Studies

in the History of Statistics and Probability

Vol. 8. Collected Translations

Most papers describe the moral situation

in Soviet science

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Notation

Notation **S, G,** i means that an English translation of the appropriate paper is available on my cite www.sheynin.de which is being copied by Google, Oscar Sheynin, Home, in Document i.

General comments on most items

[i] Karl Fedorovich (Ferdinand?) Hermann (1767, Danzig – 1838, Petersburg), was an economist and statistician. Since 1795 he lived in Russia, was professor in Petersburg. In 1821, he and three other professors were compelled to leave the university, and his books were banned. Being an extraordinary academician, he continued his scientific work and was elected full academician in 1835 (*Great Sov. Enc.*, 3rd edition, vol. 6, 1971).

Druzinin (pp. 44 – 45 of his *Khrestomatia*) noted that, contrary to his § 2, Hermann criticized Russian reality in his lectures and that that reality refuted his optimistic views expressed in § 12.

Hermann did not mention either political arithmetic, or insurance of life (which at that time, however, hardly existed in any civilized way in Russia) or medical statistics, and in this respect he regrettably followed Schlözer (and apparently Achenwall). Did not visitations of deadly epidemics belong to remarkable features? And at least until the 20th century neither did population statistics study medical statistics.

Hermann (§ 2) defined the theory of statistics as the science of the initial notions of statistics, but did not even mention those notions. He (§ 9) also provided a pattern for statistically studying a state.

[ii] The author was evidently the diplomatist Petr Borisovich Kozlovsky (1783 – 1840). This statement is indirectly supported by his quotations from ancient classics and references to recent history (Napoleon, Stuarts). He certainly knew something about the history of probability but not really enough (see Notes 1 and 7), and his story about Duke Dalberg should have been provided in a supplement, if at all. I doubt that his contribution had any impact; Gnedenko (1951, p. 108) only mentioned it (and provided a mistaken year of its publication), but did not recall it in his booklet of 1984 devoted to Ostrogradsky. I note finally that the Editor of the *Sovremennik* was Pushkin.

[iii] Over many years, the Petersburg Academy of Sciences awarded prizes for contributions in various branches of knowledge. The money was donated by Pavel Grigorievich Demidov (1798 – 1840) who became an honorary member of the Academy. The lyceum in Jaroslavl was apparently called after him. It had been in existence in 1803 – 1918; at least in 1834 – 1868 it was associated with Moscow University. Its curriculums seem to have been good enough; they included political arithmetic, and, as Stroganov's recommendation suggested, some probability as well.

Chebyshev indeed succeeded in proving important stochastic theorems only by algebra; he had not mentioned that he only dealt with independent variables but at that time this omission was general. Note, however, that even Jakob Bernoulli had managed by algebra with respect to his great theorem.

I believe that a general survey of the theory of probability as well as its applications by Daniel Bernoulli and Quetelet, to name only two scientists, would have been much more important the more so since Chebyshev's derivations were necessarily burdensome.

After compiling his *Essay* (1845), Chebyshev (1846) published its *valuable original supplement* (Bernstein 1945/1964, p. 412). See its detailed study (Prokhorov 1986). Note also that Chebyshev himself (1879/1880, 1936, pp. 162 – 163) described one of his intermediate results in a simpler way. He consistently estimated the errors of pre-limiting relations, and (1846, p. 259) admonished Poisson: *The celebrated geometer* did not *provide the limits of the error allowed by his approximate analysis and* […] *the demonstration does not possess the appropriate rigor*.

Here, indeed, is Poisson elsewhere (1837, § 84):

There exists a very high probability that these unknown chances little differ from the ratios …

Actually, Poisson followed Laplace who had resolutely transferred probability theory from pure (in the sense of mid-18th century: Jakob Bernoulli, De Moivre, Bayes) to applied science.

The concluding part of Prudnikov's extract is unsatisfactory. *First*, it hints at Chebyshev's interest in insurance, but he never discussed that subject. And, in his lectures (1879/1880, 1936, p. 214), when he described lotteries, he even stated that they are equally fair if the expected gains are the same in all of them. For the gambler, this is patently wrong since it contradicts both common sense and the opinions of eminent philosophers and mathematicians. The possibility of large gains, which have extremely low probabilities, is best ignored. But did not Lyapunov only make fragmentary notes, as Prudnikov (1964, p. 183) decided, when writing down Chebyshev's lectures? And, finally, it is well known that up to the mid-19th century insurance had been inseparably linked with cheating.

Second, Buniyakovsky's main contribution to insurance was a chapter in his treatise (1846). There, on p. 215, he noted that *the moral benefit* of the insured consisted in ensuring his future. True, in the same chapter he discussed marine insurance by means of Daniel Bernoulli's moral expectation.

Prudnikov (1964, pp. 28 – 30) reprinted the text of the entire extract.

[v] Vygodsky made a few mistakes in his references. In general, his description of Nekrasov's activities ought to be corrected, but he provided related useful information. Here are my additional comments.

1. At the end of the 1890s, Nekrasov did not stop studying mathematical problems, cf. Note 6.

2. In his correspondence with Markov, Nekrasov dropped his refusal to substantiate the statements of the 1898 paper, although added that the required work was tremendous. That paper was not compiled *in a bureaucratic manner*, it was a report providing his results. Something similar happened with Chebyshev's proof of the central limit theorem (Sheynin 2009, § 13.1-4).

3. Markov requested to be excommunicated from the Church in 1912 rather than in 1902. This is a common mistake occasioned by Tolstoy's excommunication in 1901. However, a few days before his death the Synod discussed whether his excommunication ought to be revoked but resolved to let it stand (newspaper *Rech*, 8 Nov. 1910, p. 3, anonymous note *The Holy Synod and L. N. Tolstoy*). In 1912, this episode was hardly forgotten. Markov was not excommunicated; the Synod decided that he had *broken away* from the Church (Emeliakh 1954, p. 408).

I have published many papers which dealt with Nekrasov and translated many pertinent materials. Nekrasov (2004) is a collection of translations of six papers of Nekrasov, two of Markov, one of Liapunov and of a very rare paper by Bortkiewicz as well as of the correspondence of Markov and Nekrasov and of related materials and the Report (1916). Then, see Sheynin (2003 and 2009, § 14.5). This latter source discusses Soloviev (1997) also mentioned in Note 6.

[vi-b] Lasarevsky Nikolai Ivanovich (1868 – 1921) and Tichvinskiy Mikhail Mikhailovich (1868 – 1921) were shot together with 57 others. They both (and perhaps the others) were rehabilitated.

I doubt that the Appeal, published in Russian, was at least somewhat effective.

[vii] The following archival letters sent by Oldenburg are published by Sorokina on pp. 111 – 119 of the same source: To Gorky, in 1920, 1922, 1929 and 1932; to Lunacharsky, in 1922; to wife, in 1929; to Enukidse in 1926, signed by Karpinsky, the President of the Academy, and Oldenburg. (Enukidze was chairman of the Commission of the Political Bureau of the Central Committee of the Party, shot in 1937). The Commission controlled the everyday activities of the Academy.

Here is a phrase from the letter to Lunacharsky (p. 112):

The [current] *phenomenon is exceptionally menacing since it is a sure indication of the occurring loss of the life of Russian scientists and, consequently, of Russian science*.

On Oldenburg see also Tokareva (2007, p. 125).

[viii] Apart from the stated in my Notes, I indicate that Ermolaeva had not compiled her Bibliography sufficiently good: in many cases the page numbers are not provided. Then, in some places her story is too detailed. Tokareva (2007b) discussed planning in mathematics (partly, in science in general). She indicated that in 1931, at a high-ranking conference in Moscow, quite reasonable proposals were formulated, for example, on planning the work of scientific bodies. Ermolaeva had only touched this subject (in § 4).

[ix-b] Kantorovich stated that his main aim was to apply the Marxist theory to the socialist economy and forcefully declared that the true-blue Marxists were afraid of losing their leading positions. He had to conceal his thoughts, to pass over in silence many negative aspects of Soviet economics, for example, the ruin of stimuli for effective work, especially in agriculture. He did not negate the Marxist law of value and even thought of leaving it (aided by computerized systems) as the governing law of economics, but experience proved his mistake.

He and his followers including some volunteers from other branches of science understood the role of capital in economics and probably had to conceal it under smokescreen. Thus, some of their invented terms were directly derivable from *capital*. The absence of direct references to Marx in some cases was, however, sufficient for their mighty opponents to anathematise them. L. B. Sheynin

I myself believe that Marx had only formulated *principles*, and had certainly understood that they should be somehow applied in practice, but left this goal for the future. His followers and Soviet followers in particular, had been, however, treating those principles as Gospel truths, categorically forbidding modification even in the least possible extent.

[x] The Introduction by the Editorial Staff is carelessly written, see Note 1. In addition, some minor defects of the author were not corrected/improved and a few abbreviations, only understandable to physicists, not explained.

