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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 translated by me into English.

General comments on some items

[vi] Chetverikov (Letter No. 1) mentioned supernormal dispersion. He somehow did not know that Chuprov had all but done away with the Lexian pertinent classification (Sheynin 1990/2011, pp. 140 – 143). Then, Chetverikov extensively studied the seasonal wave but this notion seems to be abandoned.

A comment is needed on the measures to immortalise the memory of Chuprov. At the time of his death, the Soviet statistical elite considered him an alien *bourgeois statistician* (Sheynin 1990/2011, pp. 159 – 160). Only one obituary appeared in the USSR. Compiled by his close colleague it was hastily written and only appeared in a Leningrad newspaper.

Not so abroad! Several Russian statisticians published obituaries outside Russia. His portrait appeared in *Biometrika* (vol. 18, No. 3 – 4, 1926). Previously, only the portraits of Chebyshev and Markov were published there. Then, the Royal Statistical Society passed a *Resolution of Condolence* (Sheynin 1990/2011, p. 156) which was certainly published abroad.

See other materials in Chuprov (2009). Contained there are **1.** The text of the planned memorial collection (never published). **2.** A list of posthumous manuscripts kept by P. S. Prokopovich (Prague). **3.** Letters to Kohn from Anderson and Chetverikov. Anderson called Chetverikov *the closest and the most devoted* to Chuprov among his students. He also called Chetverikov the *most knowledgeable*, but obviously such was he himself.

And now Letter No. 8. There, Chetverikov mentions some of Chuprov's unpublished materials which apparently arrived in Moscow from Leningrad. Among them most interesting seem Chuprov's *two thick notebooks* with notes about the theory of probability. Did he hear lectures on that theory? Who read them? And where are these notebooks? In 1989 I studied the fund of Chuprov and his father in the section of rare books of Gorky Library, Moscow State University, and compiled a booklet (1990), but had not seen any notebooks. They were lost [v, Bibliography of Chuprov, section on lost materials].

[viii] Being disillusioned with the Soviet regime, Kolman managed to leave the USSR in 1976. Until the end of his life in January 1979,

he lived in Sweden and compiled the manuscript of the posthumous book of 1982. He (p. 231) had been ending that compilation in November 1978 and possibly had time to send his manuscript to New York. Anyway, he could have arranged almost everything in good time.

Kolman provided useful and little known information about Sholokhov (p. 158), Lysenko (p. 213) and Bertrand Russel (p. 250), and he (p. 201) stated that *Stalin was congeneric with Hitler*.

Kolman (p. 184) clearly stated that Stalin had killed Kirov. Someone compiled a rhymed Russian verse:

Small cucumbers and tomatoes//Stalin killed Kirov in a corridor.

(*Ogurchiki, pomidorchiki//Stalin Kirova ubil v koridorchike*)

He was killed (in a corridor) by a mentally disturbed man, but there is a strong suspicion that the *organs* had known about the impeding act and actually assisted that man. (The same was stated about the assassination of President Lincoln).

I have commented on Kolman's (p. 329) opinion about Israel.

Kolman (pp. 300 – 301) religiously trusted Stalin. Read: trusted that the extermination of millions was needed.

Here, however, is the main point. Kolman (p. 266) *actually* states that revolutions ought to be avoided in principle and he (p. 122) remarked that the October revolution (more precisely, a coup d'état) led to a dictatorship of *a vicious and criminal man*. On the other hand, he (p. 263) declares that that revolution was needed and even *opened a new era in the history of mankind*!

[ix] Lozovoy mentioned a few books so that his subtitle is misleading; moreover, he had not listed the editions of his main target. Below, I adduce some additional bibliographic information.

I have not seen the books that he reviewed but it is safe to state that their authors and editors were not stupid as Lozovoy would have it. Then, they had to toe the Bolshevist line and comply with the situation of the day which explains much about Lozovoy's attacks.

As far as statistics was concerned, Lozovoy was a non-entity; in my Notes, I have mentioned only a few of his unbelievable mistakes and passed over his amateurish astronomical example. Lozovoy's ignorance likely explains why the alleged culprits were not persecuted: it was absolutely impossible to reveal the horrible choice of that ignoramus by the leading Party theoretical periodical, the *Bolshevik*.

Lozovoy makes it abundantly clear that **1.** Instead of the theory of statistics the reviewed books had to be largely concerned with economic statistics (at the end of his paper Lozovoy even complains that the main reviewed book contained too much mathematics). It follows once again that at least to the end of the 1950s Soviet statistics had been restricted to social statistics. **2.** Consequently, Soviet statisticians hardly mastered contemporary statistics. Actually, Lozovoy said noting about the essence of the reviewed textbooks.

I

V. S. Kirsanov

Newton and his epoch

Voprosy istorii estestvoznania i tekhniki, No, 1, 1993, pp. 16 – 18

Newton was born on the year Galileo died. His work is the culmination of the scientific revolution of the 17th century and his life covers a whole epoch with many events, and in the first place, the bourgeois revolution in England in 1640 – 1660. Yes, the epoch was extremely stormy: the old monarchies crumbled and new states emerged. However, on the outside Newton's life remained in a quite normal routine and pretty calm way.

Newton outlived six kings, a civil war, Cromwell's protectorate, restoration of the Stuarts and a change of a dynasty. But all that barely reflected on his fate. He never married, never left England and hardly had any students. Nevertheless, his creative life had not at all been less tense and just as rich in events as his epoch.

At the end of the 17th century Newton's name embodied all but the science itself. His glory as the creator of modern mechanics which laid the foundation of a scientific picture of the world was all-embracing and unparalleled. Neither the isolation of the island native land of that great Englishman, nor the domination of the Cartesian physics over continental Europe, nor the disgracefulness of the priority strife with Hooke and Leibniz, nothing was able to shake his authority.

More than three hundred years ago, in 1687, he published one of the most remarkable works in the history of culture, his *Mathematical Principles of Natural Philosophy* (*Principia*). It contained the fundament of the entire new science and signified the most important methodological transition from likely speculations to a quantitative theory and precise experimentation.

Today, after these three hundred years, we can hardly imagine that psychological and intellectual explosion which *Principia* had generated. For approaching at least marginally an adequate understanding of that fact we should recall the state of natural science before Newton. He himself, when appraising his contribution to science, said that he was able to achieve so much and look so far ahead since he stood on the shoulders of giants.

Newton's statement was not original, and it is even possible that his phrase was only a formula of politeness: he was aware of himself better than anyone else. However, that phrase certainly contained some truth although the majority of his results was his own and independent discoveries.

Only now, at the end of the 20th century, after gigantic work had been accomplished by those the world over who had studied his contributions, after the publication of many volumes of his correspondence and previously unknown writings, it becomes clear how inadequate were the previous judgements about him, about his work and ideas to say nothing about obvious mistakes of his biographers.

This is not surprising since Newton's genius is such a complicated phenomenon: it is contradictory, not confined in any boundaries so that a really unrestricted region of work is awaiting future students. Even a simple listing of some facts of his biography astonishes us by the appearing problems. How could a young man without any definite inclinations to exact sciences, who enters a university being almost ignorant of mathematics; furthermore, who has no time for reading Euclid even by graduation, how was he able, in a few years after that, to make such an epochal discovery, the invention of the new analysis?

Why Newton, a son of extremely rich parents[1], was compelled to reconcile himself to being a subsider[2], a social outcast in a Cambridge society? How Barrow (who, incidentally, never was his teacher) who first met him at the final examination (in which Newton, in his own words, answered the questions in the worst way), could have recommended Trinity College to retain Newton? Why, after discovering in 1666 the law of reciprocal squares, he had been delaying for twenty years the publication of the law of universal gravitation[3]? Why the proofs in the *Principia* were explicated by the synthetic geometric method rather than by the new analysis, which would have considerably facilitated both the understanding of that book and its future application? It is difficult to answer these questions although they constitute only a little bit of the problems which are left for the researchers of his life and work.

And now, the giants. At the mid-17th century the European science found itself in a peculiar situation. The work of Kepler and Galileo delivered a shattering blow to the Aristotelian ideas, but those ideas still prevailed in university education. However, Kepler's search for universal laws which govern the universe (of his cherished *harmonices mundi*) proved unsuccessful although he himself, the first after Copernicus, essentially facilitated the development and establishment of the heliocentric model of the universe [of the Solar system]. He postulated the ellipticity of the planetary orbits and ascertained the now generally known three laws which reflect the main regularities of celestial kinematics[4].

Galileo's achievements were also restricted to kinematic regularities, whereas his attempts to approach dynamically the problems of physics were based on qualitative and sometimes wrong considerations.

The merits of Kepler and Galileo are certainly not reduced to the above, but it is important to stress here that Galileo was the first to introduce into science the method of thought experiments in their modern understanding, i. e., of introducing experiments made under ideal conditions which yield to mathematical description and the correlation of that ideal world (of *il mondo di carta*, as he expressed it) with the world of physical reality.

Be that as it may, after the *Astronomia nova*, *Harmonices mundi*, *Dialogos* and *Discorso*[5], the Aristotelian physics as well as the entire picture of the world which he portrayed, ceased to exist. But, to repeat, neither Kepler, nor Galileo was able to construct a new world which would have been adequate for the new approach to the explanation of nature. Galileo was not even inclined to formulate such a problem for himself. Quite in the spirit of the methodology of contemporary Italian academies, he directed his efforts to the solution of particular problems. Not without reason he wrote in one of his letters:

I prefer to discover the truth even in insignificant matters than to debate for a long time about greatest issues without attaining any truth.

The overthrow of the Aristotelianism led to a gap which was remarkably filled by Descartes. He created an extremely attractive mechanistic picture of the universe which took into account the newest advances of science. It had been rapidly becoming the leading doctrine, which in its orthodoxy was not inferior to the Aristotelian teaching[6]. However, in spite of all its attractiveness the Cartesian methodology of physics which centred on mathematics, the Descartes' model of the universe was mostly qualitative, almost lacking calculations to say nothing about him constructing a mathematical theory to confirm or describe his fundamental statements.

At the same time it is paradoxical that it was Descartes who made a new essential step to foster mathematics: he created the methods of analytic geometry which revolutionarily influenced the entire further development of science and soon became the main source of Newton's interest in mathematics.

Finally, we ought to mention Huygens, a senior contemporary of Newton, among those giants whose contributions led to the appearance of the new science. He seems to be the last scientist of the pre-Newton epoch who obtained new results by previous methods. In this sense there was no one like him in mathematics.

In general, the history of science is essentially the history of new solutions of previous problems. In physics, such were the problem of the fall of bodies, of the flight of shells, of collision, the problem about the essence of gravitation, of light, about the existence of vacuum, the essence of substance etc. Even from antiquity, these

problems constitute the main object of scientific inquiries, and many of them continue to interest modern science.

Huygens was the first to solve the problem of collisions, to formulate the wave theory of light and to obtain a number of most essential mathematical relations in physics (in the first place, the formula of the centrifugal force).

And so, let us summarise. What was achieved before Newton, and what he had to do? The Aristotelian understanding of a hierarchical space with its dichotomy of motions had been done away with and was replaced by the Copernican idea of a heliocentric universe [Solar system] and an isotropic Euclidean space. Galileo proved that the physical laws are universal and can be mathematically written down. The first such law was the law of the fall of bodies. It stated that the distance of fall is proportional to the square of the passed time.

Kepler discovered the mathematical relations which describe planetary motion. Descartes (and Galileo before him but with some reservations) formulated the principle of inertia: rest and uniform linear motion have the same ontological status (none of them needs to be justified). Finally, Descartes attempted to picture the world. He based himself on the idea of a universe entirely filled by matter (extent cannot be distinguished from matter!) in which all the processes and phenomena are conditioned by collisions of the particles of matter. Some ideas about the laws of conservation were also advanced (Descartes, Huygens).

Nevertheless, the programme for which even Galileo had stood up and which Descartes unsuccessfully attempted to realise, namely, the mathematical description of the book of nature, was not fulfilled. A new mathematics and a new science, dynamics, had to be created for its realisation. The solution of these problems fell to Newton.

Notes

1. Newton was a posthumous child. He was born after his father's death.

2. In modern English, that word denotes a donor.

3. Newton wished to confirm his law by Flamsteed's observations. Flamsteed, however, never hurried to publish them and impeded Newton (Sheynin 1973, p. 109). And still, after those twenty years (C. Truesdell, letter of 1992):

Newton did fudge, make errors, use wrong data etc.

Quod licet Jovi, non licet bovi!

Rosenberger (1895, pp. 183 – 184) was the first to note that fudging.

4. Kepler attempted to fit a closed curve to the Tychonian observations and accomplished an enormous amount of calculations. This indeed was ascertaining, but hardly postulating.

5. Galileo published several *Discourses* on various objects.

6. *Orthodox* certainly meant *generally accepted*. The author used quite a few philosophical terms barely suited for non-philosophically minded readers.

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II

Oscar Sheynin

On the history of the De Moivre – Laplace limit theorems

Istoria i metodologia estestvennykh nauk, No. 9, 1970, pp. 199 – 211

I consider the relations between the results of Jakob Bernoulli and De Moivre and the appearance of the normal law in the latter's work. This subject was certainly studied previously, but my explication is more detailed and some of my conclusions are original, see also Sheynin (1968; 1971).

The *Ars Conjectandi* (AC) of Jakob Bernoulli essentially influenced the development of the theory of probability and was always considered classical. I dwell somewhat on the AC, mostly in connection with Karl Pearson's [1] extremely negative and downright wrong opinion about its fourth part, i. e., about Bernoulli's law of large numbers, as Poisson named it.

I briefly describe this law. Given, a binomial $(r + s)^{nt}$, $t = r + s$, r and s are natural numbers and n is a large natural number. If nt is sufficiently large, the sum of $2n$ middle terms of the expansion of that binomial, even excluding its middlemost term, becomes an arbitrary given number c times larger than the sum of the other terms.

This algebraic fact is applied for a stochastic reasoning. Let $r/(r + s)$ be the constant probability of success in a trial, and nt, the number of these (independent) trials. Then, if nt is sufficiently large, the probability that the number of successes is restricted by the interval $n(r \pm 1)$ can be made higher by more than c times (c is arbitrary) than the probability of the opposite event. In other words, J. B. proved that

$$\lim P(|\frac{\mu}{n} - p| < \varepsilon) = 1, \quad n \to \infty. \qquad (1)$$

Here, μ is the number of the occurrences of the studied event (of the successes) in n independent trials and p is the constant probability of success in each trial. A detailed description is in [2] and [1]. Then, Bernoulli inverts his problem and states (but does not prove) that, if after a series of trials a posterior probability of success in a trial is $p = r/(r + s)$, the probability that the true value of p is contained in the interval $p \pm 1/(r + s)$ can also be made c times higher than the probability of the opposite event. J. B. also provides a less known estimate: for $r = 30$ and $s = 20$, which means that $t = r + s = 50$, $1/(r + s) = 0.02$, and it occurs that for $c = 1000$, $nt = 25{,}500$; for

c = 10,000, nt = 31,258 etc. The increase in nt by 5758 leads to a ten-fold increase in c:

$$nt = 25{,}500 + 5758 \lg(c/1000) = 8226 + 5758 \lg c, \qquad (2)$$

$$c = 10^{(nt - 8226)/5758}. \qquad (2')$$

It is not difficult to replace 10 by e and we may certainly say that J. B. understood that his numerical estimate led to a logarithmic and an exponential function. It follows that he had actually introduced a prototype of the density

$$\varphi(x) = (m/2)\exp(-m|x|),\ m > 0$$

which appeared in Laplace's early memoir [3].

Again, neither (2) nor (2') are density functions, they only establish a determinate relation between nt and c which means however that Bernoulli's law was a prototype of a local limit theorem.

In 1913, the bicentenary of the AC, Markov edited a Russian translation of the fourth part of the AC [4]. That same year Markov published the third edition of his *Calculus of Probability* [2] which he called a jubilee edition and illustrated it by Bernoulli's portrait. Finally, again in 1913, the Petersburg Academy of Sciences devoted a special sitting to the AC and heard out the reports of Markov, Vasiliev[1] and Chuprov.

However, only in the posthumous edition of his *Calculus of Probability* [2] Markov improved the numerical estimate of Jakob Bernoulli and replaced 25,500 by 16,655. The main improvement (to 17,324) was the result of specifying Bernoulli's intermediate inequalities. Markov had not applied here the Stirling formula, apparently since J. B. could not have known it. (More precisely, Markov applied it in a special investigation, see his p. 55ff.)

Markov achieved an additional improvement by rejecting the divisibility of the binomial's exponent by $r + s = t$.

At about the same time Pearson [1] applied the Stirling formula and attained a practically precise coincidence of the obtained estimate with that which used the normal approximation to the binomial law and pronounced an utterly wrong verdict (p. 202):

He [Bernoulli] *gets most exaggerated values for the needful number of observations and for this reason his solution must be said to be from the practical standpoint a failure. It would ruin either an insurance society or its clients, if it were adopted. All Bernoulli achieved was to show that by increasing the number of observations*

the results would undoubtedly fall within certain limits, but he failed entirely to determine what the adequate number of observations were for such limits. That was entirely De Moivre's discovery.

And on p. 210:

After all, I think we must conclude that it is somewhat a perversion of historical facts to call the method […] *by the name of the man who after twenty years of consideration had not got further than the crude values* […] *with their* 200 *to* 300 *per cent excesses. Bernoulli saw the importance of a certain problem, so did Ptolemy, but it would be rather absurd to call Kepler's or Newton's solution of planetary motion by Ptolemy's name*! *Yet an error of like magnitude seems to be made when De Moivre's method is discussed without reference to its author, under the heading of* "*Bernoulli's theorem*". *The contributions of the Bernoullis to mathematical science are considerable, but they have been in more than one instance greatly exaggerated* [?]. *The Pars Quarta of the Ars* […] *has not the importance which has often been attributed to it.*

It hardly makes sense to stress the practical uselessness of the Bernoulli estimate, and especially to disregard his ignorance of the yet unknown Stirling formula and to compare inadmissibly his result with the wrong Ptolemaic system of the world. On the contrary, it was necessary to point out the very existence of Bernoulli's estimate and of his existence theorem (1), and the great importance of his law of large numbers for the entire development of the theory of probability at least until Laplace and Poisson.

Pearson [5] also remarked that J. B. had not introduced any measure of precision of the type $1/\sqrt{n}$, but why should we require so much from a very early scholar?

After Bernoulli's death but before the publication of the AC Nicholas Bernoulli, in a letter to Montmort of 23 Jan. 1713 [6], derived an approximate formula for estimating the ratio of the middle part of the binomial series to its other parts and applied his formula for stochastic inferences about the sex ratio at birth. His conclusion was an almost obvious corollary of the proof itself of the law of large numbers in the J. B. form: the probability of the number of yearly male births (m) is contained in the interval $7200 \pm l$,

$$7200 = \frac{14,000}{18+17}18,\ m:f = 18:17.$$

Here f is the number of yearly female births and $14{,}000 = n$ is the yearly number of all the births.

N. B. calculated the ratio of the terms $(fr + 1)$ and $(fr - l + 1)$, $r = n/(m + f)$, of the binomial $(m + f)^n$ and got

$$\alpha = \frac{u_{fr+1}}{u_{fr-l+1}} = \frac{mr+1}{fr-l+1}\frac{mr+l-1}{fr-l+2}\ldots\frac{mr+1}{fr}(f/m)^l. \qquad (3)$$

Then he assumed that the consecutive fractions in (3) constitute a geometric progression. If all the magnitudes there are constant, N. Bernoulli's estimate is precise to terms of $O(1/p^2)$, $p = m/(m + f)$. If $l = \sqrt{n}/2 \approx 60$ (De Moivre, see below, especially noted this value), the ratio of that progression will be $q = f/(m + f) \approx 1 + 1/3500$, and the error of the sum of the progression will be

$$(1 + 2q + 3q^2 + \ldots + lq^{l-1})O(1/p^2).$$

This series diverges if $l \to \infty$ and, for a finite l and given p the error increases as l^2. Therefore,

$$\ln\alpha = \frac{l}{2}[\ln\frac{mr+l}{fr-l+1} + \ln\frac{mr+1}{mr} + \ln\frac{fr}{mr}],$$

$$\alpha = [\frac{(mr+l)(mr+1)f}{(fr-l+1)mr\cdot m}]^{l/2}.$$

Then Nicholas applies the obtained ratio α as a *scale*: he notes that the ratio of the terms fr and $fr - l$ is larger than α, of the terms $fr - 1$ and $fr - l - 1$ is still larger etc. He separates the first part of the binomial series into *classes* with l terms in each (the first class, from the term fr on the right until the term $fr - l$ on the left, the second class, from the term $fr - l - 1$ on the right to term $fr - 2l - 1$ on the left etc.) and notes that the ratio of the sum of the terms of the second class to the sum of the terms of the first class is less than α, of the sum of the terms of the third class to the sum of the terms of that same first class is less than α^2 etc. and that the sum of the terms of all the classes except the first class is less than $\sigma/(\alpha - 1)$, where σ is the sum of the terms of the first class and it is assumed that $l \to 0$. So it follows that for small values of l the ratio of the sum of the terms of the first class to the sum of all the other terms of the series from u_1 to u_{fr-l-1} is less than $\alpha - 1$.

Similarly, when considering the ratio

$$\beta = \frac{u_{fr+1}}{u_{fr+l+1}} = [\frac{(fr+l)(fr+1)m}{(mr-l+1)fr\cdot f}]^{l/2},$$

N. B. gets the final answer: the sum of the terms of the series from

u_{fr-l+1} to u_{fr+l+1} inclusive even without the maximal term u_{fr+1} is not less than $(t-1)$ times larger than the sum of the other parts of the series where $t = \min(\alpha, \beta)$.

Here is the stochastic essence of this statement (the author's own example):

$$\frac{P(|\mu - rm| \le l)}{P(|\mu - rm| > l)} = t - 1, \; P(|\mu - rm| \le l) = \frac{t-1}{t}.$$

If, as previously, l is of the order of $\sqrt{n}$, then

$$t \approx [\frac{(mr+l)}{(fr-l)}\frac{f}{m}]^{l/2} \approx [1+\frac{l(m+f)}{mfr}]^{l/2} \approx \exp(-\frac{l^2(m+f)^2}{2mfn},$$

$$P(|\mu - rm| \le l) \approx 1 - \exp(-\frac{l^2}{2pqn}). \qquad (4)$$

An exponential function of a negative square has thus first appeared, although in an indirect way and the reasoning itself, just like in the case of Jakob Bernoulli, was a prototype of a local limit theorem. Note that $pqn = \mathrm{var}\mu$, and that formula (4), multiplied by $\sqrt{2/\pi} \approx 0.80$, could have been applied for calculations according to that theorem.

Now, De Moivre (1667 – 1754). See general information about him in [13; 14; 9; 15; 16] (the first two sources are the most important). French by nationality and a Huguenot by religion, he had to leave France after the revocation of the Edict of Nantes (1685) and settled in London.

His mathematical education (in particular, he studied under Ozanam) proved very incomplete, but, all by himself, he managed to fill in the gaps in his knowledge, and in 1697 he was elected to the Royal Society. That Society appointed him, together with other scientists (in particular, with Arbuthnot[2]), member of a special committee for establishing the truth in the strife of Newton and Leibniz over the discovery of the calculus of infinitesimals.

Todhunter [9, § 233] testifies:

Newton himself, in the later years of his life, used to reply to inquirers respecting mathematics in these words: Go to Mr. De Moivre, he knows these things better than I do.

In the long list of men ennobled by geniuses, virtue and misfortune, who had found an asylum in England, it would be difficult to name one who had conferred more honour on his adopted country than De Moivre.

Todhunter was probably in the right, at least as far as natural scientists and mathematicians were concerned, but I ought to add that De Moivre's adopted country had not secured him, even in the slightest degree, a fitting way of life. He had to earn his living by private lessons and consultations. In 1735 De Moivre was elected to the Berlin Academy of Sciences, and, in 1754, to the Paris Academy of Sciences as a foreign member. In spite of his wish, he had no time to submit any manuscript to Paris.

Todhunter [9, § 336] also states that the theory of probability

Owes more to him than to any other mathematician with the sole exception of Laplace.

If applications are also considered, Daniel Bernoulli has to be mentioned as well. Now, Todhunter (Ibidem) lists De Moivre's merits: investigation of the duration of play, theory of recurring series and an *extension of the value of Bernoulli's theorem by the aid of the Stirling formula*. The last-mentioned investigation included the introduction of the normal distribution.

De Moivre's main contributions which interest me are: **1.** *The Doctrine of Chances* [18]. It appeared in 1718 (I had not seen this edition), 1738 and, posthumously, in 1756. It was a great extension of his paper *De mensura sortis* of 1711, translated into English in 1984. **2.** *Miscellanea analytica* … [1730, French translation 2009] with two apparently later bound Supplements. Pearson [5; 19] noted that not all copies of the *Misc. anal*. have the first Supplement and only a few have the second Supplement dated 1733. **3.** This second Supplement, just as the *Misc. anal*., was written in Latin, translated into English by De Moivre himself and included in the second edition of the *Doctrine*. Its extended version occupies pp. 243 – 254 in the third edition of that *Doctrine*. It is preceded there by an explanation (p. 242):

I shall here translate a paper of mine which was printed Nov. 12, 1733, and communicated to some friends, but never yet made public.

It is therefore likely that that second Supplement was bound to the copies of the *Misc. anal*., not yet sold by 1733. Here is the title of that paper in translation:

A method of approximating the sum of the terms of the binomial $(a + b)^n$ *expanded into a series from whence are deduced some practical rules to estimate the degree of assent which is to be given to experiments*.

The first (after the *Misc. anal.*) publications of the original Latin text of that paper were due to Archibald [20], see also his note [21], and Pearson [5], but De Morgan in 1864 and then Eggenberger [23] (cited by Czuber [24]) were the first to note the appeared normal distribution.

Eggenberger described in detail Bernoulli's result (prior to Haussner, see [25]). On p. 158 Eggenberger called the function $\exp(-2x^2/n)$ by

De Moivre's name, attributed to him the first appearance of the calculus of infinitesimals in probability (although only in life insurance) and of a curve of distribution (see however Huygens's letter of 1669 below). Finally, Eggenberger indicated that De Moivre

Had in essence provided the Laplacean analysis of the Bernoulli theorem. He derived the approximate value of the binomial coefficients and $\Gamma(x)$ [more precisely, $n!$] *and obtained the Laplacean integral*.

Now, De Moivre's religious views. They are best seen in the last edition of the *Doctrine*, in the *Approximation*. (p. 253). He agrees that *chance* and *probability* can certainly be studied, for example in games of chance, but

Chance in atheistic writing or discourse is a sound utterly insignificant … It can neither be defined nor understood …

However, if Arbuthnot decided that the prevalence of male births among the recently born was the result of Providence, an atheist will explain it by a (statistical) law of nature.

A similar vigorous statement was due to another member of the Royal Society, the publisher of Hooke's manuscripts and a clergyman, Derham (1657 – 1735) [26, p. 313]:

Should we be so besotted by the devil and blinded by our lusts, to attribute one of the best contrived [by God] *pieces of workmanship* [man] *to blind chance, or unguided matter and motion, or any such sottish, wretched, atheistic stuff*?

This statement was possibly known to De Moivre. In 1714 Derham [10, vol. 2, p. 520] asked Newton to fulfil his promise and send his *castigations* about one of the earlier editions of his, Derham's, *Physico-Theology*.

De Moivre himself [18, 1756, p. 251] stated that

Altho' chance produces irregularities, still the odds will be infinitely great that in the process of time these irregularities will bear no proportion to the recurrency of that order which naturally results from Original Design.

And here is Pearson's comment [19, p. 552]:

The causes which led De Moivre to his Approximatio or Bayes to his theorem were more theological and sociological than purely mathematical, and until one recognises that the post-Newtonian English mathematicians were more influenced by Newton's theology than by his mathematics, the history of science in the 18th century, in particular that of scientists who were members of the Royal Society, must remain obscure.

And now the Dedication of the first edition of the *Doctrine* to Newton [18, 1756, p. 329]:

To Sir Isaac Newton, Kt. President of the Royal Society

Sir, the great help I have received in writing upon this subject having been from your incomparable works, especially your method of series; I think it my duty publicly to acknowledge that the improvements I have made in the matter here treated of, are principally derived from yourself. The great benefit which has accrued to me in this respect requires my share in the general tribute of thanks due to you from the learned world.

But one advantage which is more particularly my own is the honour I have frequently had of being admitted to your private conversation wherein the doubts I have had upon any subject relating to mathematics have been resolved by you with the greatest humanity and condescension. Those marks of your favour are the more valuable to me because I had no other pretence to them but the earnest desire of understanding your sublime and universally useful speculations.

I should think myself very happy, if having given my readers a method of calculating the effects of chance as they are the result of play and thereby fixing certain rules for estimating how far some sort of events may be owing to design than chance, I could by this small essay excite in others a desire of prosecuting these studies and of learning from your philosophy how to collect by a just calculation the evidence of exquisite wisdom and design which appear in the phenomena of nature throughout the universe. I am, with the utmost respect,
Sir, your most humble and obedient servant A. de Moivre.

Note that the main goal of De Moivre's theory of probability was the separation of the necessary and the random without specification of randomness.

Pearson's opinion is thus confirmed, at least with respect to De Moivre. However, there was an exception: another fellow of the Royal Society, Thomas Simpson, who never mentioned anything connected with religion. David [27, p. 36] scornfully mentioned him and played down his achievements[3]. However, he studied life insurance (Hald 1990, pp. 515 – 546), applied generating functions and actually introduced random variables (§ 6.1.3) and introduced a problem which directly bore on the future statistical control of quality (Sheynin 2017, § 10.4-6). But the relations between De Moivre and Simpson became terrible and Pearson (1978) called the latter *a most disreputable character* (p. 145) and *an unblushing liar and a thorough knave at heart* (p. 184).

Both Lagrange and Laplace thought about translating the *Doctrine* into French, see Lagrange's letter to Laplace of 30 Dec. 1776 [28, p. 66]. This fact once more stresses the importance of the *Doctrine*, but what did each of the two French giants (and especially the atheistic Laplace) think about De Moivre's philosophical ideas?

To end this subject, I describe a telling episode [13, p. 184] as quoted in English by Walker [15, 1756, p. 363]:

To a man who, apparently intending to pay him a compliment, remarked that mathematicians had no religion, he replied: I will prove that I am a Christian by forgiving you the insult you are offering.

Book 5 of the *Misc. anal*. is called *Binomial* (*a* + *b*) *raised to a large power*. Here De Moivre quotes a long passage from Jakob Bernoulli, describes the letter of Nicholas Bernoulli to Montmort (see above) and solves two problems about the expected gain in a game of chance as well as two algebraic problems which he later applied in the *Approximation*. Concerning Nicholas Bernoulli De Moivre [17, p. 98] remarked that he

Had not studied the probability for the probability of the number of successes and failures to be contained in definite boundaries.

Indeed, N. B. solved another problem.

The mentioned stochastic problems were included in the *Doctrine* [18, 1756, Problems 72 and 73]:

A and B playing together and having an equal number of chances to win one game (Problem 73: *the chances are as a:b*) *engage to a spectator S that after an even number of games n is over, the winner shall give him as many pieces as he wins games over and above half the number of games played* (*A shall give him … over and above* [*a*/(*a* + *b*)]*n and* [or] *B … over and above* [*b*/(*a* + *b*)]*n*). *… How the expectation of S is to be determined*.

I reprinted the texts of these problems since De Moivre considered them as transitions from the previous text to the *Approximation*. The Contents of the 1738 edition (no Contents were prefixed to the edition of 1756) characterized these problems as inclinations to the establishment of the degree of accord which should be attached to experiments, whereas the *Approximation* was modestly described as the same, continued.

Problem 73 was followed by a Corollary which stated that the ratio of the probabilities of success and failure in a trial is very near to the same ratio after the number of the trials is large, and the nearer the larger is that number. But De Moivre also noted that, even when the number of trials was large, deviations from the expected result can happen. He (p. 242) concludes that the study of these deviations is *the hardest problem that can be proposed on the subject of chance*, and that this problem is treated in the *Approximation*.

The texts themselves of Problems 72 and 73 were not really needed, they only served as examples for posing that *hardest problem*. Note the clear formulation of the inverse problem, the determination of a probability given observations. This problem is certainly present in the AC, but Jakob Bernoulli had not mentioned the transition from the

direct to the inverse problem. And De Moivre, when considering *the hardest problem*, actually returns once more to the direct problem, see below.

Just as Part 4 of the AC, the *Approximation* consists of an algebraic and a stochastic part[4]. In the former he (p. 243) notes:

It is now a dozen years or more since I had found what follows.

Thus, he thought about the *Misc. anal*. He writes out the ratio of the middle term of the binomial $(1 + 1)^n$ to the sum of all of its terms and the logarithm of the ratio of that middle term to the term removed from it by *l*, obtains the sought ratio of an arbitrary term to the sum of all the terms of the expansion, and finally determines by integration the ratio of the sum of the terms situated between the middle term and the term removed from it by an arbitrary distance *l* to the sum of all the terms of the expansion.

1. The ratio of the middle term of the binomial to 2^n is

$$\frac{2A(n-1)^n}{n^n\sqrt{n-1}} = \frac{2A(1-1/n)^n}{\sqrt{n-1}}, \qquad (5)$$

$$\ln A = \frac{1}{12} - \frac{1}{360} + \frac{1}{1260} - \frac{1}{1680} + \ldots, \quad \frac{1}{12} = B_1\frac{1}{1\cdot 2}, \quad -\frac{1}{360} = B_2\frac{1}{3\cdot 4}, \ldots$$

B_i are Bernoulli numbers. Denoting

$$\ln B = 1 - \ln A,$$

De Moivre rewrites the ratio (5) as

$$\frac{2}{B\sqrt{n-1}} \approx \frac{2}{B\sqrt{n}}$$

and, referring to Stirling indicates that $B = \sqrt{2\pi}$.

De Moivre had calculated the ratio (5) in Book 5 of the *Misc. anal*. There, he wrote out the fraction

$$C_{2m}^m = \frac{(m+1)(m+2)\ldots 2m}{m(m-1)(m-2)\ldots 1},$$

developed the logarithms of the ratios $(m + 1)/(m - 1)$, $(m + 2)/(m - 2)$, … into series and summed the obtained series term-wise (all the first terms, all the second terms, …). He applied the Bernoulli numbers and, as it seems, experienced no difficulties at all.

He calculated the constant A ($= e/\sqrt{2\pi} = 1.08444$) in the first Supplement to the *Misc. anal*.

Also in that Supplement De Moivre derived an approximate formula for $n!$ independently from and at the same time as Stirling who only communicated to De Moivre the value of the constant, see above. Some commentators [2; 5] justly decided that the Stirling formula should be named after them both. In addition, in that same Supplement De Moivre published a table of $lgn!$, $n = 10(10)900$, reprinted [18, 1756, p. 333] which increases his weight as compared with Stirling. A comparison with a modern table [29] shows that eleven or twelve decimals were correct although a misprint appeared in lg380!.

2. The logarithm of the ratio of the middle term of the binomial series to the term removed from it by l is

$$(m + l - 1/2)\ln(m + l - 1) + (m - l + 1/2)\ln(m - l + 1) - 2m\ln m + \ln[(m + l)/m], \qquad (6)$$

where $m = n/2$. If $m \to \infty$ this expression is equivalent to $-2l^2/n$ and the ratio itself is therefore equivalent to

$$1 - \frac{2l^2}{n} + \frac{4l^4}{2n^2} - \ldots \qquad (7)$$

Actually, as confirmed by De Moivre's further calculations, he bore in mind the inverse ratio of the removed to the middle term.

De Moivre also calculated expression (6), although not its equivalent form in Book 5 of the *Misc. anal*.

3. The ratio of the sum of terms situated between the middle term and the term removed from it by an arbitrary distance l, to 2^n is

$$\frac{2}{\sqrt{2\pi n}}\left[l - \frac{2l^3}{1\cdot 3n} + \frac{4l^5}{2\cdot 5n^2} - \ldots\right]. \qquad (8)$$

Here, the sum in the brackets was obtained by integrating the series (7).

In the stochastic part of the *Approximation* De Moivre applies either series (8), after calculating its sum in case of small l (less than $l = \sqrt{n}/2$), or, otherwise, by integrating the function $\exp(-2l^2/n)$ by Simpson's approximate formula of numerical integration (the *three eight rule*).

Pearson correctly remarked that De Moivre was applying here the integral of probability just like it is done nowadays. And De Moivre

(Corollary 6) fully recognized the value $\sqrt{n}$ as the measure of precision of the observations. He noted that $p = 1/2$ is attained when $l \approx \sqrt{2n}/4$ (Corollary 5) and that his formula (i. e., the normal distribution) ensures a good approximation even at $n = 100$, see also below. However, a decrease of this value of n was quite possible.

Nevertheless, the initial appearance of $\sqrt{n}$ in the *Approximation* was due to an algebraic fact: as mentioned above, $l = \sqrt{n}/2$ was the bordering value for the two methods of integrating the function $\exp(-2l^2/n)$. De Moivre had not named the value $l \approx \sqrt{2n}/4$ at all or proposed it as a measure. Much later the value $p = 1/2$ became connected with the probable error.

On p. 247 De Moivre remarked that the *rule* here given will be *tolerably accurate* if $n = 900$, *nay not even* 100, *which I have had confirmed by trials*. He did not describe these trials, but the very fact of that experimental check is remarkable. Walker [15, 1756, p. 355] mistakenly stated that

De Moivre never resorted to physical experimentation. He did not weigh and measure and count to secure objective verification of his discoveries in the theory of probability. Even in his Doctrine of Chances his work is deductive, and he does not set up experimental checks on the outcome.

De Moivre's trials did not belong to natural science, but they, just like his table of lgn!, like his work on life insurance and mortality (Hald 1990, pp. 515 – 546) proved that much of his efforts was connected with induction. Furthermore, the entire *Approximation* was written exactly for ensuring objective *verification*, see Problems 72 and 73 above as well as its title.

The significance of induction in the early theory of probability can be seen in Huygens' letter of 1669 [30, p. 530]: he constructed a graph of a continuous empirical density function of mortality in different age groups.

It may be assumed that De Moivre understood his *Approximation* as a specification of Jakob Bernoulli's estimate and, for that matter, for any l rather than for its single value as his predecessor had provided. At the end of that piece De Moivre justly noted that his explication was most easily generalized on the case of $(a + b)^n$. Since this generalized binomial is mentioned in the title of the *Approximation*, we may safely conclude that he proved the local and integral limit theorems on the convergence of the binomial distribution to the normal law, this being the simplest case of the central limit theorem. Neither he, nor Laplace had yet any notion about universal convergence.

On Laplace's study of the De Moivre limit theorems see Sheynin (2017, § 7.1-3). The name *De Moivre – Laplace theorem* (perhaps not generally recognized) was due to Markov.