Novikov is probably the only author, and a most eminent at that, who described the horrible situation which had for a long time reigned in the Soviet Academy of Sciences as was unavoidable under the Stalinist and post-Stalinist regimes. A similar source, though not really connected with the Academy, is Yaroshevsky (1991). I myself (1998) described the situation in Soviet statistics.

[xi] Novikov [x] described the disgraceful aspect of the work of the Soviet Academy of Sciences in the 1960s – 1980s, cf. Note 3. Here, he continued in the same vein about a later period, although this time the disgrace was occasioned by a different kind of unimaginable antiscientific stupidity. Elsewhere, he (2000, p. 159) quoted Zalizniak (2000): *during the lifetime of Kolmogorov* [who died in 1987] *nothing of the sort would have happened*. One of Fomenko's discoveries, as Zaliznyak (p. 163) noted, was that *Formerly London was located on the shores of Bosporus*, and he ended his paper thus:

Hardly had anyone inflicted such a heavy blow on the authority of mathematics and mathematicians over public conscience as Fomenko. Only recently the representatives of the humanities could have judged the possibility of a fruitful participation of mathematicians in solving their problems by Kolmogorov's excellent works, but now they will have to judge by Fomenko.

How was it possible for several academicians to side with Fomenko? I have only one explanation: they applied the *Shiryaev method*, the method of Kolmogorov's student, and judged Fomenko's books by his own abstracts! They would have hardly allowed themselves anything similar in their own fields.

For the benefit of the readers I add one more reference: Fomenko et al (1989). Fomenko continued in the same vein (Nosovsky & Fomenko 2004): Jesus was the Czar of the Slavs! Nowadays the Russian Academy of Sciences is all but subordinated to the Russian Orthodox (medieval) Church, and it is opportune to recall the great Markov (Sheynin 1989, p. 340). In 1912, he asked the Most Holy Synod to excommunicate him from that Church. One of his arguments was that he did not *sympathise* with religions which

Like the Orthodox Church are supported by, and in turn lend their support to fire and sword.

The proper way for Fomenko was to compile *beforehand* a list of a hundred, say, most important events pertaining to the critical period, and study their dating, one by one, but invariably having in mind the opinion of Gauss (*Werke*, Bd. 12, pp. 201 – 204):

It is possible to err much when applying the theory of probability if only issuing from numbers […] *and disregarding any other knowledge*.

A short visit to Google convinced me in that Fomenko had flourished right up to his death in 2015, and that Logunov (cf. Note 3) is still flourishing.

And now, to immortalize Fomenko I suggest to refer to previous events in a new way: specify the dated abbreviation BC by adding /BF or /AF (before Fomenko and Anno Fomenko), and specify similarly the AD. Alternatively: **Tar and feather him**!

Now, Shiryaev: above, I have already introduced the term, *the Shiryaev method*, so let it stand.

[xii] The seminar proved quite useless, see my general comment to Novikov [xi], and the report of Sventitskaya was a smoke screen. I am sure that historians were justifiably afraid of opposing leading mathematicians, cf. Note 1. And since Fomenko continued to propagandize successfully his discoveries up to the end of his life (in 2015), see once more my general comment to Novikov [xi]. His story throws light on the horrible moral climate in the Russian Academy of Sciences.

[xiii] It had been widely known that I. Grekova was the pen-name of Professor Elena Sergeevna Ventzel, and *igrek* is of course the French (and Russian) name of the letter *y*. One of her books (1977) was devoted to elementary probability.

Her paper had been and likely still is interesting and instructive although not sinless, see my Notes. Some points of her subject had been discussed in the framework of statistics, see for example Borgatta (1978) and a few other items in Kruskal & Tanur (1978).

[xiv] Orlov apparently had not known the Grekova paper [xiii]. According to the biography of Orlov (Wikipedia), in 1989 – 1992 he headed the All-Union Centre of Statistical Methods and Informatics at the All-Union Economic Society and was a main organizer of the All-Union Statistical Association. Its constituent conference took place in October 1990, and Orlov became its vice-president and head of the section on statistical methods. In 1992, the Russian Association of Statistical Methods was established on the basis of that section, and in 1996 it became the Russian Academy of Statistical Methods.

The surplus of statistical information had been discussed from the beginning of the 19th century (*legions* of new data had appeared). In the second half of that century the same fact troubled meteorologists (Sheynin 2009, §§ 10.8 and 10.9.3). In 1904, Newcomb (*Carnegie Instn* 1905, p. 180) suggested to organize an *institute or bureau of exact science* for dealing with data. The opinion of other scholars were collected, and Karl Pearson (p. 184) noted that about a half of the data was useless, that its rejection will require experts (p. 186) whose work

will lead to *controversy, possibly uproar* (p. 186) and suggested instead a *Statistical and Computating Institute* (p. 187). Nothing was done.

In a letter of 1914 or 1915 Chuprov (Sheynin 2011, pp. 130 – 131) expressed his wish to see an *Institute for the Statistical Study of Russia*. Again, nothing comparable was done even to this day.

Orlov twice, in § 1, mentioned the application of the statistical method to various branches of knowledge. I (2016) suggested that the statistical method is the same as the theory of statistics and, again, as theoretical statistics. The terms medical, stellar, etc. statistics are therefore the application of the statistical method to the appropriate branches of science.

I

K. Hermann [Karl Theodore Herrmann]

The general theory of statistics[1]

Vseobschaia Teoria Statistiki. Petersburg, 1809. Extracts by N. K. Druzinin, *Khrestomatia po Istorii Russkoi Statistiki* (Reader in History of Russian Statistics). Moscow, 1963, pp. 45 – 61

[1] The upheaval in the science of political economy created by Adam Smith necessitates an upheaval in statistics. This discipline always described a man who lived in a state; consequently, it included answers to the questions which he as such was possible to formulate. In ancient times, they mostly concerned the conditions of the army and the finances; in Middle Ages, questions about the civil law and the structure and governing of the state; and in most recent times, statistics is including materials about the study of the conditions of life of the inhabitants of a state.

Quite new questions for the statistician follow from the new basis of the political economy. In addition, he ought to reveal a new theory whose foundations are found in the newest regulations about political economy and politics. Statistics is now so rich in materials and acquiring still more of them, but it still has little pertaining to political economy. And those who collect statistics very often forget the purpose of the statistical information about that science which aims at establishing as precisely as possible the relation of all the classes of citizens to each other and of each such class to all the other ones.

False notions about many issues of the national wealth became the reason for the part of statistics belonging to political economy to be until now so dry, wrong and fragmentary, to lack clear notions, to remain always a collection of dead numbers which crowd memory but are of no use for the mind. Even with a greatest surplus of most detailed statistical information such a collection is unable to provide any clear notion about the might of a state, the well-being of its citizens or their more or less favourable condition. […]

The totality of knowledge having the general and necessary notions as its only or main subject is called science. On the contrary, the totality of knowledge only based on particular events or random experiments is called cognition. […]

Any science or thorough knowledge about external (?) subjects has two parts, a *formal* (or figurative) and a *material* (or constituent). The former part is the theory, and the latter part, the subject itself. They both, after being united into a single entity, constitute the system of a science or of knowledge. The distinctive feature of a theory is its generality and necessity, and that of all the subjects of the experiments, is individuality and randomness. […]

Statistics, like any science or thorough knowledge, consists of two parts, formal and material, and it therefore has a theory. If that theory is sufficient, statistics is a philosophical rather than an arbitrary science. […]

[2] The theory of statistics is a science of its initial notions, showing its importance, and of the essentials necessary for describing its parts as perfectly as possible. […]

The theory of statistics has a *general* and a *special* part. The former considers the notion, boundaries, kinds, sources and usefulness of statistics. The latter offers the main indicators according to which we ought to judge the subjects belonging to each of its special parts. […]

Statistics in its extended sense is a thorough knowledge of the conditions of a state in some known time. […]

Statistics is knowledge rather than a science since it deals with deeds but not notions. It is a thorough knowledge of deeds which are real, detailed and philosophically ordered according to the indicators of the theory, and thus according to the notions. […]

Statistics deals with the conditions of a state. 1) It is therefore a political science and constitutes one of the main parts of the course in politics which ought to precede other political sciences.

2) Its scope is very wide since everything even most remotely bearing on the state is already a subject of statistics; for example, all the branches of industry, all the sciences and arts, the conditions of the people living in the same state, all its laws and decrees.