Notes

1. Aleksandr Vasilievich Vasiliev (1853 – 1929), professor in Kazan. Actively supported Lobachevsky's ideas, studied the history of mathematics. In 1885 published in Kazan a course on the theory of probability. Is primarily known in this branch of mathematics as Markov's correspondent.

2. John Arbuthnot (1667 – 1735), physician and mathematician, member of the Royal Society since 1704. Was friendly with the writer Swift and the poet Pope. Published a few pamphlets. The hero of one of them, John Bull, is not forgotten. His books include *An Essay on the Usefulness of Scientific Nature* (1701, reprinted in [7]) and *Tables of Ancient Coins, Weights and Measures*, 1727. The translation of a booklet of Huygens called *On the Laws of Chance* (London, 1738, fourth edition) is attributed to him. In our context, the most interesting is his paper (1712 for 1710). There, he was the first [8] to test a statistical hypothesis, although only from the viewpoint of *determinate or random*. Becoming sure of the determinate prevalence of male births he attributed it to Divine Design.

3. For his time the triangular distribution which he introduced was not primitive (as David called it) and, in addition, Simpson had introduced its continuous version.

4. De Moivre began his *Approximation* by indicating that only Jakob and Nicholas Bernoulli had studied a similar problem:

Tho' they have shown very great skill and have the praise which is due to their industry, yet some things were farther required. For what they have done is not so much an approximation as the determining very wide limits within which they demonstrated that the sum of the terms was contained.

This is what Pearson noted with regard to the former, but De Moivre never allowed himself to denigrate him.

Second thoughts. 1. Pearson stressed the religious influence of Newton and in this connection I mention Hessen [x] who studied the socio-economic roots of Newton's *Principia* but apparently did not say anything about religion. For that matter, Pearson himself had much to say about the same subject, see the title of his book (1978). **2.** Concerning the difference between the direct and inverse laws of large numbers which neither Jakob Bernoulli nor De Moivre really understood, see Sheynin (2017, § 5.2) where I dealt with the discovery of Bayes. He numerically described the loss of precision of the inverse law as compared with the direct law.

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III

A. L. Dmitriev

The Letters of M. V. Ptuhha to V. I. Bortkewicz

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The letters of the famous Russian and Ukrainian statistician, an alumnus of Petersburg University M. V. Ptukha (1884–1961) to V. I. Bortkevich (1868–1931) are published for the first time. They are stored in the library of Uppsala University and therefore have not been broadly known. The letters shed light on Ptukha as an economist and allow us to understand the academic interrelations between the two scientists. They also provide a picture of the economic science at the law faculty of Petersburg University.

Mikhail Vasilievich Ptukha is mainly known to demographers and historians of statistics because of his fundamental contributions [10; 11]. He was born in 1884 in Oster, Chernigov province, into a family of a clerk in the Oster Zemstvo board. In 1891 he began his school education in Oster and graduated in 1898. For three years he had been a *peoples' teacher* and at the same time participated as a registrar in statistical estimations which were carried out by the Chernigov Zemstvo.

In 1902 Ptukha had entered the Rostov mechanical-chemical technical school but was expelled for being a member of a social-democratic organization. Soon he was arrested, spent two months in prison, then released under police surveillance. In 1904, without attending any lessons, he passed the examinations for a school-leaving certificate. Being politically suspect, he was unable to enter a university. Only in 1906 he became a lecture goer at the law faculty of Petersburg University.

During the first years of study he ascertained that his main interest was in statistics and began to study it in earnest under Prof. I. I. Kaufman. During Kaufman's two-year absence abroad Ptukha managed the statistical room. Kaufman paid much attention to political arithmetic and inculcated Ptukha with an interest in the history of demography [12, p. 322]. In 1910 Ptukha, now a full student, graduated with a diploma of the first degree and, on Kaufman's recommendation, was left at the university at the chair of political economy and statistics to prepare himself for professorship.

Many years later Ptukha used to recall that Kaufman, a highly educated economist[1] and statistician and a *pioneer of Russian financial statistics*, paid special attention to demography. He read a special course in demographic statistics and students who participated in this work wrote down and mimeographed his lectures.

Kaufman's deep knowledge of demographic statistics and his ability to pass it to his students awakened my interest in this branch of knowledge [as quoted in 13, p. 288].

In 1911 Ptukha published his first work devoted to the aid of homeless children [7]. Then, in 1910 – 1911, he prepared himself for his master's examinations although not in his alma mater, as was then supposed, but on Kaufman's insistence, in Berlin University and under the direct guidance of Prof. Bortkevich who had left Russia in 1901. Bortkevich read lectures in theoretical statistics, and Karl Ballod, who also came from Russia, read economic statistics.

Ptukha became friendly with Bortkevich; later, he minutely informed his former teacher about the situation at the law faculty of Petersburg University, and this constitutes the significance of his letters which are adduced below. Indeed, they add curious features to the history of the economic education at that University, characterize many prominent economists and reveal the complicated relations between scientists then and there.

Upon returning to Russia, Ptukha had successfully passed his master's examinations in political economy and statistics (1912) and was confirmed as a privat-docent of the University. From 1913 he conducted the seminar on statistics. At the same time A. A. Kaufman invited him to teach statistics at the Bestuzhev courses. In the beginning of 1914 Ptukha was sent to London for two years to prepare himself for his master's dissertation. There, he persistently worked in the libraries of the British Museum, the Royal Statistical Society and the Institute of Actuaries and indeed prepared an *Essay* [8], the dissertation which he defended in 1917 in Moscow University. .

In 1916, the University established a branch in Perm, and Ptukha was sent there to prepare the education in statistics. In 1917 he was confirmed as acting ordinary professor of political economy and statistics. Then, in 1918, he was sent for a year to Kiev, and, from the autumn of that year, he taught statistics at the Peoples University cum Polytechnic School. In January 1919 Ptukha was elected director of the Demographic Institute of the Ukrainian Academy of Sciences established on Tugan-Baranovsky's initiative. He remained there until that institute was shut down in 1938[2].

During that same period Ptukha had been teaching in the Institute for National Economy and other Kiev institutes. In February 1938 he was arrested but released in January 1940 as innocent. In 1920 Ptukha was elected full academician of the Ukrainian Academy of Sciences and, in 1943, correspondent member of the Soviet Academy of Sciences.

In 1940 – 1950 Ptukha had been in charge of the statistical section at the Institute of Economics, and, from 1944, chairman of the Class of social sciences of the Ukrainian Academy of Sciences. After retiring in 1950, he continued his scientific work turning to the history of statistics. He died in 1961 in Kiev. See the biography of Bortkevich in our paper [6] but his most comprehensive scientific biography is [16].

The Bortkevich papers kept in Uppsala include 29 letters from Ptukha (1911 – 1928). Here, we only adduce twelve of them which cover the Petersburg period of his life and work (1911 – 1914). All letters are hand-written. The first and the last are undated but their dates can be approximately estimated. It was thought that the

posthumous materials of Bortkevich were lost, but G. Rauscher (Austria) discovered that most of them are kept in Uppsala (Sweden). They apparently came from Bortkevich's sister who kept house for him. J. Chipman (USA), C. Wittich (Switzerland) and O. Sheynin (Germany) found some other materials. I am very grateful to the last-mentioned for sending me the copies of the appended letters.

Letter 1 [1911]

I am here […] for two days now but am still unable to enjoy myself to my heart's content. […] I see that I was in the right. [The source is] [H.] Dietzel, *Theoretische Sozialökonomie*, Bde 1 – 2. [Leipzig, 1895].

Letter 2, 23.3.1911

[…] I am confirmed as a person left at the University although without a stipend. I will receive the leaving certificate and am going to Petersburg. I came to your place, wished to thank you for your kind and warm attitude which you had invariably manifested. I terribly regret that I returned you the first part of your contribution (1894 – 1896) spilled with ink even if I myself was not guilty. I hope that you will remember me although not because of that spoiled copy of your work. Allow me to thank you once more for your kindness the like of which I had rarely come across.

Letter 3. Petersburg, 25.4/8.5.1912

The Minister of peoples education suggested that three chairs of the law faculty should be filled by recommendation, and one of them is the chair of political economy[3]. So I happened to hear such talks and rumours which are apparently interesting for you. At a sitting of 9 April, old style, the law faculty elected Prof. I. I. Kaufman and Georgievsky to nominate candidatures for filling the vacant chair of political economy. For five years and now also I. I. upholds the candidature of Tugan-Baranovsky. On the contrary, Georgievsky had suggested that Prof. Manuylov nominates himself, but the latter categorically refused.

Therefore, as it is rumoured, without asking your opinion, he nominated you. The Ministry of peoples education requires information about candidates having Russian diplomas, and Georgievsky therefore suggested that the faculty confer a doctor *honoris causa* on you. The members of the faculty very highly estimate your scientific merit but still think that we should not connect the two problems, the conferment of a doctor's degree as though ad hoc, and the filling of the chair. Prof. Kaufman agrees with that opinion but he will uphold with all his heart and soul the conferment of the degree which you have deserved more than anyone else.

The [university] Statutes require rather complicated formalities for conferring a scientific degree and quite a lot of time is needed to allow for all the circumstances from which depends the success of the business (2/3 of the council's vote, troubles with the ministry etc.). At present, much depends on what response you will send to Georgievsky and, in general, on whether you wish to be nominated as

a candidate under such a situation. All these questions ought to be resolved on the 11th and 13th May, new style, since the Minister had determined a short space of time.

My examination in political economy is set on that same 13th May, but it is difficult to say whether it will take place.

Letter 4 [25.4.1912]

I hasten to inform you about all that I know or heard recently about the filling of the chair of political economy. Tugan-Baranovsky was elected by 13 votes against 1 (Georgievsky). You were not balloted *on formal grounds*, because you still do not have a Russian degree of Doctor of political economy and statistics. Only Tugan-Baranovsky was balloted. This concluding information comes from Migulin since Kaufman only spoke a bit about that with me.

Georgievsky put forth very long objections to Tugan. It is said that they contained *a complete denunciation and an absolute distortion of his scientific activity*: *a socialist, a landowner, corruption of young men, etc*. flash on each page of that *objection*. The discussion dragged on for a very long time.

Kaufman read out his presentation but had not answered Georgievsky's political attack. Kovalevsky and partly Migulin took this aim upon themselves. The latter said that, *without touching Tugan-Baranovsky's political face, Georgievsky's testimony allows us to conclude that there is no candidate in Russia worthy of being opposed to Tugan-Baranovsky. There is only a professor of Berlin University, but he still does not have a Russian scientific degree and, in addition, he does not want to be balloted*.

From the end of this phrase I perceive that Georgievsky had in some way (probably in an indefinite manner) informed the faculty about your unwillingness. Answering my question, Kaufman told me that Georgievsky had likely said nothing definitely (but his answer was vague). For me, what had Georgievsky said and how did he say it remain very indefinite.

Sirinov (who referred to Georgievsky himself) said [to whom?] that Georgievsky was especially distressed by the treachery of Foinitsky, Ivanovsky and some other members of the faculty. They promised to back him but retreated at the decisive moment. If the Minister of peoples education will not confirm Tugan (which is unlikely because of his connections) the problem of filling the chair will occur once more. My *personal* opinion is that the issue of conferring on you the doctor's degree will be therefore (?) postponed or even shelved.

I do not remember whether you know that the 25 years of Kaufman's pedagogic activity ends in June 1914 [and he will not be obliged to read lectures] and the chair of statistics becomes vacant. *Before* the issue about the conferment had arisen, Tugan told me that Kaufman will soon become an honorary professor and that the problem of conferring on you the doctor's degree should be put on the agenda. He asked me whether you will agree etc.

I *personally* think that, if only his disposition does not change, Kaufman will wish a five-year extension. I am informing you about all this, dear Vladislav Iosifovich, and hope that everything will remain

between us. Indeed, if Kaufman finds out that I am discussing his chair, he will gobble me up alive. I suppose that he is awaiting a letter from you, but I have not said, and will not say anything to him about any letters coming to me from you since I do not know how you will regard it.

I have passed my examinations although not without incidents. The examination in statistics will be in September. In the summer, I will not go abroad. Shaposhnikov will defend his dissertation here. Schwittau's defence is set on May 6, and Solntzev's, on May 13.

Letter 5. 15/28 May 1912

Kaufman recently told me something new about the removal of your candidature before the faculty's sitting. Georgievsky told him then that he received two letters from you the contents of which you had let me know. Kaufman added that they both decided not to speak about you since you had clearly formulated your unwillingness [to be balloted] and because they hesitated to expose you to the risk of getting a minority of votes. […] This information once more differs from what Georgievsky had written you.

In his statement Migulin undoubtedly indicated the unwillingness to be balloted (and two professors repeated it). Kaufman told me that this autumn he will suggest to the faculty the conferment of a doctor's degree on you. While desiring to strengthen his statement, I had asked him whether I may inform you about it but he disagreed and told me that, in summer, he himself will tell you that.

Today, conversing with A. A. Kaufman about Georgievsky, I. I. Kaufman said, had he suggested you, Georgievsky *would have protested* so he will suggest it in autumn. Then A. A. asked him to ensure his, A. A.'s, participation. I think that you will excuse me if I allow myself to express my opinion about that conferment.

I had repeatedly indicated to I. I. Kaufman that such a conferment is only a proper deed (regrettably, very belated). Without discussing at all your scientific achievements *in general*, the great benefit which you secured to *Russia* by your scientific investigations and by your mentoring *young Russian scientists* [apparently, Russian students of Berlin University] already means that the conferment is indeed an act of just gratitude. And so, I think that whoever will be instrumental in bringing about this act, and whichever considerations will guide him, is not, and cannot be essential.

As to my examination, I still feel its very unpleasant aftertaste. The examiners did not want to examine the two of us[4] since the sitting was delayed, but we *insisted* and succeeded. I came there at $7^{h}30^{min}$ but the examination began at $11^{h}30^{min}$ (and ended at $4^{h}\,30^{min}$) and I developed a splitting headache.

When I came up to the examiners, Kaufman and Tugan-Baranovsky, the former told me very sharply: *Take a ticket, fail and be off*! I was so taken aback that thought of going away, but the dean, Golmsten, who noticed my embarrassment, asked me to take a ticket. I was so upset that *did not hear myself* and barely understood Kaufman's questions (Tugan kept silent).

A few times he sharply told me *No, you do not know it*, etc. All the time he sat with his back facing me (which is his peculiar method). But when I managed to arrive at Böhm-Bawerk, Marshall et al, about whom he knew nothing, he drooped and went away. I had finally found my feet and everything went on well enough.

An amusing incident happened with Migulin [a third examiner suddenly appeared!]. I did not answer his question:

How many copecks do they require in the USA for a pood [16.4 kg] of wheat? [A worthy question indeed!]

Then, very loudly, he began to debate the *actual* money circulation in Russia. I answered that nowhere in Russia hard cash was accepted after the war of 1812 by the treasury (at the rate of exchange) as opposed to the banknotes. He declared very sharply: *Nothing of the kind had anywhere happened*! I told him, also loudly: *But Prof. Kaufman* [4, such-and-such chapter] *wrote so-and-so.* Migulin whispered: *He had not written that* … […] A row erupted.

Solntzev's defence went on very, very mildly. Tug[an]-B[aranovsky] mentioned a *deep contribution to science* and *translation into foreign languages* whereas Migulin, as it was said, *revealed his socialism and ignorance*.

Letter 6. Oster, Chernigov province, 31 May 1912

Today, I had finally run away from Petersburg. It was hot, dusty and badly since the work somehow was not getting on at all. Kaufman almost daily required and required some new and uninteresting study for the examination in statistics. This is why I thought it right and proper to go to my home town. I will work in June, hunt in July and return to Petersburg on the first of August.

Kaufman had recently written down your address and asked me how to get to your place. He begins travelling in the end of June but when he will come to Berlin, at once or on his way back, I do not regrettably know. He is now very busy with issues concerning banks, he is somewhat nervous, very tired and wishes to rest.

I am recently working on the statistics of mortality and nuptuality, rummaged through the Berlin censuses and yearbooks, trying to find all the essential in Böckh. He writes or rather wrote especially vaguely. For the present, my relations with Kaufman are good and it seems that he does not think about postponing my examination. If I pass it in statistics in the beginning of September I will sit for my examination in finance in the end of October and, from 1913, I hope to be sent according to all the rules, but for now it is a dream.

I only wish that my dependent examinational efforts end and I will finally begin working. I often have a look at my notes of your lectures which helps me very much. How do you like the new contributions of Shaposhnikov [14] and Sirinov [15]?

Letter 7. Petersburg, Oct. 1912

I had taken up my pen after each essential event in the University and in my own personal life, but, until the end of all of my examinations, never wrote anything. Although I fear that some of my

news are very dated and known to you, I shall now describe everything in chronological order.

Prof. Pokrovsky was transferred and resigned and a strong current stirred up among the professors. A few times some of the members of various groups of professors even expressed a desire to resign. Finally, about the 15th of September a populous meeting of the leftist professors of all the faculties took place. Three groups had formed there. The first reproached [everyone?] for failing to resign during the Moscow incident[5] and indicated that it was necessary to resign even now so as to come out *with credit*. The second, most populous group, remained undecided, and the third group were the opponents of the first group. The speech of Grimm played a certain role. He indicated that the decision of Kasso had rigorously conformed to the *University Statutes* and that therefore any action of the professors will only be politically significant. Finally the meeting resolved to abstain from *political demonstrations* and resignations.

Kaufman had been so upset that he became unable to work, lost weight and turned horribly yellow. I think that it is unnecessary to say that he had not participated in that event at all. The newly appointed Gribovsky and Nikonov militate but are unable to do anything since Migulin and Pilenko (who had now went to war) passed on to the old ones who are now in majority. The relations have aggravated, Zhizhilenko and Gribovsky do not offer hands to each other etc. Further negotiations (concerning Beneshevich) are discussed and appointments (Chistiakov, for the chair of political economy, and Krassnozhen for the ecclesiastic law). The dean and the secretary of the faculty resign although because of personal matters.

Migulin was afraid of reading lectures and they were postponed until the third term. At present, no one will read them. Nothing is yet said about Shaposhnikov's dissertation but he settled down in the Polytechnic School rather well. Kaufman will be apparently able to promote Orzhensky, he wrote favourably about him. Solntzev is being voted in Yuriev [Tartu] and Sirinov, as it seems, hopes to defend his dissertation in Kazan.

I have concluded my examinations up to the written exam. They themselves ended even well enough but before that I had to experience much bitterness. The next siting of the faculty will discuss the issue of my journey, I am really worried that I handed in my application too late. I seriously disagree with Kaufman about my future work and the places which I have to visit. He requires studies of economic statistics (mostly of England) and my immediate journey to London, then to other English cities and towns (4 months), to Paris and Rome (4 and 2 months), Vienna and Munich, but by no means to Prussia (to Berlin).

However, I have resolutely and definitely decided to study mostly demography, and to go first of all to Berlin, there to consult with you when my theme becomes clearer, then go only to London. Travels are very interesting but they strongly impede work. Since I am badly informed, I had only barely outlined my

theme. However, I know that it will not be mortality, but a bit of nuptuality and fertility.

Anyway, I will not begin my journey before March, and during five months here become somewhat acquainted with the literature and hopefully ascertain for myself my future problem, at least approximately. I wish so much to come to Berlin, at least for a month, to speak with you, share my doubts, but at present this is impossible. I had also studied Böckh, but barely successfully and, for the tine being, will not return to him. Am much busy with bibliography, try to compile a possibly complete list (on cards) of books and papers. Will be very glad to hear from you.

Letter 8. Petersburg, 11 Dec. 1912

I am very grateful to you for your letter and advice. I agree that Petersburg is a city least disposing you to work, and I yearn to, but regrettably cannot, brake away from here. I wish to read lectures and mostly to become a privat-docent. But the main point is that my destiny is not yet decisively decided. The ministerial commission sits at the end of December, and it is this body which decides to allocate the money [for the sojourn] or not. The chances of travelling are slim, less than a half of the applicants will be satisfied. It is desirable that someone petitions for me, but there is no such person and it is really possible that, for an indefinite period of time and still in an indefinite position I will have to remain in Russia.

Kaufman had clearly reconciled himself both with my studying demographic statistics and with beginning my journey by going to Berlin. For the present, there is no apple of discord. On 16th December Orzhensky at last defends his dissertation. I proved to be too pessimistic although it was difficult to allow for the very often visits of Pergament to Kaufman and his pressure. The second opponent is Tugan. At first he declined, then agreed. Because of Orzhensky [the defence of] Sokolov's dissertation, although approved by the faculty, will hardly take place before Christmas:

These days Sirinov is defending his dissertation in Kazan. Solntsev was unanimously elected chair in Yuriev [Tartu], which should be approved by the ministry. He is much afraid that Kasso will not approve it, but, according to the general opinion, there is nothing to be worried about. I saw A. A. Kaufman a long time ago when he began reading lectures. It seems that he is really satisfied with the second edition of his book [2] and declares that now he does not wish to see its first edition. I still intend to see his statistical room which he praises very high.

Migulin had not yet begun reading lectures and it was remarked that he will be transferred to Tomsk. Yesterday, at the latest sitting of the faculty, he stated that he is already healed and will next term continue the course in financial law which Kaufman had begun.

A tragedy occurred at the faculty. The dean, Golmsten, is gone and Zhizhilenko is going (he had handed in his application, but his resignation is not yet approved). The former suffers from dropsy, and the latter ought to complete his doctor's dissertation. A number of the

holders of the master's degree had promised in writing to complete, by 1910, their doctors' dissertations[6].

And so, none more or less leftist can *rule and govern over us*[7], absolutely no one. It remains to elect some people from the appointed, although the old ones are still slightly in majority. Among the appointed Gribovsky, Nikonov and Udintsev sometimes violently row. I am now reading, mostly some old books.

Letter 9. Petersburg, 19/4/II1913 [?]

It seems that I wrote you not long ago, but many changes had occurred in the academic world. Solntsev is not approved as an acting extraordinary professor at Yuriev [Tartu]. Rumour has it that the reason is that a still unknown person is now writing his dissertation and will fill that chair. Sirinov has defended his dissertation in Kazan and is now free to choose both a chair and a university. Chistiakov had not shown himself particularly. It is said that he is very touchy, even oversensitively so, extremely nervous and that he was *the most decent professor of the law faculty* in Odessa.

Orzhensky defended his dissertation just before Christmas. His official opponents were I. I. Kaufman and T.-B. and A. A. Kaufman was an unofficial opponent. Your name was mentioned many times and A. A. called your book on the law of small numbers [18] *great*[8]. Orzhensky rather strangely remarked that he barely takes it into consideration since poorly understands it. He attempted to introduce humour, mentioned buttons which tear off all the time whereas no one is able to invent an *untearing* button etc.

I. I. Kaufman is the dean until Petrazhitsky is confirmed the chance of which is very slim. I happened to hear that Shaposhnikov settled down in the Polytechnic School well enough, brought over a German wife from Berlin. In a word, he is flourishing. Tugan wishes to replace Posnikov in that same School, but the election did not yet take place. The Institute of Higher Commercial Knowledge is turning into an almshouse[9]. All the ousted professors are gathered there. A. A. Kaufman recently told me that he reads various courses in statistics either 8 or 9 hours weekly.

Schwittau left for himself only 24 hours at the University and 2 hours at that Institute devoted to the *working-class issue*. Solntsev conducts classes in political economy. Sokolov recently defended his dissertation, but, as I think, rather poorly. Kovalevsky kindly but thoroughly pinched him. The printing of I. I. Kaufman's book [5] will soon be completed.

A *misunderstanding* regrettably happened with my journey. I am included in the second turn among those who will be sent on the first of July if only the ministry has the necessary means. The ministry explains that the University only sent them an application without any information but Zhizhilenko assures that that was simply a new blow on the faculty.

I have collected all the necessary documents and they are sent to the ministry. Yesterday I read my first lecture, will read the second one in a fortnight and plead for the post of privat-docent. Everything is very mournful but I am unable to do anything. It was not what I had

thought about, not what I had hoped for. I would have liked to work some more and apply your advices and indications, but the hope for a speedy realization of my wishes is very little. Horribly want to work but the lectures leave no time for it.

Letter 10. Petersburg, 29/9/1913

I sincerely thank you for your contribution [20]. All this time I did not write to you, I wished to attain definite results from my efforts to process a part of my future book. I thought of expounding the complete history of the first table of nuptuality compiled by Muret[10]. I thoroughly attempted to find the well-known paper in the *Berliner Börsen-Zeitung*, did not find it in any library. I directly turned to the Petersburg office of that newspaper and they gave me their file for the proper year.

Already then I had a presentiment that that table which served as the basis for the calculations of Wittstein [21] and Zeuner [23] is connected with Muret. Now, however, I have established this without fail. The unknown author had just recalculated the nuptuality table of Florencourt: from a thousand 16-year old girls 13 are married [during ?] etc. And from 35 numbers only 12 differ by 1 or 2 from what they should have been.

But one point is misfortunate: I am still unable to reconstruct Muret's calculations. He was obviously mistaken. I computed his table anew but it is interesting to find out how he calculated it. I have apparently tried out all the wrong methods, failed to attain a satisfactory result but hope to succeed.

I collected everything what I know in Berlin and I can find here the materials which I have not seen. Even while in Berlin I discovered a good library, the library of the insurance society *Rossia*. I am here for exactly a month, but achieved barely sufficient results. The Commercial Institute requires much time although my brother helps me a lot[11]. I will conduct classes after Christmas since A. A. Kaufman thinks that students should first attend his lectures which he will conclude during one term after reading 4 hours weekly.

The studies in the University began yesterday. The first class which I conducted left no bad feelings. There were about 18 students and 7 themes were chosen, the other students were afraid of managing reports and did not venture to follow. I will conduct classes in the theory [of statistics] and demography, Stepanov, city and Zemstvo statistics, Bukovetsky, financial and economic statistics, and we work independently from each other. For the time being, 75 students (the maximal number) registered with Stepanov and 25 (the minimal number), with me.

Already in Berlin I began to quarrel with I. I. Kaufman. He told me that my present study cannot become a dissertation because 1) the members of the faculty will not understand it; 2) and the main point: it belongs to insurance (!) rather than to statistics. There is a problem in statistics, as he declared, about the family circle of the population, but it is ascertained, as he stated once more, in his celebrated programme and it is reflected in the censuses. A family as a social phenomenon includes children so that etc.

In a word, he does not even recognize the existence of an independent scientific problem. His studies of population statistics were apparently restricted to mortality. This conversation was not repeated and the immediate cause of the quarrel was his rude attacks on me and on the conduct of the future classes. Now, however, everything has calmed down. My earnings (1600 roubles) are sufficient but I have to spend much time.

Many newspapers stated that Solntsev was confirmed in Tomsk, but he himself does not know it for sure. Sirikov was not retained in Yuriev [Tartu] and he is mightily glad. A. A. Kaufman will soon read a report at the [Russian] Geographical Society about the census of 1915[12]. He will argue for the unification of the censuses of population, industry and professions. I have recently met Kurchinsky and we spoke much about you. Near Christmas and during vacations I hope to process the theoretical part of my work, but am not sure about the success. Just came here and already long for coming back. I would like to work freely for a year or two. Neither Spektorsky nor Vilkov said anything about the invitation to Warsaw [?]. I thank you for the book and for everything.

Letter 11. Petersburg, 22/1/1914

Much has changed in Petersburg and in my personal life after my latest letter. The appointed professors became wholly and definitively the masters of the situation. They elected their dean (Prof. Udintsev) and secretary (instructor Rosin). One *old* professor voted for them, and it was none other than Kaufman. As he explained, he *cannot tolerate anarchy and lack of superiors*.

Solntzev was confirmed as an ordinary professor in Tomsk and they [apparently, he with family] went there. He is very glad. Sirinov took root in Yuriev [Tartu] but lives here, at times he affects great importance as though being an ordinary professor. He is addicted to *politics*, became an Octombrist[13], began to declare that *we have always been on guard of law and order*, but in general he remains the same chatterbox as previously. He is writing a doctor's dissertation on the basis of three fat volumes of the works of some commission and threatens to complete it soon. He hopes to make a rapid successful career either in science or administration.

You probably know that Troinitsky died, and that it was thought to fill his post by Sudeikin, but Georgievsky told his superiors that he will then go since it is customary that that post is filled by the director of the Central Statistical Committee (CSC). Newspapers had already reported that Sudeikin was appointed, but Georgievsky won. Beliavsky, the former professor of police law in Yuriev [Tartu], filled his previous post.

During all this time I had interviewed Sirinov in detail about the proposed post for Ballod and previously got an *impression* that he himself does not want Ballod to fill the official post of head of section. I explained it to myself by Sirinov's slight sin: perhaps he himself thinks about that post.

I spoke to Georgievsky after his appointment once more and he told me that he still intends to see Ballod among the heads of the three

sections of the reorganized CSC but when will that reorganization take place is yet unclear. Sirinov also told me that they do not anymore think about inviting Ballod into the provisional census commission and that there are already candidates for its highest posts (Solntsev and Brunneman).

Goldstein was not confirmed as a professor of the Moscow Commercial Institute. Shaposhnikov, as it is said, is not satisfied with his situation. I read with great pleasure your criticism [19] of Wolf [22] and am very, very thankful for sending it to me. It serves such impudent fellows right! That Wolf was here and I saw him during Orzhensky's defence. Kaufman is recently all the time unwell. He repeatedly asks me, if I will write to you, to thank you for the rare pleasure which the reading of your paper provided him.

Now about myself. None of my good hopes about which I wrote you had come true. My earnings and serious troubles in my personal life left me little time for working on my future book. The loss of earnings threatened me and I had to change much, so now I am overburdened with work which, generally speaking, is not well-paid. But kind people still exist on our Earth. From January 1 to September 1, 1916, the Ministry of Peoples Education is sending me abroad. I had nearly forgotten about such possibilities, but, being prompted by Schwittau, I had at the last minute submitted an application to the faculty. They approved my application since Kaufman was absent. Otherwise he would have certainly argued that two years are impossible. I did this secretly, said no one about it, was afraid to be dismissed from the Institute. It became clear now that my fear was wholly founded. A. A. Kaufman was horribly angry and his kindness to me had disappeared. He scolded me very seriously although I will fulfil all my obligations towards the Commercial Institute, and, to ensure this, I remain here until the end of March.

By mid-April new style I will thus be in Berlin and live there, as I suppose, for a week or two. Then I go to London but by September I think of returning to Berlin and staying there until concluding my dissertation. I cannot even say how I am happy to work without hindrances for 1 3/4 of years and develop my very modest knowledge. And so, I will see you soon, if, as I hope, you will be then in Berlin and I will be very distressed otherwise, will be unable before going to London to share with you my doubts about my work.

Letter 12. London [1914]

I am living here for two months now. I cannot say that my work is going on as rapidly as I thought. Because of my inexperience I am apparently unable to estimate the time needed for processing the material. At first I had been unwell, then very unhappily moved house and am now once more living in the previous place. Following your advice I am writing without any drafts, but I am a very bad writer and have to rectify and rewrite everything over again.

I have prepared for printing the chapter about the first table of nuptuality (but will perhaps have to rewrite it once more). I have to comply with the requirements which are usually imposed on

dissertations and write lengthily, partly also because of my inability to write. The chapter is 2 1/2 times 26 or 27 thousand letters long.

I am now processing English tables of nuptuality which are likely interesting since they are compiled for various occupations. My search for French tables of nuptuality is yet unsuccessful. Is it possible that the French *do not apply such tables for insurance*? I wish to finish definitively all the historical chapters so as not to return to them, and, which is the main point, not to find out later that the compiled materials are somehow defective. This has indeed happened to me previously.

Bearing in mind that it is always possible to cut something out I write each chapter in detail. I do not know when I will conclude my historical chapters but wish to proceed to the theoretical part before going to Berlin. I am now recalling the differential calculus and solving problems. Time flows by so swiftly that each evening I feel some unpleasant resentment that too little is accomplished.

At first I barely understood the English and had therefore rarely spoken with them. Now, on the contrary, I understand better then I speak. Two or three times I had studied with an Englishman. He corrected my translations from Russian into English and I corrected his translations from English into Russian and we both are successful. I will nevertheless have to go to the library of the Institute of Actuaries since I was unable to find some books, even in English, in the [British] Museum.

Our boarding house has two tennis courts so I bought a racket and am diligently losing the 10 pounds which I had managed to add to my weight during the two latest years. Schwittau is here and asks me to send you his greetings. I received a letter from the secretary of the professorial council. The faculty resolved to keep Kaufman for the next five years. The locality in which I am living is the best in London. Gardens, gardens, no end of them, we have two. I pay 24 pounds weekly for everything, which is dirt-cheap. The meals are very good. In the morning, I work in the [British] Museum and return home for lunch. Only one circumstance is unpleasant: the weather changes surprisingly often. Now, it is cold, and now, very hot.

I am very much interested whether you have not changed your mind to conduct, *in the next term, a seminar in mathematical statistics*. This is connected with the time of my arrival in Berlin.

Notes

1. This highly educated economist was ignorant of political economy, see Letter 5. O. S.

2. A Demographic Institute of the Soviet Academy of Sciences was established in 1930 but abolished in 1934 (Tipolt 1972). The bloody Stalinist regime was revealed in demographical statistics which was therefore too dangerous. Here is a telling episode. In 1954, an extremely important statistical conference was held in Moscow. In his report, Kolmogorov listed the main fields of application of the law of large numbers but omitted demography (Sheynin 1998, p. 531). O. S.

3. Why statistics was not mentioned either here or in the beginning of Letter 4? O. S.

4. The second of the two was not examined. He defended his dissertation, see end of Letter 4 and below. O. S.

5. That incident was a collective resignation of the teaching staff of Moscow University, a protest against Kasso's reactionary policy. A. D.

6. Otherwise their professor will be blamed. A. D.

7. Ptukha indirectly referred to the Norman theory, see for example Riasanowsky (1947). O. S.

8. Concerning that alleged discovery see Sheynin (2017, pp. 248 – 249). O. S.

9. The Petersburg Institute of Higher Commercial Knowledge was established in 1910 instead of the Higher Commercial Courses. In 1917, it was renamed Petrograd Commercial Institute. A. D.

10. The first attempt of compiling a nuptuality table was due to the Swiss demographer Muret in 1764. Ptukha [9] described the history of such tables. A. D.

11. That brother was apparently Vladimir (1894 – 1938) who became a high-ranking Communist functionary and an active participant of Stalinist witch-hunting. Then he himself perished. In his preface Dmitriev mentioned Ptukha's arrest and release in January 1940. A brother of an enemy of the people released? Something went wrong! O. S.

12. Because of the war that census never took place, but Kaufman's report was published [2]. A. D.

13. Octombrist, member of a liberal reformist monarchic party. O. S.

Information about Those Mentioned

Ballod (Ballodis) Karl Mikhailovich, 1864 – 1931. Economist, statistician and demographer. Lecturer at Berlin University from 1905. Professor in Riga from 1919

Beliavsky Nikolai Nikolaevich, 1869 – 1927. Statistician. Graduated from the law faculty of Petersburg University. Professor of police law at Yuriev [Tartu] University and Imp. School of Jurisprudence. Director of Central Statistical Committee, 1914 – 1917.

Beneshevich Vladimir Nikolaevich, 1874 – 1938. Graduated from Petersburg University. Lawyer and Bysantyne student. Professor, chair of canon law, Petersburg University, from 1911.

Böckh Georg Friedrich Richard, 1824 – 1907. Demographer and statistician. Director of Prussian statistical bureau in Berlin, 1874 – 1902. Professor, Berlin University. See Ptukha [10] about his work in demography.

Brunneman Yuliy Wilhelmovich, no dates provided. Statistician at the Central Statistical Committee. Managed the library of the Russian Geographical Society.

Bukovetsky Antoniy Iosifovich, 1881 – 1972, economist and financier. Graduated from the law faculty of Petersburg University. Custodian of the statistical room of that faculty from 1907, privat-docent at the chair of financial law from 1912

Chistiakov Ivan Ivanovich, 1873 – not before 1918. Economist. Ordinary professor, chair of political economy from 1912, Petersburg University. Managed the economic room from 1914, secretary of law faculty from 1916

Foinitsky Ivan Yakovlevich, 1847 – 1913. Graduated from law faculty, Petersburg University. Criminologist, taught criminal law 1873 – 1913.

Georgievsky Pavel Ivanovich, 1857 – 1938. Economist. Ordinary professor, chair of political economy and staistics, Petersburg University, 1890 – 1911, Chairman, Central Statistical Committee at Ministry of interior, 1911 – 1914

Goldstein Iosif Markovich, 1868 – 1939, economist. Graduated from Munich University. Docent of economic policy and statistics at Zürich University, 1898 – 1902. From 1906 read economic policy at Moscow University

Golmsten Adolf Khristianovich, 1848 – 1920. Lawyer. Privat-docent from 1889, extraordinary professor from 1895 and ordinary professor from 1899, chair of civil law and legal proceedings , dean of law faculty, Petersburg University, 1912, rector, 1899 – 1903.

Gribovsky Viacheslav Mikhailovich, 1867 – 1924. Lawyer and fiction writer. Graduated from Petersburg University. Privat-docent, chair of encyclopaedia and history of philosophy of law. Professor, Novorossiysk University, 1909 – 1911. Appointed by Ministry of peoples education as chair of history of Russian law, Petersburg University. Professor, law faculty of that university, specialist in the history of Russian law.

Grimm David Davidovich, 1864 – 1941. Specialist in civil law. Professor, chair of Roman law, law faculty, Petersburg University. Rector of Petrograd University, 1914 – 1917

Ivanovsky Ignatiy Aleksandrovich, 1858 – after 1926. Lawyer. Instructor at chair of constitutional law, Petersburg University, from 1896. Dean of law faculty, 1904 – 1905

Kasso Lev Aristidovich, 1865 – 1914. Reactionary Minister of Peoples Education, 1911 – 1914

Kaufman Aleksandr Arkadievich, 1864 – 1919. Graduated from law faculty, Petersburg University. Statistician. Taught statistics at that faculty, 1910 – 1913. Professor, Bestuzhev courses, 1907 – 1916

Kaufman Illarion Ignatievich, 1848 – 1915. Economist and financier. Privat-docent, 1889 – 1893. Extraordinary professor, 1893 – 1901, ordinary professor from 1901, Petersburg University

Kovalevsky Maksim Maksimovich, 1851 – 1916. Sociologist and historian. Professor, Petersburg University, 1906 – 1916

Krassnozhen Mikhail Egorovich, 1860 – 1941. Lawyer. Professor of canon law, Yuriev [Tartu] University

Kurchinsky Mikhail Anatolievich, 1876 – 1939. Economist and financier. Graduated from the law faculty of Petersburg University. Privat-docent, 1905 – 1915. Professor, chair of financial law at Yuriev [Tartu] University from 1915

Manuylov Aleksandr Appolonovich, 1861 – 1929. Economist. Professor, Moscow University from 1900, later its rector

Migulin Pavel Petrovich, 1870 – 1948. Economist and financier. Professor, chair of financial law, Petersburg University, 1911 – 1917

Nikonov Sergei Pavlovich, 1868 – after 1920. lawyer. Ordinary professor at law faculty, Novorossiysk University, 1909 – 1910. Transferred to Petersburg University, chair of commercial law and legal proceedings, 1912 – 1917.