3) The status of a thing depends on the properties of all its parts so that statistics ought to be as detailed as possible; the more general it is, the less well we know about the status of its special parts, the less perfect is statistics. However impossible it is to publish a detailed statistics about a vast state, there is certainly no reason for ignoring a tiniest detail about a subject belonging to statistics. Any such detail indeed belongs to statistics in its most extended sense. […]

Status is the quality of a thing in a certain time during which it does not essentially change. Therefore, statistics is describing a state during a certain time when no such changes take place. It is thus not a description of a continuous sequence of conditions so that it is necessary to mention the appropriate years. For its inhabitants, the most interesting are its present conditions which therefore constitute the usual subject of statistics. Previous conditions of a state are not, however, excluded from statistics. […]

Statistics only describes its subject but does not judge, praise or blame. Designs or general considerations do not belong to statistics. […]

In the *narrowest* sense, statistics is a thorough knowledge of everything noticeably influencing the well-being of a state in some definite time. […]

[3] And so, everything essentially influencing the well-being of a state, or its remarkable features, as Achenwall and Schlözer called them, is the subject of statistics in its narrowest sense. But how can we find out what constitutes a remarkable feature of a state? […]

A state, just like anything else which exists in nature, has its essential parts without which it is either destroyed or its aims, i. e., its security and the well-being of its inhabitants, are directly influenced. The state also has random parts which can at least be mentally taken away without destroying the state itself.

All, which most thoroughly improves or worsens those essential parts, constitutes the remarkable features of a state. […]

The subject of such a remarkable feature, even belonging to some other science or art, will also be a subject of statistics if it is not merely described but its influence on the essential parts of the state is shown as well. The statistician thus borrows materials from all sciences and arts. […]

Remarkable features often remain unrecognized at once as such even by those rather knowledgeable. Such recognition requires a special teaching, since these features are isolated in the circle of other sciences and arts and, in addition, great many insignificant observations only become important when considered from a single point of view. […]

The observed subjects cannot be expressed more precisely than by numbers whereas descriptions of status become the more perfect the more definite they are. It follows that most statistical information is useless if not expressed precisely by numbers. […]

Not many authors defined the science which they offered and only a part of them justified their explanations. Suffice it, however, to repeat the best of them provided by Achenwall or Schlözer[2]. Here is the former: [take] everything that concerns a state, and actually exists in the civil life of men and on the land which they inhabit. And [isolate] everything remarkable which noticeably concerns the well-being of the state. Those remarkable features constitute the subject of statistics which is thus a thorough knowledge of the really existing remarkable features of a state. […]

[4] Schlözer (1804, pp. 27 and 37) justified the definition of statistics in much more detail and much more thoroughly. The natural man, he says, is a social man. He is living in his home and in a civil society, and, finally, in the society of his state. That last mentioned society is a union of many families who granted liberty and power to one or a few individuals to attain common happiness. Only such societies can be and are worthy of being the subject of statistics, which selects from all the particular descriptions of a state [compiled by specialists of several sciences] only that which has an obvious or unobtrusive stronger or weaker influence on the well-being of the state. Only the thus chosen can be called remarkable features of a state, and statistics is the information about all of them. Elsewhere Schlözer (pp. 95 – 96) mentions an extremely special type of knowledge only pertaining to the state and its government which constitute the scope of political sciences. These consist of four main parts:

1) Politics, or the general rules of the state.

2) The theory of governing a state (Staatskunst).

3) Staatskunde [Staatswissenschaft, University Statistics], or statistics, i. e., the description of a state under its really existing conditions and at a certain time, or the history of the state at a certain moment (stillstehende Staatsgeschichte)[3].

4) History of a state, or a narration of how did the state become such as it is now. […]

It is especially necessary to note that

1) Already Achenwall had correctly shown the subject of statistics in its narrow sense whereas Schlözer had only expressed it more precisely. It seems, however, that Schlözer's expression (p. 86),

Statistics is history standing still and history is statistics flowing

is only an intricate play on words[4].

2) Schlözer (pp. 12 – 13) includes statistics in the realm of political sciences as it existed in Greek and Roman societies which Büsching[5] all but denied. […]

[5] Statistics differs from some sciences.

From geography by its subject. The subject of statistics is the state whereas that of geography is the earth. Geography describes either the location of a region or the space, which it occupies, and it is then called Mathematical Geography; or the climate and the soil and its produce which is Physical Geography; or the changes of the earth's surface created by man, and then it is Political Geography. If geographical descriptions are very detailed in every respect, they constitute Topography.

All that can also be said about regions lacking states. Their inhabitants are only living without any social connections in family or civil societies. Only geographical, physical and historical descriptions of those lands and their inhabitants are possible, but statistics is not.

It follows that statistics and geography essentially differ. However, because of indefinite notions and their mistaken applications there occurred a delusive opinion that statistics was only a new name for the science which had until now been called geography and that there exists a worldwide statistics just like there is a worldwide description of the earth. Yes, the earth is everywhere, but only its very small part is occupied by states. […]

Statistics *differs from history* both by its subject and the method of research. All the remarkable features created by man everywhere and at any time constitute the subject of history whereas the subject of statistics is restricted to the remarkable features of a state. […] It only deals with those regions which are occupied by states […] and only during constant conditions of their existence. […] Great upheavals and their causes are the most essential subjects of history whereas statistics is restricted to the remarkable features of a state. […]

Splintered or *individual* descriptions of all the parts of a state at a short period of time during which no essential changes are occurring in the structure of a state is the main subject of statistics. Eloquence is peculiar to history whereas statistics attempts to represent numerically all the described issues, and can only be glorified by properly arranged tables[6]. […]

Statistics *differs from the study of nations* in its subject and the method of research. That study describes all nations whereas statistics only deals with those which constitute a state. The former historically considers the physical and moral qualities of a nation whereas statistics only leaves remarkable features of a state and of their relations with politics. Indefinite notions led to the delusive opinion that the study of nations is a part of statistics rather than a separate science. […]

Statistics *differs from civil law*. That latter is a political science which considers the structure of a state in general with respect to its aims and essence and examines the rights and obligations of sovereigns and subjects whereas statistics describes the real status of a state at a certain [calm] time. Civil law of a certain state includes many statistical issues (courtiers, the national emblem, titles, orders etc.), but it does not follow that civil law in its entirety should be a part of statistics. The essence of civil law consists in a philosophical indication and definition of the rights and obligations of sovereigns and subjects and thus creates it as a special science not belonging to statistics. […]

Statistics *differs from political economy*. The subject of the latter is the preservation and increase of national wealth which it studies in a philosophical manner. National wealth is, however, only one issue of the statistics of a state, and statistics only describes it as it presently exists. […]

Statistics *differs from politics* as understood in its usual meaning. Politics is a science of the most beneficial structure and government of a state and in this sense it is a philosophical science. However, if politics is understood as the entire political course (*cursum politicum*), then statistics, just like history of a state, will undoubtedly belong to it. […]

But still, although all the mentioned sciences essentially differ from statistics, they partly explain it, partly include materials, and if they are presented in another manner, those sciences will constitute essential parts of statistics; or, in other words, their theories will essentially influence the method of considering statistical subjects. […]

[I omit several lines which in essence repeat those just above. O. S.]

And so, all these sciences are auxiliaries for statistics. […]

[6] Historical events constitute a very fruitful introduction to the special parts of statistics. A sketch of the previous status fosters a survey of the present situation. A political subdivision of a state, the number and quality of its towns, harbours, commercial centres, villages; seas, rivers, lakes, canals; climate, soil and its produce, − all this, if described in a way different from the geographical manner, constitutes the proper part of statistics. The same is meant about the population, its strength, morals and manners and education; about courtiers, the national emblem, titles, orders as well as about the rights and obligations of each state.

Statistical descriptions of all these subjects consist in studying their influence on the well-being of the state. For example, a geographer mentions the universities of a state since they are located there. A statistician, however, mentions them in respect of the means which the government applies for disseminating education. He describes their structure and action [on the population], shows the ratio of the student population to the population at large, to that of public establishments and educational institutions. […] [Beginning with the word *ratio*, my translation is only tentative. O. S.] All subjects are treated in a similar way. Their statistical description selects and reveals that which constitutes the remarkable features of the state.

Finally, political economy and politics powerfully influence statistics. Indeed, statistics ought to collect and arrange its materials in a manner ensuring answers to all the questions possibly formulated by these sciences. A statistician should therefore know what do they require, and each major change in the elements of the political sciences necessarily ought to bring about changes in the collection and arrangement of statistical information, and therefore in the theory of statistics. […]

The subject of statistics is the state. It is such an arrangement of a large society of men which means that one or many individuals commands/command or governs/govern, and all the others obey him/them. The government is called the supreme power and those who are ruled, the nation or citizens. Each state consists of these two classes of the inhabitants, so that statistics, being the knowledge about the situation of a state, ought to consist of two most essential parts, on the status of the nation and of the government. […]

All the inhabitants of a political society are akin in that they consist of a certain number of individuals possessing more or less of the national wealth, are more or less educated and enlightened. Even the poorest nation has some belongings which are its property. It would have been proper only to apply the term *national wealth* to prosperous nations, but it is commonly used and we follow suit although clearly meaning that the appropriate nation is prosperous.

Just the same, each nation has its proper enlightenment and education. We understand these terms not only as something ensured by a scientific or school education, but education of any kind pertaining to industry[7], morals and manners, religion and learning.