Orzhensky Roman Mikhailovich, 1863 – 1923. Statistician. Professor, Petrograd University, 1918 – 1919. Head of department of statistical methodology, Central Statistical Directorate of Russian Federation from 1919

Pergament Mikhail Yakovlevich, 1866 – 1932. Worked at chair of civil law, Petersburg University, 1906- 1907. Permanent dean of law faculty, Bestuzhev courses. Acting ordinary professor , chair of civil law, Petrograd University from 1917

Petrazhitsky Lev Iosifovich, 1867 – 1931. Lawyer. Petersburg University, chair of encyclopaedia and philosophy of law, dean of law faculty, 1904 – 1906, member of disciplinary court

Pilenko Aleksandr Aleksandrovich, 1873 – 1948. Lawyer. Graduated from Petersburg University. Privat-docent from 1900, ordinary professor, chair of international law, 1911 – 1917

Pokrovsky Iosif Alekseevich, 1868 – 1920. Specialist in civil law. Professor, chair of Roman law of Petersburg University, 1903 – 1911. Dean of law faculty, 1910 – 1912. In 1912, transferred to Kharkov University.

Posnikov Aleksandr Sergeevich, 1845 – 1922. Economist. Chair of political economy Petersburg Polytechnic Institute, 1902 – 1912, director of that institute, 1911- 1913. Member of Fourth State Duma, 1912 – 1915

Rosin Nikolai Nikolaevich, 1871 – 1920. Lawyer. Graduated from the law faculty of Petersburg University. Ordinary professor, chair of criminal law and legal criminal proceedings, same University, from 1912. Dean of law faculty, 1916 – 1917

Schwittau Georgiy Georgievich, 1875 – 1950. Statistician and human geographer. Privat-docent, chair of political economy and statistics, Petersburg University, 1908 – 1916. Left Russia in 1919, returned back, worked in the Agricultural Academy, 1929 – 1933.

Shaposhnikov Nikolai Nikolaevich, 1878 – 1939. Economist. Professor, Moscow Commercial Institute, 1913 – 1927

Sirinov Mikhail Aleksandrovich, 1878 – after 1959). Economist. Privat-docent, Petersburg University, 1908 – 1913, extraordinary professor, chair of political economy, Yuriev [Tartu] University from 1913

Sokolov Konstantin Nikolaevich, 1883 – 1927. Lawyer and journalist. Graduated from the law faculty of Petersburg University. Taught there. Head of foreign department at newspaper *Rech*.

Solntsev Sergei Ivanovich, 1872 – 1936. Graduated from Petersburg University. Managed the economic room, 1907 – 1913. Privat-docent, chair of political economy, 1913. Extraordinary professor, chair of financial law, Tomsk University from 1913. In 1921 – 1926 professor in Petrograd Iniversity.

Spektorsky Evgeniy Vasilievich, 1875 – 1951. Lawyer, defended master's dissertation. From 1913, professor in Kiev University, chair of constitutional law. Elected dean of law faculty, 1918, and later rector of the University

Stepanov Viktor Vladimirovich, 1868 – 1950. Graduated from the law faculty of Petersburg University. Statistician. Headed statistical department of Petersburg city Duma, 1906 – 1918. Taught statistics in several institutions (in Petersburg University, 1895 – 1902, privat-docent from 1909, professor from 1943). Works in population statistics and history of statistics. Member, International Statistical Institute.

Sudeikin Vlasiy Timofeevich, 1857 – 1918. Economist. Privat-docent of financial law, law faculty, Petersburg University from 1892

Troinitsky Nikolai Aleksandrovich, 1842 – 1913. Statistician. Director of Central Statistical Committee, 1883 – 1897, chairman of Statistical Committee at Ministry of Interior from 1897

Tugan-Baranovsky Mikhail Ivanovich, 1865 – 1919. Economist. Privat-docent, Petersburg University, 1895 – 1899 and 1906 – 1911. Taught in Petersburg Polytechnic Institute, 1905 – 1907, professor from 1913

UdintsevVsevolod Aristarkhovich, 1865 – 1945. Lawyer professor, chair of commercial law at Kiev and Petersburg universities. Dean of law faculty 1913 – 1917.

Vilkov Aleksandr Aleksandrovich, 1872 – 1958. Economist. Instructor at Warsaw University. From 11925 lived in Czechoslovakia.

Zhizhilenko Aleksandr Aleksandrovich, 1873 – not before 1930. Lawyer. Graduated from Petersburg University. Professor, chair of criminal law from 1901

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IV

A. L. Dmitriev

Letters of M. V. Ptukha to V. I. Bortkevich (1921 – 1928)

Vestnik Peterburgsk. Univ., ser. 5, No. 1, 2012, pp. 88 – 107

This is a sequel to our publication [iv]. These later letters cover Ptukha's Ukrainian period of life and work. In spite of the remoteness from Petersburg and Moscow and extreme occupation with present-day problems he continued to maintain scientific contacts with scientists (E. E. Slutsky, S. I. Solntsev, S. M. Orzhensky, V. V. Stepanov, A. A. Chuprov, C. Gini), but first of all with his teacher, Bortkevich.

The scope of his interests essentially widens: from theoretical problems in demography he turns to practical studies of mortality of the population of Russia and Ukraine which is clearly seen in the bibliography of his works. Ptukha devotes much attention to his creation, the Demographic Institute of the Ukrainian Academy of Sciences. It was established in 1918, to a large degree according to M. I. Tugan-Baranovsky's intention. He presented a note to that Academy, an outline of the structure of its branch of social sciences. T.-B. suggested to subdivide that branch into economic and law classes with eleven chairs and two institutions in the former class. The first of these institutes at the chair of political economy should study economic conjuncture, and the second, at the chair of statistics, for studying statistically the population of Ukraine [2, p. 53].

In January 1919 Ptukha was elected Director of the Demographic Institute and thus obtained the rank of an academician of the social-economic class of the Academy. In 1934 – 1938 his Institute was named Institute of Demography and Sanitary Statistics. Until 1934 the Institute suffered from a veritable personnel dearth and Ptukha had been inviting his students from the Kiev Commercial Institute in which he actively taught then.

At first, in 1919 – 1922, the Institute mostly collected and systematized statistical materials about the population of separate territories of the former Ukrainian provinces and thus created a basis for future research. One of its initial problems was the compilation of complete mortality tables for Ukraine. Ptukha also attached much attention to the calculation of the indexes of nuptuality (of the inclination of potential bridegrooms and brides from the same social group to merry each other)[1].

After critically examining various patterns of indexes suggested by foreign statisticians he explicated his own ideas [3]. He highly estimated this work and it was discussed in the correspondence of Bortkevich and Chuprov (Letter 176 of 2.8.1922, see [vi]). Chuprov wrote:

Many thanks for Ptukha. He is a bit hard up for scientific inspiration and not quite sound in considerations about numerical

relations. […] *But on the whole his work is decent and of a really good quality*.

In 1923 – 1926 the Demographic Institute established close contacts with the statistical organs of Ukraine and the Soviet Union. At that same time regular publications about the population of Ukraine began to be published. Ptukha provided a historical essay about the Kiev province. He [5] described the increase in its population during 1797 – 1923, its distribution, structure and its probable losses during WWI. And the first volume of the Transactions of the Demographic Institute had appeared [6].

In 1926 – 1930 the Institute had considerably improved its material security. Means for long-time scientific journeys became available and the staff was strengthened. Six volumes of the Transactions (vols. 3 – 8) were published as well as two volumes of *Demograficheskiy Zbornik* (Collection). The Institute devoted much attention to the reproduction of the population and especially to mortality and Ptukha suggested a new method of the compilation of summary tables for the USSR. Causes of death of the population of Ukraine in 1918 – 1927 were minutely considered. Several statistical collections had been prepared under Ptukha's editorship and he took pains to send all the main works of the Institute to his mentor, Bortkevich, and awaited his severe criticism.

Then, in 1931 – 1938, on the instruction of the republican Planning Committee, the work of the Institute became connected with the estimation of the future population of Ukraine. Numerous publications in Russian and Ukrainian studied the history of population statistics [2, pp. 69 – 71].

The period of mass repressions did not spare the Institute. In February 1938 Ptukha was arrested and in June of the same year the Institute was abolished on the decision of the presidium of the Ukrainian Academy of Sciences. The employees of its demographic section were transferred to the Institute of Economics in which a group devoted to the censuses of population was formed.

The letters below are kept in the Bortkiewicz fund at Uppsala University, Sweden. We are very extremely thankful to O. Sheynin who had sent us the copies of those letters. All the letters except the typed NNo. 6 and 8 are handwritten. Most of them contain underlined words or phrases and notes left by Bortkevich but we did not reproduce them. A number of letters are written on forms of the Ukrainian Academy of Sciences (department of social sciences).

Letter 1. 26.2.1921

Allow me, after a long interruption, to describe briefly what occurred in my life during that time. At the end of 1915 I returned to Petrograd after a two-year absence and began to type the appended work [7] which appeared in October. In the summer of 1916 Vilkov invited me to nominate myself for the Donskoy (formerly Warsaw) University and [or] to be sent as an acting professor to the newly established Perm branch of the Petrograd University.

I accepted the latter suggestion since the faculty gave me to understand that, if my [future] dissertation makes a favourable

impression and I properly discharge my duties, in a year or two I will be returned to the chair in Petrograd. In February 1917 I defended my dissertation at Moscow University and came to Petrograd. My candidature was sympathetically discussed by the faculty and the Council, but the following events had thwarted everything.

I. I. Kaufman died in the end of 1915 but his chair was filled in the end of 1918 by Orzhensky with A. A. Kaufman as supernumerary professor. In the spring of 1919 the former moved to Kiev and the latter died that same year.

In the summer of 1918 I received the right to a year-long scientific journey and went to Moscow. I was not allowed to go abroad but got permission for a trip home. The family of my late father lives near Kiev and I attempted to settle there.

After being recommended by the late Tugan-Baranovky I was elected director of the Demographic Institute of the Ukrainian Academy of Sciences, established in the end of 1918, and professor of the Ukrainian University. In the summer of 1919 I was elected to the chair of statistics at the Kiev Commercial Institute and as a supernumerary professor of St. Vladimir University. The Demographic Institute was established in Kiev rather then in Kharkov, the capital of Ukraine until 1934.

In 1920 I was elected a supernumerary academician at the chair of statistics held by Orzhensky. All this is still unchanged. But owing to different causes all my belongings in various places were lost. Only books were left and I attempt to collect them in Kiev, so I visited Petrograd and would not refuse to live there again. I brought back everything that was still left in Perm, and now I am in Moscow on my way to Kiev.

N. A. Kablukov had also died and P. A. Vikhlyaev is the chair of statistics (appointed; the former head of a provincial Zemstvo statistical bureau). A statistical institute is envisioned here, and I am asked to remain here, but I do not want to live in noisy Moscow.

In Kiev, the libraries are bad and life is very difficult. The whole 1920 I was director of the Cooperative Institute and secretary of the third department of the Academy and had very much work on my hands. Little time was left for pure science or for finishing my doctor's work (nuptuality of the population) which strongly depressed me. I typed about five *lists* [1 *list* = 40 thousand symbols] of small political-economic papers and prepared the theoretical part of my second work [3] for printing. Incidentally, in this part I offer a positive theory of the indexes of nuptuality since in my book (?) I only criticized all the existing indexes.

The historical part describes and discusses all the existing tables of nuptuality. I only need to compile a few such tables and finish some §§ of the theoretical part. There is nowhere to publish my work. And this is all I can say about myself. I cannot express how I incessantly feel the lack of your guidance. If my book has something sensible, it is the result of your school. It is my most cherished wish to break away at least for some time and work once more under your guidance. Time will show whether I succeed.

Letter 2. 1.12.1922

On 1 September received from you your contributions [8; 9; 10 – 12] and sincerely thank you for remembering me. They are the only works in statistics published after 1917 and available here, in Kiev. So I felt a new burst of energy but had no time even to thank you. I had been ill, spotted fever, thrice almost died, my memory failed me almost incessantly. But I managed to pull through. No complications except general feebleness and weak feet especially.

I began working a little. For me, the most oppressive is that I lost the possibility of printing my work, cf. [13]. […]

Gini invited me to participate in his *Metron* and I think of sending him my tables, cf. [14]. I would like to see my contribution [3] published in a foreign language if only it deserves attention and translation. There is no one here even to talk about it. Prof. Orzhensky never studied [this subject].These days he is moving to Kharkov, it will be somewhat better there for his scientific work.

Please excuse me, highly respected Vladislav Iosifovich, for troubling you, for asking you to send around copies of my contribution [3]. I had no other means for achieving it and I am really asking you to send me also, if possible, your earlier contributions and to inform Chuprov on occasion that I will be thankful to the highest degree for sending me his new works. In the near future I will apparently work on more concrete subjects. It is difficult to study the theory. My sincere thanks for the help.

Letter 3. 30.12.1922

I have received your letter of 14 December as well as five papers. I am very, very thankful. And the same day I received three of Chuprov's papers. Until receiving your letter I, just like others, never assumed that German books were so expensive[2].

During the next few months I hope to save some money, buy German marks and send them to Berlin to buy at least two or three books. I have plunged into work. These days I am again over my head in work, will finish the first part of *Mortality of the Population in Nine Ukrainian Provinces at the End of the 19th Century*[3]. It is now very difficult to study theory but work of that kind is possible.

Almost each evening I am *sitting* somewhere or reading a lecture or examining etc. and it is often necessary to do the same in the course of the day as well. Here in Kiev, remuneration for such work is less than modest and I am often unable to buy myself even a book in Russian. Still, I hope that the situation will improve or that I will be able to become a consultant of the Kiev provincial statistical bureau where the remuneration is not as bad as elsewhere.

Prof. Orzhensky moved to Kharkov because of pecuniary considerations, left his family there and went abroad for medical treatment. Here in Kiev, I have a few young men who began to study seriously statistics. But it is only bad that I myself am so pressed for time that cannot duly help them.

I am living as previously in the building of the Academy, in a one-room flat with an installed small stove, a consolation in our lives. I have completely recovered although my legs lack their former might.

As to your indication that my indexes [the following description can only be understood after reading [3]].

I do not know how to thank you, highly respected Vladislav Iosifovich, for the trouble which you underwent when sending my work to those scientists who might be interested. Please do not bother to send me your book [16]. I will somehow manage, I cannot even think of adding a financial loss to your troubles. Please convey my regards to your sister[4].

Letter 4. 5.1.1923

I will not journey to Bruxelles. For some reason the consul had not a permit for me[5]. I sent an urgent telegram but still have no answer. It is a misunderstanding since Stepanov, Popov and Litovchenko have left, the consul had permits for them. I wish to see you in the nearest future but do not know when it will be most convenient for you.

Letter 5. 14.2.1923

I have profited by a sudden occasion and allowed myself to forward six dollars to Prager [see below] in Berlin for buying books according to your indication. In the first place, I would like to have your work, then books on the theory of statistics and demography and the *St*[*atistisches*] *Jahrbuch f. d. DR* [für das Deutsche Reich] (the two latest issues). I have Майр 3 том, *Lex*[*iko*]*n* 1 – 4 but 5 and later are needed, And the new edition of Конрад. If money will be enough, then some more books of your choice.

I am really, really asking you to excuse me for the troubles, but, living in Kiev, I am absolutely helpless, do not know either what was published or the prices of books. After tomorrow I give my work, cf. [5], to the publisher. I will send a copy to you. I wrote it to earn money.

Letter 6. 29.4.1923

For a long time now I wished to write to you and clear up a possible misunderstanding with the purchase of books for me in Berlin. It became obvious that all my efforts were to no avail, the money is somewhere in Kharkov, and, what is most important, a thought is worrying me: my attempt could have influenced your attitude towards me.

I think that even you, highly respected Vladislav Iosifovich, can hardly imagine with complete distinction the picture of my life here four months ago. It is this picture that exonerates such an unforgivable courage, as it seems on the face of it, of burdening you by the purchase of three or four books for me.

We have no foreign books and our earnings are such that, for leading a simplest life, we have to sell something. After saving three dollars with the same difficulties as in the previous times almost a thousand roubles, I found myself in difficulties of the same order: how to convert the money into books? Quite unexpectedly, through a good friend, I found out that a Mr Gurevich goes to Berlin that same evening. My friend ensured me that Gurevich is a courteous and proper man. So I was unable to devise anything better than, after

borrowing another three dollars, to ask him to give the money to your bookseller Prager for the purchase of books according to your indication.

But I have no idea about what happened. I gave Gurevich a letter for you and asked him to arrange everything in a way that ensures the least possible trouble for you. This did not happen and I am now very much worried lest there had occurred some unpleasant misunderstanding the like of which my present life encounters so often.

For some time I have lost the hope of becoming acquainted with the works which interest me and, in addition, I have perhaps displeased you because of my boldness which I allowed myself with respect to you. Once more I am asking you to excuse me after considering all the peculiarities of our life.

And if it will not make difficulties for you I would like to know your opinion about the expediency of my new work, *Mortality in Various Regions of Russia*[6], and, the mean point, about its methods. I try to study scientifically the city and rural mortality allowing also for the nationality of the population. To this end I compile a number of short statistical tables of mortality. For the ages 0 – 4 years I apply the English method as described by Newsholm [17, pp. 271 – 273] and the usual method for ages 5 – 9 years. Then I calculate the number of those still living l_x and the mean duration of life for the extreme years of the intervals 10 – 14, 15 – 24, 25 – 34 etc. After a large number of trial calculations I decided in favour of the method of Bertillon for calculating the values of the column of l_x since it provides the best results beginning from about 35 years (the best approximation to the complete mortality tables for Ukraine and European Russia). Here, I followed Ballod [18, p. 134] […]. However, for calculating the values of the column of mean duration e_x the best approximation occurred when, instead of distributing deaths proportionally during a ten-year period, we calculate a few intermediate values of the number of still living. Here, Hayward [1899, p. 478], see also Newsholm [17, p. 285], enlarged on the method of Farr [19, pp. 456 – 457].

The results. [Ptukha provides a table of l_x and e_x for men, separately for Ukraine and European Russia, for ages 15, 25, 55, 75, 85 (l_x) and 10, 15, 25, 35, 55, 75, 85 (e_x).] For women the results are about the same.

For ages 95 – 104 I took the yearly values of l_x (supposing that for 10 years p_x = Const, but for the periods 86 – 94 and 75 – 84 I calculated intermediate values for 87.5, 90 and 92.5), for other ages I took a single value.

In that way I consider short tables of mortality (do not know whether it is proper or not) like standards and think that they are much better than general coefficients. But they are certainly not like the real and irreproachable complete tables of mortality. I came across references to the latest works of King[7] about such tables but they are here unavailable and I cannot say whether they are suitable for our Russian materials.

In the mean, my tables can be compiled in three or four days without any interpolations, perhaps excepting the values of e_x for ages

ending by zero. I think that they are absolutely unsuitable for old men because the accumulation of the living whose ages end by a zero occurs at the expense of the five neighbouring and earlier years. However, this circumstance hardly influences more or less essentially the values for the ages of the young. My short tables are defective in that **1.** The Bertillon formula is only suitable for ages with an increasing mortality. **2.** It is improper that the calculation of the columns of l_x and e_x are calculated by issuing from two absolutely different hypotheses about the law of mortality during each age period

I have compiled my tables of mortality for Ukraine exactly the same way as Novoselsky [1916]. They are somewhat rough. They will be printed soon and I will send you a copy. After corresponding with Chuprov I decided, according to his advice, to send a paper to *Metron*. It will describe mortality in Russia at the end of the 19th century and I will insert there my short tables of mortality. It will be therefore especially beneficial for me to know your opinion about the method of their compilation.

Before the Passover I participated in the Moscow congress of regional studies organized by the Russian Academy of Sciences. Some young statisticians had asked me to move to Moscow since Kiev is apparently fated to vegetate. Indeed, in Moscow literature is available and people are valued higher.

From Moscow I went to Kharkov and earned there some money. By means of a restricted technical arsenal I am now developing the old and partly the new demographic statistics of Ukraine That money will allow me to exist easy in my mind until autumn and perhaps to the new year. I am ever stronger craving for studying only theoretic statistics. Circumstances for this seem to be somewhat better, and in autumn I will plunge into theory if only something will not send me off the rails just like it happened last year. Orzhensky is now ordinary professor of Warsaw University and even lives there.

Letter 7. 30.7.1923

I am sincerely thankful for your letter of 20 May which I have received in due time. I had not written to you until now because of an accident. My wife had to undergo a medical examination in a hospital, then followed an operation for appendicitis. We are alone, only the two of us, so I had to nurse her, cook etc.

Because of all this I can by no means finish my paper about Russian mortality for *Metron*. I still did not rest at all and am very tired. About 10 August I hope to go to my mother who lives near Kiev and return here once weekly. We have finished the summary and the examination of the materials for the movement of population in nine Ukrainian provinces.

I took upon myself this work for earning some money. These days I will send my earnings (50 dollars) to Berlin, to the brother-in-law of my friend, docent E. E. Slutsky. He will kindly buy books for me and send them here. Yesterday I returned from Kharkov after leaving there my work. It turned out that my protracted (almost four years long) efforts to secure a scientific journey abroad can, under some circumstances, become successful in the nearest future. It is painful to

be deceived by hopes, but it is even more painful to let your chance slip without trying to grab it.

Perhaps you know that there is in Kharkov, just like in Moscow, a Central Statistical Department acting like a people's commissariat [like a ministry]. Its head is the former docent of our institute, A. M. Volkov who is helping me financially by his assignments. He promised to arrange a scientific journey and the money if I will participate in the next session of the International Statistical Institute (ISI, 15 October, Bruxelles) after receiving its invitation.

Moscow received the suggestion of the Institute to send there its members and Stepanov was named. (He seems to be the only member in Russia.) Volkov said that had the Institute sent a similar proposal to the Ukrainian government in Kharkov and indicated its wish to see me at their session, he will arrange everything and I will be able to work abroad for about three months.

Depending on various circumstances the proposal to send me can be made in two ways. **1.** An application to the Council of People's Commissars [to the government], and this is the best way, as he says. **2.** An application to the Academy of Sciences since I am the chair of statistics.

And so, highly respected Vladislav Iosifovich, if it is at all possible and if for some reason it will not be inconvenient for you, I would very much ask you to assist me in realizing my incessant dream, about journeying abroad. I also ask Chuprov. Incidentally, I read in a Moscow newspaper that P. I. Popov, the head of the Central Statistical Department, is sent to that session.

Life is going on as previously, but, as you see, I can already buy books, although for extraordinary resources.

Letter 8. 16.2.1924

I had not written to you until now mostly because of the tempo of my life here. I returned quite safely but all at once I had to compile accounts of the work of the Academy for 1923 and for all the five years of its existence. During 10 – 17 December I was in Kharkov at the All-Ukrainian statistical conference and, during 14 – 21 June, in Petrograd at the session of the Central Bureau (of which I am a member) of regional studies at the Russian Academy of Sciences.

As you see, that's how it should be: one journey is not enough. I ought to say that in many respects these journeys are very beneficial for me. It is exactly three months that I have returned from Germany. During that time I compiled somewhat more reports than scientific materials. In particular, I wrote the biography of Orzhensky [20]. Now, however, I plunged into a rather peculiar work. At the conference, I was asked to report about the general features of the latest statistical works in the West. I did report after which, as I may say, they humbly asked me to publish it.

For a long time I disagreed, but am now compiling such a paper. It will only cover books and there will certainly be gaps, and, in general, I am not sure of success (but see [21]). I raise most modest aims: provide a general impression about each book. I only know that my work is extremely useful for myself. I have almost all of those books,

but ought to describe some of them by depending on reviews. Our statisticians assure me that, because of our complete ignorance, my work will be useful. It is this that prompts me to go on with that work.

At the same time I continue to work on mortality in Russia by enlarging on my paper sent to *Metron* (see [15]). Thus, for the time being my time and my strength are fully engaged. Yesterday I received a letter from Gini. He had sent the proofs to Schlemer[8] which gladdens me very much. Until now, even my financial circumstances are not bad at all. I earn about 150 roubles monthly. I am only distressed by the situation with the comptometer (Archimedes). I had instructed my friends to buy it, they wrote me that they had paid the money, obtained the receipt but left it for being packed. But the firm does not give it and assures that something was only done conditionally. I cannot understand anything from here and fear that I will incur heavy losses.

Solntsev reads political economy everywhere: at the University, at the Polytechnic school, Commercial and Forestry Institutes, at the former Quartermaster courses and is also earning, summarily, about 150 roubles. In essence, he has no time to work scientifically whereas I manage to devote to such work 4 – 6 hours daily. The materials about the demography of Ukraine are in the press. If you have reprints, I would very much wish to obtain them.

Letter 9. 5.2.1925

I had been ever awaiting the publication of my bibliographic work, but do not hope anymore for its speedy appearance. Life is somehow running so rapidly here that I scarcely have time to finish anything. In the summer I had been finishing my bibliography and worked much to organize the All-Ukrainian Congress of the study of the productive forces and national economy of Ukraine (Kharkov, 26.12.1924 – 5.1.1925). I also went to Moscow, to the Congress of regional studies. In all, I journeyed for about two months.

I was asked to remain in Moscow, but the academic situation is much worse there than in Kiev. Relations between scientists compel to wish them to become much better, and in addition the remuneration for scientific work is also worse than here. I do not wish, and cannot have any wish to move.

The possibility of printing (although not of everything) and the earnings of a part of the professors have rather essentially increased [here in Kiev], life became not bad at all. I personally worked and am working much, but, regrettably, not on the theory. During the work on my bibliographic notes I had acquainted myself with much, but a study of something definite was until now impossible.

Yesterday I gave a rather large monograph on the population of the Kiev province to the publisher and hope that it will appear (in Kiev) comparatively soon. The bibliographic book is in the print in Kharkov, eight *lists* [1 *list* = 40 thousand symbols] are printed, five more remain. Even in the summer of 1913 I wrote the history of the first nuptuality table, and it is now in the print in a collection devoted to the memory of Kablukov [22].

These days I will probably sign an agreement to compile a contribution on the population of Ukraine and begin writing it bit by bit. At the same time I will once more work, little by little, on the theory which entices me for a long time now. In 1924, a scientific journey abroad became impossible owing to the lack of means. I am now pleading for it again. I will succeed if the ISI invites me once more, otherwise hardly.

Schwittau wishes to return to Odessa and I am helping him. Sincere thanks for sending me your works.

Letter 10. Rome, 19.10.1925

I arrived in Berlin on 25 September and regretted very much that you were elsewhere. This time, although after surmounting many difficulties, I came to the session of the Institute. It had cost me much time, strength and even money. Stepanov, with whom we lived together, will tell you about all the peripeteia of life in Rome.

I will live [in Kiev] better than before. I have an apartment, as previously in the building of the Academy. My bibliography of the book literature has appeared, regrettably 1 1/2 years later than it was necessary. I will send a copy from Kiev since I have none now.

Financially I am better off, I earn about 300 roubles. My son is growing. Chuprov is in hospital in Italy. He suffers from two illnesses: bronchitis and something else which the doctors still cannot identify. He feels himself better.

Letter 11. 2.11.1925

I sincerely thank you for sending me the review (apparently [23]). Your letter arrived when I was absent, in Kharkov, at the statistical congress. On 26 November an All-Union Congress devoted to censuses should have taken place in Moscow, but quite unexpectedly I received a telegram informing me that it is postponed until 20 January. It was thought to carry out an agricultural census in May, an industrial and commercial census somewhat later and a census of population in December.

I am tired of travelling and am very pleased with the delay. The journey to Rome was very difficult (five nights, four days), almost all the way I travelled in the third class and slept little. For coming back I managed to obtain a transit visa through Czechoslovakia and Poland so that the journey lasted a day less. To my deep regret I was unable to make due use of my journey. There were difficulties with the money. I received 200 roubles and was compelled to spend all I had and go home after that.

I wished to work for some time in Vienna, collect the statistics of the Ukrainian population, but the Austrians had dragged out the permission for an entry visa. I do not know whether Stepanov managed to go there. And the prices abroad are so high that I had for a long time disturbed my budget equilibrium (a debt of 400 roubles). Muscovites were given 1500 or 1300 roubles but I got only 200. I am now studying the materials of the Congress and compiling a pertinent

paper, for the time being only for myself (see [24]). My lectures were transferred to the second and third term so that I can rest and work.

The episode with the comptometer ended quite badly. […] I am subjectively sure that I was deceived. I am left without the money and without the machine. […] No one has any news about Chuprov which stirs up a great uneasiness.

Letter 12. 7.3.1926

I sincerely thank you for your letter of 20.12.1925. Stepanov had not told you about our troubles with visas. Indeed, he went to Vienna with only a transit visa. Breisky helped him to settle. He would have helped me as well, but I did not want to run the risk and in addition I was hard up with money. Financially, I only recovered in February, but I am still very happy to have journeyed abroad.

I am only extremely distressed since I was unable to see you. I dream of going to the session in Cairo, then I will certainly meet you. I only fear that the Ukrainian finances will let me down once more. Indeed, Stepanov got more than a 1000 roubles whereas I got only 200 and spent 650 over and above that. I followed your advice and wrote to the firm Sabielny but have no answer although it is still too early.

Your opinion about my latest work made me unspeakably happy and encouraged me. Indeed, in our situation we somehow lose much. To my deep regret Slutsky moved from here to Moscow and I am left quite alone. I stayed in Moscow for two weeks, from the end of January to 10 February. A statistical congress devoted to the census (population, industry, commerce and cooperatives) of 1926 and an agricultural census in 1927 took place there and I participated in the work of its demographic section. I assisted as much as was able in the improvement of the programme of the census of population and its elaboration.

After returning from Italy I studied the materials of the 16th session and wrote a paper [25] on the ISI. In February I began reading lectures and conducting classes right up to headaches. I am making up for the first term when I had not read lectures and I earn money at provisional courses to send my wife to Yevpatoria for healing her up of rheumatism. I am working little by little on the demography of Ukraine and on a book on mortality in Russia but this work is going on slowly because of my lectures and other pursuits. I wish to begin compiling a handbook on statistics, but am afraid of running the risk.

I allow myself to trouble you with a request to send my book (which I am now dispatching to you) to Aleksandr Aleksandrovich [Chuprov]. I think that that is my only real possibility. Then, I have once forgotten to inform you about the following. Chuprov, while being in Rome, answered the request of F. Zahn to send him something for the *Allgemeines stat. Archiv*: my work [26] was suitable. I do not know whether Zahn was simply courteous (he is the editor of this journal) and I wish very much to know your opinion about that. I bitterly regret that I made some blunders when explicating your ideas. Should I send [my] book to Altschul?

After my journey to Moscow I became convinced once more that it is better to work in Kiev.

Letter 13. 28.5.1926

I returned home from Petrograd, found your paper [27] and sincerely thank you. As soon as I learned about Chuprov's death I thought of writing to you. A wretched impression does not leave me at all although the fact itself was not sudden. His students and friends are now pleading for the publication of all of his works, they collect letters etc. I am also assisting in that activity.

While in Petrograd, I met Stepanov and we spoke about the need to write a biography of A. A. I made a copy of his service record at the Polytechnic Institute which means that the older materials are available. We thought that it will be best if you take this task upon yourself. Indeed, who else can write about A. A.'s work? I will send you the materials which I now have so as to facilitate that task at least a bit if you decide to write about Chuprov. […]

The second term was especially hard for me and it was difficult to work scientifically. My research workers and I myself are now devoting most time to processing materials on the demography of Ukraine. I passed three weeks in Petrograd and studied the history of Russian statistics. I will remain in Kiev until 20 July, then go to Oster.

Letter 14. 19.2.1927

I feel myself very guilty towards you, but still hope that you are not very angry with your student of old days. Indeed, I attempted to free myself from mid-April and transferred all the pedagogic work to the first two terms. And it somehow happened that, together with conducting classes, I am now busy 24 hours a week. Add sittings and the drawing up of a new course in theoretical (mathematical) statistics and something terrible emerges. Weeks and months pass by and all the time I am about to do something which should have been done long ago.

After Slutsky had left Kiev, I took upon myself his course in theoretical (mathematical) statistics. It is this duty which requires work most of all. However, I am studying eagerly since much is forgotten and much should be gone into anew. Apart from a general course in statistics I am also reading a course in *Methods of Economic Statistics* (two hours weekly). It turned out therefore that for creative scientific work almost all the time from October to April was lost. Between times I am writing a book, *Population of Ukraine*, materials for which are being processed by my Institute.

In November I attended a Ukrainian statistical conference in Kharkov and was suddenly offered a post. Mikhailovsky had died, the demographic section of the Central Statistical Department was split into sections of censuses and movement of population, and I was asked to head the latter. On reflection, I declined.

In Moscow, professors are remunerated still worse than in Kiev (180 roubles for nine hours through a year) and it makes no sense in becoming a pure administrative statistician. I attended a conference there also at the end of January and became convinced that my decision was correct. They are now doing away with the remnants of *Popovshchina* and Zemstvo statistics[0].

I dream often about going abroad and plead for it. The chances of success are now certainly worse than in 1923 and 1925[10]. I would like to go in September, work in Berlin and London, come to the 17th session [of the ISI] in Cairo and return back. But it is necessary to receive an invitation from the Institute just as previously. It seems that it should come at least because of the following. In December Gini wrote me and suggested to nominate me for membership of the Institute and promised to actually put into effect his nomination. At that time I thought of asking you to assist in this project. An election would be extremely important for me in various ways. All the attempts made by Chuprov to promote a Russian statistician were to no avail. I certainly cannot say what happens with Gini's attempt.

Everything is well in my personal life. The day before yesterday my son Bogdan became three years old. Another son, whom we named Roman, was born in October. My financial situation is not bad, but I have to work too much.

Letter 15. 5.4.1927

I sincerely thank you for your letter of 19.3. I have recently received a letter from Prof. Gini. He writes that new elections will be held in 1928 after the session in Egypt. He asked a French statistician whose name he did not mention to sign the [electoral] bulletin for me and he asks me to which Russian statistician should he apply[11]. I mentioned Stepanov. Gini also informed me that the Egyptian government will only assist members of the Institute and that the journey will cost very much, so do I wish to receive an invitation and come under such conditions. I answered that the chances for journeying are very, very slim but that they will rise when the journey is registered. Well, I fear that this year it will prove impossible to go abroad.

In a week I will be free from the lectures and begin working once more. I ought to acknowledge that the hard work on preparing lectures in theoretical statistics (for last-year students) and methods of economic statistics provided positive and perhaps even good results as well. For me, much became clearer, refreshed and essentially livened up, also because of teaching the first-year students the general course in statistics. In future, it will be much, much easier.

Until autumn I will apparently write [conclude?] the first volume of *Population of Ukraine*[12] the material for which will soon be ready. It turned out that Dr. Roesle liked very much my work on population statistics and is praising it to everyone. He had sent me his more recent works.

I have grounds to believe that beginning next year the conditions for my scientific life here will improve. I am sometimes thinking that in addition to studying theoretical statistics I can begin to write a manual. Here, this is now a very sore subject. Kaufman's book [23] is rare, other books are simply feeble.

Nothing new at home. My sons are growing, the younger will soon be six months old. I myself had been somewhat sick: the fashionable influenza.

Letter 16. 22.5.1927

Many thanks for sending me your photo and works. It was especially pleasant to receive the photo which consoled me. I fear that it will be absolutely impossible to see you this year. My pleading for a scientific trip ended unsuccessfully. I am comparatively free from mid-April. Until 15 June I have only four hours of lectures weekly which is very, very good as compared with last year. Next year I will try to make my life more rational, have less work which is apparently really possible.

Nowadays I am working on a very large contribution, on statistics of the population of Ukraine, and I am also reading up a little on theoretical works on statistics. In connection with the former I will go to Petrograd and Moscow, perhaps in July, for acquainting myself more thoroughly with the archives there. Perhaps however (and more probably) I will only send there two assistants. I managed to engage three senior workers and two counting assistants and am much satisfied since the processing of materials is going on more speedily.

Solntsev thinks about moving to Kiev and we are now nominating him for the Academy. If we succeed, Kiev will have another good scientist and man. From time to time I correspond with Slutsky and Chetverikov. Their lives are not honeyed at all[13].

In a week or two I will send my family to Oster but remain here and work until the 20th days of July, then go there myself to rest and hunt. […] My sons are healthy, grow up before my eyes, cause many petty delights, sometimes distress but the former prevail. Solntsev wrote that Schwittau with wife and little son is returning from London and will settle down in Odessa or Petrograd. One of my students, a demographer working in the sphere of administrative statistics in Kharkov, Korchak-Chepurkovsky, is to go to Berlin. He will then visit you.

Letter 17. 3.3.1928

I should have written you long ago and indicate at least that I am alive and kicking, working as much as strength and time allow me. From the summer of 1927 I thought all the time about a journey abroad, but was unsuccessful, am pleading this year as well but my hopes are not high. From autumn I sat to compile a book on mortality in Russia and Ukraine (see [29]), will soon finish it. It is generally important and I wish to publish it in Russian but am not sure about that. This latest half-year I am working very much, more than previously, but because of various causes the former calmness of spirit left me in a great measure.

In particular, I am much influenced by the impossibility of journeying abroad. I wish to see you, to discuss my plans and intentions. The direct cause of my letter is the recommendation for the election to the ISI signed by Gini. I had sent it to Stepanov for him to sign it and on 27 February he sent it back to Gini. Today I am writing my last letter asking to arrange everything with the three more signatures and in particular to send the recommendation to you (and provided your address). Somehow I do not really believe in success

but wish very much to be mistaken. My chances of regular journeys abroad and scientific work in the best libraries would have much risen.

My domestic life is flowing variably. My sons are growing, one of them was four years old recently, the other one will soon be 1 1/2 years old.

Letter 18. 25.4.1928

with Bortkevich's note: *answ. 9.7.1928*

I sincerely thank you for your troubles about me and for the sent review. As soon as I had received your letter, I wrote Gini and now I received a letter from him. He had no time to send the recommendation so that it is signed (except Gini and Stepanov) by the president of the Austrian [statistical] bureau Breisky and Hersch, professor at Geneva University. One more Italian member of the ISI will sign as well. I will surely fail since certainly not even a half of the members will vote for me.

To tell the truth, I thought that Gini (as he informed me) will enlist the support of more influential signatures, but what can be done? This is fate. I am pleading for a journey once more, although without real hopes for success. I had not yet finished my book about mortality in Russia and Ukraine. Everything is well at home, the children are growing up.