[7] The first part of statistics, on the status of the nation, therefore consists of the following subdivisions: On the population; on the national wealth which is acquired by agriculture, manufactures, and commerce; on education and enlightenment. We begin by the status of the people since without them government is meaningless. […]

The supreme power, or sovereign, can either be an usurper or a legal ruler (*aut dominus, aut imperant*). They differ in power or in the right of power. The former ensures power not by right, but by violence and oppression, and we do not dwell on such rulers. The latter ensured the right to will certain aims in the name of everyone and to carry it out. He acts to attain these aims, i. e., he reigns or governs.

The right of reigning (governing) is established by state decisions, and the actions of reigning, by state government (administration). The second most essential part of statistics, or the status of the government, should therefore consist of two sections, on state decisions and state administration. […]

The right of reigning, and all the power of the sovereign consists in the right to introduce laws, to try accordingly and to carry out these judgements. That right is therefore subdivided into the legislative, judicial and executive branches of power. The naming of the person/the people who by right possess such power curtailed by certain restrictions constitutes the items of the first section of this second part devoted to state decisions. […]

Whatever are the aims of a state, it is already known that all the enlightened governments have established institutions tending to preserve and strengthen both security and the national well-being. The statistician should therefore only bear in mind these subjects, and the section on state administration is accordingly subdivided into measures directed at the security of the people and at their well-being.

The former constitute the most important aim of a government and those measures should be stated beforehand. However, the latter measures directly influence the issues belonging to the former, and statistical descriptions will therefore be better unified if those measures are also stated beforehand. This argument is not, however, as compelling as to forbid any statistician to choose the arrangement of those two subdivisions. In addition, I will only remark that my own arrangement above provides the best unification of statistical descriptions. Besides that, the measures necessary for the security require greatest expenses and therefore depend on the condition of the state finances which those descriptions will also consider. On the contrary, measures necessary for the well-being require very small expenses or none at all[8] […]

Laws are the heart and soul of the statistical administration. Who wishes to act reasonable, ought to keep to rules which for him are laws. Therefore, just before describing the state administration, which is the business of the government, and after describing state decisions, which indicate the proper right of acting for the government, it is appropriate to discuss laws or rules following which the government intends to act. These rules differ according to the objects of their action so that statistical descriptions should lack any special sections on laws and which will thus avoid unnecessary repetition; instead, it is better to suggest statutes for each institution concerned with state administration. […]

[8] *Measures directed at the well-being of the nation* differ in the objects fostering that aim. Here they are. Those having to do with

1) Population concerning medical establishments; poorhouses and houses for foundlings; establishments dealing with the voluntary resettlement of inhabitants of overpopulated regions to a scarcely populated land or with adoption of migrants from other states.

2) The people's property concerning agriculture, manufactures and commerce.

3) Education and enlightenment concerning religion, good behaviour, erudition and people's education.

The laws will not necessarily constitute an entire code (codex) but they ought to form a main system, to show the spirit governing the mentioned measures and the appropriate establishments. […]

Measures preserving and strengthening security directed against those who either violate, or are expected to violate it. These people called enemies [or expected enemies] are living either in the state itself or beyond it. Accordingly, such measures are subdivided into those directed against the former and the latter.

The former measures are aimed at preventing all that can violate security and requires speedy help which is the duty of the *police*; or, at breaking off any quarrels caused by the violated security, which is the

duty of *justice* or of its administration. Justice has its own laws, establishments and orders of proceedings (criminal and civil laws, courts and processes).

In European countries, a special class of citizens, *the military*, is appointed for combating external enemies. They also have their own laws, courts and boards. Wars are ending by peace treatises whose main aim ought to be the security of the belligerent powers. Those, or special treatises benefit a nation, tend to increase its well-being at the expense of another one, but security invariably remains their main aim.

For this reason we additionally reckon on measures which bear on the *foreign relations* of the state. Here, we deal with boards, treatises and the governing political system. […]

All government affairs consist in the following. The sovereign (*imperant*), being a single individual (or many people), has the right and power to manage all the state work. This, however, is impossible, and he therefore chooses a certain number of subjects and distributes among them that work which they carry out in his name.

Being citizens but not managers, these subjects spend some of their time and efforts, and may expect to be secure and prosperous. By right they only ought to draw remuneration from the sovereign equal to those personal benefits which they would have ensured by their own abilities. That remuneration is usually monetary and constitutes their salary.

Although the sovereign enjoys all his rights he is unable to govern without such a division of state affairs and he should secure those helpers a decent salary commensurate with their work. The sovereign therefore has the right thus to spend some part of the national capital by transferring it to the state capital. The condition of this capital, or of the *finances*, is not a duty of the state management, but only a means conducive to setting it into motion. State finances, or the state revenue should therefore be considered in conclusion. […]

[9] After breaking up the subject of statistics as shown above, we arrive at its following subdivision.

Part 1. On the situation of the nation

Section 1. On the population

Its strength, subdivision, physical properties, relation to the size of the territory

Section 2. On the national wealth

On agriculture, manufactures, commerce

Section 3. On knowledge and education

On religion, erudition, practical technical and commercial knowledge, knowledge of morals and manners

Part 2. On the status of the government

Section 1. On state decisions

On the legislative, judicial and executive braches of power; on the rights of the various classes of citizen

Section 2. On state administration

Subdivision 1. On establishments directed at the well-being of the inhabitants

With regard to the strength of the population, to the wealth of the nation, to the education

Subdivision 2. On measures directed at preserving security

Against internal enemies. On police, judicial trials, laws, judicial institutions, administration of justice

Against external enemies. On military power, military boards, laws, strength of the army, foreign relations, boards, treatises, the political system […]

This break-up of the statistical subjects will achieve the aim of the entire statistical information. It indicates, with all possible precision, the relations between separate classes of the citizens of a state and the totality of those classes and those between each special class and all the other ones. This break-up also allows us to find out correctly the strength of the government, i. e., allows to show the revenue, military force and connections with foreign powers. The measure of security and well-being of the citizens of the state will also be felt.

Statistics of any state can be thus described. However, with respect to the second part of the break-up, and especially the second section of that part, there exist some differences between various states. In this case, should we also keep to [this] speculative (?) break-up, or describe the statistical information by practically copying the situation?

Under the latter alternative, after wrongly subdividing the material and adopting that mistaken subdivision for usual practice, we will very easily overlook some part of the description or the possible disproportion between the various measures concerning state administration will remain unclear.

Time and circumstances, power and chance, prejudices and wisdom participated in the creation of states now existing in different places. The statistician should therefore issue from a precise theoretical break-up which includes all the parts pertaining to a well organized state. Peculiar features of the structure of some state can be indicated at the end of each item. […]

Land is a remarkable feature of a state almost in the same sense as is water, air or climate. Statistics does not include a detailed description of the land, air or climate which will constitute political and physical geography. It is only necessary to describe the relation of the land etc. to, and their influence on the security and well-being of the people. All this will naturally be represented when compiling the special parts of statistics. A very good statistical description occurs if all the statistically remarkable features are borrowed from geography, united into a single general picture and represented as an *introduction*.

Land is a remarkable feature of the state with respect to industrial, administrative and military considerations. The statistical introduction ought to view the land from this point of view. […]

Statistics is a description of not many, but of a single situation. Statistics of the present therefore supposes that statistics of the previous time already exists, at least with respect to the most important political changes. However, that statement is possibly only partly true, or the reader can be ignorant about the previous events. Therefore, when compiling each part of his description, the statistician

ought to borrow the statistically remarkable from the statistics of ancient times, or, if lacking, from history. He thus ought to begin each part by *a historical introduction*.

The general geographical and statistical introduction ought to precede the statistics of a state and each of its part should also have its special historical introduction. However, to insert general history or an abridged description of the region is contrary to the essence of political science, that is, of statistics. […]

[10] The *sources* of statistics are state papers, journals and gazettes, descriptions of journeys and oral information. […]

It is seen that the worth of these sources essentially differs. The art of determining it is called statistical criticism[9]. […]

For approaching the truth as nearly as possible the state papers ought to be considered the main source but subjected to strong criticism, then compared with all the other sources, and especially with the oral information.

Truth, in the strongest sense, is the first and most sacred aim of the statistician. […]

Statistics, in the words of Sinclair[10], numerically describes for the government the measure of the well-being of the nation. Even an enlightened minister, who is unable to glean anything new from the statistician, from the man who himself ought to obtain all his materials from the minister[11], even he will be pleased to know how the learned men independently, thoroughly, free from any prejudices, estimate the situation of the state. […]

The measure of the well-being, as numerically determined by the statistician if only it consists of objects capable of enumeration, allows to see its benefits or shortcomings by means of a statistical review, of the art to represent everything as it really is and to unite scientifically various subjects. The statistician is able to show much of that which is not seen in usual information. Criticism reveals shortcomings in the formulas underlying the compilation of such information, which is so influential and on whose precision so much is depending, makes it possible to correct all of them, indicates errors which crept into the circle of lower establishments and even penetrated those higher up. […]

In states favourable for the statistician he is really a public herald of both good and bad and an inspector of the government. Indeed, by appointing statisticians the government keeps people who describe the real situation of a state rather than an exemplary state. […]

An official of the state must be in charge of some part of the government affairs, and his first and natural duty is to ensure its precise knowledge. This is why all government institutions felt the need to compile historical and statistical descriptions of the most important issues considered in their work and of the previous manner of dealing with them. Such clerkly compilations can certainly be very good if their composition was directly entrusted to knowledgeable people. However, those materials are usually fragments haphazardly compiled from many documents and are therefore no substitute for the work of a thoroughly learned scientist.

In addition, each subject of the state economic affairs, each part of the administration of the state, is connected with other related subjects, which means that especially the highest officials need at least a superficial survey of the statistics of the entire political body. Therefore, each man bearing in mind high government positions should prepare himself not only practically, but also by scientific knowledge, by studying statistics, state economic affairs and political sciences if his aim is a position connected with national economy; and statistics and jurisprudence, if thinking about honourably serving in government offices.

Such scientific training of state officials for work in establishments concerned with security or the well-being of the nation became just as necessary as military science is for an officer. In this, the 19th century, we can hardly maintain that the knowledge of the military science little influences military success [or failure], that it is only necessary for generals, whereas only obedience and courage are required from the officers. And just as hardly we may state that the knowledge of politics and jurisprudence does not influence the administration of the state, that it is only necessary for a minister whereas nothing but honesty, skill and common sense are required from the other officials.

However, if scientific training is necessary for an official of the state, as at least the Romans acknowledged during their periods of enlightenment, the knowledge of statistics is necessary for future officials even if they prepare themselves for work in establishments having to do with checking security or with increasing the well-being of the nation. In both cases those hopeful ought to know the real status of the might of the state and the method of managing it. They should understand the really existing social circumstances. […]

[11] Compilation of such tables, in which the statistically important and remarkable is seen at once, which do not contain anything excessive, but, on the contrary, contain everything required by a wise government for the well-being of the state, − such compilations belong to an important but very difficult art.

Perfect to the possible extent and very rare specimen of lists and tables of the dying and born, on the strength of the population, forests and arable land, industry, etc. are the fruit of a long-lasting experience. The completeness and precision of the most part of statistical information collected by governments depend on the perfection of such tables. The population, national wealth, measures of the people's education, the staff of government establishments, military might and finances, all that can be enumerated and therefore cannot be more precisely and clearly represented than by tables. Obviously, not the entire statistics consists of tables, but they are undoubtedly essential for more than a half of it.

Governments usually provide forms for such tables and lower officials ought to conform precisely to them. However, those forms can sometimes be partly insufficient so that the study of statistical contributions which include the possibly perfect exemplary tables should certainly be very useful. A high government official who chooses such forms will be able to compare them with those published

in the best statistical writings and decide whether something ought to be changed. […]

[12] Finally, each concerned citizen judges the administration of the state. This is his inalienable right which he always exercises underhandedly if not publicly. This can only be prevented by cruellest despotic oppression, when destroying human abilities by disseminating gloom and slavery and thus depriving the nation of the ability to think and reason[12]. It is extremely difficult to attain that goal even in case of a half-educated nation and absolutely impossible if the nation is enlightened.

It is impossible to forbid a somewhat educated citizen to judge the situation of his state, and it is therefore much better and simpler to take measures for guiding such judgements and even to determine public opinion. However, no one is able to attain better the former aim without abandoning the policy of useless secretiveness and, instead, frankly informing the public about the affairs of the state[13].

Writings describing these affairs with possible rigour and precision will necessarily really contribute to the determination [direction] of public opinion, and they, the writings, can only be statistical. Loyal citizens (who invariably constitute the majority of the people) will then learn how to answer the cunning slander of the malefactors.

On the contrary, when forbidding the loyal citizen such learning of the truth he will have to agree with the aspersions cast at the government. And, although his own inner feeling will suggest the contrary idea, some sting of the calumny will always remain in his soul. The impossibility of refuting a doubtful event begets trouble. Bans are here absolutely ineffective and only enlightenment can be successful, and the only source of enlightenment is statistics. […]

Notes

1. Translated from Druzhinin (1963). Each extract ends by dots […].

2. Achenwall was the most prominent German [no other had existed in his time – O. S.] university statistician, the founder of the Göttingen school of descriptive statistics, professor of common and international law and statistics in Marburg, then, from 1748, in Göttingen. In 1749, he described his theoretical views. Achenwall's proposition that the remarkable features of a state constitute the subject of its statistics had been firmly established in the Staatswissenschaft. Schlözer, the author of a book published in 1804, was another professor at Göttingen. In the 1760s he had been working in Russia. He was the most eminent representative of the Achenwall school. N. K. D.

3. See below.

4. Not a play of words but a mistaken statement. Even Leibniz (manuscript, 1680s) indicated that different times in the life of a state, and different states considered at the same time should be compared with each other, and Schlözer himself (1804, p. 37) stated the same about the remarkable features, that is, about statistics. It follows that statistics should not be *standing still*.

5. A. F. Büsching was a German geographer and statistician of the second half of the 18th century. For some time he had been living in Russia. As a geographer, he founded the comparative economic description of various countries. Such descriptions at the same time represented a new direction of the German [a useless specification] university statistics. N. K. D.

6. Hermann several times positively characterized the use of statistical tables. Their introduction was due to Anchersen (1741) but *tabular statisticians* had been despised (Knies 1850, p. 23). Schlözer (1804, pp. 41 and 90) somehow expressed himself both in a negative and then in a positive sense.

7. Hermann many tines mentions both manufactures and industry. What did the latter mean?

8. Below, in § 8, Hermann actually refutes this strange statement.

9. A very narrow definition.

10. John Sinclair, an English statistician of the second half of the 18th century. As compared with German university statisticians he attached more importance to economic descriptions. N. K. D.

Sinclair was the author of a statistical treatise (1791 – 1799). O. S.

11. But how about the other sources of information?

12. Here Hermann provides a philosophical reasoning which proved entirely wrong. Even in the same 19th century *enlightened* French citizens hardly criticized Napoleon who waged several wars quite unnecessarily and even contrary to the national interests of France. Tolstoy, in his *War and Society* (*War and Peace* is a misnomer), was possibly the only one who mentioned Napoleon in an utterly negative sense. And brainwashing has been quite successful even without despotism.

13. Schlözer (1804, pp. 52 – 53) was of the same opinion. According to Wargentin (Nordenmark 1929, p. 249), the lists of the dying and born in Sweden had been concealed until 1766.

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II

Prince Kozlovsky

About hope

Sovremennik, No. 3, 1836, pp. 23 – 47

Ex fumo dare lucem
[Give light from smoke]
Horace

1. Many superficial people confuse that elevated feeling which our sacred religion considers as a Christian virtue and the fuss of everyday hope. In the Christian sense, hope means hoping for God's mercy, confidence in, so to say, the unbounded love of the Creator of man. Here, nothing is faithless, nothing is conjectural and religion justly deems it as the most valuable treasure when it is kept in our heart and fortifies our power in illness, misfortune and sorrow.

On the contrary, everyday hope is that which the ancients, when vividly describing the life of a mortal, thought has been left on the bottom of the Pandora jar, regarded it as some dreamy comfort ensuing from the possibility of a groundless change of chance.

Non si male nunc/Et olim sic erit, quondam Cithera taceniem
Suscitat musam, necque semper arcum./Tendit Apollo
[Translation of a part: Apolo is not always straining the bow.]

This is Horace's poetic description of that which occurs in each hopeful heart. There is no cause for hoping for the better only since it is now bad, but the miserable gambler poorly acquainted with mathematics and ignorant in that at every deal of the cards the chances against him are the same as they were in the beginning of the game, passionately yearns for losing once more after having lost ten times in succession. He thinks that the eleventh time will certainly be luckier only because of the past misfortune.

Almost all our everyday hopes liken us to that gambler and sometimes lead us to consider a most obvious impossibility or at least ignorance as a consoling hope. How many young men enter the real world with golden hopes and return back spiritually bankrupt, hating mankind and becoming absolutely unable to do anything useful either for themselves or others.

I happened to know a very gifted young man so utterly blind that he still hoped when his love agreed to marry his richer rival. He was forbidden her house but did not abandon hope. That house became candlelit and preparations for the wedding began there. The miserable man saw all that but thought that a *possibility*, on which his hope was based, had still existed that the bride, while standing at the altar, will answer *no* to the priest's question.

As a consequence of such a horrible hope his most elevated mind and most brilliant talent went blank. More than twenty years have passed, but he still complains to each visitor, who comes to him in the mental hospital, about the treachery of the priest who had performed the marriage of his lover and whom he kills daily in frenzy.

Who of those who visited Vienna did not know the rich banker Parish, and the beautiful woman and generally beloved actress ruined by his love? Parish was a shiftiest American speculator, and his business was, and even now the businesses of his brothers in Hamburg and London are quite excellent. However, false hopes in the firmness of the Spanish Cortes involved him in deceptive speculations.