Letter 19. 21.7.1928

with Bortkevich's note: *answ. 19.3.1929*

I sincerely thank you for the information concerning the election to the ISI. I think that my chances are slim and I therefore resigned myself to failure. Under the present circumstances a collection of 2/3 of the votes is extremely difficult. A week ago I have sent the manuscript of my book *Mortality in Russia and Ukraine* (see [29]) to Kharkov. It will be published in Ukrainian, but, in addition, chapters 2 and 3 will appear in Russian (see [13]).

This year I am tired more than usual. Tomorrow I am going to Oster where my family is living for two weeks now. I ensured a journey abroad, but still do not know whether I will receive the money or not. Without the money I will not go since remitting it is very difficult. Otherwise, contrary to expectation, I will go to Berlin in September, then to Vienna. There, I intend to collect data about Ukrainians in Austro-Hungary.

Oster, Pedagogic secondary school

Information about those mentioned

Altschul Eugen, 1887 – 1959. German economist, banker and journalist. Born in Libau, educated in Germany. Head of the Frankfurt Soc. for the Studies of Conjecture. Emigrated to England 1933, then to the USA.

Bertillon Jacques, 1851 – 1922. French statistician and demographer. Head of Paris statistical bureau, 1883 – 1913. One of the founders of the International Statistical Institute.

Breisky Walter, 1871 – 1944. Austrian politician and statistician. Chancellor, 1922. Chairman of Austrian statistical bureau, 1923 – 1931.

Farr William, 1807 – 1883. Leading British demographer. One of the organizers of the censuses of population, 1851 – 1871. Provided a number of mortality tables and tables of health and mortality for some regions of Great Britain.

Gini Corrado, 1884 – 1965. Italian statistician, demographer and sociologist. Professor of statistics in universities of Cagliari (1909 – 1913) and Padua (1913 – 1925), professor of sociology, Rome University, from 1925. Chairman, Italian Institute of Statistics, 1926 – 1932, founder of *Metron*.

Hayward Thomas, British officer of the medical service.

Kablukov Nikolai Alekseevich, 1849 – 1919. Economist and statistician. Head of statistical section, Moscow province Zemstvo, 1885 – 1907. Professor Moscow University, chair of statistics, from 1903.

Korchak-Chepurkovsky Yuri Avksentievich, 1896 – 1967. Demographer, sanitary statistician. Worked in the Kiev Demographic Institute, 1922 – 1925.

Litoshenko Lev Nikolaevich, 1886 – 1937. Statistician and economist. Assistant, Moscow Commercial Institute, 1911 – 1917. Worked in the Central Statistical Department from 1918. Professor, Timiriazev Agricultural Academy from 1929.

Mikhailovsky Vasiliy Grigorievich, 1871 – 1926. Statistician and demographer. Head of statistical department in Moscow city administration, 1897 – 1922. Head of section of demographic statistics, member of the board of the Central Statistical Department, 1918 – 1926.

Newsholm Arthur, 1857 – 1943. British demographer and statistician,

Novoselsky Sergei Aleksandrovich, 1871 – 1953. Statistician and demographer. Chair of sanitary statistics, Leningrad Inst. of Advanced Medical Studies, 1920 – 1930.

Popov Pavel Ilyich, 1872 – 1950, statistician. Head of estimations and statistics, Tula province zemstvo, 1909 – 1917. First director of the Central Statistical Department of the Russian Federation, 1918 – 1926 ranked as people's commissar (minister). Organized Soviet state statistics. Dismissed 1926 but continued to fill important posts. My guess: dismissal occurred under the influence of the notorious Bolshevik troglodyte Maria Smit.

Roesle Emil Eugen, 1875 – 1962. German medical statistician.

Vikhliaev Panteleimon Alekseevich, 1869 – 1928. Head of statistical section, Moscow province Zemstvo, 1907 – 1917. Member of board, Central Statistical Department, 1918 – 1926. Chair of statistics, Moscow University, from 1919. Chair of statistics, (future Timiriazev) Agricultural Academy, 1920 – 1928.

Volkov Aleksandr Mikhailovich, 1891 – 1954. Statistician. Head of Kiev statistical bureau, 1917 – 1921. First director of the Ukrainian Central Statistical Department, 1923 – 1926.

Notes

For some reason Prukha continued to mention Petrograd instead of its new name, Leningrad. Again, he wrote Russia, hardly ever USSR, but perhaps at least sometimes he meant the Russian Federation.

1. Quetelet (1848a, p. 77; 1848b, p. 38) introduced inclinations to marriage (but not necessarily within the same social group). Also here I note that the author had passed over in silence the horrible man-made hunger of 1932 – 1933. A demographic institute working during that hunger? Too difficult to imagine!

2. After WWI life in Germany became horrible, and books in particular had been very expensive. However, foreign currency had been so highly valued, that Chuprov, living then in Dresden and earning such currency remained comfortable, see his later letters to Bortkevich [iv]. This explains Ptukha's worries.

3. No such source is mentioned in the appended bibliography.

4. In many (although not in each) letter Ptukha repeats this request. I had left out this repetition.

5. It is perhaps possible that those responsible were cautious because of Ptukha's brother, see Note 11 in [iii].

6. No such source is mentioned in the appended bibliography.

7. Kendall & Doig (1968) list three papers of G. King published in 1908, 1915 and 1916.

8. I did not find any suitable Schlemer or Schlemmer.

9. Popovshchina: the surroundings of Popov. See possible explanation in the *Information about those mentioned*. Zemstvo statistics was independent from officialdom, an inadmissible feature even in the beginning of the Stalinist regime.

10. A possible explanation: the Stalinist regime attempted to curtail the contacts between Soviet citizens and foreigners.

11. According to the by-laws of the ISI, at least one of the recommending members should have been a compatriot of the proposed new member. Incidentally, the Russian Wikipedia states that Ptukha became (when?) a member.

12. No such source is mentioned in the appended bibliography.

13. That was life in the Soviet Union. See the life of Slutsky in **S, G,** 6 and Chetverikov' life is described in contributions mentioned in Sheynin (2011, § 7.7).

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V

V. I. Bortkevich, A. A. Chuprov, Correspondence (1895 – 1926)

Berlin, 2005

Translated below are selected letters from that correspondence.

My original Russian text was prefaced by a foreword kindly written by Irina I. Eliseeva, Corr. member of the Russian Academy of Sciences, and I am now reprinting it in translation.

Foreword

The *Correspondence* of V. I. Bortkevich and A. A. Chuprov which became available due to Oscar Sheynin is a specimen of scientific contacts. It covers the period from 1895 to 1926 which includes the first Russian revolution of 1905 – 1907, WWI and the October coup d'état of 1917, but these events hardly influenced the *Correspondence*. The participants concentrated on the problems of the theory of probability and statistics, of elucidating the essence of the appropriate terms and concepts and on specifying the proofs of the theorems involved. Both attempted to remain well acquainted with all that was happening in their sphere and to share their pertinent impressions and opinions.

A large part of the letters had appeared before 1902, when Chuprov as a scientist was still in the making. Even after a tiny interruption in their correspondence he wrote to Bortkevich: *How I miss the lively exchange of our thoughts* (Letter 30 of 5 – 10.3. 1898). Bortkevich was older, although only by six years and had already been recognized in science. This, however, does not mean that Chuprov was only his admirer. Being like-minded in the main, they differed in concrete cases and could have dressed each other down not only in letters but in scientific periodicals as well[1]. However the feeling of seniority did exist, especially in the beginning, see for example how Chuprov was Bortkevich's protégé as the author of the entry *Moral statistics* for the *Brockhaus & Efron Enc. Dict*. (Letter 22 of 3/15.3.1897).

From 1902 – 1903 onward, the contacts of many years became friendship, they began to address each other as You[2]. Later, especially in the 1920's, the letters became briefer, both often exchanged postcards rather than letters, they used many more phrases in foreign languages (mostly in German). It was the time when mathematical statistics had been in the making which was reflected in numerous remarks about the desire to attract the attention of mathematicians since the theory of statistics *is becoming* mathematical (Chuprov, Letter 8 of 17.11.1896).

At the same time they both realized the great inertia which hindered the dissemination of knowledge, the co-existence of scientists who imagined the evolution of statistics in differing ways. Doing justice to

methodology, Chuprov realizes that it is impossible to lose contacts with empirical findings, with practical materials.

This leads us to the appearance of another subject in their correspondence, to the part played by the theory of probability in the study of mortality and in general, to the movement of the population. In his early publications, Bortkevich had long ago studied the mortality of the Orthodox population in Russia, and in his dissertation on the theory of measuring mortality which he had defended in Göttingen in 1893, see the list of his publications at the end of this book.

The presence of at least two other dramatis personae is always felt in the correspondence: of Aleksandr Ivanovich Chuprov on the side of his son and Wilhelm Lexis on the side of Bortkevich. Chuprov's attitude towards A. I. becomes clearer: not only respect and acknowledgement but disappointment with his father for being generally available to everyone. This, as A. A. decided, had not allowed his father to leave a weightier scientific legacy. He himself concentrated on scientific pursuits and only digressed from them for earning his living[3].

All his life A. A. had been reckoning with his father's advices. Thus, he coordinated the subject of his dissertation written in Strasbourg with his father (Letter 30 of 5 – 10.3.1898). And it is also clear that A. I. Chuprov helped both of them: in 1902, he helped his son to fill a chair at the newly established economic faculty of the Petersburg Polytechnic Institute; and he helped Bortkevich to fill a job as a teacher at the prestigious Aleksandrovsky Lyceum in Petersburg (1899 – 1901) where he, Bortkevich, ardently wished to work since his relatives lived there.

A. A. Chuprov thinks that Lexis, the recognized head of the Continental statistics, attaches to much importance to the *element of time* [Letter 5 of 11.11.1896], but had not the subsequent development of statistics confirmed Lexis' insight? The creation of the theory of time series, survival analysis (and, as a particular case, of the analysis of panel data) corroborated the ever increasing significance of studying processes which included time.

The main subjects of the entire correspondence are the problems of the theory of probability and mathematical statistics, but the wide extent of the interests and pursuits of both scientists, especially of Bortkevich, is also seen there. The contacts between them covered not only the tine when mathematical statistics had been formed, but also the period when the new classical economic theory was created. Bortkevich is known to have essentially contributed to the exposure of the contradictions in *Das Kapital* and to have studied the ideas of Pareto and Walras (with whom he corresponded).

The letters of the *Correspondence* throw light on the relations between their participants, on their special friendship. They apparently held the same opinion about Soviet Russia. Chuprov's pamphlet *La décomposition du bolchevisme* which Sheynin discovered in France's National Library in Paris, and was translated into Russian by Prof. A. A. Semenov[4] evidently means that Chuprov had clearly distinguished his friends and students who remained in Russia and with whom he

continued to correspond and cooperate, from the powers that be. He had no intention to make a compromise with them, never promised to return back and remained abroad.

I. V. Tunkina commented on a book by M. I. Rostovtsev *Izbr. Publitsistich. Statyi* (Sel. Publicistic Papers), *1906 – 1923*. Moscow, 2002, p. 180. Referring to the manuscript section of the British Museum (Williams Collection), Tunkina reported that the Russia Liberation Committee (whose uncompromising stand with respect to Bolshevism is well known) had regularly corresponded with Chuprov during the initial period of his emigration when he had been working in Stockholm as the head of the statistical bureau of Zentrosoyuz and *representative of the Russian embassy and colony in Sweden* (1917 – 1919)[5].

Owing to his natural inclination Chuprov could have never developed into a political figure. The statistical method, its universal essence, the penetration of statistics into various branches of science, that was the foundation of his interests. This is exactly why he was so attracted to a statement by Goldschmidt (Letter 30 of 5 – 10.3.1898); *The theory of probability is not a discovery, but an invention*.

The correspondence includes important comments made by Bortkevich on the contributions of Poisson (especially) and Poincaré to the theory of probability, since, while working on his law of small numbers, he naturally separated his own contribution from that of his predecessors.

The publication of this *Correspondence* is a new page in the history of statistics, and the honour of its discovery is due to Oscar Sheynin, who revealed Chebyshev's ideas for Russian and Western readers and in a sense returned Bortkevich to Russia.

Notes

1. Chuprov had hardly ever publicly contradicted Bortkevich. O. S.

2. In Russian and in some other languages the *thou* retains its usual meaning, but on special occasions its first letter is capital (Thou). In translation, this is lost. O. S.

3. No, not at all. He himself had spent very much time helping his students, former students and statisticians in general, see Sheynin (2011). O. S.

4. A. A. Chuprov and the Bolshevik revolution. An introductory paper and the publication by A. L. Dmitriev & A. A. Semenov, *Voprosy Istorii* No. 10, 2003, pp. 3 – 18. I. I. E.

5. I managed to find only one letter written by Chuprov to that Committee in Jan. 1919. He advocated an all-out intervention of the West to crush Bolshevism since the *tabula narratur* […] *about the fate of the European culture* (Sheynin 2011, p. 36). O. S.

General Information about the *Correspondence*

I am reprinting extracts from the extant *Correspondence* between Vladislav Iosifovich Bortkevich (Ladislaus von Bortkiewicz, 1868 – 1931) and Aleksandr Aleksandrovich Chuprov (1874 – 1926):

I collected that *Correspondence* which contained 83 letters from Bortkiewicz (1895 – 1914) and 128 letters from Chuprov (1896 – 1926) and was (and presumably is) kept partly in Moscow and partly in Uppsala and published it in its original Russian. True, in 1917 and 1918 Chuprov, from Sweden, sent postcards written in German to Bortkevich in Berlin (Letters 142 and 143). He obviously was careful

not to put the German military censors on guard. Some mathematical considerations in his letters are difficult to understand since many letters from Bortkevich are obviously lost. Now, I left out most mathematical calculations, especially those which pertained to the law of small numbers, see Sheynin (2008; 2017, pp. 248 – 249).

Chuprov dated his letters written in Russia in the old style (still in use by the Russian Orthodox Church) whereas Bortkewich, living in Germany, naturally used the new style. When ordering the letters I took account of that difference. Another circumstance was also present: the dates of writing and posting a letter could have differed. Thus, letters 132 and 133 are ordered properly, but it is seen at once that the order of their compilation was opposite.

I was unable to decipher some words in Chuprov's letters and he himself (Letter 2 of 1896) apologized for his *insufferably nasty writing* […] *but the leopard cannot change his spots, and I cannot cope with my hand*.

Nonsense! His texts were unbelievably dense, at the end of many lines the letters were crammed still denser. He turned some pages upside down and wrote a line in the bottom, wrote something else in the narrow margins. Especially difficult was the reading of names of people, place names and some letters in his formulas. He could have written slower, perhaps should have worn other glasses. And, after acquiring a typewriter he changed the ribbon too rarely. In general, he did everything to lessen the impact of his letters. His closest student, Nikolai Chetverikov (brother of the eminent geneticist Sergei Chetverikov), managed to read (and type) many of his extant manuscripts kept in Moscow.

The compilation of the bibliography was also difficult. Full bibliographic information was rare. Page numbers could have corresponded to reprints which differed from those in the final publications. Some German periodicals had a double paging, one for each issue, another one for the entire volume. And, when I managed to complete a source cited in the *Correspondence*, I transferred it to the bibliography. Items mentioned in this Introduction are inserted in that bibliography.

References to my Comments (which follow after the *Correspondence* itself) indicate the number of the pertinent letter, for example, **124.1, 124.2**, … And, even if in my translation I left out 124.1, I had not changed that numeration. Finally, my present new comments are numbered **124.0. 124.00**, …

Words or phrases underlined by Bortkevich or Chuprov are italicized. Some places were underscored by fat sweeping lines apparently by Bortkevich, now they are shown in bold type. Stars* mean *see Bibliography*.

Bortkiewicz was well acquainted with Chuprov's father; Aleksandr Ivanovich Chuprov, an eminent (non-mathematical) statistician. Thus, in Letter 25 of 1897 he asked A. A. to thank his father for recommending him, Bortkiewicz, for a teaching post at the prestigious Aleksandrov Lyceum. It is therefore probable that the *Correspondence* between Bortkiewicz and A. A. had not begun by chance. Having a common field of scientific interests and probably

like principles of life and work, and almost always living in different cities or even countries, they actively corresponded. Moreover, they became very close to each other; indeed, here is Andersson (1931, p. 19), Editor of *Nordisk Statistisk Tidskrift*. At the death of the great Tschuprow, von Bortkiewicz wrote to the former's relations [but certainly in Russian. Where is this letter and who translated it?]:

My intercourse with him gave me much good for both mind and soul. I feel his death as if something very important and valuable had dropped out of my personal life and reduced its meaning and import. I need hardly say that there was no living person with whom I could carry on such interesting and fruitful conversations on subjects within our own special province.

In their *Correspondence* the friends discussed scientific topics (opinions about their own manuscripts, published and planned works, about the works of others, and discussions of general scientific and social and political news). They both followed statistical literature and attempted to know everything going on in their vast scientific field. They also paid special attention to Bortkiewicz' unsuccessful attempt to return to Russia and Chuprov's most serious troubles provoked by an essential worsening of the conditions of his life as an exile. During the first years after WWI the economic situation in Germany had been horrible, but royalties in foreign currencies allowed Chuprov to live comfortably. Then the situation normalized and foreign currencies did not help much anymore.

Nothing essentially new in the characters of either participant of the *Correspondence* emerges, but the reader will clearly perceive them as scholars, will see how widely and deeply they had been studying the scientific literature and attempting to know everything occurring in their vast sphere. Bortkevich, professor at Berlin University from 1901 to his death, had retained strong connections with Russia, but it is quite possible to understand P. D. Azarevich (Fortunatov 1914, p. 237) who wrote in 1912:

Each time I see Bortkevich, I regret that we had let him go. He is a veritable man of science!

We distinctly feel how great the sphere of Chuprov's interests was. Just fledged, in 1896, he earnestly discussed the newest literature on statistics, economics, and history of the theory of probability, and in 1898 he critically considered the main writing of Pareto. During that period, he continued studying the logic of the probable which had occupied a prominent place in his candidate composition (i. e., in his serious diploma). It seems however that this subject and especially his considerations about the plurality of causes and conclusions, was a blind alley. Many years later Chuprov himself (Letter 162, 1921) remarked that *in the late years* he *turned aside* from philosophy to mathematics. This turn had naturally begun during his correspondence with Markov (1910 – 1917).

The *Correspondence* also suggests ideas about the German – Russian scientific ties. Many Russian statisticians and economists aspired to perfect their knowledge in Germany, but this process was not unilateral. Suffice it to mention that both Bortkevich and Chuprov, and later Anderson (who had not studied in Germany at all) became

leading German statisticians, and later Anderson (1957, p. 97) indicated that Lexis, Bortkevich and Chuprov had created the previous German mathematical-statistical school.

Some passages from my book on Chuprov (1990/2011) can supplement the *Correspondence.* Thus (2011, p. 55), Chuprov described to his father his relations with Bortkevich. And, on pp. 38 – 39 you will find the text of Chetverikov's letter to Chuprov: N. D. Kondratiev, Director of the Conjuncture Institute in Moscow, invited Chuprov to return to Moscow and work in that Institute.

I can also refer to Chuprov's letter to T. Andersson, of 10.8.1925 (in German). Chuprov thought that Kondratiev had offered excellent conditions, but concluded:

In spite of all the temptations, I had not hesitated even for a moment. Better to stay here in Prague a few months more in my uninviting situation [without a prospect of a permanent post] *than breathe the Moscow air. For those, who had not gone through the school of these severe years there, it is in essence impossible to acclimatize*.

In a previous letter to Andersson of 23.7.1925 Chuprov remarked that his *shares* in Russia had become *higher than ever,* probably due to his being elected honorary member of the London Royal Statistical Society. More interesting is Chuprov's letter to Guldberg of 20.5.1923:

If I return to Russia, I will at once receive invitations to fill a professorial post in various institutes in Moscow and Petersburg [Petrograd] *and, in addition, I will probably be elected to the Academy of Sciences. However, owing to some personal and other causes I do not want to return under present conditions. I hope that they will change, but many years can pass before this happens*.

Chuprov was a correspondent member of the Academy, so perhaps he thought of an election to a full academician.

Other additional sources are the reminiscences about Chuprov and the papers of I. I. Eliseeva and A. L. Dmitriev (Petersburg) about him. They are partly mentioned in the Bibliography and collected in English translations (Sheynin 2004). It is my pleasant duty to thank Hakan Hallberg, the librarian of the Uppsala University (Sweden) for sending me copies of Chuprov's letters which are kept there in Bortkevich's papers. The existence of those papers was recently discovered by Guido Rauscher (Vienna). Claus Wittich had sent me copies of Knapp's letters to Borkevich and of Chuprov's letters to Andersson and Guldberg.

Another part of the *Correspondence* of Bortkevich and Chuprov (the drafts of Chuprov's letters and the letters of Bortkevich) came from Moscow (items from the papers of Chuprov and his father, Box 21, section of rare books and manuscripts, Gorky Library, Moscow University) owing to the kind assistance of Eliseeva and Dmitriev. However, only a few of those drafts were useful since all the rest (and not even drafts but final texts) are kept at Uppsala University. The titles of these Moscow items are italicized.

Wittich (C. W.) and Dmitriev (A. L. D.) essentially helped me to understand many places in the *Correspondence*, and Wittich in

addition corrected the copied German texts. Only because of their help the *Correspondence* seems to be now in a desired form.

Much more about Chuprov is contained in my publications (2004; 1990/2011), which to some extent even directly complement the *Correspondence*. Also relevant are the letters of Chuprov to Gulkevich (Chuprov 2009b).

Supplememt. Bortkevich and Russia. In 1901, Bortkevich became extraordinary professor at Berlin University, but had not broken his ties with Russia. He published a few more papers in Russia (the last one, in 1921), and in 1916, during WWI, he somehow managed to send reprints of his paper [61] to Markov and Chuprov, see Ondar (1977, p. 102). In translation (Ondar 1981, p. 93) only Markov himself is mistakenly mentioned.

After the war, he began corresponding with Slutsky (Slutsky, Bortkevich 2007/2012) and the *Correspondence* mentions letters from M. V. Ptukha to Bortkevich. Below, letters from A. A. Kaufman and N. S. Chetverikov are also mentioned.

1. Before WWI Ptukha [iii] informed Bortkevich about the situation at the Law faculty of Petersburg University. The chair of political economy was vacant and M. I. Tugan-Baranovsky was likely to be elected. Then, however, P. I. Georgievsky nominated Bortkevich, and, since V. I. had no Russian scientific degree, he also suggested the conferment of that degree *honoris causa*. Many professors supported that suggestion, but the business required time and efforts, whereas Bortkevich refused to participate in the ballot.

Ptukha [iii, Letter 5] informed Bortkevich that he had been *greatly useful* to Russia by his contributions and by mentoring Russian young men (apparently, Russian students at Berlin University). Finally, Ptukha [iv, Letter 2] informed Bortkevich that for everything achieved in science he, Ptukha, was obliged to Bortkevich's school.

2. Kaufman described the pre-war situation at the Law faculty of Petersburg University much like Ptukha. He also took some measures for electing Bortkevich to correspondent membership of the Petersburg Academy of Sciences. V. I. did not object, although election would have required his return to Petersburg, but, anyway, no vacancies were available.

In 1910 Kaufman attempted to publish collected translations of papers by Lexis and Bortkevich, coordinated with the latter the contents of the proposed collection, and sent him a specimen of translations written by his employee. Usual difficulties and WWI buried that plan.

Finally, Kaufman thanked Bortkevich for a favourable report about his treatise after which it appeared in German (1913). I note that Bortkevich was deadly wrong.

3. In 1924 – 1927 Chetverikov discussed with Bortkevich his thoughts about time series and index numbers and informed him that A. A. Konüs [see vol. 3, p. 62 of *New Palgrave* about him] had applied V. I.'s results in his work.

I note that everyone concerned testified that Bortkevich had been invariably sending reprints of his papers to his Russian correspondents.

And I especially note that the Archive of the Berlin University is keeping the texts of the letters of condolences sent by the rector after V. I:'s death to the Russian Scientific Institute and the Russian Academic Society and their joint answer (code UK PAB 347) with the following phrase:

Deeply grieved by the sudden demise of the great scientist and excellent man.

Here are some little known facts about the life of Borkevich.

He was member of the German Society of the Science of Insurance (*Deutsche Verein f. Versicherungswissenschaft*), of its leading committee and later president of its mathematical section, see its booklet (Berlin, 1904).

He doubled as teacher in the Berlin Commercial College (*Handelshochschule*) from its creation [in 1906, Berlin Univ. Archive, code UK PA B347] to 1922/1923. (It was later renamed *Economic College*.) Its document of 11.2.1938 (Ibidem, code WHB603), apparently compiled for its own archive, stated that the portrait of the late Bortkevich had disappeared from its assembly hall. The secretariat suggested that

An outsider had taken the portrait since he mistakenly believed that Bortkevich was not of German blood.

He was a Russian Pole, so that the theft was quite justified!

Brief biographies of some scientists. I adduce information compiled by Wittich and Dmitriev about those eminent German and Russian scholars and closest Chuprov's colleagues who are often mentioned in the *Correspondence.* I myself added one more figure, Gulkevich. All are listed in the order of the Russian alphabet.

Eugen Altschul (Liepaja, Latvia 1887 – Kansas City 1959), economist. He was educated in Germany and remained there. Banker, journalist, head of the association for studying conjecture (1926), lecturer (Frankfurt/Main, 1927). In 1933 emigrated to England, then to the USA, became professor in Kansas City. Maintained ties with Russian scientists, studied application of mathematical methods in economics.

Karl Ballod, Karlis Ballodis (governorate of Livonia, 1864 – Riga, 1931), economist and demographer. Educated in Tartu and in Germany, socialist and a Utopian. Lecturer in Berlin University from 1905, professor in Riga from 1919.

Alfred Weber (Erfurt 1868 – Heidelberg 1958), economist, sociologist, professor of economics in Prague and Heidelberg. Jointly with Lederer (see below) Director of Institute of Social and University Statistics in Heidelberg, one of the editors of the *Arch. f. Sozialwiss*. (1922 – 1933), then became an *internal emigrant*. After WWII returned to active scientific work.

Max Weber (Erfurt 1864 – Munich 1920), economist, co-creator of sociology. Professor of economics in several universities (in 1919 – 1920, in Munich). Co-published and actually edited the *Arch. f. Sozialwiss. u. Sozialpolitik*. Was interested in events in Russia and learned the Russian language.

Paul Darmstaedter (Berlin 1873 – Montreaux, Switzerland 1934), historian of economics with wide scientific interests. Professor in Göttingen from 1907, emigrated in 1933.

Vladimir Eduardovich Den (1867 – 1933), economic geographer and statistician (geography of the branches of economic, economic statistics). Graduated from the Law faculty of Moscow University (1890), attended lectures in German universities. In 1894 – 1896 worked in Ministry of Finance, then privat-docent of Moscow University. In 1902 – 1930 docent and professor, the first Russian chair of economic geography, Petersburg – Petrograd – Leningrad Polytechnic Institute. In 1930 – 1933 professor, Leningrad Industrial Institute.

Konstantin Nikolaevich Gulkevich (1865 – 1935), an eminent Russian diplomat. After the Bolshevik coup d'état of 1917 he remained in Stockholm as the envoy of the former Provisional Government. He dissuaded Chuprov from returning to Russia and became extremely friendly to him. Later was assistant of Nansen, the League of Nation's Commissioner for refugees.

Aleksandr Arkadievich Kaufman (1864 – 1919), statistician and economist (theory of statistics [hopelessly lagged behind life], the agrarian problem). Graduated from the Law faculty of Petersburg University (1885). In 1887 – 1906 worked in the Ministry of state properties, after which studied land use and the economic conditions of the life of peasants in Western Siberia, then scientific work and teaching of statistics (the Bestuzhev courses and Petersburg University). Doctor of political economy and statistics (1908). Participated in the creation of Soviet statistical organizations.

Bogdan (Fedor) Aleksandrovich Kistiakovsky (1868 – 1920), lawyer and sociologist, publicist. Learned at historical-philological faculties in Russian universities, attended the lectures of Windelband and Knapp in Germany. Helped to establish the periodical *Osvobozhdenie* [Liberation] in Stuttgart. Returned to Russia and edited the journals *Kriticheskoe Obozrenie* [Critical Review], 1907 – 1910, *Yuridich. Zapiski* [Legal Trans.], 1912 – 1914, and *Yuridich Vestnik* [Legal Herald], 1913 – 1917. Master of constitutional law (1909). Taught at Moscow University, Moscow Commercial Institute and Demidov Lyceum.

Georg Friedrich Knapp (Gießen 1842 – Darmstadt 1926), economist and statistician (theory of mortality, peculiar theory of money, history of economics). Professor of economics and head of seminar on university statistics in Strasbourg, 1874 – 1918.

Johann von Kries (Rogenhausen, Prussia, now Rogoz in Poland 1853 – Freyburg in Baden-Württemberg 1928), physiologist. Professor in Freyburg, 1880 – 1924. His book (1927) influenced Keynes and Chuprov.

Emil Hans Lederer (Plzen, Czech Republic 1882 – New York, 1939), economist, sociologist. Editor of *Arch. f. Sozialwiss.*, 1918 – 1922, then its co-publisher. Published contributions of N. D. Kondratiev and V. Leontiev. Privat-docent (from 1912) and professor (1918 – 1931) in Heidelberg, in Berlin until 1933. Together with Alfred Weber (see above) Director of Institute of Social and

University Statistics in Heidelberg. Emigrated to England, then to the USA, became professor at New School for Social Research.

Wilhelm Lexis (Echweiler near Aachen 1837 – Göttingen 1914), statistician and economist. Professor in several universities (in Göttingen from 1887 until retirement). Created the Continental direction of statistics (which was prepared by Poisson and Bienaymé and continued in particular by Bortkevich and Chuprov).

Sergei Nikolaevich Prokopovich (1871 – 1955), economist (the agrarian problem, cooperative system, national income) and political figure. Graduated from Bruxelles University, became member of the Union of Russian Social-Democrats Abroad. In 1905, member of the central committee of the party of constitutional democrats. Doctor of Philosophy (Berlin University, 1913). Taught in the Moscow Shaniavsky People´s University. Minister of commerce and industry, then of foodstuffs in the Provisional Government. In 1921, member of the committee on the help to the starving. In 1922, deported from Russia and continued scientific work in Berlin, Prague, Geneva, and, from 1939, in the USA. Published *Russkii Ekonomich. Zbornik* (Russian Econ. Collection) and *Ekonomich. Bulleten*.

Mikhail Nikolaevich Sobolev (1869 – not later than 1945), economist (Russia's customs-tariff policy, the peasant problem). Graduated from Law faculty of Moscow University (1891), taught geography and history of commerce in the Aleksandrov commercial school in Moscow. In 1899 – 1912 professor at the chair of political economy and statistics, Tomsk University, then, until second half of the 1920s, professor of financial law, Kharkov University and professor of Moscow Industrial-Economic Institute. Master (1898) and doctor (1912) of political economy.

Gustav von Schmoller (Hellbronn 1838 – Berlin (?) 1917), economist, historian, state and public figure, head of the *new historical school* in economics. Opponent of *theories* in social sciences.

The Correspondence (excerpts)

I mention the place of the compilation of a letter
when it is needed for ascertaining the calendar style

Letter 2. Chuprov – Bortkevich. Berlin, 29.10.1896

Many thanks for your paper[2.1]. I read it with greatest interest. I was certainly unable to derive the same benefit or pleasure as I did from your theoretical papers in the *Jahrbücher* [*f. Nat. Ökon. u. Statistik*] and the *law of small numbers*. The more delicate nuances of your thoughts escape my notice because of my ignorance in this subject and its literature. […]

Your paper provides a weighty argument to the proponents of class antagonism […]

[A detailed but definitely insufficient criticism of the mathematical reasoning in Bortkevich's law of small numbers follows, see its devastating criticism in Sheynin (2008) in which Chuprov's discussion of some mathematical reasoning had not however been studied.]

Letter 3. *Bortkevich – Chuprov, 22.10/3.11.1896*

[…] Quite unexpectedly I discovered such an attentive reader and critic in your person that no one better can be hoped for. […]

I cannot agree with you in that the title [of my law of small numbers] is deceptive. Lexis advised me to leave it and adduced one more pertinent argument: this name can be used as a repoussoir [a contrast between it and the law of large numbers]:

You, gentlemen, mathematicians and non-mathematicians, ignorant of the statistical reality, you are invariably requiring large numbers, but it occurs that small numbers are more regular.

Letter 5. Chuprov – Bortkevich. Berlin, 11.11.1896

[…] *I personally have no special liking for expectations*. Too much subjective content is somehow unintentionally connected with them, and I prefer to deal with mean, probable etc. errors[5.2]. […]

As I see it, *an excessive attention to the element of time* in problems about variance and suchlike questions is one of the fundamental deficiencies in the work of Lexis, and your previous paper is not quite free of this deficiency either. Indeed, Lexis arrived at these problems from studies of mass phenomena in social life in which time is certainly playing an essential role. […]

You are certainly familiar with Chebyshev's contribution [1845]. Is it really possible to say that his essay is within reach of readers who have no mathematical schooling? […]

Prof. Nekrasov to whom I had submitted my [student] composition, understands these problems so insignificantly, that, when noting the word *dispersia* [variance], he timidly asked me:

So do you really apply the theory of probability to the dispersion of light?

Letter 6. *Bortkevich – Chuprov, 3/15.11.1896*

[…] Following your advice, I got hold of Darboux, but will hardly read him: he exceeds my mathematical understanding. […]

Better to share the obtained rather than wait [for improving it]. […] Later, it will be possible to publish, for example, *New Studies of the Law of Small Numbers*. […]

Letter 7. *Bortkevich – Chuprov. Strasbourg, 3/15 – 4/16.11.1896*.

[…] I was able to find an extremely simple proof that the sum of the squares of the deviations is *npq* […]. And now I am prepared to insert the change in my work which Markov demanded but without generating functions and successive differentiations. [See Feller (1950/1957, § 11.1)][7.0]. […]

Notebook: Bortkevich & Chuprov, Conference. Dec. 1896

[The two scholars met five times and registered the essence of their talks in a notebook which was appended to one of the letters of their *Correspondence*. I only mention the subjects of their discussions: mean duration of life; connections between the laws of small and large

numbers; logic of the probable; mathematical foundation of the theory of statistics. However, almost nothing is stated about the last subject.]

Letter 14. *Borkevich – Chuprov, 31.12.1896/12.1.1897*

[Bortkevich discusses the *merkwürdiger Lehrsatz* of Gauss which points to § 10 of the *Theory of combinations*.]

Differentiation with respect to *f* is senseless. What do you think? […] No one reviewed this *Lehrsatz*, and, as it seems to me, this indicates that others also thought that something was wrong there. […][14.0].

Letter 19. *Bortkevich – Chuprov, 26.1/7.2.1897*

[…] [About Poincaré (1896):]

The excessively respective attitude towards Bertrand is surprising. No traces of a special acquaintance with the literature on probability are seen. The treatise is written in such a way as though Laplace and Poisson, and especially the latter, had never lived. […]

Compile an item on *Moral statistics* for the *Brockhaus & Efron Enc. Dict*. I received a permission to transfer this obligation to you. […] I do not at all wish to write it myself since there is much else on my hands. I have recently sent my piece on *Accidents* [12] and am now busy with *Illegitimate births* [for that source]. All this distracts me from the law of small numbers.

Letter 20. Chuprov – Bortkevich, Berlin, 10.2.1897

[…] I enlarge on your remark about Poincaré. That he treated Bertrand respectively but kept silent about Poisson are not independent events. Bertrand is a faithful admirer (and almost a student) of Poinsot whereas Poinsot and Poisson are enemies and rivals. They differ both in their scientific aspirations and because of personal relations.

[Chuprov refers to Poisson (1836), a paper which includes the discussion of his report of that year. I (Sheynin 2017, § 8.9) described Poisson's clash of 1836 with Poinsot but not the arguments of Dupin to whom Chuprov also refers.]

[…] Little by little I am enjoying myself while reading Gauss. How an outstanding mind is felt! Almost everything is known, read and reread in various textbooks so that you simply leaf them through, but I am reading his exposition with pleasure. […]

Letter 22. *Bortkevich – Chuprov, 3/15.3.97*

[…] You were surprised that I am compiling a piece about illegitimate births. What will you say when I'll tell you that it […] will not be published. Arseniev [the Editor of the *Enc. Dict*.] apparently had not regarded in earnest my promise to send him that piece and commissioned someone else to write it. I had sent him that piece, but *it was too late*. My deadline was 10 – 12 Feb. and I sent it on the 8th. Bear this in mind! I have sent Arsentiev a rather sharp letter and certainly will not cooperate with him anymore. […]

Letter 23. Chuprov – Bortkevich. Berlin, 20.3.1897

[…] It seems that I have found the source of regarding the law of large numbers as an imperative law, as a commandment: take large numbers and be blessed! It seems that the sinner was Littrow[23.2]. […]

Letter 25 (Postcard). *Bortkevich – Chuprov, Petersburg, 3.10.1897*

[…] From the 1st of September I am an employee of the Department of state railways. […] Thank your father for recommending me for the [Aleksandrovsky] Lyceum. No decision is yet reached. […]

Letter 26. Chuprov – Borkevich, Strasbourg, 1.11.1897

[…] In Petersburg you will have a worthwhile society, a possibility of widely applying your efforts in public work of most various kinds. But … the conditions for activity in Russia are so unbearable, the regime is so senselessly severe that for a man who had once walked away it will be difficult to get accustomed to the life there. However, the main point is that it is terrible to feel that you will abandon your scientific work. […]

Letter 27. *Bortkevich – Chuprov, 9/21.11.1897*

[…] Let me recall my latest three-hour talk about the law of small numbers with Markov. It only annoyed me. Once more he demanded a change of the name of that law. [Then Bortkevich discussed other topics, mostly the law of large numbers.]

Concerning the application of the theory of probability to statistics, Markov, ignorant of the literature, put forward uninteresting arguments and did not venture to pronounce his opinion about the scientific significance of the law of small numbers since, as he thinks, it belongs to statistics. He also believed that the publication of my work in German will all by itself prevent its publication in Russian.

[…] I informed the perpetual secretary of the Academy about the results of my talk with Markov and he confirmed that the German publication will hinder the appearance of my work in the journals of the Academy. […]

Letter 28. Chuprov – Bortkevich. Strasbourg, 18.12.1897

Your story about the *academy* and academicians and about *how they regarded your work awfully revolted me*. It is bad when the cobbler does not stick to his last, but it is hardly better if he refuses to do even that. Markov is not a judge at all! Wait a while, our day will come. *In three or four years you and I will set the pitch in statistics, I am quite sure of that.*

[…] Here in Moscow the shares of statistics as an independent science are very high. […] I heard that *you will be asked to draw up a programme of teaching statistics for the planned higher commercial* (actually, social) sciences school in which the Moscow businessmen are very much interested. […]

I have spent much time in preparing a talk for Knapp's seminar, *The rural population and the prices of cereals in Russia*, and possibly *The agrarian economy*. The talk was very successful. Knapp said that he was quite startled. […] [Chuprov described Knapp's

complimentary estimate of that talk. He ended by stating that] Russian statistics is a specimen of what statistics should and can offer to the social sciences.] […]

Just as previously, Knapp charms me, *I enjoy his lectures on applied economics*. […]

Letter 30. Chuprov – Bortkevich. Strasbourg, 5 – 10 March [1898]

[…] How I miss the lively exchange of our thoughts … […]

The Baye[30.2] theorem indeed can and should stun a man who galloped through the initial notions without analysing them. Its, so to say, psychological value for the philosophy of the probable as well as its methodological importance is very high. However, its role in regard to the formal logic of the probable […] is very, very second-rate, and, on the basis of a distinct construction of the notion of probability, it is ascertained in a word.

Not without lawfulness is the idea which in its most vivid form reads that the theory of probability is not a discovery but an invention.

[…] I have recently thoroughly delved into the history of the theory of probability. Found much instructive and curious. I see now the picture of the development of the problems connected with the *probable*, and in particular I see the main features of the history of the relations between the theory of probability and statistics with a complete distinction and even the details are almost clear. To apply myself a bit more and my own system in its historical margins will occur here and now. […]

Knapp almost by force dragged me into a talk about my plans which I had diligently avoided since I foresaw its consequences. Knapp sympathetically considered my intention of sitting here for my doctor degree, not without interest heard about my work. But he refused to regard it as a dissertation: according to its philosophical essence it does not suit a state science faculty (a faculty of university statistics?). ... *Choose some work of an agrarian-statistical kind based on Russian sources, treat some problem of the Zemstvo-statistical literature in the spirit of your talk*. […]

During autumn I became much interested in the agrarian economy and even independently from Knapp I was attracted to a work of the kind I will have to accomplish. But it is disappointing to abandon a theoretical work.

[Chuprov describes the work of Pareto and other economists.]

Letter 33. Chuprov – Bortkevich. No place (Russia), 18.6.1898

[A special police permit is necessary for possessing a typewriter.]

[…] For the Germans a work [of the kind which Chuprov has to undertake] can also be essentially interesting. […] For me, the acquaintance with all these materials can prove really interesting. […] Very promptly I have become familiar with all of them and am now

freely able to abstract 300 – 500 pages daily, and calculations are for me very easy. […]

Letter 35. Chuprov – Bortkevich, Mukhino [Russia], 27.8[.1898]

[Chuprov severely criticises the structure and even the essence of Bortkevich's paper on Pareto [15]].

My own work is going on rather successfully, but the further you get the harder the going. […] I have got used to my work and am now enjoying myself quite a lot. […]

Letter 37. Chuprov – Bortkevich. Moscow, 18.9[.1898]

[…] I am familiar with both works of Guerry (1833; 1864) and consider them very interesting. […] He has a really thought-out system. […]

Letter 61. Chuprov – Bortkevich, Strasbourg, 8.7.1901

[Chuprov passed his doctor's examination and had become Doctor of University Statistics.]

Letter 65. Chuprov – Bortkevich, Sosnovka (Russia), 10.3.1903

[Chuprov is now chair of statistics, Petersburg Polytechnic Institute.]

[…] In general, I am satisfied with my life here, but I am very tired. I am recently barely able to work. […] It will certainly become easier. This year I had to compile the entire course, one lecture after another. My course is rather complicated so it was impossible to follow any specimen.

[…] I read lectures and guide practical classes. Apart from the three obligatory hours of these classes I had up to eight hours weekly of colloquiums and talks with the students. Much time was spent on the establishment of a statistical room, no time was left for working on a dissertation [foreign degrees were not recognized] or on something else. Without being sure that […] in good time things will sort themselves out I would have sent to the devil both the free apartment and the post of a docent. […] A travel to Petersburg in both directions lasts not less than three hours. When the tram begins functioning, the time will be reduced to half an hour, but while the grass is growing the horse starves.

Letter 78. *Bortkevich – Chuprov. Charlottenburg, 11.7.05*

[…] The Department of State Savings Banks invited me to fill a new job of an actuary. They do not object to my doubling at the Polytechnic Institute. […] You had asked me whether I would agree to work in your institute and it seems that you thought about a chair of insurance […]. But here is the point. If they took me directly from Berlin, they could have appointed me professor in spite of my having no Russian degree […] because of my exceptional case. The case will be possibly different for an employee of the Finance Ministry.

[…] And there certainly are some circumstances against this. Here, I reckon on becoming a docent of insurance mathematics in the opening School of Commerce [see Supplement. Bortkevich and Russia]. […] And I am not keen to return to Russia, and in addition, to be an employee. The suggested work (life insurance by the state through the savings banks) can prove abortive, especially now. In general, the present moment is hardly favourable for a return to Russia. […]

Letter 79. *Borkevich – Chuprov. Berlin, 22.7.1905*

Very grateful for the detailed letter [lost]. You are prepared even to sacrifice the prospect of an advancement in the near future. But I had no intention at all of filling the chair of statistics, even if only nominally. […] I reckoned on the establishment of a chair of insurance. Your argument that it is impossible to connect a professorship with work [in the Finance Ministry] had wholly convinced me and I wrote to Nikolsky [at that Ministry] that I am definitively declining. […]

I added that I received an impression from a certain person that for an employee of a Ministry a participation in a ballot for a professorial post is very risky. Here, I do not feel myself badly at all. On the contrary, it is wonderful as far as the kind, the conditions, and the place of work are considered. Only one circumstance is troublesome although not really in earnest: the remuneration is comparatively small. Even if Lexis' intention of transferring to me his chair [in Göttingen] is realized, it will not essentially improve my situation.

As compared with the present, in Petersburg, if employed in the Ministry and being a professor, I will at once become rich. And I will not have to resort to mean actions and will not quit writing in German, i. e., will not abandon the occupation which I value much more than teaching. I do not appreciate myself very high as a lecturer or guide.

[…] We should bear in mind the possibility of an abrupt change [in Russia] of the general direction of the official policy if not of the entire structure of political life […].

Even under normal conditions of studies in universities I will hardly barter my extraordinary professorship for an ordinary professor of statistics in Petersburg. […]

A certain Azarevich, a Zemstvo statistician from Saratov, had visited me on the recommendation of Fortunatov. He […] became acquainted with statistics all by himself, had read my contributions and even applied the law of small numbers to statistics of fires.

Max Weber […] became interested in Russia (in economics and sectarianism), studies the Russian language and intends to visit Russia. […]

Letter 81. *Bortkevich – Chuprov. Berlin, 2.10.1905*

[…] Knapp had published his *Theory of Money* (1905)[…] He indeed negates the *economic* theory in its entirety, but considers it unnecessary to say so in his book. […] Knapp forces an open door, he intends to overcome the metallists [and to replace their viewpoint by

the idea of Staatswissenschaft, science of the state]. In spite of this, his book likely contains much valuable […].

Letter 87. *Bortkevich – Chuprov. No place, 5.10.07*

[…] Illarion Ignatievich Kaufman thinks that you are the only worthy successor to the chair of statistics [at Petersburg University] and very much wishes to become in touch with you. […] He considers himself a representative of mathematical statistics although his claim is doubtful. […]

Letter 92. Chuprov – Bortkevich. Sosnovka, 4.4.1909

[…] You are wrongfully slighting Gini. […] He gets tangled, but that will come with time. [Gini criticized the law of small numbers.]

I have received a proposal from Edgeworth to become the correspondent from Russia for the Roy. Econ. Society. […] I decided to agree. […]

Letter 103 (postcard). Chuprov – Bortkevich. Munich, 10.9[.1910]

[…] Did you read Orzhensky (1910)? While reading his book, my impression became ever less favourable […]

Letter 104. Chuprov – Bortkevich, Petersburg, 30.11.1910

[…] A fearful opponent of the Lexian theory of dispersion and of its description in my book (1909), Markov, had appeared. This autumn he began reading it and entered into a very lively and sometimes very interesting correspondence with me. He intends to conclude it by a special paper[104.1]. He is earnestly up in arms against statisticians.

[…] You will also have to beat off his attacks, since, judging by his letters, you will also have something to endure although his main target is likely my book.

I became burdened by a dullest and taxing work of preparing a census of Petersburg. […] I had to take upon myself the management of a certain section of the city. The census was badly prepared, and I have much trouble.

Letter 106. *Bortkevich – Chuprov. Berlin, 29.3.1911*

[…] [N. A.] Troinitsky sent a circular letter to the members of the International Statistical Institute concerning your election. He praised you so many times as can only be compared with his wrong accent aigus and accent graves. Such a method of an election campaign is unheard of! […]

In 1893 […] Georgievsky began to list his merits in statistics which, in his opinion, justified his incontestable right to fill the vacant chair in the [Aleksandrovsky] Lyceum. Is he not a statistician? […]

I. I. Kaufman […] is not anymore appreciating you at all, and much higher values not only Achenwall as previously but even Orzhensky. […] In your report (1912a, p. 267) you have not regrettably stressed that the precision of the result [of sampling] only depends on the absolute number of observations, although certainly given the degree of the uniformity of the entire mass. […] From his viewpoint

Kaufman is in the right that the central point is the method of selection. […]

Markov sent me his paper (1911). I will return sometime to his reasoning that normal dispersion does not at all represent maximal stability. […] Actually, he did not add anything to Kries and revealed his gross conceit since he thinks that *statisticians* had not noticed the point to which he had turned his attention. […]

Letter 108. Chuprov – Bortkevich. Sosnovka, 20.3.1911

[…] Troinitsky, who had informed me about my failure, asked me whether I agree to participate in a new ballot, so I know that I am a candidate once more. […] I do not understand his attitude to my election. […]

This half-year had been more agonizing than during the revolutionary period, and during some time I had all but decided to abandon teaching and go to the newspaper[108.2]. […] Now the situation is not favourable at all. […]

Letter 109. *Bortkevich – Chuprov. No place, 22.10.1911*

It was wrongful for you not to come to The Hague. Troinitsky had a grudge against you. […] He told me that he had patronized you in memory of your late father. […] My report [57] compelled G. von Mayr to announce that in statistics, mathematical formulas are useless. […]

Letter 110. Chuprov – Bortkevich. Petersburg, 12/25.10.1911

The main reason why I did not go to The Hague was that I had learned too late about my election. […]

Letter 116. *Bortkevich – Chuprov. Berlin, 15.3.1912*

[…] Astronomer [C. V. L.] Charlier began to publish papers in English on math. stat. (*Arkiv for Matematik, Astronomi och Fysik*, Bd. 7, No. 17)[116.1] and showed, already in his first paper, that he did not understand Poisson.

Letter 122. Chuprov – Bortkevich. Sosnovka, 14.9.1913

[…] Not only Furlan (1911) but even more importantly Gini had anticipated your work on the distribution of incomes. He sent me a number of his reprints. One of the papers (Variabilità e mutabilità. *Studi Economico-Giuridici R. Univ. di Cagliari*, Anno 3, parte 2. Bologna, 1912) is essentially devoted to the propaganda of the mean difference as a measure of the mixed character of the mass. […] He applies in great detail sums rather than integrals. In particular, he establishes the relation between the mean square difference and the mean square error. He also discusses the application [of that] to the statistics of the distribution of incomes and refers to the work of other Italians who had apparently applied the same measure under his influence. […] I can send you Gini. […]

Letter 123. *Bortkevich – Chuprov. No place, 21.11.13*

[…] You indicated Gini's papers published in […][123.1]. The local [Berlin] Royal Library does not have this source so that I have every right to disregard them.

[…] Sorrowful the man will be if he calculates the correlation coefficient as Kaufman did. Non-mathematical statisticians ought to abstain completely from adducing formulas. […] I am wholly absorbed in the doctrine of incomes. […]

Letter 124. Chuprov – Bortkevich. Sosnovka, 17.11.1913

[…] I agree that all this public is rather hopeless, but in spite of the muddle and the lot of nonsense we ought to welcome the turn to the theory and mathematics. The next generation will stand somewhat higher.

This autumn, in connection with the law of large numbers, I am looking over physics, radioactivity, heredity etc.[124.2] from a logically formal side. It is tempting to be occupied with a serious work about the history of how the statistical viewpoint won over modern science. […] But to make the result to some extent neatly an excessive and laborious amount of preparatory work is needed. So in a fortnight I will only read a report and by Christmas perhaps write a paper in German. […]

In connection with the work of my student[124.3] (who intended to develop and justify the method od Hooker (1908) […]), when improving the composition of his not really elegant calculations I had at the same time attempted to render by expectations the constructions of the English into the language of pure theory of probability. The exaggerated empiricism of the English (the urge towards shielding mathematical probability by frequency) impedes them to explicate their ideas quite distinctly in the logical sense. […]

Letter 125. Chuprov – Bortkevich. Sosnovka, 19.11.1913

[…] We are disordered once more, this time because of an *academic* issue […]. An almost complete strike. […] Today no lectures were read in the morning. […] All this is wretched. I have no more strength and time is spent on various conferences which are barely useful but unavoidable. […]

How do you justify that EA/EB can replace EA/B[125.1]?

Letter 126. *Borkevich – Chuprov. Berlin, 7.12.13*

[…] See justification in [59, p. 55]. The replacement can only be made if M(B) is insignificant as compared with M(A)[126.0] […].

Letter 133. Chuprov – Bortkevich. Sosnovka, 4.3.1914

[…] For the third week now I do not touch my work. Conferences, candidate dissertations, editorial work with the translation of Yule[133.2] and a lot of matters, significant or not […].

Letter 135. *Bortkevich – Chuprov. Berlin, 27.3.1914*

[…] About shelving the [Lexian] Q I do not agree with you at all.

Letter 137. Chuprov – Borkevich. Paris, 2.7.1914

[…] Did you see the papers of Soper (1914) and Whitaker (1914) […] which directly concern you? […] The second paper is rather properly directed against you by proving that in your examples the agreement of experience and theory is bad. […] Soper provides tables and thus develops your paper. In that same issue of *Biometrika* there is a note by Student (1914) which partly covers the work of my student Anderson (1914).

Letter 138. *Bortkevich – Chuprov. Berlin, 3.7.1914*

[…] The paper of Whitaker (1914) is wholly stupid but typical of the purely formalistic direction of the Pearsonian school. She proves that [the formula $(p + q)^n$ describes suicides with positive fractional values of all the parameters and, in addition, with q and n taking negative values].

Letter 139. Chuprov – Bortkevich. Paris, 5.7.14

[…] *The reproach upon Whitaker seems not quite proper*. True, I had no time to read her article, I only smelled it. Thus, I do not know how to calculate the parameters, but the idea itself to begin with a binomial seems to be not uninteresting. And since for most of your examples, given normal dispersion, it provided absurd results, an explanation is required. The matter can be rather simple but in any case it justifies scrutinizing. It would have been worthwhile to ponder over purely experimentally selected examples. […]

Letter 141. Chuprov – Bortkevich. Sosnovka, 25.6.1914

[Chuprov discusses the work of E. Abbe who was also mentioned in earlier letters. The main point is that Abbe had derived the chi-square distribution, see Sheynin (1966; 2017, § 9B-1).]

Letter 142 (postcard). Chuprov – Bortkevich, Stockholm 22.8.1917

[This letter and the next one are written in German, see beginning of my Introduction. Chuprov discusses some of his previous works, see Sheynin (2011, §§ 14.2 and 14.4.]

Letter 143. Chuprov – Bortkevich. Stockholm, 21.9.1918

I am glad that you succeeded in taking away your sister. The conditions of life in Petrograd are now worse than at any time previously. All the news which reach us are such that I am once more postponing my departure. The prospect of being retained by the Bolsheviks as a hostage is perhaps [not?] really attractive. [Die Perspective von der Bolscheviki als Geißel [Geisel] festgehalten zu werden ist eben [nicht] wohl allzu verlockend.] My chances [here] are pretty high.

[…] In the near future I will deliver a report to the Society of Actuaries [in Sweden][143.2]. I will try to present it in Swedish. […]

In Pearson's latest works I detected horrible mistakes. His insufficiently thought-out approximate methods awfully avenge themselves exactly when he attempts to go further than the first approximation[143.3].

Letter 144. Chuprov – Bortkevich. Stockholm, 3.11.1919

[…] Papers [74; 76] are very curious and extremely intelligible[144.1]. […] Is there anywhere a less gratifying problem than a description of statistics of population on 41 pages [72]? The second part is very interesting, especially the chapter about Malthus. [Some criticisms nevertheless follow.]

This year I have also abandoned mathematical statistics. After deciding that it was senseless to go to Petrograd, I had to think about the means of subsistence. […] There were many most various offers. […] It became clear […] that all our cooperative establishments, around which concentrated everything still viable in Russia, had remained entirely ignorant of everything going on abroad. They needed information and I agreed to tell them what is happening in the world economy.

[…] Twice monthly we put out a rather voluminous lithographic bulletin[144.2] […] but contacts [with Russia] which had always been far from perfect were abruptly severed. I will hold out until the end of the year. […]

Perhaps will occupy myself with […] the business of publication. A Russian – Scandinavian publishing house is being established and I am offered the management of its section of economics. […] If organized seriously and widely enough, I will not mind to undertake it. [Chuprov outlines his plans of studying in the most general way connections between two or more variables.] Life is here unbearably expensive. […]

Letter 146. Chuprov – Bortkevich. Dresden, 4.9.1920

I was unable to find a flat and am now the only tenant, rent two large light rooms in a small apartment. […] Czuber sent me reprints of two of his papers (1920a; 1920b) from a new international statistical journal, *Metron*, edited by Gini, both of little interest.

Letter 151. Chuprov – Bortkevich. Dresden, 20.1.1921

I began putting in order the third part of my paper (1918 – 1919 and 1921). […] But only now the *revolutionary character* of an inference from my general formulas which arrested my attention is embarrassing me.

If $m_1 = Ex_i$ and $\mu_2 = E(x_i - m_1)^2$ remain constant during all the trials and in addition if, for any pair of trials, $\mu_{1,1} = E(x_i - m_1)(x_j - m_1)$ is also constant, the expectations of *y*, *w* and *u*, see (1918 – 1919, First essay, chapter 1, end of § 3], are equal to each other and equal to $\mu_2 - \mu_{1,1}$. And if, once more in addition, μ_2 is constant for any natural *r*, $\mu_{r_1 r_2}$, $\mu_{r_1 r_2 r_3}$ etc. (i. e., if not only the law of distribution but also *the law of connection between any pairs, threes,* etc. trials is constant), then the argument on pp. 224 – 225 (1918 – 1919) holds and $Ew/y = Eu/y = 1$. From a mathematical point of view this is a new curious generalization of the coefficient of dispersion on connected trials but statistically it resembles a *complete denial of the Lexian theory* of stability. EQ^2 is unity not only if $\mu_{1,1} = 0$, i. e., when the trials are independent, but also if $\mu_{1,1} = c$, where *c* is any positive or negative constant. The meaning of the criterion $EQ^2 = 1$ is quite different from

that which is understood by the Lexian theory of dispersion. The Lexian method cannot empirically distinguish between normal and non-normal dispersion.

The **third powers** of the deviations do not help either since we have not only for independent trials, given *r* series consisting of *n* trials each,

$$\mathrm{E}\frac{nr}{(nr-1)(nr-2)}\sum_{i=1}^{nr}[x_i^{'}-x_{(nr)}]^3=\mathrm{E}\frac{n^2r}{(r-1)(r-2)}\sum_{i=1}^{r}[z_i-x_{(nr)}]^3=\mu_3.$$

Indeed, in the more general case of coincident laws of connection between trials we have [instead of μ_3] $\mu_3 - 3\,\mu_{2,1} + 2\mu_{1,1,1}$.

Incidentally, bear in mind that my formulas (32) – (36) on pp. 97 – 98 of the third essay in (1918 – 1919) are wrong[151.1] […]. In formukla (32), the denominator of $\mu_{[3,2]}$ should be n^2r^2 rather than *nr*, and, in formula (33), it should be again n^2r^2 rather than n^2r. Then, on p. 98 there are a few misprints. In formula (36) the coefficient of the double sum should be $(nr-2)(r-2)\div[r^3n(n-1)(n-2)]$.

In formula (61) on p. 109 $(nr-2)(r-2)$ should replace $(nr+r-1)(nr-1)(r-2)$.

It is somewhat more complicated for the **fourth powers** of the deviations since not only μ_4 but in addition μ_2^2 enter in

$$\mathrm{E}\frac{1}{n}\sum_{j=1}^{N}[x_j'-x_{(N)}]^4.$$

However, when replacing the fourth power of the square bracket by the second power we can eliminate one of those two magnitudes. Then for independent trials we will have the expectation of

$$\frac{n^2r^2-3nr+3}{nr(nr-1)(nr-2)(nr-3)}[\sum_{j=1}^{nr}(x_j'-x_{(nr)})^2]^2 -$$

$$\frac{1}{(nr-2)(nr-3)}[\sum_{j=1}^{nr}(x_j'-x_{(nr)})^4]=\mu_2^2=$$

the expectation of

$$\frac{n^2(r^2-3r+3)}{r(r-1)(r-2)(r-3)}[\sum_{i=1}^{r}(z_i-x_{(nr)})^2]^2-\frac{n^2}{(r-2)(r-3)}[\sum_{i=1}^{r}(z_i-x_{(nr)})^4].$$

However, for independent trials we will once more have [the initial and the final expressions remain as they were but the expression in the middle will be not μ_2^2, but] $\mu_{2,2} - 2\mu_{2,1,1} + \mu_{1,1,1,1}$].

The same story is thus repeated, and the following conclusion is outlined: At best, the comparison of the scatter of the partial means from the general mean with the scatter of the separate results from it ascertains whether an invariable law of connections between the trials

exists as a *basis*. It is impossible to ascertain in the same way the existence of a normal dispersion.

The simplest example of a stochastic pattern with an invariable law of connections is provided by a ticket unreturned in an urn which you call the second *Spielmodus* (manner of game). Denote by N the total number of tickets in the urn, then $\mu_2 - \mu_{1,1} = [N/(N-1)]\mu_2$,

$$\mu_3 - 3\mu_{2,1} + 2\mu_{1,1,1} = [N^2/(N-1)(N-2)]\mu_3,\ \mu_{2,2} - 2\mu_{2,1,1} + \mu_{1,1,1,1} =$$

$$[N^3/(N-1)(N-2)(N-3)]\,\mu_2^2 - [N/(N-2)(N-3)](\mu_4 + 3\,\mu_2^2).$$

The usual Lexian methods are unable to distinguish empirically this case from the case of normal stability.

The next question therefore presents itself. Can the normal character of the dispersion be somehow established empirically or whether in all cases in which the expectation of one or another entirely empirical expression μ_2 occurs when the trials are independent and $\mu_2 - \mu_{1,1}$ occurs when the law of connection between the trials is invariable?

I think that an empirical separation of $\mu_{1,1} = 0$ from the case in which $\mu_{1,1}$ is a non-zero constant is however possible, for example, by iterations. This, however, should be thoroughly studied. These topics exceed the limits of my work which began by the publication in *Biometrika* and which was devoted to the derivation of formulas but not to their statistical use and I therefore intend to describe my considerations in a special note (1922g). […]

Veniamin Khwostov, a lawyer and professor of Moscow University, took his life which produced a strong impression in Moscow, even on the Bolsheviks. And, as I was told, became one of the incentives for improving the nourishment of the professors. […] Lyapunov took his life […]

Even in summer I had sent Mises a reprint of my Scandinavian paper but have not received any response. It was perhaps lost. On occasion have a look at Smoluchowski*. It is not without interest both in the positive and negative sense. […]

Letter 152. Chuprov – Borkevich. Dresden, 5.2.1921

I have drafted my paper (1922g). It came out even stronger than I had written to you. Without prior information it is impossible to distinguish mutual independence of the trials from the case of invariable connection between them, and not only by the means of the Lexian method, but by any other methods either.

I had tried out various approaches. A few times I thought that I had succeeded, but after making the necessary calculations and eliminating the last prior magnitudes from the formulas they invariably lost everything which distinguished both those cases. I experienced most troubles with the insufficiently known to me *iterative* approach, but managed little by little. I cannot say, managed exhaustively, but sufficiently for convincing myself that there also the matter is the same. Here, for example, is an indicative calculation. […]

Letter 153. Chuprov – Bortkevich. Dresden, 17.2.1921

[…] It seems that I have informed you that a few years ago I had written Pearson a number of letters […] indicating errors made by him and his students. No answer came […]. My letters did arrive and made an impression. […] One of my students […], Mordukh, tells me that an editorial in *Biometrika* [vol. 12, 1919, pp. 259 – 281] acknowledged me and systematically corrected the committed transgressions[153.0]. […]

Letter 158. Churov – Bortkevivh. Dresden, 27.5.1921

[…] My Mordukh is carried away by mathematics. He has a pretty knowledge of this science and a high level of mathematical ability. […]

Letter 162 (postcard). Chuprov – Bortkevich Dresden, 23.8.1921

[…] I have lately somehow turned away from the *philosophy* of the probable to mathematics. With a greater definiteness of its conclusions and more precise considerations it is stronger attracting me. […]

Letter 167. Chuprov – Bortkevich. Dresden, 13.2.1922

[In spite of an insistent letter of Alfred Weber, Chuprov hesitates to move to Heidelberg university, but, for an unexplained reason, other cities, Dorpat (Tartu), Varna, Riga, Prague, seem good enough. He describes the difficulties of working in Heidelberg.]

I know about the existence of the Kharkov Bernstein although my notion about him is slight, and I am somehow confusing him with the Göttingen [Felix] Bernstein. He is a pure mathematician almost belonging to the French school[167.4]. I saw some of his works, but they did not adjoin my own interests. I think that as a statistician he should be about the type of Polya.

Letter 168. Chuprov – Borkevich. Dresden, 16.2.1922

[Chuprov continues to discuss the difficulties of working in Heidelberg.]

To live only on literary earnings is difficult and it will become more difficult with age, the more so since the [favourable] currency conjunction […] will end sometime […].

[Chuprov hopes that, if the New Economic Policy in Russia develops, he will be able to retrieve some of his materials from there.]

Letter 176 (postcard). Chuprov – Bortkevich. Dresden, 2.8.1922

[…] Many thanks for [M. V.] Ptukha. He is a bit hard up for scientific inspiration and not quite robust in considerations about numerical relations. […] But on the whole his work is decent and of a quite good quality[176.1]. […]

I am busy with my student [Oskar] Anderson who is now in Budapest. He had written a sequel to his work about the so-called method of variate-difference correlation [Anderson (1923)*][176.2] and sent it to me for reviewing. His manuscript is voluminous. Calculations are extremely entangled and the approaches are not quite

proper. Difficult to trim it up. I had spent about a week on this work but had not finished. But there is something of interest, and it is necessary to launch properly the fellow and his work.

Letter 177 (postcard). Chuprov – Bortkevich. Dresden, 22.8.1922

[…] Do you have [reprints] of Charlier (1909) and of his theorems of Poisson and Lexis and of those papers in which he constructs his curves of the A type? Can you send me these reprints for a short time?

[…] These days I have been busy with the works of my students. We have put a finish on Mordukh (1923). The work of Anderson is interesting in many respects, but awfully unwieldy and confused. Together with Kohn and Chetverikov I am working on *Vital Statistics of Russia during the War* for the Carnegie foundation. The business is near to completion[177.4]. […]

Letter 178 (postcard). Chuprov – Bortkevich. Dresden, 27.9.1922

[…] I have abstained from sending reprints to Falkner-Smit [M. N: Smit-Falkner] since I dimly understand that she hardly belongs there to the group of those the ties with whom I value. And there exists a confirmation of my understanding: she is apparently reckoned among the *red* professors. In any case, at the outset of Bolshevism she was a communist[178.2]. […]

Letter 179. (postcard). Chuprov – Bortkevich. Dresden, 11.10.1922

During Monday and Tuesday I was in Berlin to see some of the Muscovite exiles[179.1]. Came to your place but no one answered my knocks. […] With regard to Falkner-Smit your situation is different. I ought to allow stronger for the mutual relations there.

Among the exiles is [A. A.] Kisewetter. […] In our universities he is now considered the best specialist in the history of Russia. […]

Letter 180 (postcard). Chuprov – Bortkevich. Dresden, 13.2.1923

[…] I received a letter from [E. E.] Slutsky from Kiev. He attended the statistical congress in Moscow. Tells me that a mathematician from Central Asia [Bortkevich: Romanovsky] had read a report. By like methods he obtained some of those results which I had published in *Biometrika*. It's amusing. […] Slutsky returned to mathematical statistics. […] Laments the lack of fresh literature. […]

Letter 181 (postcard). Chuprov – Bortkevich. Dresden, 12.6.1923

[Chuprov discusses the personal difficulties experienced by Andersson as the Editor of *Nordisk Statistisk Tidskrift.*] I intend to send him a testimonial about his journal. […] His journal will always remain militant. […]

Letter 186 (postcard). Chuprov – Bortkevich. Dresden, 25.11.1923

I returned home [from Rome]. […] I wrote and read out my report in Italian (but before that I read it with an Italian). Everything went quite successfully[186.1]. […] The session was colourless. […]

Finding means of subsistence will soon become serious. […]

Letter 187 (postcard). Chuprov – Bortkevich. Dresden, 20.2.1924

[…] My money reserves are being exhausted and the rent is heightened. […] It seems that my happy existence as a private scientist will soon end. […]

Letter 192 (postcard). Chuprov – Bortkevich. Berlin, 5.9.1924

[…] Apart from Prague I think about Riga. […]

Letter 195 (postcard). Chuprov – Bortkevich. Dresden, 11.1.1925

These days I am moving to Prague. [...] It is difficult to say what happens. […]

Letter 197 (postcard). Chuprov – Bortkevich. Prague [precise address], 27.1.1925

I succeeded in finding a comfortable abode. […] It is a rare piece of luck, but there are rather essential drawbacks. […] I am awaiting information from you about the journal which you and Mises are envisaging[197.2].

Letter 198. Chuprov – Bortkevich. Prague, 2.4.1925

[Chuprov describes various possibilities of work which do not seem however good enough.] I can settle down in Geneva where prices are comparatively low. Gulkevich has a small three-room apartment and one of them is held for me. Even without extra earnings my reserves will allow me to hold out for more than a year. But it seems terrifying to stake on this card. [What will he do after that? And illnesses are also possible.] […]

Letter 199. Chuprov – Bortkevich. Prague, 2.4.1925

[Chuprov describes the possibility of living and working in Riga. He will move there if the promised conditions are ascertained.]

Letter 200 (postcard). Chuprov – Bortkevich. Prague, 24.4.1925

[Oslo University is more inviting than Riga] but *the decision is not easy*.]

Letter 201 (postcard). Chuprov – Bortkevich. Prague, 26.5.1925

If your information about Heidelberg will be confirmed, nothing better can be wished. However pleasant Oslo is for me, […] it is more proper not to block the road to local candidates. […]

Here in Prague nothing worthwhile is yet seen. […] E. Schoenbaum found out that I am in Prague and became very interested in me. […] A lively mind, broad scientific interests, continues to follow the literature although heads an enormous establishment. He began speaking about me in the Ministry of Peoples Education. And Struve spoke about me in the Ministry of Foreign Affairs. […]

Letter 202 (postcard). Chuprov – Bortkevich. Prague, 4.6.1925

[…] For the time being *I adjoined the Prokopovich Economic Cabinet*.

Letter 203 (postcard). Chuprov – Bortkevich. Prague, 9.6.1925

I congratulate you on your election [as honorary member of the Royal Statistical Society]. […] No honorary members were elected in 1920 – 1923. In 1923 (!) they elected *Charlier, Czuber and me*. And in 1924 […]. After the war, you are undoubtedly the first German. […]

It is annoying that politics prevents Gumbel from the business which for him as a mathematician who transforms himself into a statistician is more than sufficient. Did the present shock make a proper impression upon him[203.0] or is he incurable?

Letter 204. Chuprov – Bortkevich. Prague, 1.7.1925

Prokopovich told me what he had heard from you: somewhere here I had either lost an election or was struck from the ballot papers because of my publication in *Vestnik Statistiki*[204.1]. Please let me know in more detail what had reached you and where did that happen. […]

Letter 205. Chuprov – Bortkevich. Prague, 2.7.1925

[…] For a long time I thought of studying sampling once more and in real earnest. […] *My relations with the Charles University in Prague seem to begin by an invitation to deliver* […] *a few lectures*. They have such a custom. […] Schoenbaum is planning my invitation. I think that my subject will be *The role of sampling in the theory of probability and statistics*. Actually more interested are the university mathematicians. […]

In Rome [at the session of the International Statistical Institute] I think about suggesting at least an initiative of establishing a statistical bibliographic edition, something like the *Fortschritte der Mathematik*[205.4]. […]

I wholly agree with you about the *harm occasioned by the ignorance* of *sylleptik*[205.7]. It is even more harmful than the ignorance of statistics. […] As to the prophets of conjuncture, they, in general, and W. M. Persons[205.8] in particular, are muddle-headed. […]

Letter 206 (postcard). Chuprov – Bortkevich. Prague, 15.7.1925

[…] I have obtained some rather amusing results in sampling. They distinctly reveal that in many cases the urge to attain representativeness of samples is a prejudice. When solving some problem a non-representative sample can provide better results [206.1].

[…] I received from Romanovsky a long an interesting manuscript written in poor English and sent it to *Metron* (1925) […].

Letter 207. Chuprov – Bortkevich. Prague, 23.7.1925

[…] Recently something aches a little, most often it is the neck and the back of the head or […] or […]. By observing myself and experimenting I have established almost surely that it is not rheumatism. This is my theory. There are external and internal causes. Internal causes [become more pronounced with age]. […] But possibly the most important is that heat is very good for me. […]

Letter 208 (Postcard). Chuprov – Bortkevich. Prague, 4.8.1925

[…] [Chuprov suffers from acute sleeplessness.]

Letter 210 (postcard). Chuprov – Bortkevich. Rimini, 9.9.1925

[…] I continue to look sour to such an extent that I finally intend to consult a doctor. […] Each evening the temperature is higher than 38°C. […] Feel irritation in the cheek-bones and cough. Most likely the main cause is connected with the cheek-bones. […]

Letter 211. Chuprov – Bortkevich. Geneva, 19.1.1926

Excuse me for writing in pencil. For more than six months now I am shivering with fever (recently once more high temperature, near to 39°, often higher and once even 40.°2) which confines me to my bed, am unable at all to reach my desk. […]

I have once more received a number of attractive offers from Russia but I do not want to go there. […] The doctors are unable to reveal confidently the cause. At first they thought that it was a malaria-like disease. […] Then they diagnosed *endocarditis lenta*. With this diagnosis I arrived in Geneva. [New detailed investigation began without any definitive results.] […] During these months I have very much weakened.

Notes

2.1. It will become clear that Chuprov discussed Bortkevich's paper [11].

5.2. Chuprov had to calculate expectations time and time again.

7.0. Nowadays this method is well known. It is applicable to random variables in general, not only to the case of the binomial distribution. Bortkevich's attitude was unreasonable.

14.0. That *f* was a constant, one of the limits of an integral, and Bortkevich had shown his insufficient knowledge of mathematical analysis. In Letter 15, which I did not translate, Chuprov explained that Gauss was not mistaken, but, curiously, he did not say anything about the differentiation of an integral.

23.2. In 1828 – 1831 the astronomer Littrow published papers on life insurance, widow funds, and on the influence of the weather on the spread of cholera epidemics. Two years later appeared his book on the applications of the theory of probability* strongly affected by Laplace's *Essay* (1814). The *sinners* were apparently those who had been uncritically applying the law of large numbers.

30.2. Chuprov mistakenly translated *Bayes* as though that scholar was a Frenchman. What exactly did he mean by saying *galloping* […] *without analysing*? Once more, he was mistaken: Bayes was very deep (Sheynin 2017, § 5) and Chuprov himself at least partly said so. Anyway, Chuprov, a self-conceited unfledged chick, was able to become a statistician of the very first rank.

104.1. See Ondar (1977/1981). Markov did not publish any concluding paper but reviewed Chuprov's *Essays* (Markov 1911).

105.1. [I have replaced this comment from Letter 66 which is omitted from the translation.] Troitsky was a founder-member of the International Statistical Institute and its vice-president in 1897 – 1913 as well as president of Russia's Statistical Council (Nixon 1960, p. 161). At least later that Council had been called Central Statistical Department.

108.2. By that time Chuprov had published no less than 44 articles in the Moscow newspaper *Russkie Vedomosti* and 22 articles later. Their list is in Sheynin (1990/2011, pp. 181 – 182).

116.1. By 1906 Charlier had published three papers in that journal.

123.1. In 1910 – 1912 Gini published four papers in the *Studi … Univ. Cagliari* at least one of which Bortkevich had *every right to disregard* (see Letter 122).

124.3. This student was Oskar Anderson. His later works were published in 1923 and 1926 – 1927. In connection with his paper of 1914 he received a letter from Pearson who had attributed the Hooker (1908) method, the variate difference correlation method, to Cave-Brown-Cave (1904, p. 407ff), see Sheynin (1990/2011, p. 153).

In 1925, anticipating the publication of his last paper in *Biometrika*, Anderson sent two letters to Pearson with an explanation of its essence. Ploshko & Eliseeva (1990, p. 183ff) described the subsequent history of the variate method. Strecker (*Jahrbücher f. Nationalökonomie u. Statistik*, 2004) devoted two papers to its applications. He was also the author of the item *Variate difference method* in vol. 9 of the *Enc. Stat. Sci*. in which that method was attributed to Anderson and Student (1914) whereas Hooker was not mentioned.

125.1. Chuprov himself repeatedly studied the possibility of that substitution, see Ondar (1977/1981, Letter 80 of 1916, pp. 94 – 100) and Chuprov (1918 – 1919, p. 156; 1922).

126.0. Bortkevich [59, p. 19, formula (53)] had not directly explained the meaning of M, but he noted that (in my notation) $M^2 = E\xi^2 – (E\xi)^2$. In the next letters which I did not translate, Chuprov discussed this point but suffice it to refer to his paper (1922a).

133.2. Chuprov edited the translation of Yule (1912) which was never completed (Sheynin 2017, p. 31).

143.3. See Letter 153.

144.1. Chuprov apparently discussed Bortkevich's papers [74] and [76] which only appeared in 1920. He had probably seen their proofs.

144.2. *Biulleteni Mirovogo Khoziastva* (Bull. of World Economy), 36 issues. Not a single one was found.

151.1. Chuprov corrected all formulas in his copy of that contribution and the Russian translation of (1918 – 1919) took into account his corrections.

153.0. See Sheynin (2011, p. 75).

167.4. Bernstein published many papers abroad, mostly in the *C. r. Acad. Sci. Paris*, was elected foreign member of that Academy (1955).

176.1. Elsewhere Chuprov mentioned Ukrainian mortality tables compiled by Ptukha. Here, however, he apparently meant Ptukha's indices of nuptuality.