Until the very last week of his misfortune he was still able, as all of his correspondents thought, to put his business right at the expense of some losses, but he never lost hope that everything will change. And, when finally he decided to kill himself, simplest prudence and honesty should have compelled him to set something aside for the unlucky woman, whom he was leaving without subsistence. Indeed, he had deprived her of her advantageous position in the Vienna theatre. He hoped that it was sufficient to give her 50 thousand gulden in cash with a strange understanding that she ought to keep the money until he demands it back as though his creditors will not think about questioning her after his death. The wretched woman told the truth and was required to give back the entire sum to the bankruptcy fund after Parish had drowned himself in the Danube.

How many fathers leave the beloved of their heart in poverty, sometimes even in slavery, recklessly hoping for an entirely unjustified continuation of life! How many people, being in sound mind and physically fit, remain idle in the trial hour of happiness and hope for a certainly possible but unlikely course of events!

Thus, Napoleon in Dresden and Frankfurt, even in Chatillon itself, could have still remained the sovereign of France, and, while in the first two cities, took a fancy to imprisonment and premature death.

Thus the miserable Stuart family for 60 years had been multiplying the number of the misfortunate victims of their devotion who accompanied their own downfall. The Stuarts hoped for some favour from fortune without any reasoning about the facility of the occurrence of each resumed undertaking.

Thus Spain, depressed by misfortunes of all kinds, proudly turned down the millions offered by Southern America in return for the already gained freedom. Spain preferred bankruptcy, poverty and weakness to a complete payment for its debts: it hoped to return the irretrievable.

Thus, finally, the entire romantic Portugal had for a few centuries been expecting the return of its beloved Sebastiao I [1554 – 1574] who had perished in the African deserts!

Everywhere and almost always hope is being connected with something similar to madness or at least to such impudence which prevents us to see coolly our position and look for convenient means to amend it. Many will object: a man never loses hope, and one comforting idea follows another one for all his life and thus are the real hardships lessened. I answer:

1) It is absolutely contrary for the mind to praise such a state of the soul which is obviously similar to drunkenness.

2) Such dreams, had they not deprived us from acting in the proper direction, would have still been somewhat justified. However, when relying on the possible, we barely choose the path for achieving the easily occurring. And, finally,

3) Most important for the adherents of deceptive hopes is that after any obvious loss of some hope the blow to the heart will be accompanied by a suffering which exceeds hundredfold the delight of the former deception.

Suicide, hate of mankind, despair and criminal activity almost always inevitably follow seductive hopes. A gambler becomes a thief; the thief hopes only to steal but finally kills; a hero sacrifices his life's glory to the falsity of one single event. Finally, the very remorse and the last reconciliation with heaven, to which we are already tending, dampen after a cruel and deceptive charmer whispers: *there is yet time*!

Walter Scott, when depicting Napoleon's negotiations of 1814 with the allies, properly adduces the words of a poet who described the appearance of a shadow to the recreant Alp who had besieged Corinth [in Greece]:

There is a light cloud by the moon –
'Tis passing and will pass full soon.
If, by the time its vapoury sail
Hath ceased her shaded orb to veil,
Thy heart within thee is not changed,
Then God and man are both avenged!

Alp and Napoleon hoped, but the cloud passed by and the time provided for repenting as well.

2. I dare to ask each reader to add the fruit of his own experience and reminiscences to the above for becoming convinced in the great danger and bitterness of deceptive hopes. But how to protect ourselves from them? How to generalize a means for such protection? I know only one way, the approach of that philosophical mathematics which is called calculus of probabilities, or, better, the science of calculating the facility of the occurrences[1].

Together with the first algebraic notions it becomes clearly and deeply impressed in the most middling minds. This does not seem to me as difficult as many imagine out of fright by algebraic formulas. I will attempt to describe the elements of that science without supposing that my readers have any knowledge of higher mathematics. My reward is that, after reading this paper, the reader will say: each child will understand it.

3. We will begin by a few words about fractions. […] I see no great difficulty in understanding that if some three roads out of four lead to Moscow, I have 3/4 for the facility of coming to Moscow and 1/4 for being mistaken, or that the probability of my mistake is 1/4. This simple and obvious example allows me to conclude that the *facility of the occurrence of events is expressed by a fraction* […]. If my

considerations are correct, nothing will change when only 2 rather than 3 roads, or all 4 lead to Moscow. In the second case the fraction will be 4/4 = 1.

In the science of the calculus of the facility of the occurrences certainty is expressed by unity. And the nearer such a fraction approaches unity, the more reasonable will be my hope. On the contrary, the more that fraction decreases, the less reasonable will be the expectation of a favourable outcome. […] In case of moral events, it is certainly difficult to enumerate all the possible and facile chances, but the importance of science[2] consists in that, when knowing the rule, the mind gets gradually used to determine those chances and the relations between them. Most important, however, is that at the same time we break with the habit of invariably confusing the possible and the facile occurrences.

4. I have described the mathematical expression of a hope depending on *one* event but who will not go farther, who restricts his reckless passion for hopes and wishes?

Magnaque numinibus vota exaudita malignis!
Our wishes are fulfilled by embittered gods. Juvenal

Human dreams are boundless since there is nothing except eternity which can satisfy our souls.

5. We will try to determine quantitatively the measure of our hopes for such cases in which we expect that our wishes come true when they depend on two, three or more favourable events. I am afraid that, had I dared to show here even the simplest development of the Newton binomial which is here necessary, the reader, after he recalls that long since forgotten friend of his youth, will not glance at these pages anymore.

So let us leave that binomial aside and take instead two counters, one of them black, the other one, white, with letters *a* and *b* written on the opposite faces of each. We will see how many different chances can be shown when both counters are tossed on a table. There will obviously be one case for *aa* and *bb* each and two cases for *ab*. Write this down as 1*aa*, 2*ab*, 1*bb*, and let us see how we can adapt this to our subject.

6. Suppose that I bet on getting heads when tossing a coin. Since the facility of each outcome is 1/2, my own stake and the stake of my rival should be the same. However, if I bet on getting heads twice in succession, my just stake should be balanced with the facility of my gain. Let us list all the possible cases which will be represented by our counters. The outcomes of the tosses can be two heads (*aa*) which is my only favourable case; two tails (*bb*), heads and tails (*ab*) and tails and heads (*ba*). I will lose in each of these three cases, so I have the facility 1/4 for gaining and 3/4 for losing, and the just stake of my rival ought to be thrice larger than mine.

Who will believe that this science is so delicate, that it requires the mind to penetrate it so deeply that the glorious D'Alembert for a long time resisted the certainty of the consideration above. He thought that the denominator [of the first fraction] should be 3 rather than 4. He

reasoned thus: If the outcome of the first toss was tails, the game is over, and the case of two tails should not be considered at all. He was mistaken since he did not take into account that the bet had stipulated *two* tosses rather than one toss and that, consequently, the calculation of the facility of the occurrences should have allowed for all the cases which occur after two tosses. Such mistakes happen when we, expecting something advantageous which depends on two joint events, do not consider the case in which they both can be unfavourable for us.

7. Let us now find out the facility of three heads occurring in succession. We will therefore list all the possible outcomes represented by a black, a white and a red counter. There will be one case for *aaa* and one for *bbb*, three different cases for *aab* since *b* can be white, black or red, and another three for *bba*. And so, we have 1*aaa*, 3*aab*, 3*bba* and 1*bbb* […]. In all, there will be 8 cases with only one of them (*aaa*) favourable for us. According to the explained rule, we have the facility 1/8 in our favour and 7/8 in favour of our rival, and his stake should be 7 times larger than mine.

8. And now add a yellow counter. We already know that there will only be one possibility for the occurrence of *aaaa* or *bbbb*. However, *aaab* and *bbba* will occur four times each since both *a* and *b* can occur with counters of the four different colours. There only remains the case of *aabb*. Both *aab* and *bba* can occur thrice; now add the yellow counter to each of those cases. It will repeat *b* in the case *aab* and repeat *a* in the case *bba*. We will thus have six cases

aabb, abab, abba, baba, baab, bbaa, or, finally,

1*aaaa,* 4*aaab,* 6*aabb,* 4*bbba,* 1*bbbb*.

We only have 1/16 as the facility in our favour and 15/16 in favour of our rival.

And now, after we have made so important discoveries, we can reasonably rather than by groping along study cases in which there are more joint chances. So let us write down the numbers found above and look for any common law of their origin. We know already that each line begins and ends by unity. When considering those lines more attentively, we will see that each intermediate term consists of the sum of two numbers in the previous line, one of them directly above, and the other one, to the left of the first one:

1.1
1.2.1
1.1 + 2.2 + 1.1
1.1 + 3.3 + 3.3 + 1.1, or

1.1
1.2.1
1.3.3.1
1.4.6.4.1 and therefore
1.5.10.10.5.1
1.6.15.20.15.6.1[3]

This is the so-called glorious Pascal triangle which consists of the coefficients of the Newton binomial. We have been acquainted with their philosophical aspect: they were the chances representing the measure of our hope when we expect to attain our goal which depends on many joint events rather than on one event.