177.4. See also Sheynin (1990/2011, § 11.2).

178.2. In 1930, Smit-Falkner called on statisticians to *become the OGPU of the scientific thought in statistics*. OGPU was the predecessor of the KGB. Next year, 1931, she noted with satisfaction that *the crowds of arrested saboteurs are full of statisticians* (literal translation). She herself likely helped to fill those crowds … And in 1934 she ignorantly declared that Gaus (yes, Gaus!) *wanted to subdue ferociously the world* by a single curve of distribution. Finally, in 1960 the unsinkable Smit, now a correspondent member of the Soviet Academy of Sciences (certainly!), declared that the doctrines of Marx and Lenin had penetrated the *essence* of economic laws. That same year Kolmogorov and Kantorovich thought it necessary to change economic planning. See Sheynin (1998).

Chuprov's *dimly* feelings were thus confirmed. Moreover, Chetverikov informed Bortkevich that Smit had become the leading figure in the *Statisticheskii Vestnik* (the only Soviet statistical journal) and *the conclusions are clear* [vi, Letter 7].

179.1. In September 1922 a hundred and sixty public figures, philosophers and historians, were expatriated from Soviet Russia. Among them were S. N. Prokopovich and A. A. Kisewetter who are mentioned in the *Correspondence*. See Courtois et al (1997, pt. 1, end of chapter 5).

186.1. Chuprov's report was called *Statistical Culture in Russia* (his letter of 24.11.1923 to Guldberg).

197.2. The publishing house Teubner decided to launch a new statistical journal and Chuprov repeatedly discussed this plan in his letters. No such journal had nevertheless appeared.

203.0. This episode concerned Gumbel's unreasonable statement which indirectly insulted the German victims of WWI. On Gumbel see Sheynin (2003).

204.1. Eliseeva & Dmitriev (1997) reprinted an excerpt from the reminiscences of D. A. Lutokhin who had described Chuprov's life in Prague. Chuprov was given a hostile reception there since he did not severe his scientific ties with his colleagues in Moscow and published two papers there (1924b); its somewhat extended version appeared in *Vestnik Statistiki*, vol. 19, 1925). Chuprov himself (letter to Andersson of 21.3.1925) noted that the Russian colony in Prague was torn apart by acutest political discords. We may note the reasonable difference of his attitude towards Smit and honest Soviet statisticians.

205.4. *Jahrbuch über die Fortschritte der Mathematik*, a journal of abstracts, 66 volumes, 1868 – 1942. The International Statistical Institute published a journal of abstracts *Statistical Theory and Method* (1959 – 2005).

205.7. In 1882, G. Rümelin introduced the term *Sylleptik* which meant statistics in the widest sense (Bortkevich [66, p. ix]). This new word did not take root and Bortkevich (p. 2) began understanding it as the [deterministic] regularity existing between statistical magnitudes. His example: the formal theory of population,

205.8. Persons, one of the compilers of the *Harvard Conjuncture Barometer*. Statisticians unsuccessfully attempted to foresee the economic climate by applying this barometer (i. e., by issuing only from empirical data). They miserably failed to foresee the world economic crisis of 1929.

206.1. These results had not been published. Anyway, the term representative sample, is vague (Kruskal & Mosteller 1988).

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I have included all the sources mentioned in the *Correspondence* and all other known to me items published by Bortkevich (these items are unnumbered) and, in addition, a few general bibliographic sources. The bibliography of Chuprov is reprinted from my previous book (Sheynin 1990/2011) in a somewhat shortened way.

Abbreviation

AGSA = *Archiv für die Geschichte des Sozialismus und der Arbeiterbewegung*

ASWSP = *Archiv für Sozialwissenschaft und Sozialpolitik*

Hdwb = *Handwörterbuch*

GL = Gorky Library in Moscow University

JGVV = *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich*

JNÖS = *Jahrbücher für Nationalökonomie und Statistik*

NST = *Nordisk Statistisk Tidskrift*

SAT = *Skandinavisk Aktuarietidskrift*

ZgVW = *Zeitschrift für die gesamte Versicherungs-Wissenschaft*

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VI

A. L. Dmitriev

Letters from N. S. Chetverikov to V. I. Bortkevich

Voprosy statistiki, No. 12, 2008, pp. 69 – 75

The letters of Nikolai Sergeevich Chetverikov (1885 – 1973) to Vladislav Iosifovich Bortkevich (1868 – 1930) are very interesting not only for the history of the national statistical science. They describe the scientific problems which troubled both scientists, and contain important information connected with the immortalization of the memory of Chuprov.

Bortkevich left Russia in 1901, but extensively corresponded with many Russian economists and statisticians including S. S. Kohn(1888 – 1933), A. A. Kaufman (1864 – 1919), Ptukha (1884 – 1961) [iii and iv], P. B. Struve (1870 – 1944) (and certainly Chetverikov) et al. Scientifically, he was very exacting both to himself and others, he thoroughly followed the work of those Russian scientists who were interested in the same problems as he himself was, and never forgot to send them reprints of his papers.

It was Chetverikov who initiated the immortalization of the memory of his teacher, Chuprov. He began this work soon after Chuprov's death by preparing the publication of Chuprov's collected writings and letters[1]. Only in the end of the 1950s and only partly Chetverikov was able to accomplish this work, see Chuprov (1959[2], 1960a, 1960b).

Also described in the letters translated below is the worried atmosphere which had appeared in the Conjuncture Institute directed by N. D. Kondratiev (1892 – 1938). Chetverikov worked there from 1923 to 1929 as a consultant, head of the section of scientific methodology and deputy director. While there, he actively developed mathematical methods of analysing time series and studied the application of the theories of correlation and probability to economic phenomena.

In 1929 the Institute was closed and Chetverikov began working in the Fruit and Vegetable Institute as head of the department of prices. However, he was soon arrested together with other former workers of the Conjuncture Institute, accused of sabotage and sentenced to do four years in a forced labour camp.

After returning, and in 1935 – 1937 Chetverikov became a scientific worker in the Institute of Medicine and Hygiene. Then he was arrested once more and, after returning to Moscow, worked as a

planner, taught in a technical school and in the Moscow Radiological Institute. Then, in 1949 – 1959, Chetverikov tended his brother Sergei (1880 – 1959), a renown biologist and geneticist, in Nizhny Novgorod.

After returning to Moscow, he actively translated foreign scientists, see in particular the collection (1968) which he edited and Cournot (1843/1970)[3]. In 1963 appeared his *Selected Works* (reprinted in 1975).

For a long time Bortkevich's posthumous papers were thought to be lost, but Guido Rauscher (Vienna) discovered them in Uppsala (Sweden) University. Oscar Sheynin sent me copies of some of these papers including the letters translated below for which I sincerely thank him. He himself published the correspondence of Bortkevich and Chuprov (cf. [v]).

Letters NNo. 1, 2, 6 and 7 were written by hand, others were typed and corrections inserted by hand in NNo. 3 – 5 and 8. The underlining of some words and phrases had been made either by Bortkevich or Chetverikov. Chetverikov dated all the letters except No. 5 which was likely written in the end of 1925 or beginning of 1926, but certainly before the death of Chuprov (19 April 1926). Letter No. 1 was written in German and translated into Russian by L. A. Tsapalin.

Letter No. 1, 25 August 1923

I have received both parcels with your works and am sincerely grateful. Particularly interesting was your paper *On the theory of dispersion of statistical series*[4]. In his letter Prof. Chuprov (Dresden) had acquainted me with your main ideas, but here, in Russia, I was certainly unable to see the paper itself. Perhaps you will be interested to know that here, in Moscow, an auxiliary method for measuring correlation was developed. Its main idea belongs to my friend V. S. Yastremsky[5] and it is apparently akin to your own idea.

When studying the dependence of the harvest on the weather, we (?) concluded that even smallest correlation coefficients for the region of Poltava (rains in May – June and the harvest of the same year) allow us to conclude safely that the correlation itself could have changed over those 22 years during which we have the necessary results. An elementary graphical method convinced us in that such sharp changes from + to – are quite possible. And an idea suggested itself: the numerator of the correlation coefficient can change in time and each magnitude connected with evolutionary changes could then reveal a supernormal dispersion. And exactly then I received from Chuprov his paper (1923). There, this problem is theoretically considered and a reference to your paper is provided.

The possibility of sending you this brief letter considerably and sincerely gladdens me[6].

Letter No. 2. 6 May 1924

I have received your writings and sincerely thank you. I was able to see your paper (1923 – 1924) since Nikolai Nikolaevich Shaposhnikov[7] gave me its reprint. I am now incessantly encountering calculations of index numbers although at heart I have no special liking for that section of statistics.

I think that the method of indexes has a few ineradicable defects which appear partly from its own properties and partly from the main peculiarities of the most usual field of its application, the statistics of prices. **1.** If we concentrate on the study of prices, it becomes clear that the weights should remain unchanged from date 0 to date 1 which utterly contradicts reality and disturbs the purity of the entire construction. **2.** If, while answering our task, we ought to ascertain the *tendency of prices* (of their movement either in the *secular* or *random* sense over a given interval of time), we discover many independent movements. When combining them in a single indication we will have to consider, on one hand, the economic significance of each tendency (for example, the turnover) and on the other hand the purity of the manifestation of each kind of the *tendency* (*the probable error of the parameter which characterises the movement of one or another group of commodities*).

These two principles of calculating the weights are irreconcilable and cannot be synthesised. **3.** When following the history of the prices of a given (say, of a sufficiently homogeneous) group of commodities we easily note that it is repeated, especially during given years, not precisely at the same time. Therefore, the general mean indeed combines differing phases of the history of prices into a single whole. **4.** Statistics of prices registers the ordinates of some continuously fluctuating function after each given interval of time. If these intervals are insufficiently small, the appearing picture can strongly distort reality and we will determine more or less distinctly only the waves of such order which contain a number of the moments of observation.

If during the movement of prices two commodities at least little differ, the possibility of comparing the *random* movements [of their prices] becomes impossible. Actual data was not yet checked that way.

I became very much interested in your formula of comparing the price of a *pack* of commodities at moments 0 and 1 when both the prices and the composition of the pack change. However, it is possible to isolate the changes of its price under its constant composition in a single multiplier, and, in another multiplier, the changes in the opposite sense. Exactly such a case happened to appear in the task suggested by our cooperative centre: to constitute an index which allows for both sides of the turnover for their commodities. I will

attempt to apply the idea of your formulas (9) and (10) on p. 380 [1923 – 1924].

I am really asking you to excuse me for writing this letter instead of briefly thanking you for the attention which you had paid me.

Letter No. 3. 9 November 1924

With deep gratitude I inform you that I have received the reprint of your paper (1923 – 1924/1924, pp. 208 – 251). Being unwell, I have time on my hands and am studying it.

Letter No. 4. 30 March 1925

Today, I have received the reprint of your work (1923 – 1923/1924, pp. 494 – 516) and the report about the sitting of the Society for Social Policy [Verein f. Sozialpolitik] and sincerely thank you.

Many people are applying the writing which you had sent me, among them is A. A. Konüs[8]; his paper (1924) is known to you. I am now finishing my study (1925) of the dependence of the prices of cereals on the harvest. My main attention was directed to revealing the evolution of that connection during the period 1894 – 1913. I am now extremely interested in constructing a *current* correlation coefficient by replacing the derivation of the means (of products and squares) in the formula of that coefficient by calculating parabolas (or straight lines) by the method of least squares. Until now, this procedure leads to awkward formulas but it is still possible to arrive at interesting conclusions.

Letter No. 5

I received your letter three weeks ago but postponed my answer until the time of your planned return to Berlin.

It is unnecessary to tell you that any letter from you inspires my work. This time I was especially interested to know that the problems of the so-called conjunctional statistics are not alien to you. I am far from any idea of burdening you with some pertinent letters, I know that your time is sufficiently occupied by other subjects. Indeed, I had often found myself an *insolvent* correspondent in cases which probably mostly interest you. But still if you do not directly forbid it, from time to time I will allow myself to share with you the interesting problems which crop up in our Institute as well as those methods of work which had been developed during these three years.

The work of our group which is guided by Slutsky[9] and me and which is consisted partly of my students and partly of mathematicians, graduates of the [Moscow?] University (all of them females) is going on very freely. It is possible to work at problems required by practice and to experiment widely with new methods and to apply them to most diverse materials or even to *urn problems*.

For a long time now the problem of the seasonal wave (Chetverikov 1928) occupies one of our central places. All textbooks are now separating the changes in time series into secular movement, seasonal waves and disorderly fluctuations (conjunctional in the narrow sense).

I think that that separation is wrong. These items are not the constituent parts of statistical series but types of movement. The so-called seasonal wave can change evolutionarily both in amplitude and form. Conjunctional fluctuations themselves can change in time either evolutionarily or disorderly; we can try to find the seasonal wave of the ability of some indication to fluctuate etc.

Duality of terminology is inconvenient since it can lead to awkward expressions, but we have to use the generally adopted terms concerning both the movement and the composite parts of a time series. Owing to the complicated net into which the different types of movement are interlaced, we encounter a number of difficulties when establishing seasonal waves.

The requirements of the calculations all by themselves suggest the need to think out thoroughly the main definitions, at least of the seasonal wave. At the same time those requirements reveal almost non-removable internal discrepancies.

For establishing a seasonal wave of some economic indicator we may proceed in two possible manners. We may understand it as EVERYTHING conditioned by some materially determined factor, for example, by the change of the seasons, the approach of a harvest etc. Then, however, a study of the seasonal wave will only be possible in connection with a causal analysis of phenomena and the wave will lose its *statistical-morphological* essence. This approach is obviously groundless. Indeed, if the given phenomenon only conceals a seasonal wave in itself as one of its composite parts, then we can hardly exhaust any solely seasonal factor by that term without diverging from its usual sense.

Even the approach of spring which is as though a seasonal phenomenon, has a partly evolutionary essence, if only we may speak about the evolution of the climate. An analysis of a phenomenon is not exhausted by a reference to the seasonal essence of a factor (?) since the factor itself requires the same analysis and thus we can [we will] move *ad infinitum*.

Another approach to the seasonal wave is formal, statistical-morphological. It is based on the idea of something recurring from year to year, but a number of difficulties immediately appear and reservations are needed. Many examples show us that, while repeating its form from year to year (repeating the ratios of the monthly deviations) the seasonal wave changes its amplitude. In some cases these changes evolve according to a definite law, but in other cases the

changes are disorderly. For example, the autumn issue of paper money depends on the harvest.

It is clear that, even with all those difficulties, what we call a seasonal wave (as a component of a statistical series) still retains the right to its name. More difficult is the case in which we consider the form of the wave rather than its amplitude. It can be invariably repeated from year to year, but it can also evolve regularly. For example, the seasonal wave of the export of bread from Russia ever more concentrated on the autumn months as the old-fashioned type of trade when the important buyers kept the grain and awaited the spring increase in price had been replaced by the new type of trade. It included agents who were only interested in the rapidity of the turnover of the money obtained on credit and in ensuring the brokerage.

The notion of a regular evolution is the own brother of the notion of *constancy*. We are unable to reject the name *seasonal* to a wave with an evolutionary form. But what can we do if the form of the wave begins to change disorderly from year to year and becomes a random variable[10]? Here is an example. We have a series of observations of the harvest and the monthly fluctuations of the price during many years. Either can derive the seasonal wave of the force of the dependence of the price on the harvest or break up our series into series of productive and lean years[11]. We will then derive a special form of the seasonal wave of this connection for each part of the series. However, when considering these waves chronologically we will derive a disordered alternation of those seasonal waves of various forms. Will they still be seasonal? May we reject that name but retain it in the first case? Such an essential characteristic of the seasonal wave as its form depends on a random factor (on the harvest) and therefore becomes random itself.

So what then remains from the statistical-morphological definition of a seasonal wave and does not here occur a shift in the direction of the deeply penetrated into our terminology definition of a seasonal wave according to the materially determined factor?

On the solution of all these questions depends one or another development of purely practical tasks of conjunctional studies. And I am often doubting which way to follow, to what should I hold on.

I began by ensuring you that I have no intention of infringing on your time but now I am finishing the eighth page of my letter. To deprive the time for reading is less sinful than it is for writing, but it still is a sin.

Letter 6. 1 May 1926

A great and common misfortune emboldens me to turn to you with this letter. I wish to tell you about our supposition and ask your advice.

The friends and students of Chuprov intend to arrange a solemn meeting of the Executive Commission of the statistical congresses[12] to carry out a number of practical steps. **1.** To solicit the publication of a collection of Chuprov's scientific works from the Academy of Sciences. The Central Statistical Department (CSD) will hardly be able to cope with such a problem. **2.** To publish a collection in memory of Chuprov based on analogous themes which ensured a similar publication devoted to Kablukov (1925). **3.** To proceed at once to the collection of Chuprov's correspondence and of materials for his biography.

Chuprov's manuscripts are kept in two places. Those preceding 1917 are in Leningrad, in the statistical room of the Polytechnic Institute[13]. They are there looked after by Karpenko[14] who is the chief librarian of the rooms of the seminars. Later manuscripts are concentrated in Prague under the care of Kohn[15]. I corresponded with him but do not have any information about his plans. Just in case, here is his address […]. Only a little which has to do with Chuprov's report to the Rome session of the International Statistical Institute should be in Geneva.

Most difficult but necessary is the collection of his *correspondence*. It is hardly the proper time to publish it at once, but the collection of copies of his letters should begin immediately. It would be very important to know your opinion. I took the liberty to make some steps in that direction. I wrote to Dr. Isserlis[16] (London) and asked him to try (preliminarily) to find out whether the [London] Royal Statistical Society will initiate [necessary measures] and I also wrote to prof. Guldberg[17]. There is a small fund abroad which can cover the necessary expenses.

In Russia, there are only a few of Chuprov's students[18], and only four of them were closer to him: Vinogradova[19] (Leningrad), Karpenko (there also), Khotimsky[20], a communist [vi], and I myself. I am quite sure that Slutsky will essentially help to overcome the unavoidably encountered difficulties of preparing the manuscripts.

This was the most essential, the *business* side with which I intended to inform you. The Russian translation of Chuprov (1925) will appear just after the [1 May] celebration.

Letter 7. 25 December 1926

With deep gratitude for the attention which you paid me I inform you that your reprints have arrived.

For our Conjuncture Institute the passed autumn [and a part of winter] was scientifically almost fruitless and I fear that the [end of

winter and the] spring will not be better. All the time we are being afraid of our closure, all the time someone or other initiates plans of a *centralization of the study of conjecture*, of *transferring all the conjectural materials into a single whole*, of *merging, subordinating*, etc., etc.

It is quite understandable what a nervous atmosphere is created by such an uncertain tomorrow. We have to work by straining all our strength and nerves which we do not have so much anymore. Everyone, who works to the slightest extent conscientiously, i. e. who bothers not only about fulfilling the required but about obtaining necessary results, complaints of complete exhaustion of strength and nerves and of hopeless overtire.

You are apparently interested to know the situation with Chuprov's heritage. Regrettably, and partly to my shame, I ought to say that it was only possible to achieve insignificantly little. I have made arrangements with the library of Moscow University about transferring Chuprov's papers to them, but their transportation from the Polytechnic Institute is delayed until January. Until then their study cannot begin.

Maria Aleksandrovna[21] informed me about your kind consent to order and prepare for publication your correspondence with Chuprov, certainly including your preliminary examination of the materials. The publication of any of Chuprov's writings or letters is not now urgent. True, Bernstein[22] was elected member of the Academy of Sciences, so that now the theory of probability has a representative there. On the other hand, my attempt to publish my brief biography of Chuprov was unsuccessful although I had activated some personal connections for pushing it forward. The CSD had been reorganised and *Vestnik Statistiki* fell into new hands. Smit[23] is in charge there and the conclusions are obvious!

But I had not yet lost hope. In about two months that critical period in the life of our Institute will end. It was additionally conditioned by the attempt to accelerate our activity and intensify the role of investigations in our everyday work. Then, in two months, I will perhaps be able to redistribute my working hours, find time for a systematic work on Chuprov's papers. For me, this problem is a very sore point. Please excuse my extensive letter about all these details.

Letter 8. 17 May 1927

I have received the reprints which you had sent me and am deeply thankful. I gave the second copies to Slutsky. His work (1927) has recently appeared and you certainly have its reprint. However, perhaps you are interested in the entire collection of [the pertinent] papers, and, again, do you have Bernstein's book (1911/1927)? Just let me

know if you need both sources and I will be glad to fulfil such an assignment.

I am continuing to sort out Chuprov's papers. My work is going on extremely slowly, partly because of the enormous quantity of extremely condensed materials, and partly owing to external causes. You know well enough what kind of handwriting had Chuprov! With all my practice I am sometimes busy deciphering for a long time. Right now, it is the turn of his student notes on the theory of probability (two thick notebooks) and the notes of his lectures. There also is an enormous material of mathematical calculations, mostly concerning his first publications abroad. It will perhaps become possible to begin this work owing to a young mathematician, a student of Khinchin, who is now in real earnest studying probabilistic statistics.

I myself am continuing to study our (pre-war) grain export, in particular, the evolution of the seasonal wave. My calculations provided such awkward results that I fear that their treatment will require much time.

Notes

1. See Letter 8 and [v, beginning of Introduction]. O. S.

2. Chetverikov published Chuprov (1909/1959) without a single commentary and we are sure that he was seriously mistaken, see Sheynin (2016, Chuprov), (2017, Beginning of Preface) or (2018, Chuprov). O. S.

3. That book has many deficiencies. In 2013, I translated it into English (see Bibliography) and introduced essential comments. O. S.

4. No such paper is mentioned in any source. At the end of this Letter Chetverikov mistakenly stated that Chuprov had mentioned it. O. S.

5. Yastremsky spitefully mentioned D. F. Egorov, the great mathematician who was later exiled to Kazan and soon died in a prison hospital. O. S.

6. This remark apparently means that sending letters abroad had only then became possible. O. S.

7. Nikolai Nikolaevich Shaposhnikov (1878 – 1939), economist. In 1913 – 1927, professor, Moscow Commercial Institute; 1923 – 1928, deputy chairmen of section on money circulation and credit, Institute of Economic Research, Finance Ministry and scientific advisor, Conjuncture Institute; 1931 – 1936, chief engineer, central administrative board, Ministry of Heavy Industry. Main works devoted to policy of credit and money and external trade. A. D.

8. Aleksandr Aleksandrovich Konüs (1895 – 1990), economist and statistician. 1923 – 1929, employee, sector of indexes and prices, Conjuncture Institute; from 1945, worked in the Institute of Economics, Academy of Sciences and the Research Institute of Labour; from 1960, in Economic Research Institute, State Planning Committee. A. D.

See *New Palgrave*, vol. 3, p. 62. O. S.

9. Evgeny Evgenievich Slutsky (1880 – 1948), mathematician and economist. 1913 – 1926, taught in Kiev institutes; 1926, work in the Central Statistical Department and consultant, Conjuncture Institute; from 1934, Moscow State University; 1939, Mathematical Institute, Academy of Sciences. A. D.

Literature about him includes Kolmogorov (1948/2003). His *Selected Works* were published (in Russian) in 1960. See also his *Collected Statistical Papers* (2010) in

English. They contain, in particular, our paper of 1999 about him and his biography by Chetverikov. O. S.

10. A random variable is not disorderly. O. S.

11. The discussion below is senseless: the general time series should have been definitively subdivided into two pertinent series. O. S.

12. That Commission was probably established by the International Statistical Institute. O. S.

13. Deleted.

14. Boris Ivanovich Karpenko (1892 – 1976), statistician and economist. 1919 – 1938, head of the Chuprov room, Petrograd/Leningrad Polytechnic Institute; from 1921, taught in Petrograd institutes; 1921 – 1938 and 1955 – 1976, docent and professor Petrograd/Leningrad Polytechnic Institute; 1938 and 1949, repressed. Main works devoted to methodology of statistical research, theory of indexes, financial statistics. A. D.

15. Stanislav Salesievich Kohn (1888 – 1933), statistician and economist. 1914 – 1918, statistician, Special Conference on Foodstuffs; 1918 – 1920, taught in Tiflis (Tbilisi) Polytechnic Institute; from 1921, in Paris, collaborated with Russian Financial-Industrial Society and editorial office, *Ekonomicheskie Zapiski*; from 1923, in Prague, taught in Russian law faculty. A. D.

16. Leon Isserlis (1881 – 1916), English statistician and mathematician. Student of Karl Pearson. Translated Chuprov's works into English. A. D.

17. A. Guldberg (1866 – 1936), Professor of mathematics, Oslo University. A. D.

18. See however Anderson (1959, p. 294): the names of many promising students of Chuprov suddenly disappeared from scientific literature. O. S.

19. Nadezhda Matveevna Vinogradova (1889 – 1975), statistician. 1924 – 1935, taught statistics in Leningrad institutes; from 1936, taught in Moscow Ordzhonikidze Institute of (инженерно-статистический) Engineering Statistics (?). Works on theory of indexes and statistics of harvests. A. D.

20. Valentin Ivanovich Khotimsky (1892 – 1939), statistician. From 1924, scientific and pedagogic work, Moscow Institute of National Economy and Institute of Red Professorship; 1927 – 1932, scientific worker and head, mathematical section, Communist Academy; 1935 – 1937, head, section of population statistics and public health, Central Directorate of Accounting in National economy. 1938, arrested; 1939, shot. A. D.

See Tolts, here in Bibliography, [vii, Additional information, Kolman] and [ix] about his participation in compiling statistical textbooks. O. S.

21. Chuprov's sister. A. D.

22. Sergei Natanovich Bernstein (1880 – 1968), mathematician, member of Soviet Academy of Sciences. Leading Soviet mathematician, foreign member of Paris Academy of Sciences. O. S.

23. Maria Natanovna Smit-Falkner (1878 – 1968), economist and statistician. Correspondent member of Soviet Academy of Sciences. A. L. Dmitriev lists the places of her work.

See also [v, Note 178.2]. Unbelievably ignorant, fanatical communist. O. S.

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VII

E. Ya. Kolman

Sabotage in science

Bolshevik, No. 2, 1931, pp. 73 – 81

The saboteurs from the Industrial party pleaded guilty of preparing explosions of, and setting fire to our mills, of espionage, organization of crises in the spheres of power engineering, in fuel, metal, textile plants, transportation and of preparing an intervention[1]. Being engineers-designers and engineers-economists, they were complete saboteurs, agents of French imperialists and former *national* mill- and landowners. But as soon as their scientific activity began to be heard in court, they took a defensive position. Ramzin reiterated:

In my scientific works, which I have more than a hundred and fifty, I never ever said anything contrary to reality. In my scientific work I did not carry out any wrecking directives, any wrecking aims. My scientific contributions published at home or abroad never had any wrecking aims.

And he persistently stated:

I had precisely distinguished my practical wrecking activities and my scientific work. In that work I never carried out any wrecking ideas.

It occurs that, being engineers and arch-wreckers, as professors they had been blameless and keeping to the *purity* of their *objective* science. In spite of all his evasions, the court exposed Ramzin. His theoretical statements about high and low pressure and the decision of the Combustion Institute which he headed to burn the coal extracted from the coal-field near Moscow in a powdered form were wrecking, deliberately aimed at delaying the development of science and production.

It seems senseless to prove extensively all the groundlessness and nonsense of the statement that the theoretical work of practical wreckers can remain unsullied by the wreckers' poison. As though there exists somewhere a *free* from politics, from the Weltanschauung of the scientist, immaculate *objective* classless science which had by some miracle avoided the general fate in our world, sharply separated into two camps of irreconcilable class struggle[2]. However, all the attempts made by the confessed complete practical wreckers to shield their allegedly objective theoretical scientific work have a definite essential meaning. They show that the crushed class enemies never think about definitively surrendering. No, they attempt to entrench themselves in most inaccessible, cunningly camouflaged positions on the theoretical front since they wish to go on holding the commanding heights in science. [A long quotation from Lenin's *Complete Works* (actually, from *Proletarian revolution and the renegade Kautsky*) follows. It is about the hate felt by the defeated exploiters.]

The theoretical scientific sabotage had not been confined to a certain branch of science. In economics, whole *schools* decorated by such names as Bazarov, Sukhanov, Groman, Rubin, Yurovsky, Finn-

Enotaevsky, Kondratiev, Tschayanov (Chayanov), Falkner , liberally disseminated their idealistic and mechanistic theories, their scientific falsifications of the theory of value, reproduction of goods, money, agriculture, in research and educational institutes etc.

Economics in general and particularly in planning was *lucky*: sabotage was most rapidly and comparatively completely revealed, although its recurrence is met with even nowadays. But the scientific sabotage of the bourgeois professors had not been restricted to the sphere of social sciences. In engineering, natural science and mathematics the forces of dialectical materialism are incomparably weaker and pretty little is done yet to reveal the work of scientific wreckers. However, even the known isolated facts convincingly tell us that, however abstract and *harmless* some branches of science seem on the face of it, the wreckers had stretched out their sticky tentacles to them[3].

Thermal engineering, the theory of refrigerating, human geography, technique of rationalization, theory of land reclamation, forestry, and mining, the technique of high tension, microbiology, accountancy, statistics and ichthyology, each became the field of the wreckers' onslaughts. They aimed at a *scientific* justification of their own practice and at guiding the training of the rising replacement of the workers of science and engineering.

Here, it is impossible to analyse the definite manifestations of theoretical sabotage in separate branches of engineering or natural science. This task should be the business of the workers of separate sciences. They should thoroughly and critically survey the entire scientific, technical and educational literature so as to *separate the good from the evil*. It is certainly wrong to believe that everything written by the wreckers is complete sabotage and great work is needed to select that, which at least temporarily may be left there intact.

It is clear that according to its special content wrecking theories in ichthyology, say, have nothing in common with such theories in the compilation of balances, but in the final analysis the social sense is the same everywhere. Ichthyologists, like for example Nazarovsky, *prove* that the natural laws of propagation of fishes absolutely preclude the fulfilment of the five-year plan in fishing. At the same time they wrongly indicate the natural habitat of various kinds of fish so that the Soviet fishing will indeed get smaller catches[4].

The compilation of balances is based on such an economic theory from which it follows that there is no difference between the USSR and a capitalist country[5] and offers such indications, for example, about the method of determining commodity remains which will lead to the concealment of shortages.

From the numerous examples of theoretical sabotage one case is especially standing out: the Vishnevsky *straw billions*. He was a closest employee of Groman who had until recently directed the compilation of the summary agricultural balance in the State Planning Commission. Thanks to the Vishnevsky method of estimation the production of straw and hay became equal to 2/3 of the value of the [harvested] cereals, and about equal to the same value in 1929/1930. These statistical manipulations (Starovsky 1930)[6] were aimed at

understating our achievements in the industrialisation (?) and secure a basis for the known theories about the *unprofitableness and degradation* of our agriculture. Had they been adopted as a foundation for compiling the planned figures, planning would have been thrown into confusion[7].

Not without special colour are the counter-revolutionary onslaughts on the pages of the journal of local lore, *Okhrana Prirody* (Protection of Nature). They conceal their wrecking nucleus under the cover of combating agricultural pests (rodents, harmful insects etc.). In No. 3 of that journal [1931?] N. Pod'japolsky declares:

The current complete ploughing up of vast territories in our gigantic state farms and large kolkhozes can ruinously tell on themselves. The owls that live on mice (! E. K.) make their nests in old worthless trees which are sometimes found isolated in the fields will disappear.

The editorial in No. 7/8 [1931?] is devoted to the same propaganda. What is the journal and what are the students of local lore dreaming about is evident from the article *The last days of the Yamskaia steppe*. It demands that that steppe be declared a protected area. Deeply melancholic, the author contemplates how the desert steppe gives way to *an immense space of black ploughed fields*. The author *looks into the distance* and heaves a sigh:

The spirit of primitiveness is felt and the thoughts carry you back into the pre-agricultural part of the territory.

Exactly so: *protection of nature* becomes protection against socialism[8].

And so, the essence of all the wrecking theories is necessarily the same, the aims of the wreckers of every stripe is the same: disruption of our socialist construction, restoration of capitalism. Even after a superficial glance at the writings of the professorial saboteurs it often becomes evident that all is thoroughly coloured: a quotation from Marx is followed by another from a bourgeois economist, then a quote from Lenin is followed by another from the next bourgeois scientist etc. and the reader is granted the choice.

Not less typical than the crude forgery of *the Soviet style* is the exceptional abundance of mathematical calculations and formulas which are flashing in the works of the saboteurs. Piles of most complicated calculations and formulas, numerous diagrams of harmonic (?) and exponential functions are characteristic not only for collections of the Conjuncture Institute, not only for such masterpieces of Rudanovsky's *theory of balance calculations* (he *solves* the pertinent problems by differential equations of mathematical thermodynamics), no! A certainly gross exaggeration of the mathematical method is also seen in such *highly specialized* works, as for example the estimation [the choice?] of the most favourable direction of new railways (a collection published by the Moscow Institute of Railway Engineers) or even in the theory of horse-breeding.

Matter disappears and only equations are left, this characteristic of scientific clerical obscurantism in modern physics by Lenin[9] is the clue to understanding the wreckers' preferences for mathematizing

any science. Indeed, the wreckers certainly will not write in plain language that they advocate the restoration of capitalism, they must look for a disguise. And there does not exist a less penetrable screen than the veil of mathematical abstraction. Pretty often mathematical equations attach a pseudo-passionless, objective, precise and irrefutable essence to propositions hostile to socialist construction and conceal their true character.

The *story about centrography* can be indicative of mathematical charlatanism, about the method which for a very long time took shelter under the wing of the State Planning Commission. The centrographers, creators of *the new statistics*, refuse to understand that such economic means like *mean price* have a real sense since they describe a moment in a really occurring economic process (for example, the movement of prices regulated by the law of value).

Centrographers construct their geographical *centres* just like centres of gravity are determined in mechanics. Thus, they imagine a unit force situated in the place of life of each inhabitant and directed towards the centre of the Earth. They add up those forces according to the parallelogram law of forces and call the *centre of population* the point of application of the resultant force.

They certainly apply many sinuses, cosines, sigmas etc. and discover such laws as

The centre of the distribution of pigs is west of the centre of population, and of the sheep, south of that centre

(Mitelman 1929, p. 128). However, they do not disclose the mystery of fateful conclusions, of what happens if the cardinal points become reversed. This is not only eyewash. American and French colleagues of *our* national centrographers, Heyford, Meuriot and others apply the conclusions of centrography as a an argument for imperialist annexations. And here is a remark of their student on the Soviet soil (Ibidem, p. 113):

As a peculiarity of one of the regional centres I note that the centre of the population of the Far Eastern region, because of the bending of its territory, is located beyond it, in Manchzhuria.

It is only doubtful whether the suggested by itself rounding of the region be implemented under the slogans of Zhang Xuellang or Ustrialov[10]. Not accidentally the wrecking mathematization of science is defended in *Poslednie Izvestia* and *Vozrozhdenie* (10 and 17 Nov. 1930). They spitefully responded to an article in *Pravda*, shield the geometrical mean and other mathematical methods (?) of Kondraytievs and Bazarovs:

In a country which is alien to the lifeless formal logic, twice two equals four tells us nothing, but twice two is a five-year-plan, means something quite different[11]!

That insipid snake hissing which ought to look ironically contains the entire secret of the White Guards' liking for *pure* science. What in the Trotsky-Syrzov's formulation was ringing like the defence of *arithmetic*, which *should be not rightist, not leftist, but correct*, is stated here openly, and only the Ryabushinsky's *commercial arithmetic* with a profit of 500% for the overthrow of the Soviet power

and murder of millions of working people can call in question the clearness of that formulation.

The attempts of applying the mathematical method anti-scientifically, contrary to Marx, which appear now and then in our own ranks, seriously assist the saboteurs' mathematical mystification of science. Thus, we see serious attempts to derive the law of the development of the productive forces in the USA by equating them with engineering and assuming that the number of the patented discoveries is a measure of progress. Then follows a mathematical determination of the dependence of *engineering* and time, of the laws of the movement of wages, of the rate of profit, of the index of prices etc.

Such rough empirical exercises which only touch the surface of events[12] encourage those wreckers who mathematise science. Indeed, the class enemy greedily catches up each our mistake. Thus, N. V. Ignatiev (*Voprosy Koniunktury*, vol. 3, No. 1, 1927) hurries to fix the moving unity of the head of the American bourgeois politico-economic school and a communist scientist and does not miss the opportunity to express his deep gratitude to Kondratiev, the Editor of the pertinent collection. His paper includes the following statement:

By the simplicity of its formulation the quantitative theory of money which has been confirmed by empirical data as well (*I mention at least the work of Prof. O. Schmidt*[13] *for the period of emission*) *essentially tempts me into a statistical check*.

The still sufficiently wide liberal attitude to the bourgeois professorial *erudition* directly assists wrecking in science. A feebleness about any idealistic statements which are shielded by their *scientific character*, if only uttered by academic circles, directly assists wrecking in science.

This policy diametrically opposes Lenin's indication (*On the significance of military materialism*, 1922) that communists ought to

Wage war against modern <u>educated</u> landlords who advocate serfdom, reactionaries, qualified lackeys of clerical obscurantists.

That policy had built its robust nest, in particular in the natural scientific department of the *Great Soviet Encyclopaedia*. The authors of the main methodically guiding items of that department had been selected in such a way that the hunt for *eminent* bourgeois scientists is obvious. Our own Marxist forces are not applied. And the quality of those items naturally agrees with the selection of their authors. A number of articles are saturated through with mechanical philosophy, Machism, conventionalism and subjective idealism and are methodologically an eclectic medley.

Those articles reflect the methodological turmoil which exists in the bourgeois natural science, but they do not at all testify to the editorial staff's Marxist-Leninist guidance. For example, the articles *Waves* and *Hydromechanics* are written by the Prague physicist Philipp Frank, one of the main modern representatives of Machism. In *Die Naturwissenschaften* he came out against Lenin's *Materialism and Empirio-criticism*.

In the former article correct statements are artfully interspersed with a Machian nucleus[14]. Light is treated as though it exists side by side

with matter, and, in accord with Schrödinger's newest theory, the movement of matter is reduced to the concept of waves. The article *Perpetual Motion Machine* says nothing about the connection of the pertinent problem with philosophy, about the impossibility of such a machine which follows from the main fact: all what is happening, is only a change in the form of the movement of matter, so that a new form of that movement which cannot originate out of nothing.

The article *Substance* defines materialism and idealism, but the ignorance of that definition can be revealed by any student of a Soviet and party school. The physical aspect of the problem is considered from the viewpoint of *the identity of the finite particles of substance*, i. e., from a gross mechanical point of view. And, finally, in the heat of a vulgar animation it is stated that modern discoveries confirmed the beliefs of the alchemists[15].

In the biographies of *Harvey, Galileo* and *Gauss* only an insignificant place is devoted to their scientific methodology among a historical description, sometimes of a far-fetched essence. And the methodology is wrongly appreciated. Thus, Prof. Kagan is silent about Gauss' struggle against the Kantian view on geometry, Prof. S. Vavilov wrongly and contrary to Engels' views contrasts Galileo and Kepler, and Prof. Samoilov mechanically identifies Harvey's method with methods of modern natural science[16].

The Editor (and the author of a prevailing majority) of mathematical articles, Prof. Kagan, carried out a clear Machian policy. For example, axioms (article *Axiom*) are understood as arbitrary, conjectural propositions which do not reflect reality[17], and are only suitable for practice. The article *Probability* (Asmus) provides a complete idealistic concept:

Probability is not an indication belonging to the events themselves.

That most important cognitive category is thus thought of as belonging to either Divine Providence or the immanently creative human mind[18]. The London Professor Bowley, a Machian, refers to this article and provides a mathematical definition of probability by *specifying its subjective meaning*. Such a concept of probability is in harmony with the subjective schools in political economy which are fashionable in the capitalist world, but it is patently unable to explain why do the insurance offices, which are guided by *the mathematical measure of our subjective ignorance*, ensure quite an objective profit.

Even these few examples show that it is necessary to study thoroughly and minutely the published articles of that department, and not only those which describe inorganic nature and mathematics, but articles pertaining to biological and psychological sciences as well. Thus, articles like *Association, Affect*, are not free from Freudism[19]; *Will, Perception*, from mechanical philosophy.

The collective of authors, the method of planning, and selection of the personnel ought to be revised. All this should be taken out of the system, which is based on the notorious *personal connections*[20], into the path of social discussion, into an environment of activists in science and popularisation should be created, and an involvement of scientific communist forces organised.

The reorganisation of the national scientific department of the *Great Soviet Encyclopaedia* is extremely important in connection with the great and ever increasing significance which natural science acquires during this present stage of socialist construction. However, a similar situation exists in the *Technical Encyclopaedia*, and the *Medical Encyclopaedia* ought to be thoroughly examined. The Communist Academy, the Society of Militant Materialists-Dialecticians and all our scientific Marxist-Leninist societies and institutes should at once begin participating in this work.

Great educational work is needed in our own ranks as seen even in the following example. After the disclosure of the sabotage of the Industrial party the engineers, communists, of the NIS[21] were commissioned to analyse the works of Ramzin. Two of them, after considering his proposal to convert tractors into steam traction, reported: all the calculations are correct, from the viewpoint of engineering everything is irreproachable, it was only overlooked … that there can occur shortages of water or straw (as fuel).

After all, that *only* is unique. Such a friendly criticism, such a narrow calculative approach reveals how narrow is the political horizon, how deficient is the political flair and economic education even among many engineers – communists[22]. The substitution of the Bolshevist policy in science, of the struggle for the Party spirit by liberalism is all the more criminal since the bearers of reactionary theories are such eminent professors like the Machian Frenkel in physics, vitalist Gurvich and Berg in biology, like Savich in psychology, Kol'tsov in eugenics, Vernadsky in geology, Egorov and Bogomolov in mathematics. From their own science they derive most reactionary social theories.

Is it not typical, if only the events of the past month are considered, that the Moscow Mathematical Society refused to expel the recognized leader of the reactionary Moscow mathematical school and the former director of a mathematical institution, a churchwarden who did not wish to join a trade union, Prof. Egorov. And when Egorov declared that

Only the imposition of a standard Weltanschauung on scientists is the true sabotage,

the reporter, a communist, in his concluding remarks, not only did not repulse him, but rejected a proposal to adopt proper practical conclusions, and explained that statement as a misunderstanding.

Such is the policy of some communists which they are carrying out in the most reactionary professorial milieu among the custodians of the traditions of Zinger, Bugaev and Nekrasov who had been developing the theory of probability, the science of numbers [the number theory] and analysis as proof s of the firmness of *Orthodoxy, autocracy and nationality*[23] which should have supported Lopakhin's philosophy among those, who quite consistently refused, during their recent congress, to send a greeting to the 16th Party congress [1930].

It is quite obvious that the behaviour of those communists who are carrying out a policy of such protection of the reactionary professorship is the standpoint of the bourgeois democratic fellow-travellers of our revolution. They are people with Party cards about

whom it is said *over there*: *He is a communist, but he is our man*, people who value their *good name* as formulated just above.

It is quite understandable that, when such people are guiding the practical policy in science, their conducting the Party policy on that militant territory is out of the question. Not accidentally therefore, that in spite of the direct indication of the November plenary session of the [Party] Central Commission, the scientific department of the State Planning Committee did not yet develop a five-year-plan for technical and scientific personnel. The planning of science and research is still done more in words than in reality[24].

We must struggle most ruthlessly with rotten liberalism and patronage (?), with kowtowing to science alienated from life, with attempts to transfer the customs of the bourgeois academic caste into our scientific milieu, with the ideological enveloping the communists who are working in the region of scientific theory and are guiding scientific work. This struggle presupposes a maximally intensive and truly mass work of our new and rising generation of cadres at mastering the best achievements of the bourgeois science, at their critical remaking on the basis of materialistic dialectics.

The enemies of the proletarian revolution attempt to portray Bolsheviks as persecutors of science. They understand well enough that, had their libel become true, it will ensure their victory. Therefore, they exert every effort for imposing on us the idea to leave the pursuit of the theory to *specialists*. And so, those same followers of Kondratiev, the mathematical calculations in whose papers dazzle us, preached to us, in the works of Kondratiev himself, that we ought

To abstain from numerically expressing those elements whose variation we cannot at present quantitatively foresee[25]*;* that we should *categorically renounce the fetishism of numbers*;

that we are *hypnotized by numbers and arithmetic*, that *an unaccountable passion for mechanical, minute calculations* is ruining us[26].

Those same gentlemen who had pulled the wool over our eyes by their pseudoscientific statistical calculations were acting quite reasonable as seen from their viewpoint, when, particularly in commissions of inquiry into the compilation of curriculums for our educational institutions, they decreased the number of hours for statistics and mathematics to such an extent that those sciences were practically deleted (for example, in the Economic Planning and the Power Engineering institutes)[27].

Those gentlemen intend to preserve their monopoly on scientific theory. Most of our economists, planners, et al are mathematically and technologically illiterate, in each trifling case they have to turn to a specialist whose work they are hardly able to check. Most of them regard mathematics with such respect that they do not dare to study or apply it.

The saboteurs register all that pretty well and retain the entire subtle mathematical arsenal for their own aims but warn us even against the multiplication table. Ignorance of mathematics, statistics, human geography, economics of separate branches of industry, of mechanics, chemistry, of elements of engineering, this is the specific expression

of a most essential condition which enables sabotage. It is the expression of our low cultural level, of one of the obstacles to socialist construction which should be surmounted.

Is it not shocking that in the country of Dneprostroi, Magnitogorsk[28], construction of plants for producing tractors, in the country of socialist industrialization, which cannot abstain from giving rise to new engineering, isn't it shocking that such a movement as for the mass technical enlightenment (the society Tekhmass) drags out a miserable existence and is barely honoured by public attention?

Is it not less shocking that the Communist Academy still has not transformed its technical section into an active organ guiding all the country's technical thought? That its Association of Natural Scientific Institutions, Sections and Societies is far from being that vigilant ideological guard and an active builder of the party, of the communist science, which it should have been?

Only now, against the background of the revealed sabotage, we clearly distinguish the entire decisive significance of the revolutionary measures taken according to the Party's indication and under its guidance, measures to speed up the training of proletarian specialists, to entirely reorganising the high technical school. Even a blind man will understand the role played by the objections of the rightist opportunists to the reorganisation of the higher educational institutions undertaken by the Party, will understand the objective meaning of all of their talk about trusting the old engineer personnel.

However, alongside tens of thousands of red specialists commanding socialist engineering and economics which the Party selected and trained from the ranks of the working class, we ought to arm millions of rank and file builders of socialism with knowledge much more extensive than ordinary literacy.

It is apparently unnecessary to prove that for a successful participation in mass rationalisation and mass inventiveness, calculations and for an increase in the productivity of the labour of each worker, we need to master technology, economics, technical drawing, mathematics, etc. A mighty wave of counter-industrial and financial plans has risen at the plants and in transportation, and we ought to underpin it by a robust scientific base. Then each worker will be able to participate most productively in planning from the bottom[29]:

So that each will be able//So that each//Each will be able to watch and to check//And do!//Do!//Do!

So that the working class will strike blows by those plans on the very possibility of sabotage, so as to convert counter-measures into a mass method of general planning, to convert each worker into a planner and each plant into a cell of state planning, – to achieve all this we ought to unite and legalize all the efforts, all the energy which is developed in the workshops and will surmount all obstacles by the enthusiasm of the working mass.

At the same time it is necessary to organize the needed mass studies. Mass planning will realize Lenin's thoughts about the role of registration and control as the main elements necessary for the proper

functioning of the first (the socialist) stage of communism, about the participation of the working masses in the daily management, in the organisation of production in the entire country. This [according to Lenin] is the condition whose fulfilment ensures the working class such a force which will throw back capitalism with its survivals like a straw or dust[30].

It is time for the unification of the work of productive commissions, cells of inventions and of the Tekhmass society, the teams of counter-measures, commissions on rationalisation, various courses in raising the level of skill, and for the State Planning Committee to establish its own centre of mass planning and to become a mass organisation so that the idea of mass planning, of planning down to the lathes, into an organisation which unites the shock workers of Soviet planning. All this insistently requires a ruthless battle with sabotage in science and a resolute uprooting of all of its traces. Our proletarian communist fighters, builders of socialism, will thus incessantly perfect themselves and be tempered.

Notes

1. They pleaded guilty quite voluntarily, certainly no one compelled them to confess … I cannot say how should the coal be burned, but if (if!) Ramzin was mistaken it was an honest mistake.

2. *An irreconcilable class struggle* only existed in the Bolsheviks' heads who dreamed of a new war and a world revolution (of the Second Coming!).

3. Kolman thus proved that the terror of the beginning of the 1930s was a dress rehearsal for the Great Terror which did not fail to strike a few years later. The Bolsheviks understood that a horrible pest had visited the country; that it mostly affected the talented, the eminent people; that to curb the epidemics those people should be shot or at least sent to work under barbaric conditions; that the more of those latter perished the better; that their wives and children ought to be severely victimized; that the greatly reduced birth rate and the negative genetic after-effects of those measures ought to be ignored …

Kolman had not shot anyone, he only indicated the habitats of the wreckers and pointed out some of them personally. The Bolsheviks did much better than the Catholic inquisition.

4. See below a still more disgusting example of the Bolshevist destruction of nature. And much later they managed to do away with the Aral Sea and, about 1984, prepared to make the great Siberian rivers flow backwards. The USSR was sacrificed for ensuring the world revolution, the most important aim of those crazy monsters. To compare: the most important aim of Great Britain was to ensure the freedom of the world trade.

5. Balances in the USSR and in a capitalist country should differ, but how?

6. That same Starovsky (1933, p. 280) alleged that

The theoreticians of the bourgeois statistics (Süssmilch, Quetelet, Lexis, Bortkiewicz, Pearson, […] *Chuprov, et al) had attempted to prove the invariability and eternity of the capitalist order and the stability of its laws.*

His statement was certainly a fabrication through and through and it is not surprising that in 1958 Starovsky was elected Correspondent Member of the Academy of Sciences and had been holding highest administrative position (in 1940 – 1975, head of the Central Statistical Department).

7. *Planned (*or *control) figures* best explained by an example. A book had appeared in Moscow entitled (in translation) *Control Figures for the National Economy of the Russian Federation for 1927/1928*.

8. Here are other facts (see also Note 4). After the draught and famine of 1946/1947 (which prevented Stalin from carrying out his wild plan to conquer at least the continental Europe) he published a scheme of planting woodland belts. It was partly implemented and proved successful, but after his death Khrushchev ordered to fell the belts. Many of them were destroyed, the area of ploughed land

increased, especially at the expense of virgin lands. In 1962/1963 erosion of the soil led to an ecological catastrophe. The Bolshevist reshuffle, Khrushchev's updated plan (see Note 29).

9. Kolman quoted Lenin from chapter 5 of his *Materialism and Empirio-Criticism*, but why did he mention the disappearance of matter?

10. Kolman obviously thought about the indefiniteness of the borders of Manchzhuria. The statement about annexations was attributed to Mitelman in the usual Bolshevist manner. In general, centrography by itself has nothing in common with annexations.

11. The fulfilment of the five-year-plans had been fictitious. At best, they were fulfilled in physical units (e. g., so many tractors were produced), but not always at all in quality, whereas the cost price of, say, those tractors was forgotten

12. Kolman could have referred to the Harvard-Barometer, to the monthly forecasts of the economic situation of the USA made by extrapolating the parameters of the situation. In 1929, suddenly occurred the Great Depression and even the Soviet economists, armed with the perfect Marxist theory, had not foreseen it. Oh, but that was certainly the doings of the saboteurs!

13. Ignatiev thought about Schmidt (1923) who had kept to the quantitative theory of money. Referring to Falkner, he (p. 12) stated that in the case under consideration Marx had applied that theory. E. K.

14. Frank was unable to deceive the vigilant Kolman! Concerning light and waves in a physical context Kolman's remark about forgetting matter was worthless. His statement about the perpetual motion machine, as well as his subsequent utterances are incomprehensible.

15. In principle, alchemists were not mistaken: transmutation of elements is indeed possible.

16. Gauss never struggled with anyone, Galileo refused to recognize the Keplerian laws of planetary motion, and the statement about Harvey was not justified.

17. Mathematics is not obliged to portray reality. Even the natural numbers 1, 2, … do not exist in nature.

18. The *Encyclopaedia* explained two viewpoints about the notion of probability, but the choice of Bowley as an author was unfortunate (and Kolman's description of his opinion is hardly understandable). It was necessary to turn to *our own Marxist forces*, E. E. Slutsky, but his work in the Conjuncture Institute crushed in 1930 compromised him at least after that. But there still was Khotimsky, the not yet exposed saboteur …

19. Freudism: the teaching of Sigmund Freud, the founder of psychoanalysis (which many authors had not recognized).

20. The implied personal connections of Kagan with Western scholars were most commendable. During long periods of time Soviet scientists had been almost banned from contacts with their foreign counterparts.

21. The first two letters apparently meant *Nauchno-Issledovatelsky* (Research) but for me the third letter remains incomprehensible.

22. So what should have those pitiful reviewers written? Kolman however obviously knew the answer.

23. Kolman mentioned eminent scholars (but forgot the mathematician Luzin whose persecution he, Kolman, had initiated) and showed himself as a true Stalinist. Egorov was exiled to Kazan and soon died after a hunger strike while in prison. Kolman separately mentioned three more worthy scientists. See Sheynin (2017, § 15.5) about Nekrasov.

The celebrated triad (*Orthodoxy*, …) appeared in 1832 and had been widely recognized perhaps until 1917. Its author was S. S. Uvarov.

24. Planning science is an extremely delicate operation. The quite recent attempts to plan the scientific work of the Russian Academy of Sciences had been most severely criticised.

25. A quite reasonable statement.

26. Kolman repeatedly stated that the mathematisation of science was sabotage. Excessive efforts had indeed occurred, even Euler was considered guilty (but at the same time he at least fostered analysis). Sometimes mathematisation happens thoughtlessly (Grekova 1976), but much oftener as a pursuit of selfish ends:

Wherever money is abundant, charlatans are brought forth by spontaneous generation

(C. Truesdell 1984, p. 117, with an incomplete reference to another author). Abundant money means its wrong allocation. But in principle one and the same differential equation often describes quite different phenomena which Kolman had not known. For that matter, he revealed his ignorance in a few other cases (and his crass stupidity as well, see his utterances about the protection of nature).

Statements about excessive mathematisation meant sabotage as well!

Here is a quote from Kolman (1930):

Now, in this last stage of the monopolistic capitalism, mathematical political economy notably revived once more. Being the most reactionary of all the bourgeois economic doctrines, it serves as the scientific shield of fascism. … I will not enlarge on the scribbles of Messrs Kondratievs and Bazarovs who had only relatively juggled their wrecking aspirations on the pages of our respectable periodicals: mathematical theories of capitalist cycles, of the processes of reconstruction, conjuncture and principles of planning.

The anti-Marxist nature of these theories which tend to frustrate socialist construction is sufficiently obvious.

Poisonous rubbish!

27. I rather believe that that episode had happened because of the zeal of enthusiastic and ignorant communists.

28. Dneprostroi, construction of a hydroelectric station on the Dnepr. Magnitogorsk, Cheliabinsk oblast, centre of iron an steel industry.

29. Updated plans, planning from the bottom etc. had been the Bolsheviks' attempts to stir up popular support. They exceeded the indications of the plans drawn up by specialists. It is hardly doubtful that the new plans led to unnecessary expenditures and/or lower quality of the commodities. Similar plans for exposing saboteurs were compiled locally and sometimes resulted in arrests of investigators, prosecutors et al. About 1934, we, school students, were told that the driving force of capitalism was competition, of socialism, socialist emulation. This was apparently true until ca. 1927.

A trustworthy friend told me a real story about an updated plan: A worker came to his shop superintendent and declared that production should be doubled. – *An how to achieve this? – That's your business*. I do not know the author of the admirable verse below.

30. Lenin's statement of 1919.

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Additional Information

Kolman E. Ya., 1892, Prague – 1979, Stockholm). Philosopher-Marxist, professor of mathematics but not really knowledgeable about it. Filled important positions in the party hierarchies of the USSR and Czechoslovakia, in 1936 – 1938 was head of the science department of the Moscow city party committee. Actually, from the early 1930s he became the ideological watchdog of the scientific community.

In Czechoslovakia Kolman was head of the propaganda department at the Central Committee of the Party. Arrested in 1948, imprisoned for more than three years but never accused in court. Became disillusioned with the Soviet regime and managed to leave the USSR in 1976. His book (1982) was published posthumously.

His paper here translated was published in the leading theoretical Party periodical and was certainly meant to be taken very seriously. His style was too good for a Czech and I thought that someone edited it. However, after reading Kolman (1982) my doubts have all but disappeared. Much more important, it seems obvious that its draft was approved and perhaps somewhat changed in the Stalinist direction by someone higher up.

It is difficult to imagine that he had not understood the bloody essence of Stalinism and there, in that book (1982), he obviously whitewashed himself. Here are some comments on it (which I nevertheless recommend to my readers). **1.** When the genuine scientist and, at least formerly, his close friend V. I. Khotimsky was shot, Kolman thought: *When trees are felled, the chips will fly*. Actually, *When chips are needed, the best trees are felled*, but who cared? **2.** It is difficult to date many described events. **3.** Many party bigwigs and highest party functionaries are described by a few never justified words.

Asmus V. F., 1894 – 1975. Philosopher, joint Stalin prize 1943

Bazarov V. A. (real name Rudnev) **V**. **A**., 1874 – 1939. Philosopher, economist, pioneer in economic planning. Arrested in 1932, did two years in prison, then exiled, later returned to Moscow.

Berg L. S., 1876 – 1950. Zoologist and geographer. President of the Geographic Society of the USSR, 1940 – 1950.

Bugaev N. V., 1837 – 1903. Mathematician, philosopher. Advocated study of discontinuous functions, required rigor. He influenced his student Egorov as also Luzin and later Kolmogorov.

Egorov D. F., 1869 – 1931. Mathematician, honorary member of the Soviet Academy of Sciences, president of the Moscow Mathematical Society, 1923 – 1930. Founder of the Moscow school of the theory of functions of real variable. In 1929 persecuted because of religious convictions, died in prison.

Falkner S. A., 1889 – shot 1938. See Lapina (2011). Specialist in circulation of money and emission. Apparently common-law husband of the high-ranking Bolshevist troglodyte Maria Falkner-Smit, later Smit (Lapina 2011). I described just one example of her unbelievable ignorance (Sheynin 2017, Note 8a to Chapter 15).

Frenkel Ya. T., 1894 – 1952. Physicist, Correspondent member of the Soviet Academy of Sciences, Stalin prize 1947.

Kol'tsov N. K., 1872 – 1940. Most eminent biologist, Correspondent member of the Soviet Academy of Sciences. Prosecuted in connection with the virtual ban on genetics, died of infarction.

Kondratiev N. K., 1892 – shot 1936. Founded the Conjunction Institute. Advocated the development of the New Economic Policy, stressed the importance of commodity-money relations. Became persona non grata, imprisoned since 1932.

Lopakhin L. M., 1855 – 1920. Philosopher (idealist), psychologist.

Ramzin L. K., 1887 – 1948. Combustion engineer, professor, Stalin prize 1943. Death sentence after the framed-up trial of the Industrial Party commuted to long-time imprisonment.

Ryabushinsky P. P., 1871 – 1924, abroad. Big businessman and banker, leader of the counter-revolutionary bourgeoisie. I was unable to check Kolman's statement about him.

Syrzov S. I., 1893 – shot 1937. Party and state functionary. Opposed the ravage of peasantry. In 1929 criticized Stalin, attempted to struggle with him. Arrested in 1932.

Tschayanov (Chayanov) A. V., 1888 – shot 1937. Economist, sociologist, writer of science fiction, active in the cooperative movement. In 1929 accused of defending the kulaks, arrested in 1937.

Ustrialov N. V., 1890 – shot 1937. Historian, philosopher, founder of National Bolshevism. Headed the press in the territory under Kolchak, managed to avoid persecution. Adopted Bolshevism, but was considered too soft-hearted. Exiled to China, worked for the Chinese Eastern Railway. Returned 1935, struggled to find employment. Arrested in 1937.

Zhang Xuellang. In 1930, governor of a province in Manchzhuria.

Zinger A. V., 1870 – 1934 abroad. Physicist.

Centrography. A direction in socio-economic geography. It determines the centres of various social and political phenomena. Its founder in Russia was Mendeleev. He determined the centre of Russia and studied the movement of the centre of population of the USA from 1790 to 1900. In the USSR it was crushed but rehabilitated in the 1980s. See Dmitriev (2016). Kolman (1930), a bit earlier:

This science goes back to Mendeleev who is known to be not only a talented chemist, but also a business agent of oil producers and a mediocre economist.

He had not justified this statement.

Conventionalism. It requires that theories ought to be non-contradictory but not necessarily reflect reality. Its most eminent representatives was Poincaré.

Poslednie Izvestia. An expatriate newspaper in Paris, 1920 – 1940.

Quantitative theory of money. It issues from the proposition that the purchasing power of money and the level of prices depend on the quantity of money in circulation. After transformation it was included in a version of economic theory.

Trial of the Industrial party, 1930, of alleged saboteurs, 1925 – 1930. About two thousand people were arrested, all of them rehabilitated in 1989. The third edition of the *Great Soviet Encyclopaedia* (vol. 21, 1975) still called the Industrial party a counter-revolutionary organisation, and Kondratiev was still a saboteur.

Vitalism. Advocated the existence of a supernatural force in organisms. Some of its propositions are still valid.

Vozrozhdenie. An expatriate newspaper in Paris, 1925 – 1936.

The history of the Soviet Union was described by George Orwell (*Animal Farm*, 1945 and many later editions)

VIII

E. (A.) Kolman

We Should Not Have Lived That Way (Russian)

New York, 1982

Translation of its small part

Page 109. [The discussion about signing the Treaty of Brest-Litovsk.] Majority of the Party functionaries led by Bukharin decided that the war should continue, others, following Trotsky, thought that the war should end, the army demobilised, but a peace treaty ought not to be signed. Lenin however demanded immediate peace.

110. Lenin: *an indecent, dishonest, predatory peace* to preserve the Soviet power. [Russia was all but sacrificed in the name of later horrors.]

111. Kolman described an episode which revealed Trotsky as a *wilful and stupid despot*.

On 9 January 1905, a peaceful demonstration in Petersburg was fired upon by soldiers. Kolman states that actually some White Guards opened fire from the roof of a house and that all of them were discovered and exterminated.

[White Guards had no reason at all to fire. It was suggested that agents sent by Parvus, Lenin's mysterious teacher and an adventurer, had fired at the soldiers who were thus provoked and opened fire upon the demonstration.

Anyway, Lenin made the most of it and blamed the Tsar who was then, on that *Bloody Sunday*, resting with his family elsewhere.]

120. Bukharin was a typical intellectual of the Russian old school, uncommonly erudite and creative, but surprisingly confused.

121. The Party turned into a Jesuit order of sorts. … A Party member, even if disillusioned, had to lie and play the hypocrite.

[Lenin (speech in 1920): *Our moral is completely subordinated to the interests of the class struggle of the proletariat*. Reminds the Jesuits.]

About 1918, following Lenin, all of us held sacred that the world revolution will soon occur. [I happened to glance at a textbook for Soviet student-lawyers and saw a suitable example. In the first years after 1917 someone was to be imprisoned until the advent of the world revolution. The crazy speeding up of that revolution had remained the top priority of the poverty-stricken country until perhaps the 1980s, and arms and money were sent across the world. And had that Utopia occurred during the Stalin era, he would have ruled the world and exterminated hundreds of millions …]]

122. Dictatorship of the proletariat [degenerated into] the dictatorship of the Party, of the Party bureaucrats, of a single vicious and criminal person. […]

In June 1918, Lenin with his choleric nature made … an irreparable mistake. After a political gamble of the left social revolutionaries [SR's] he ousted all of the SR's from Russia's executive organs [although] a part of them condemned that act and was prepared to continue their collaboration with the Bolsheviks. [The gamble: the assassination of von Mirbach, the German ambassador in Russia.]

124. Just like Stalin, Trotsky was a tyrant.

126. After visiting Lenin in his apartment Kolman wrote: *he and Krupskaya lived unassumingly modestly and simply*.

127. Dzerzhinsky combined in himself a ruthless hate for the class enemy with a genuine humanism. But the objective consequences of his activity are absolutely different. [Still,] he (or Menzhinsky) cannot be compared with such henchmen as Yagoda, Yezhov or Beria.

128. Unlike Stalin, he acted bona fide. Only a premeditated liar or a dazzled fanatic can compare him with Yezhov or Beria.

132. Khotimsky, a genuine scientist, spoke his mind and many highly ranked statisticians became opposed to him. He was arrested in 1937 and *soon he was gone*. [See [vii, Additional information, Kolman]. Kolman apparently did not intervene for his close friend, but perhaps he had remained ignorant until it became too late, or his attempt to help Khotimsky at best would not change anything. But Kolman did not mention anything of the sort which seems strange.]

151. The October uprising in Hamburg was *cruelly subdued*, and in November the communist party was prohibited. The defeat of the revolutionary events in Germany … was a gravest defeat of the whole Lenin's concept of a near victory of the world revolution.

152. [In 1923 Leitner arrived in Moscow.] He was a member of the Central Committee of the German (?) communist party and popular among workers. [Came for being admonished for his criticism of the party. Kolman heard how a high-ranking Party functionary called him a *rascal*.] He was killed, allegedly by some criminals.

155. Just like Lenin, Krupskaya always spoke the whole truth, therefore enjoyed authority among educators and young people. [Lenin?]

156. In 1925 Krupskaya freely discussed questionable issues in education with Kolman.

157. In 1937, Krupskaya was seriously ill and depressed by the situation. When Kolman asked her why will not she herself help an inventor, she answered: *I will only harm him*.

[Apparently at about that time Stalin allegedly remarked: *If she continues to meddle, we will find another wife for Lenin*.]

158. Mayakovsky talked with Kolman about his royalties: *He treated me as though I were a real knacker*. […]

Sholokhov seemed faithless and unreal. Later he became an inveterate reactionary and a Black Hunderter. [A few people including Kolman read his submitted manuscript of the first part of *Quiet Flows the Don*.] The style was heterogeneous, the language irregular, the text contained grammatical and spelling mistakes and awkward turns of speech. [His manuscript was heavily edited.]

[This is an important testimony. Extremely serious doubts have been voiced about Sholokhov's authorship. Thus, no posthumous

papers were found after his death, and one of his texts contained a phrase: *Lopakhin came to the cherry alley,* virtually a copy of Chekhov's phrase. Obviously, the real author (in Russian: the Negro) who was denied authorship played a dirty trick on Sholokhov. Now, Kolman proves that some parts of Sholokhov's texts were indeed his own even if badly written.]

162. [Kolman accidentally found himself in the same lift with Stalin. He described Stalin's unsightly appearance.] Without asking permission Stalin took a book at random from my armful, then another one, quickly leafed them through, put them back and commented with a stressed scorn: *Only mathematics*.

163. [The eleventh Party congress (1930): Kolman was a member of the editorial commission. He read the stenographer's record of Stalin's report, found a strangely constructed phrase, dared to ask Stalin to check it.] Stalin took the report and next day I asked him about it. *Leave it as it is*. Typical stupid wilfulness and arbitrariness.

164 and 165. [In 1933 or 1934 a grand plan of reconstructing Moscow was prepared. Kolman proposed to do away with basements. Stalin came to its final sitting and decided everything.] Stalin asked: how many basements there are? [An impressive number was named.] *That's demagogy*. The basements with thousands of [crowded] flats and establishments were left. [Kolman describes how the members of that commission were mortally afraid of uttering a single word, the reason of which, as he added, he did not then understand.]

166. Ezhov was a sickly, pitiful, unpresentable little man, shabby, extremely narrow-minded, dull-witted, easily irritated and nervous. Molotov was a stupid, narrow-minded and obstinate bureaucrat. [About 1955 I read somewhere that foreign diplomats called him *iron pants* because he invariably refused to budge.]

Malenkov was an engineer, composed and reasonable. Khrushchev had removed him from his high office. [Kolman thinks that Khrushchev *was afraid that Malenkov will supplant him.*]

172. Einstein did not understand dialectics. [Really?] […] Ungifted, but a crafty careerist Rybnikov plagiarized the late Yanovskaya's work on the Marx' mathematical manuscripts.

181. The instigator of the slating in philosophy and history had been either Stalin himself, or it was carried out after his indication, or by zealous do-gooders. […]

Lenin branded bourgeois professors but had not explained that subjectively they could have been most benevolent people. [At the end of his life he became a blood-thirsty madman. Thus, when the valuables kept in churches had been confiscated (although the Patriarch himself informed Lenin that the Church will donate them to assist the starving millions) Lenin wrote that *the more* [of the clerics] *we will be able to shoot, the better*.] […]

182. [Engels: *Anti-Duhring, Dialectic of Nature*; Lenin: *Materialism and Empirio-Crticism, Philosophical Notebooks*.] These books certainly contain obvious mistakes concerning philosophical problems in natural science and mathematics.

182. Neither Engels, not Lenin were educated in natural science.

183. While declaring that the electron is inexhaustible and matter penetrated infinity deep down Lenin had not distinguished the ontological and the gnoseological aspect of the problem.

[This seems too difficult to understand. Some commentators think that for Lenin matter was a philosophical category rather than physical reality. Also too difficult!]

184. Eugenics was declared a pseudo-science. […] I conversed with Kirov, saw a usual, warm-hearted and clever man, a born leader of masses. … Stalin had disposed of him in good time since he had enjoyed too much authority and love, and Stalin saw Kirov as his possible opponent.

That villainous act served him for a general extermination of the very best party cadre. [Zinoviev hardly belonged to this cadre. See also [x, note 5].]

192. Kaganovich, ca. 1936. Superhuman capacity for work. He and Krushchev were not yet corrupted by power. They were unpretentious and accessible. Later, copying Stalin, Kaganovich turned to dirty tricks, shouting and obscene language. [Collectivisation in Ukraine caused famine (1932 – 1933) and even genocide. Both Kaganovich and Khrushchev, but Stalin even much more, were guilty.]

198. Statisticians headed by Strumilin brought much trouble. He denied the law of large numbers in Soviet economy. And mathematics was many times abused in a charlatan or gibberish way. […]

[Kolman (1968, p. 104): *a planned socialist economy is a commercial economy in which the law of large numbers does not cease to act at all*.]

199. They attempted to change the spots of a leopard, to change a hangman. Neither Khrushchev, nor the less so Brezhnev had been sufficiently honest or courageous, and what is the most important, they had no intention to say that more than 20 *mln* were repressed, that more than a third of them perished, that all the trials were falsified … that among the perished were millions of non – Party members. [The perished probably numbered at least twice larger.]

Stalinism is continuing even without Stalin.

200. Means of production were not socialised but *government-ised* so that the working people did not participate in their management and denied … democratic rights. […] The same fright, falsehood and hypocrisy reigned [as in the Stalin era. Brezhnev's cult:] his portraits and phrases *Thanks personally to Leonid Ilyich* in newspapers. […] Societies mostly advance people who are required for the ruling class as leaders.

[Brezhnev was four times Hero of the Soviet Union (three times was the maximal number envisaged), Hero of Socialist Labour and marshal! The nation stagnated (*Brezhnev stagnation*), became *Brezhnevised* (Solzhenitsyn), corruption and inefficiency flourished. I read somewhere that, answering a foreign high-ranking communist, Gorbachev said: *yes, he is old and feeble, but* [*he doesn't impede anyone* (of the Biggest Wigs)].

[The war with Finland:] Stalin provoked it. [It was a manifestation of his] policy of exporting revolution. Lenin theoretically condemned,

but practised it. … It was typical for Stalin's megalomania and great-power manner.

201. In spirit, Stalin was congeneric with Hitler.

202. [The first days of the German-Soviet war.] Stalin secluded himself in his dacha and only after a fortnight came to his senses. […]

Kolman read mathematical analysis, operational calculus and application of the theory of probability to thermodynamics at the Lenin Pedagogic Institute (Moscow).

203. The destruction of the Red Army officers bore its poisonous fruit. [Even Marshal V. K. Blyukher (without any trial) and Colonel-General Stern were shot.]

The Stalinist robbery strategy of *pushing the borders outside* was forcible seizure.

210. In Aktubinsk (now Aktobe, Kazakhstan) Kolman saw Petrovsky going somewhere. He served there his exile. I am not surprised that the Wikipedia says nothing about that exile.

In 1951 or 1952, when imprisoned, Kolman heard the heart-rending screams of L. Stern, an academician. He adds: she was then 70 [actually, 75] years old.

211. Mehlis, head of the political department of the Red Army, guided the compilation of leaflets and texts of local radio broadcasts for German soldiers on the frontline. Kolman stated that Mehlis was ignorant of his duty and his activity had a harmful effect.

213. [Kolman sympathised with Lysenko.] At first, he sincerely believed in his views. After gaining power, he turned to forcible methods of struggling with the opponents of his claims.

215. Nejedly acted inequitably, had not tolerated criticism.

216. In philosophy, Nejedly was an idealist. He voted for my unlawful arrest and deportation to the Stalinist hangmen.

221. Slansky showed me a box filled with complains against Soviet soldiers who seized watches [from Czechs].

[Slansky was the first secretary of the Central Committee of the party.]

229. Suslov is considered as the grey Eminence, the truly agent of the dictator. In 1949, he conversed with a delegation from the British communist party, in particular about the *Jewish question*. Upon returning home, some members of the delegation left their party.

231. Kolman dated the compilation of these pages: November 1978, a few months before his death. He trusts in a genuine, social democracy.

238. The court rabbis: Dymshits at the court of government; Dragunsky, of the army; Mitin, of philosophy; and Chakovsky, of journalism.

245. [About 1948] Soviet, Bulgarian, Hungarian and Romanian military forces were prepared to occupy the obstinate Yugoslavia. But the international situation and the limited technical military equipment [of all those countries] prevented Stalin from carrying out this plan.

[After the war, the Soviet Union had enough *equipment*. German armies were not included since Yugoslavs apparently hated the Germans.]

250. In 1948, Bertrand Russel demanded a preventive atomic war on the Soviet Union. After the Soviets had got their own atom bomb, he began advocating peace. … He attempted to reduce mathematics to logic.

259. After February 1948 [after the occupation of Czechoslovakia] there began to occur phenomena typical for a decaying society under a cult of personality.

260. Kolman mentions the unrestricted power of Slansky and describes a luxurious reception of guests including himself at Slansky's brother villa and the empty conversations there.

263. Lenin perhaps sincerely thought that the red terror was necessary, but it … justified the villainy of Stalin and his heirs. […] I do not at all consider that the October revolution was not needed and only led to suffering. … It opened a new era in the history of mankind.

265. [Kolman describes the essence of a totalitarian regime.] Its most important lever is the hegemonic Party. The caste at its top is united by aspiration to power, fanatic dogmatism, intolerance, suspiciousness and fear of the leader. But there exist internal contradictions.

266. [Lenin's *What Is To Be Done*? (1902):] It ruinously influenced the fate … of the Bolshevik party and of the whole international movement. [Kolman explained:] According to Lenin, the working class can only acquire a social-democratic conscience. Therefore, professional revolutionaries ought to carry out the socialist revolution. The Leninist party *of a new type* is *isolated* from the working class rather than being its avant-garde.

That was Lenin's greatest and fatal theoretical mistake. If the working class is an *element* and gives in to the professional revolutionaries' agitation and propaganda then it can give in to any reactionary demagogy. [Kolman provides examples:] the Black Hundred, the Kaiser military and later Nazi propaganda intoxication. Again, the working class of Russia was unable to withstand Stalinism.

This mistake was combined with another, not smaller mistake. The organisation of professional revolutionaries … unavoidably leads to *the elimination of democracy*. The Party, and especially its elite, will become a privileged … and *horrible power*.

[An embryo of that conclusion is contained in Dostoevsky's *Demons*, 1872 (in translations, this book was also entitled *The Possessed* and *Devils*)].

268. In emigration, Lenin witnessed petty rows but never thought that the same will happen after the revolution. In his *State and Revolution* (1917) he required that the salary of functionaries should be reduced to the wages of workers. After his death this request was *forgotten*. [Kolman then describes the unthinkable marginal benefits accrued to the elite.]

269. *In 1920 and especially in 1923* Lenin mentioned *the corrupted group on the top*. They became *the ruthless masters of the people*.

[An exception: Masherov, the first secretary of the Belorussian Party. In 1980, he published a book in which he criticised the Soviet leadership as arrogant and conceited. That same year he died in a car accident (what a coincidence!).]

270. Uneasiness and confusion are felt in Lenin's last papers.

271. Nothing could have been done. Lenin's attempts to turn a historical process around were doomed for failure. In principle, nothing would have changed even without a Stalin. [Only on a local level] the rank and file Party members were allowed criticism.

273. *Perversion* also occurred in Czechoslovakia as well. The common reason is the existence of a ruling privileged bureaucratic caste.

274. SR's again.

288. [A divergence of top military aviation specialists from Stalin's opinion, ca. 1945.] Stalin pushed through his absurd ideas which threatened the defence [the attack!] capability by hostile elimination of his opponents.

298. Aleksandrov is dishonest and mean,

300. Mikulinsky showed himself as a careerist.

300 – 301. My blind, staunch basically religious trust in the infallibility of the *Great Leader and Teacher* shook [ca. 1953].

301. Beria was the *scapegoat*. He knew too much and was a most dangerous claimant to the succession of the tyrant. Other [Biggest Wigs] were quick to sacrifice him.

302. In 1959 Bulganin, Kaganovich, Molotov, Malenkov and Shepilov were expelled from the Party for fractional activity and resistance to de-Stalinisation. I was unable to shake off a suspicion that Krushchev's main goal was to eliminate his rivals.