Let us have one more look at our approach and see whether we will be able to derive some general rule from our gradual conclusions. When I bet on getting heads once, I and my rival had the same measure (1/2) of facility. When my bet extended to two events each of them having facility 1/2, the measure of my hope became $1/2 \cdot 1/2 = 1/4$; when I consider three events, my hope of success becomes $1/2 \cdot 1/2 \cdot 1/2 = 1/8$. An important rule can be formulated: For arriving at the measure of our mathematical hope *the fractions representing the facility of each event separately ought to be multiplied.*

One more example. I wish to go to Moscow, therefore try to become a shop-assistant to a merchant who travels yearly either to Moscow, or Berlin, or London or Paris or Italy. What is the measure of my hope, if I offer him my service? He will either take me on or not $(1/2)^4$; the facility of his travel to Moscow is 1/5 and my hope of coming this year to Moscow is $1/2 \cdot 1/5 = 1/10$.

Those readers who have not yet reasoned out carefully the delicacy of these considerations can conclude that the fraction should be 1/7 when considering those 7 cases *one after the other*. This conclusion is wrong since, on the one hand, it does not take into account that my wish depends on two events rather than on one event; on the other hand, it presumes that the merchant's travel to Moscow depends on his taking me on which is not true. The correct pattern is this.

The merchant takes me on and travels to one of those five places
The merchant does not take me on but still travels as above

For me, the business is not only to be taken on, but to travel to Moscow whereas his travels do not depend on whether he takes me on or not. And so, there are two events and 10 possibilities out of which only *one* is favourable for me. We can imagine therefore that one of our counters is supplemented by a pentagonal pyramid whose lateral faces are designated by the letters *a, b, c, d, e*. Many chances occur when the pyramid is turned gradually. For the letter *a* on the counter there will be 5 events and the same for the letter *b*; in all, 10 chances only one of which is favourable for me.

9. This wonderful rule means that, when our wish depends on the combination of several circumstances, the fractions, representing the possibilities of the occurrence of each case separately, should be multiplied for obtaining the facility of the occurrence sought. This rule becomes even more convincing when we recall that the product of proper fractions is smaller than any of the factors; and common sense

tells us that, when the achievement of our goal depends on many joint circumstances, we ought to have a lower hope of success.

10. Let us count the chances of each line. Their sums are equal to 2 multiplied by itself as many times as is shown by the number of the line:

1.1, sum = 2
1.2.1, sum = 2·2 = 4
1.3.3.1, sum = 2·2·2 = 8
1.4.6.1, sum = 2·2·2·2 = 16

Suppose that instead of the counters we have a hexagonal solid with a letter on each of the six of its lateral faces. Two such solids will represent 6·6 = 36 chances, three solids, 6·6·6 = 216 chances.

11. This is the basis for the games of dice but I leave aside games [of chance] and lotteries. Let curious readers apply there the rules described above which will be very easy for them if only they consider carefully the elementary conclusions already made[5].

12. Nevertheless, we will show, as our last example, how to apply the Pascal triangle although we will only base ourselves on common sense. Let us bet that, after tossing a rouble 5 times, heads appears *at least* 3 times. We will select line 5 and count all the chances in which *a* occurs *not less* than 3 times. We will find one chance for *aaaaa*, i. e., for the only case in which *a* appears each time, five chances for *aaaab* to appear and 10 for *aaabb*, or 16 chances in all with 16 chances against us. Therefore, the hopes for obtaining or not of at least 3 heads are the same. The stakes for such a bet ought to be equal to one another. But we had obtained 1/8 for getting 3 heads in succession, and will not common sense tell us that indeed we ought to hope more for 3 heads in 5 tosses than in all three tosses.

13. Readers will certainly forgive me if I congratulate them with their acquired knowledge of compiling the Newton binomial, i. e., the product of $(a + b)$ multiplied by itself any number of times which mathematicians call raising to an arbitrary power n. […] The binomial multiplied by itself six times will be

$$(a + b)^6 = {}^1aaaaaa\ {}^6aaaaab\ {}^{15}aaaabb \\ {}^{20}aaabbb\ {}^{15}aabbbb\ {}^6abbbbb\ {}^1bbbbbb$$

or, according to Descartes,

$$(a + b)^6 = a^6 + 6a^5b + 15a^4b^2 + 20a^3b^3 + 15a^2b^4 + 6ab^5 + b^6.$$

Those readers whom we will be happy to give a taste of philosophical mathematics, i. e., of the calculus of probabilities, can

read Lacroix (first edition, 1816), or, perhaps better, De Moivre (first edition, 1718).

14. Our only reward is that this superficial essay will already prepare those readers for an easiest notion of science, which is represented in the mentioned books by mathematical means and not described as ordinarily as I attempted to do for better understanding.

However, if I do not prompt even one single young reader to penetrate deeper into science, what then will the use of my work be, of my diligent attempts at clarity, of my selflessness with which I disregarded the danger of appearing unbearably dull? I answer myself: when gleaning over these pages, the reader will be unwittingly compelled to reason which does not occur always. And a gradual transition from one conclusion to another is by itself a movement of the mind not useless for it to remain sound.

15. The science of facilities is not more than two hundred years old[6], but it has been treated by the greatest minds of our time, Pascal, [Daniel] Bernoulli, De Moivre, Condorcet, and, finally, Laplace[7] applied it not only to astronomy, but to all everyday relations in which chance whose causes remain unknown or much experience can enter.

Bernoulli proved that numbers can always check moral cases as well. Thus, he arithmetically described the advice against games [of chance] inspired by prudence: the joy of a gain never equals the sorrow of a loss. He supposed that the *ratios of magnitudes* are the foundation of the entire higher mathematics and that in our life and in all our notions about the price of things we find the same relative value since richness and poverty are only relative and depend on circumstances[8]. He reasoned thus.

Suppose that two gamblers have, for example, 200 thousand roubles each. One of them wins 100 thousand and therefore only enjoys 1/3 of his new capital, but if he loses as much, he is deprived of 1/2 of his capital. I only mention this witty argument as a historical remembrance since I know myself that it will not convince anyone to abstain from gambling. I think that a philosophical course of mathematics can better direct minds, but where is the medicine, the protection of men from their passions?..

16. I will not discuss all the applications of science, and, especially, will not mention the probable continuation of life. A sorrowful remembrance burdens my heart; describing it here, I will at the same time provide a superficial notion about the so-called *curve* of life.

17. Among friends who do not exist on our planet anymore, Duke Dalberg was one of those whose death has been most painful. If ever after my death my notes will appear in print for augmenting the understanding of modern history, my readers will befriend that excellent man whose kindness exceeded the scope of his mind and the rapid penetration into state business.

The reckless Teutonism invented in 1810 by graduates of German universities had not ceased to victimize him since, being a most ancient German baron, he became a French duke and peer. [Kozlovsky explains the reasons.]

Dalberg respected mathematics as an abstract science and the realm of highest minds. He often argued with me about its philosophical significance. Most of all he based his arguments on the chance mechanical and physical discoveries and on that almost all discoveries were made by observing rather than abstract minds[9].

In 1824, Vilelle suggested a plan for decreasing the percentage of the state debt [decreasing the interest on the debt] and based his proposal on a loan which would have added 1/4 to the state debt. Laplace, who had always supported the ministry, intended to prove mathematically to the Chamber of Peers that the profit caused by the decrease of the percentage will twice exceed the arithmetical rather than the real burdening of the state.

Dalberg heard out his comrade very attentively, as he told me the same day, but the weak voice of the great mathematician, unaccustomed to speak in the Chamber (?), and perhaps his vague explanation contributed to the failure of Laplace's speech to convince even a single peer[10].

That same evening, during our walk, Dalberg stronger than ever before argued against the usefulness of mathematics for state or private life: *How can anyone acquire a liking for a curve which tells nothing either to the mind or heart*?

Let us see, I answered him. Is it always so? Draw a horizontal line and [Kozlovsky explains, not very successfully, how to construct a density curve of mortality, although only when issuing from data on friends who had died during a number of years].

Notes

1. The term *probability* first entered Russian scientific literature not later than in 1789, in the translation of part 1 of Buffon's *Histoire naturelle général et particulière* (and, likely, in other parts of that translation in 1801 – 1812), then in Pavlovsky (1821). Kozlovsky's *facility of occurrence* was not needed at all. Note, however, the expression *law of facility of errors* (Glaisher 1872).

2. Kozlovsky sometimes wrote *science* instead of calculus of probabilities.

3. Explanation is insufficient.

4. Kozlovsky assumed that an unknown probability is equal to 1/2. According to the theory of information, that value of probability corresponds to complete ignorance.