Krushchev began to manifest the habits of an obstinate autocrat. He foolishly meddled in science and art.

Brezhnev had been creating his own cult. Human rights were trampled on.

[The story of two large-scale speculators in foreign currencies, Rokotov and Faibishenko, is hardly remembered. They bought currencies from the small fry and sold them (to whom?) and certainly fed up the Moscow militia (quite recently *militia* was renamed: *police*). But then Khrushchev found out from a foreign newspaper (apparently was told about it) that such activity was going on in Moscow. He demanded to quench it; he possibly was mainly angered by the bad publicity *abroad*.

Those two had to be arrested, arraigned for their illegal activity and sentenced to do long time. No, that will not do! Too lenient! And Krushchev demanded a change in the penal code. His command was obeyed, the case was heard anew and the perpetrators sentenced to *the highest measure of proletarian humanism* (Voinovich). A blatant violation of a main commandment: *Law is not retroactive*!

The world was outraged, but *our Dear Nikita Sergeevich listened* (to the admonishment of the cook) *but continued to gobble up the chicken* (Krylov's fable).]

303. 1919. Lenin's speech. He opposed celebrations of personal jubilees and warned that the Party can become conceited. He was not understood. […]

[Kolman:] unshakeably believed in the final victory of the genuine ideas of Marx and Lenin, even if it occurs much later than we thought previously.

304. Wiener's *Cybernetics* [1948, Russian translation 1958] was kept in the special funds of libraries [and a special permit was needed for reading it].

305. But soon that book and the books of Einstein were made generally available.

307. Zhdanov denied the red shift.

322. Lenin arbitrarily ousted him [Sorokin] and thus revealed his spiteful intolerance. [Sorokin became a notable American scientist. Kolman was mistaken: Sorokin could have well been shot for his anti-Bolshevik activities.]

329. [In 1974] anti-Semitism became a state policy. Jewish communists attempted to move to Israel, but there reign the power of the rich, poverty is there humiliated, small nations (?) are deprived of civil rights and supressed. … A vile and unjust society. [Kolman, a Jew, did not understand that Israel was not worse than the main capitalist societies of the world. Or, rather, only worse since the Orthodox Jews are too powerful.]

343. After 1961 most Russians are politically passive, indifferent and do not believe that the situation will improve under other leaders.

306. Mitin is an obscurantist.

347. Ulbricht had been especially troubled and was the first to demand sanctions against Czechoslovakia [against their strive for *socialism with a human face*].

348. Dubcek was soft-hearted.

351. The Soviet leadership was afraid that the events in Czechoslovakia will spread to the USSR.

353. Lenin feared conceit.

355. Mitin and Konstantinov were guardians of order.

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IX

A. Lozovoy

The aftermath of sabotage in the statistical science

[Review of] B. S. Yastremsky, A. Ya. Boyarsky, V. S. Novikov, P. P. Shusherin, O. S. Davidova, *Kurs teorii statistiki,* 1938

Bolshevik, No. 23 – 24, 1938, pp. 116 – 123

[1] On 15 May 1938, in his address to the all-union meeting of the workers of the higher educational institutions, comrade Molotov[1] indicated the need to compile a proper textbook for such institutions:

The decisive problem consists in ensuring proper textbooks […] *worthy of our great pursuit, the pursuit of socialism. We will then certainly elevate our institutions to a higher stage and they will shine even brighter by new successes*.

A proper Soviet textbook ought to be on the level of modern science and quite simple for understanding it. It should provide the necessary extent of knowledge and at the same time prepare the students for future practical work. Among those disciplines in which the textbooks should be compiled first of all comrade Molotov mentioned accountancy in national economy. However, an absolutely abnormal situation has occurred on the theoretical front of statistics. Lenin's heritage in statistics is being developed extremely weakly[2].

We have no periodical for discussing the problems of Soviet statistics. Student-economists have no satisfactory textbook in statistics. The theory of statistics essentially lags behind practice since the so-called school of Yastremsky and Boyarsky which monopolizes the right to speak on behalf of Soviet statistics is continuing to implant the harmful theory that the object of Soviet statistics is the spontaneous-random milieu.

Any attempt to consider the theory of Soviet statistics as a science dealing with the method of qualitative-quantitative study of planned and regulated processes[3] was and is sharply rebuffed by Yastremsky, Boyarsky and their adherents. Some of those *former* advocates turned out to be enemies of the people[4]. A proper textbook in statistics which theoretically illuminates and generalizes the many-sided daily practical experience of accounting and statistics is needed not only for the students of higher educational institutions but also for inspectors and other workers of accountancy in national economy. Only ideologically armed with the Marxist-Leninist theory of statistics the numerous troops of Soviet intellectuals will successfully cope with their practical work.

Does the reviewed textbook meet the requirements made by the Party and government to Soviet textbooks for higher educational institutions? That textbook does not meet even a single demand laid down by comrade Molotov, it is compiled not on their basis but in spite of it. For the students some of its chapters, in particular, *Law of large numbers, Ordered series, Measurement of connections* are

difficult to master. In addition, they are not connected with other parts of the book.

An attempt to illustrate the expounded propositions by examples taken from our reality is made in other chapters but the connection between those propositions and their application to the analysis of socialist construction in the mentioned chapters is either completely lacking or bears an artificial character.

For a long time the enemies of the people, Osinsky, Kraval, Khotimsky, Brand et al had been pursuing statistical science. From 1932 onward the *team* including Khotimsky, Brand and the editors of the latest edition of the *Course in Theoretical Statistics* a few times issued the textbook *Statistika*. It was run through by a wrecking directive about *the dying out of statistics under socialism*. It tore the theory out of socialist practice, presented idealistic scholastic *theories* such as *the theory of spontaneous-random processes*. The compilers of the textbook regarded that *theory* as the *very foundation* of statistics.

Such processes are the only property of statistics. That was the proposition which had been cultivated and implanted for many years in the statistical science. The need to do away with statistics under a planned socialist economy was derived from the theory of such processes. In the new edition of the *Course in the Theory of Statistics* (1938) Boyarsky, Yastremsky et al had not abandoned their previous wrecking directives. The present editors plentifully provided clamorous invocations which ring falsely since that edition retained the previous directives. Just like formerly, protrudes the celebrated slogan of the saboteurs: *Down with statistics in a planned economy*! And the lion's share is still occupied by a scholastic treatment of the law of large numbers and the random sample, by the spontaneous-random, i. e., by that which the authors call a random process.

True, in the introductory part Boyarsky, one of the editors, removed the anti-Marxist scholastic patterns with the capitalist market being identified with a game of chance. He said:

In a game, the market is only represented in the most simplified sketchy form,

but the spontaneous-random sample occupies *the central place* of the textbook in respect both to extent and content.

Under the pressure of the Bolshevist criticism the compilers of the textbook had retreated only partly. Thus, in the new edition the section about the theory of means is placed *before* the law of large numbers and, consequently, is not apparently anymore connected with the so-called probabilistic patterns. However, the only consequence of that reconstruction is ambiguity, a complete alienation of mean values from statistical series from which they are derived. Practical statistics became isolated from theoretical statistics which the authors understand as mathematical patterns.

Judging by everything the authors understand that they are in a blind alley but they do not even attempt to get out of it. They simply do not pose fundamental problems but avoid them or get rid of them by obscure empty formulas. They indistinctly mention the tercentenary of statistics and damn its ardent liquidators, but this does

not improve the content of their textbook at all. The idealistic conception [of what?] in Chuprov's spirit is ruling the textbook[5].

When the authors define statistics as *the theory of the spontaneous-random process* they only issue from the standpoint of the bourgeois theoreticians of statistics, Quetelet, Lexis, Chuprov et al. They are unable to isolate the valuable in the theories of those scientists, for example, of Quetelet, and slavishly reproduce exactly that which should be thrown away like an unnecessary bourgeois rubbish. In a letter to Kugelmann of 3 March 1869 Marx characterized Quetelet[6]:

In the past, he rendered important services. He proved that even the apparently random in social life possess an intrinsic necessity due to its periodic recurrence and its periodic mean numbers. However, he was never able to interpret that necessity. He did not advance, he just extended his observations and calculations.

Quetelet built his idealistic theory of *the average man*, and the authors of the textbook do not go far from it. In the edition of 1936 of *Statistika* they state:

An isolated individual is already only regarded like a definite deviation from the mean.

In another book, *Ocherki promyshlennoi statistiki* (Essays on Industrial Statistics), 1937, edited by Boyarsky, they (?) declare that a man is a statistical totality (a collective) of cells. Speaking against the *spontaneous-random* concept, we do not at all intend to banish the theory of probability from statistics (Lenin, 1899, *Development of Capitalism in Russia*, end of Chapter 5):

Statistics ought to illustrate the social-economic relations established by a thorough analysis[7].

Under a planned socialist economy a Soviet economist rarely has to deal with processes showing the so-called equal possibilities, i. e., equal chances, just like in the game of pitch-and-toss. It is therefore absolutely unnecessary and wrong to place the theory of probability at the centre of the entire course[8].

Meanwhile even in the latest edition of *Statistika* the authors juggle the anti-Marxist wretched so-called idea of equal possibilities as though criticizing the bourgeois theory of probability. By carrying the anti-Marxist theory of equal possibilities to the events of social life the *Course in the Theory of Statistics* disarms the students, kills their will to work creatively in the area of accountancy in national economy.

The *equal possibility* which is preached in the textbook is mechanically carried over to social relations in our socialist reality. The authors shield the wrecking thesis about the dying out of statistics by stating that statistics can only be wholly applied under capitalism whereas in a socialist economy the central place is occupied by accountancy rather than by statistics:

Contrary to the capitalist economy with its spontaneous character the socialist economy as a planned economy which is not a totality of independent elements is the object of accountancy rather than statistics. […] *The economy itself ceased to be a statistical collection of independent producers of commodities. However, when solving a number of its quite definite and often extremely important problems the socialist accountancy resorts to the statistical method*.

It occurs that when the socialism in our country gains victory, statistics and the statistical method will only be possible to apply in exceptional cases. At one time the press disclosed the antiscientific directives of the textbook. When issuing the new edition, the authors were obliged to reconsider radically their previous standpoint, but they have not done it,

On the contrary, they fraudulently juggle the antiscientific directives of the previous editions. Beginning in 1932, they preach the antiscientific wrecking definition of statistics. The only object of statistics as a science, according to Boyarsky and Yastremsky, is the spontaneous-random milieu rather than the planned economy. This is eloquently stated on p. 30 of the second edition:

For a regularity to have a specific statistical essence […] *it is necessary that the individual elements were random*.

In the latest edition of the textbook Boyarsky and Yastremsky do not provide a direct definition of the object of statistics although there is a special chapter, *The object of the statistical science*. Each previous edition of *Statistika* contained a confused antiscientific definition of the object of statistics as a science. They believe[d] that statistics studies

Totalities of intrinsically connected, qualitatively homogeneous but externally independent and isolated elements as well as the regularities which act in them.

And the authors considered a statistical totality (a collective) in *its abstract form* which covers the variety of the phenomena of the definite reality.

When studying the regularities of the development of our economy the vulgar definition of the object of statistics tears apart the dialectic unity of the concrete and the abstract. The members of the *team* juggled the viewpoint of the bourgeois statistician Chuprov who also declared that statistics as a science studies

Masses existing in stable equilibrium and consisting of externally unconnected magnitudes[9].

Instead of a clear rejection of the anti-Marxist definition of the object of statistics as a science, instead of an honest, direct criticism of their mistakes, Boyarsky and Yastremsky apply brakes and cover their tracks. On p. 438 of the *Course*, edition of 1938, they declare:

We cannot require the elements of a totality which are considered by statistics to be externally independent. Even apart from the term itself which is not sufficiently clear, we cannot speak about something external as contrary to the inner (my stress – A. L.).

We can thus understand this declaration: the contrast between the external and the inner is not obligatory but allowed, and this is a new proof that they, the authors, remain at the previous Chuprov's position and are the bearers and champions of the bourgeois statistics[6].

A peculiar picture emerges when contrasting the various editions of the textbook. In the edition of 1938 a number of coarse idealistic mistakes which were indicated by the critics seem corrected. It does not contain simple repetitions of former propositions. However, after careful consideration it emerges that the previous wrecking propositions did not disappear but were reproduced in a new form.

In the edition of 1932 (pp. 335 – 336) the idealistic theory of means was advanced absolutely plainly. The authors believed that *the difference between the means of two kinds is generally accepted*. One mean is derived for a definite collective of magnitudes (mean crop capacity productivity of labour etc.) and occupied the second place. The first place was provided for means derived from a series of observations of one and the same magnitude, but [possibly] corrupted by differing errors. For example, in astronomy, when the distance of a star from the Earth is measured, the mean of the observations is assumed as its *true* value[10].

This method was transported by the *theoreticians* of statistics onto social life. People, crop capacity, production, everything, they say, has its own *true* (i. e., mean) value. If in reality concrete magnitudes always change both in time and space, it is only *deviation*. And the aim of statistics is in essence reduced to measuring these. By issuing from this concept our authors consider such means as *means of the first kind* which generalize collectives *created by measurement*. They imagine that in statistics the substitution of an objective analysis by a subjective is quite appropriate.

In later editions this place was changed. Instead, the authors apply the following method. Means are defined as *abstract indications of collectives on the whole* and add (1936, p. 227):

On the other side, an isolated individual is now represented only as a definite deviation from the mean.

Such *constructions* can lead to statements that the Stakhanovite productivity or the increased industrial use of energy result in *deviations* from the mean norm[11]. Such is the spirit in which the authors (1938, p. 311) consider the Stakhanovite movement by applying the patterns of the theory of probability.

The falsehood of a simple transfer of a mean considered for a certain number of workers or kolkhozniks onto the entire mass is obvious. Measurements ought to separate the working people into groups according to systems of wages, use of the technical arsenal etc. They ought to issue from the knowledge of the conditions of work rather than from manifestations of *spontaneous-random* regularities which transform the entire mass of working people into some one-dimensional *general totality*.

In 1936 (p. 54) the authors had not changed that proposition but appended a remark:

This should not be confused with the so-called statistical norms.

They had thus attempted to shield their theoretical poverty, their trust in randomness. The statistical norms had been derived by calculating means of daily measurements of practical work without allowance for factors which determine the productivity: system of wages, socialist emulation[12], technical level of work, degree of organization etc. Such norms reflected backwardness rather than advancement. The authors of such norms as though *swore allegiance to our backwardness* (Stalin).

The propositions expounded in the textbook contradict all the indications of Lenin and Stalin, of the Party and government. In a

certain place Boyarsky quoted Lenin who had protested to the oblivion of the

Most elementary requirements of economic statistics which obliges to separate rigorously the owners and the hired labourers in whichever form of landownership they are united, or however numerous and variable are their transitional types[13].

Boyarsky also declares, now on his own behalf, that the mean thus (?) derived will represent not a collective but an empty space between collectives:

Only when numerous transitional types are present, this empty space will as though be filled.

Lenin requires separate means for separate collectives *however numerous* …, but Boyarsky says that transitional types create an appearance of filling the empty space between them. Slightly changed, the same is repeated in 1936. Moreover, Boyarsky juggles this revisional proposition about means into the latest edition of the textbook (1938, p. 158).

There, he attempts to pile up unconvincing words and thus to cover the tracks of his previous antiscientific statements. But, among this pile we can find the frame of his *theory*. When we calculate mean values, as his theory states, we as though disengage ourselves from observing objective mass phenomena, i. e., from real totalities and arrive in the realm of *abstractly adjusted collectives*.

Such collectives can be obtained in a spontaneous-random flow of externally independent random events. But what can this proposition have in common with Marx' clear indication about calculating means? Take for example the known beginning of the eleventh chapter on cooperation. There Marx indicates that for establishing the mean duration of a working day the sum of the working hours [a quotation follows. It ends by stating that the daylong social mean is thus obtained.].

Does Marx *remove himself from individual differences* and *adjusts them* by the method of Boyarsky, the doctrinaire? How did Lenin calculate the mean area of the rented land? [He divided etc.] Did Lenin *adjust* magnitudes and transform the peasants' homesteads into a flow of independent random events? No! Lenin always dealt with real collectives possessing definite *qualitative* contents. For him, it was far from indifferent whether the statistical totality consisted of peasant homesteads or balls of differing sizes. For Boyarsky, however, the qualitative content of a totality is indifferent[14]. He proposes to calculate mean values not of a collective but of their members by *adjusting* them.

If each member of a collective can be transformed into a mean then neither statistical totalities, nor arithmetic, nor mathematics, nor common sense in general is needed. Boyarsky presents this pedantic nonsense as his *original theory of means*.

And in an absolutely naked way appears the Yastremsky vulgar bourgeois theory of means. Elsewhere in the textbook he writes:

In the previous section we adduced an indication made by Marx that the deviations of the productivity of labour of many industrial

workers from the mean are mutually cancelled the same way as the errors of measurement are in Gauss' theory of errors.

Pursuing his fraudulent aims he then quoted the first volume of *Das Kapital* [in essence, the quoted passage does not differ from the above]. Yastremsky intentionally cuts short this quote so as to shield his falsification of statistics. Had he provided the full text its essence will be seen in a quite different light. [Lozovoy quotes the next lines but nothing new emerges.]

In the new edition of the textbook the section about mean values provides a still gloomier picture. The text is shortened but the confusion increased. The method of grouping and the theory of means are completely isolated from series of distributions (?), see pp. 137 – 142, 295 – 317, 94ff. The same absolutely unjustified isolation occurs between the arithmetic and geometrical means. The application of the former is generally known, for example when determining the wages of, say, teachers, the mean income from a work-day in a kolkhoz[15], daily mean distance travelled by a railway freight car, mean tonnage of ships. Lenin widely applied it, for example in his *Development of Capitalism in Russia*. There, he skilfully used the theory of means and heavily criticized the narodniki [populists; criticized all of them?] for their wrong application of the arithmetic mean.

The geometric mean is mathematically connected with the arithmetic mean. When analysing economic phenomena our statisticians apply both means.

Concerning the basis of the statistical theory the authors remained on their previous position. And methodically the new edition provides an even more dismal picture. The main properties of the arithmetic mean which support the entire mathematical part of statistics (!!) are not mentioned at all.

In their accountant-statistical practice the saboteurs intentionally complicated the pertinent forms, numbers became piled up and did not yield to treatment[16]. This impeded the efficiency of accountancy and statistics. Just the same, in the latest edition of the textbook (to say nothing about its previous editions) stochastic mathematical patterns are also piled up. For students, they are difficult to understand and often irrelevant. An obvious example of such an ostentatious erudition is the chapter *Dynamic series* [Time series], where, on p. 374, integrals, formulas from higher mathematics *invented* by the authors are heaped up for no reason at all.

But at the same time the textbook lacks most important mathematical sections without which the theoretical propositions remain unsubstantiated. The content of the textbook is absolutely beyond the students' understanding and the form of its description is incomprehensible even for instructors. In general, all the chapters are expounded in such a manner that a rank and file skilled statistician reads them with greatest difficulty. The typical features of the entire textbook are muddle, slipshod formulations, and carelessness.

The press mentioned the absence, in the previous editions, of a section on statistical observation. Bearing this in mind, the authors introduced such a section in the latest edition. But did the textbook gain anything? No. It would have been better to leave the gap as it was

than to add that which Boyarsky had written. Take for example the most important subject, everyday observations. The following conditions should be certainly met: timeliness, completeness, uniform distribution of the net [of observations], simplicity of the programme and compulsiveness. Otherwise an everyday registration is impossible.

Nothing of the sort is present. Instead, on twenty pages, Boyarsky endlessly and tiresomely repeats the incomprehensible separation of the time of observations into *objective* and *subjective*. On p. 82 the connection of everyday registration and a census is explained as a *correction of inaccuracies*:

Some inaccuracies inevitably accumulate in the indications covered by everyday statistics. Their correction also requires a census.

The chapter about everyday statistics testifies to the author's absolute ignorance of practical Soviet statistics. It is compiled formally, dryly, without any vivid examples and is insignificant for statistical practice.

The textbook was intentionally compiled in a manner which prevented the student to approach nearer practical work. Everywhere, in all the chapters we see an attempt to adapt alien bourgeois ideas to our socialist reality, we discern a stubborn yearning for a thrust of scholastic patterns on the analysis of our socialist economics. Issues of economic statistics are weakly elucidated. The attention of the compilers is centred on mathematical scholasticism with *economic illustrations*.

Lenin pronounced his celebrated words, *Matter disappears and only equations are left* exactly about such apologies for mathematicians like Yastremsky and Boyarsky. The conclusion about the new edition of 1938 is evident. The *team* of the compilers had not changed its idealistic and mechanistic positions either when solving general issues of the statistical theory or when treating separate categories of statistics.

They, the compilers, remained on their formal scholastic positions. Their anti-Marxist approach to the solution of a number of theoretical problems shows that their methodical directives and the very methodology of exposition did not change. Their mistakes are rooted in the Trotsky-Bukharin disregard for the significance of statistics under socialism.

The enemies of the people narrowed the problems of statistics, ousted the statistical method from accountancy and in the long run preached the elimination of statistics, advocated its disappearance under socialism. The influence of these hostile theories tells on the latest edition of the *Course in the Theory of Statistics*. We ought to pronounce it worthless.

Publisher's announcement on same page

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Notes

1. Highly ranked people invariably attached *comrade* to the Biggest Wigs. I even read somewhere that *Comrade A* (chairman of a kolkhoz) *told citizen B* (a kolkhoznik) … Molotov had mentioned accountancy but not statistics (cf. Note 4) but Lozovoy naturally did not comment.

2. *Front* (and *troops* below): never forget the *capitalist surrounding*!

Lenin's heritage: Lozovoy praised the non-existing statistical merits of Marx, Lenin and Stalin. Some of the statements of these icons were wrong (see below). Kotz & Seneta (1990, pp. 84 – 85, 78, 86) mentioned Lenin's *misleading use of means, tendentious use of statistics* and *statistical and political apologetics*. See also Sheynin (1998, p. 530). And, for good measure, at the end of his paper Lozovoy kicked Trotsky and Bukharin.

3. Qualitative-quantitative study: See also Note 7. The repeatedly mentioned *spontaneous-random milieu* was apparently a screen: it was sometimes ideologically dangerous to say *random*.

4. *Enemy of the people*: a translation of *Volksfeind*, a term invented in Nazi Germany. N. Osinsky (real name V. V. Obiolensky) was the main partisan of accounting. He was arrested in 1935 or 1937 and shot. I. A. Kraval, in 1935 – 1937 assistant chief of the State Planning Committee. Shot in 1937. V. I. Khotimsky, a genuine scientist. See [vii, Additional information]. L. Brand (real name Brandengendler), was falsely accused of corrupting the results of the census of 1937 and shot and the state statistical service was decimated: Stalin previously stated that the population of the Soviet Union numbered 170 *mln*, but only 162 *mln* were counted. Sabotage!

5. This nonsense about Chuprov was likely prompted by Starovsky's (1933) ditto. A bit below Chuprov was called a bourgeois statistic. To the name of each author who did not quote Marx or Lenin Lozovoy attached the adjective *bourgeois*.

6. Marx (1952, pp. 81 – 82). It is difficult to understand his statement. And even Süssmilch knew that the relative number of marriages was stable and Kant knew it as well (Sheynin 1986, p. 283). There also I quoted Galton (Pearson 1924, p. 420) who had stated much the same as Marx later did but quite understandably.

7. A patently wrong statement: no thorough analysis is possible without statistical data. And it was apparently this mistake which led Soviet statisticians to parrot the expression *qualitative* (=Marxist)-*quantitative* study and maintain that statistics played a subordinate role.

Cf**.** K. V. Ostrovitianov, the vice-president of the Soviet Academy of Sciences (Anonymous 1954, p. 82): It is impossible to maintain that *the same methods of research were used in economics and stellar astronomy*. He directed this rubbish against Yastremsky and another statistician.

8. Only one of many unthinkable stupidities. Lozovsky (below) even called anti-Marxist the *equal possibility* approach. Also see Note 10.

9. This quote without a reference is unworthy.

10. True value, theory of means: see Sheynin (2007). That theory was more general than the theory of errors since it additionally considered means of variable magnitudes. Hilbert, in his celebrated report of 1901, was (one of the?) last to mention it. Below, Lozovoy ignorantly discusses the arithmetic mean: he shamelessly declares that the mathematical part of statistics rests on that mean. He was unable to say anything proper about the geometric mean.

11. The Stakhanovite movement: see Sheynin (1998, Note 13 on p. 537). More coal extracted by Stakhanov meant the need for more timber, more railway cars to transport the coal and an increase in the production of the iron and steel industry. So Stakhanovites had to emerge everywhere and the Soviet Union had to pull itself up by the hair. It did not, the end result was about the same as previously.

In some cases the management should have better organized the work, but obviously each manager was mortally afraid of making a mistake, i. e., of becoming a saboteur. The *English* Wikipedia (*Stakhanovite movement*) quoted a passage from a Soviet newspaper published during the short period of de-Stalinisation. It called that movement *Stalinist propaganda maneuver*.

12. Socialist emulation: see [vii, Note 29]. A bit below Stalin ignorantly (or intentionally) combined usual statistical data with data pertaining to the (some positive) results of the Stakhanovite movement.

13. *Development of Capitalism in Russia*, end of Chapter 12.

14. See Note 7.

15. A workday: the payment for such a day in a kolkhoz. It was much less that miserable. In 1942 our school class was sent to work in a kolkhoz for perhaps 15 days. I heard the kolkhozniks mutter: *we are working for ticks* (registered by the kolkhoz accountant).

16. It was common knowledge that unnecessary information had been often required out of usual pedantry.

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Some Bibliographic Information

I adduce information about books not necessarily mentioned by Lozovoy but written/edited at least partly by the same people which he severely criticized.

Abreviation: B – Boyarsky; Br – Brand; D – Davidova; Kh – Khotimsky; S – Starovsky, Ya – Yastremsky

Teoria matematicheskoi statistiki (Theory of Math. Stat.), an insensible title. 1930 and 1931. Editors, Kh, Ya. Authors, B, Kh, S, Ya

Obshchaya teoria statistiki (General Theory of Statistics), 1930, 1931. Ed., Kh, Ya. Authors, B, Br, Kh, S, Ya

Statistika, 1932. Team led by Kh.

Same, 1936. Ed., Kh, Ya. Authors, Br, D, Kh, S, Ya

Elementy obshchei teorii statistiki (Elements of General Theory of Statistics), No. 1 – 2, 1933. B, Br, D, S without separation between editors and authors

Statistika. Osnovy obshchei teorii (Statistics. Elements of General Theory). No date. B, Br, D, S without separation between editors and authors

Same, 1936. Ed., Kh, Ya. Authors Br, D, Kh, S, Ya

X

Anonymous

Boris Hessen

The Socio-economic roots of Newton's *Principia*.
Internet. *Bibliochronica* No. 6.
Unesennye v Bessmertie (Carried away to Immortality)
No date

Introduction by the Translator

The main source about Hessen is the Russian book Korsakov et al (2016) with an appended list of Hessen's publications and unpublished archival materials which substantiates his election to the Academy of Sciences. I only mention a few relevant points. **1.** Hessen studied in Edinburgh for one year, which contradicts the statement of the anonymous author. **2.** In 1929 Hessen became professor of history and philosophy of natural science, Moscow University. **3.** In 1924 – 1936 Hessen edited the most prestigious Soviet physical journal, *Uspekhi Fizicheskikh Nauk*. From 1958 it also appears in English; its present English title is *Physics-Uspekhi*. **4.** Hessen initiated a Russian translation (by Khinchin; Moscow, 1930) of Mises' book (1928) *Wahrscheinlichkeit, Statistik und Wahrheit*. **5.** In his incomparable report of 1931[1] Hessen noted Newton's theological influence as well. I assume that this fact was barely noticed (and never mentioned in the Soviet Union). **6.** In 1957, Hessen was posthumously restored as an academician. **7.** The book describes in detail the incessant struggle of the official Soviet ideology with Hessen and a few of his followers which began even before 1931. A loathsome role in the persecution of Hessen was played by A. K. Timiriasev (son of K. A. T.).

And I also ought to add that in 1926 Karl Pearson stressed Newton's theological influence. This is also seen in his lectures of 1921 – 1933, see Pearson (1978). That same statement of 1926 is found in a weak form on pp. 303 and 353. On p. 212 Pearson mentions that influence on Maclaurin, and on p. 286, on Derham. Lastly, on p. 576 Pearson states that Lagrange studied Newton's religion and philosophy.

The second International Congress of the History of Science took place in London in 1931. There were about 250 delegates from 25 countries including the USSR. After the isolation of the 1920s, it was one of the first invitations of Soviet scientists to an official international meeting. And to this visit the highest Party leadership naturally attached not only a purely scientific, but, first of all, an

ideological significance. In the imperialist den the Soviet delegation should demonstrate that the Marxist ideology was highly beneficial for the study of the history of science.

The Soviet delegation consisted of eight members: academicians N. I. Bukharin, V. F. Mitkevich, A. F. Ioffe and N. I. Vavilov (including two later enemies of the people, the first and the last mentioned), professors M. O. Rubinstein, B. M. Zavadovsky, E. Kolman and B. M. (Boris Mikhailovich) Hessen. The official head of the delegation was Bukharin, and Kolman was its Party secretary: three delegates were Party members, Bukharin, Hessen and Kolman himself[3].

Kolman was instructed to keep an eye glued to the ideological deviationists, Bukharin and Hessen. That command was extremely serious. In a few years six of the eight delegates had been arrested and three of those six lost their lives: Hessen, in 1936; Bukharin, in 1938; and Vavilov, a member of the Royal Society, although only in 1943 and not shot, but died of terrible prison conditions.

A month after returning home, Rubinstein reported in detail and very emotionally at the Communist Academy:

The organizers of the Congress, in spite of their stressed politeness, were not especially pleased with our presence. For them, the Soviet delegation was an alien body which disturbed the cosy plan of the Congress's work. Without it, the Congress would have slipped by, quietly and peacefully, just like a number of receptions, excursions etc.: mild attention of the press and the newspapers' reports in small print. […] *Actually, the press became excited, two questions were asked in the parliament. For the organizers, all this was shocking.*[…]

Indeed, [in Moscow] the preparation for the Congress was thorough. All the future eleven reports were published in separate booklets. In London, the delegation secured their joint publication in an English book (*Science at the Crossroads*. London, 1931; reprint: New York, 1971). Hessen's report (pp. 151 – 212) became the ornament of the Congress, stirred up a great number of responses, both pro and contra. Here is its content:

1. Marx's theory of the historical process; **2.** Economics, physics and technology of Newton's period; **3.** Class struggle during the English revolution and Newton's philosophical outlook; **4.** Engels' conception of energy and Newton's lack of the law of its conservation; **5.** Machine-breakers of Newton's epoch and the present-day wreckers. (No mention of the *wreckers* in the Soviet Union, see [vii] and [ix])

Loren Graham, professor at Massachusetts Institute of Technology and an outstanding specialist in the history of Soviet science wrote in his paper (1993, p. 20):

Is it necessary to prove that, according to the extent of its influence, that report was a most important event ever heard in an audience of historians of science?

But what was the cause of that success? B. I. Kozlov, a historian and philosopher of science and technology, indicated [where? when?]:

B. M. Hessen was the first who posed the problem of the scientific and technical base of Newton's creative work, and Hessen's report caused a wide response among the circle of the historians of science. In spite of the restricted number of sources, he managed to show both Newton's incessant interest in technological practice and some more definite aspects of his scientific and technological activities. Here is how he formulates the essence of his approach to the reconstruction of the history of science [translated from Russian]:

First of all, I investigate why exactly the development of industrial rather than commercial capital raised the problem of the steam engine. This explains why that engine became the central object of study not in the Newtonian epoch, but in the directly following period although its invention occurred in that epoch (Ramsay, patent of 1630).

Thus, we will see that the connection between the development of thermodynamics and the steam engine is the same as between the technical problems of the Newtonian epoch and his mechanics. […] *Newton had not raised or solved the problem of the conservation of energy certainly not because his genius was not sufficiently strong. In every branch* [of science] *great men, however remarkable their talent, formulate and solve those problems which had been put in turn by the historical development of the productive forces and the relations of production of their epoch*[4].

It is not accidentally that in the USSR Hessen's report was published in 1933 and then in 1934. Here is how he formulated the main idea of his study:

A brief survey of the Principia indicates a complete coincidence of the physical subject-matter of the epoch as created by the needs of economics and technology with its main content. The Principia, in the full sense of the word, is a recapitulation and a systematic solution of the entire field of physical problems. All of them were mechanical, so that it is evident that Newton's work was a justification of the terrestrial and celestial mechanics.

He adds, however:

It would be a gross simplification and even a vulgarization to derive directly from economics and technology each problem which had been studied and solved from economics and technology.

Loren Graham insists that Hessen's report was in a certain way a product of Soviet politics, but that does not mean that his approach lacks sense or lacks significance beyond that politics.

Nowadays, with hindsight, it is clear that the beginning of the ^1930s was Hessen's hour of triumph. The comparatively young scientist became professor of physics at Moscow University and dean of the physical faculty. Soon he was elected correspondent member of the Academy of Sciences which meant that he was recognized as a physicist, not only as a historian of science. And if his merits were stretched, it was only in a quite small way.

In 1913 – 1914 Hessen studied at the physics faculty of Edinburgh University, incidentally, with his friend and school-fellow I. E. Tamm. After the beginning of WWI he returned to Russia, became a lecture-goer at the physics faculty of Petrograd University. In 1919 Hessen joined the Russian Social-Democratic Worker's Party and until 1924 had been working as a party and Soviet functionary and participated in the Civil War.

By the end of the 1920s suchlike people were called *Marxists-intellectuals* and it is impossible to understand the main point of that nickname: sincere respect or open hostility. That same Kolman repeatedly and very toughly criticized Hessen for his absolutely sincere attempts to combine the quantum and the relativity theories with Marxism. Graham noted [this is not a quote!]:

There were doubts about him. He wished to show: I am a sincere Marxist but also a scientist, a physicist. I respect Newton's physics and the physics of Einstein. It is a fact that Newton was a creation of a capitalist society and Einstein, a creation of an imperialist society, but this does not mean that their physics is wrong.

In 1927, in one of his papers, he stated: by itself, the fact that, by issuing from the theory of relativity and quantum mechanics it is possible to arrive at conclusions unacceptable for Marxists, is not at all a cause for discarding the physical content of those theorems. For orthodox Marxists who exactly at that time became influential in the official ideology, that approach was inadmissible. In addition, he originated from the middle class (his father was a bank employee), and, on top of everything, he studied abroad for two years.

Gennadiy Gorelik, a historian of physics, notes [no reference]:

In his papers, you will not find crushing blows on his opponents or their pillory. In 1931, not for nothing Marxists criticized him:

These papers are remote from the problems of the party. There is nothing like the Bolshevist spirit in them.

His attitude towards the new physics is clearly seen in this accusation:

In all of Hessen's work we see only one direction: a kowtowing to bourgeois scientists just like to ikons.

It is indeed essential that Hessen was able to find the objects of his kowtowing by following examples, Tamm and the teacher of that scholar, Mandelstam. He deeply respected both and, as director of the Scientific Institute of Moscow University, did all possible to protect the scientific and pedagogic life of Mandelstam's school from social elements.

On 22 August 1936 Hessen was arrested. An entry in his criminal file stated;

Participated in the counter-revolutionary Trotsky-Zinoviev terrorist organization which carried out the villainous murder of Kirov[5] *and in 1934 – 1936, with the assistance of the fascist Gestapo, had been preparing a number of terrorist acts against the leaders of the All-Union Communist Party* (*Bolsheviks*) *and the Soviet government*.

On 2 December 1936 the Military Board of the Supreme Court of the USSR under V. V. Ulrikh sentenced Hessen to death and he was shot that same day.

21 April 1956 the same Board repealed that sentence since no *corpus delicti* was found. 29 April 1938, after the event, the General meeting of the Academy of Sciences expelled Hessen and a few other scientists from the Academy since they *had directed their activity to the detriment of the USSR*.

Autobiography

Published in original Russian and in a German translation by Winkler (2007, pp. 148 – 149). I am thankful to Master Guido Rauscher (Vienna) who kindly sent me the text of that source

Born in 1893. In 1913 finished the eighth class of a gymnasium. In 1913 – 1914 studied at the mathematical department [but added in English: (Faculty of science, department of pure science)], Edinburgh University. I attended and passed examinations in Introduction into analysis and first part of Differential calculus, Prof. Whit[t]aker; Analytic geometry, Dr. Carse; Disintegration and partial forces (?) and heat, Prof. Barkla; physical training workshop, Dr. Carse; Chemistry and chemical training workshop, Prof. Walke … Dr. Dobbin.

During the imperialist war I was unable to [return] to England. In 1914 – 1916 was a student at the economic faculty of the Petrograd Polytechnic school, studied statistics under Chuprov and [L. N.] Mares[s] and mathematical statistics. At the sane time I worked at the physical-mathematical faculty of Petrograd University. Being a Jew, I was not admitted as a student.

During these two years I attended and independently studied Differential and integral calculus, [Ya.] V. Uspensky and [D. F.] Selivanov; Application of analysis to geometry Adamov; Higher algebra, and Theory of definite integrals, [Yu. V.] Sukhotsky; integration of differential equations, [V. A.] Steklov. I was certainly unable to hold any examinations. In addition, I studied by myself philosophy of mathematics and some history of mathematics.

From the beginning of the revolution I had been participating in party work and propaganda. In 1917 until October I was secretary of the organization of internationalists[6] in Elisavetgrad [Kropyvnytskyi, Ukraine]. After the October coup d'état, secretary of the Soviet of workers' deputies; in August 1919, there also [in Elisavetgrad?] member of the board of the department of peoples education. In 1919 – 1921, instructor in political work [a few words are incomprehensible]. From 1921 until now, in the Sverdlov University[7], directed the cycle of economics, then directed the course for lecturers.

Know German, French, English and Latin.

8.7.1924

Notes

1. That report was *foundational in historiography of science* (Hessen. English version of the Russian Wikipedia).

2. For a long time Newton headed the mint. He very successfully investigated the existing technical problems.

3. Graham (1993, p. 26) who had conversed with Kolman, named four Party members. The fourth was Rubinstein

4. This statement is doubtful. Suffice it to mention just three scholars: Leonardo di Vinci, Euler and Ziolkovsky.

5. Kirov, *the favourite of the Party*, became Stalin's rival. Furthermore, he dared to propose the erection of a Palace of Soviets. The palace was never built, but only Stalin had the moral right to make such suggestions! About 20 years ago, the Russian television pulled the legs of their viewers: Impossible! Kirov was well disposed to Stalin. As though that was sufficient … Someone had the nerve to ask Stalin whether he understood gratitude. *Of course I do. It is a wide-spread dog's disease.*

6. These internationalists were probably close to Trotsky.

7. That university prepared its students for government and party work.

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