5. Some problems about games of dice and lotteries are extremely difficult.

6. The official history of probability (the main correspondence of Pascal and Fermat) occurred in 1654. Then, that same year Pascal informed the Académie Parisienne (the forerunner of the Paris Academy of Sciences) of his intention to write about *geometry of chance*, see Pascal (1998 – 2000, 1998, pp. 169 – 173).

7. Kozlovsky had not mentioned Jakob Bernoulli. He should have at least formulated the main goal of the *Ars Conjectandi*: to compare theoretical and statistical probabilities. His readers did not need De Moivre's book.

8. The last lines are difficult to understand.

9. Dalberg remained alien to mathematics.

10. Laplace's speech is hardly known. Volume 12 of his *Oeuvres Complètes* (Paris, 1912) includes reprints of three other speeches of Laplace in the Chamber of Peers, and a fourth is in the *Archives Parlamentaires*, 1787 – 1818 (Paris, 1868).

Brief Information about Those Mentioned

Dalberg Emmerich Joseph, Duke, 1773 – 1833, diplomatist

Vilelle Joseph, French minister of finance in 1822 – 1827

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III

V. E. Prudnikov

P. L. Chebyshev and Moscow University in the 1840s, an extract

Istoriko-Matematicheskie Issledovania, vol. 1, 1948, pp. 141 – 183 (pp. 210 – 213)

Chebyshev successfully held the written examination, then submitted his *Essay* (1845) which he had compiled on a commission from Count S. G. Stroganov, the administrator of the Moscow educational region, as a manual for teaching probability at the Demidov Jaroslavl lyceum. On 30 May 1845 the *Essay* was approved by the second [mathematical] section [of the philosophical faculty, see below]. It was decided that Chebyshev attentively studied Laplace and Poisson and

Displayed a very commendable and successful effort in providing his own proof of extremely important theorems.

The *Essay* was accepted instead of a thesis written on a proposed subject and

Approved for publication, although obliged Chebyshev to change the description of some of its parts in accordance with the detailed remarks made by Ordinary Professor Zernov.

This decision was sent to the Council of the University with a request to *allow Chebyshev to publish his work and defend it publicly*. On 6 June 1845 the Council resolved to publish the *Essay* […], resolved

To ask the rector to fix the date for its public defence and to publish that decision in the newspaper Moskovskie Vedomosti.

Professors Zernov and Brashman were appointed as opponents, and a public sitting of the second section of the philosophical faculty took place on 8 June 1846. That same day *Moskovskie Vedomosti* published the following announcement:

Today […] *candidate Chebyshev, competitor of the degree of Master of mathematical sciences, will publicly defend his thesis, Essay* […].

He had to defend the following propositions:

1. The theory of probability is only properly useful by means of analysis.

2. The elements of algebra allows to derive the probability that several events will be repeated a given number of times.

3. The determination of the probability that the number of the occurrences of the events will be contained within given boundaries requires a compilation of a special table.

4. A notion about that table and the series required for its calculation can be provided without applying integrals.

5. By means of that table we can calculate the probability of an event given the number of its occurrences.

6. By means of an auxiliary theorem about summation it is also possible to derive the probability of mean results.

7. Only by means of the elements of algebra it is possible to show that the value of the product $1\cdot 2\cdot 3\cdot \ldots \cdot (x-1)x$ *is always contained between the magnitudes*

$$Cx^{x+1/2}\exp[-x+x/12],\ Cx^{x+1/2}\exp[-x+x/12-x^3/56]$$

where C does not depend on x.

These propositions indicate the cardinal aim of Chebyshev's thesis:

To show the main theorems of the calculus of probabilities and their principal applications which support all the knowledge based on observations without the transcendental analysis.

The achievement of that aim was essential in many respects, and first of all for the benefit of many people who restricted their study of mathematics to algebra. The administrator, Count Stroganov, had in mind exactly that benefit when he asked Chebyshev to compile such a contribution.

Chebyshev fulfilled that proposal with talent characteristic of him […]. Before that the elementary courses in the theory of probability had only been describing in more or less detail the results obtained by higher analysis. Chebyshev, however, showed that it was possible to

Check all those conclusions by a rigorous and simple analysis understandable for most students.

This was an essential advance in the method of elementarily describing the theory of probability for which his official opponents undoubtedly gave him credit. According to the records, they had offered objections to the *Essay* but Chebyshev *answered them very satisfactorily*. The mathematical section decided that he was worthy of the master degree sought.

Chebyshev is justly considered a most brilliant mathematician who conscientiously formulated and solved purely mathematical problems by issuing from practice. He repeatedly stressed this point in his speeches and manifested it sufficiently clearly even in that thesis.

At that time, insurance had only just begun to appear in Russia, and the population little trusted it and did not understand its usefulness. Buniakovsky was the first and very energetic propagandist of insurance and probability theory in Russia. He (1840) wrote:

Theory proves that it is possible to balance the premium so that the insurance office will receive a sure profit and that the moral gain of the insured will increase. *This truth reveals the usefulness of the insurance business*.

This paper essentially determined the direction and course of Zernov's speech (1843) which, in turn, much influenced Brun (1845). It is doubtless that the birth of insurance in Russia and the mentioned contributions determined Chebyshev's taste for the theory of probability.

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IV

O. Sheynin

Chebyshev's note of 1870. General information

Unpublished

Mayevski (1870) compiled a memoir accompanied by Chebyshev's note (1870). It was published together with two reviews of the manuscripts by the Belgian academy of sciences among pieces either honoured by its prizes or simply submitted by foreign scientists. Only one contribution, not belonging to the class of sciences, was however, thus honoured (*Mémoires* 1870).

Mayevski's memoir, certainly with its supplement, became widely known, at least in Belgium itself (mostly to artillerymen who undoubtedly held a high opinion about both authors). Russian artillerymen had also acquainted themselves with both publications (see below), but the Chebyshev note had not been included either in his *Oeuvres* (1899 – 1907) or, in 1944 – 1951, in the *Polnoe Sobranie Sochineniy* (Complete Works, PSS). True, Chebyshev (1859) had earlier published a much longer memoir under practically the same title, but it did not contain the formulas provided in 1870, see below.

For his time, Mayevski was a most eminent Russian specialist in ballistics (Youshkevich 1968, p. 335, see also below) and *the founder of modern external ballistics* (Mandryka 1954, p. 162), but he certainly compiled his contributions under Chebyshev's influence (Ibidem, p. 163). Indeed, in 1856 – 1869 Chebyshev had been collaborating with the Artillery Department of the Military Educational Committee and was elected honorary member of the Artillery Academy (Youshkevich 1968, pp. 335 and 336). The Archive of the Russian Academy of Sciences is keeping three letters of 1878 from Mayevski to Chebyshev about his memoir (1875). One of these letters is published in vol. 5 of Chebyshev's PSS. Prudnikov (1950) more extensively described Chebyshev's activity in artillery.

The main source of information about Mayevski is Mandryka (1954), but Mayevski's memoir of 1870 is not mentioned there. True, Mandryka appended a list of Mayevski's contributions which included a Russian memoir (1869). I have not seen it, and it possibly little differs from its later French version (1870), but, anyway, Mandryka did not mention Chebyshev (1870).

Mayevski later published a book (1872) which had indeed become the foundation of external ballistics of his time (Mandryka, see above), but Mayevski (1870) ought to be mentioned here as well.

In 1872, on p. XII, Mayevski indicated:

Chebyshev provides formulas for interpolating by the method of least squares. We have applied them for determining the projection of the path [of the shell] *on the vertical plane of the firing by issuing from the results of firing*.

On pp. 267 – 278 Mayevski almost reprinted Chebyshev's note of 1870 and added on p. 268:

Chebyshev published formulas for interpolation which successively provided the polynomials with most probable coefficients of all the powers, and allowed to see the number of terms that can be taken account of … for observations of unequal precision as well.

In 1823, Gauss abandoned most probable estimators in favour of the most reliable (abandoned the maximal value of the likelihood function in favour of minimal variance), but, as it seems, artillerymen had not followed him. For that matter, Chebyshev's lectures (1879 – 1880/1936) reveal that he had hardly read Gauss (Sheynin 1994). Moreover, that memoir made extremely difficult reading, and mathematicians, astronomers and geodesists shunned it. My note (Sheynin 2012) drastically improved that situation.

Mayevski's memoirs had to do with the determination of the pressure of the gunpowder gases on the gun barrel, i. e., with a subject previously studied, as he himself mentioned, by Euler and Lagrange. Euler's book (1745) was translated into several languages and studied in French naval schools (Fuss 1786, pp. 44/47 and 57/60), see also Mandryka (1958), and Poisson published the manuscript of Lagrange (1832). Mandryka also mentioned a few earlier artillerymen, including Neumann with whom Mayevski had been in correspondence (Mandryka 1958, pp. 32 – 33), but he also indicated that the previous studies had only been theoretical, whereas experiments were necessary.

The reviews (Liagre 1868; Brialmont 1868) were quite positive, and the third reviewer, as stated by the editors at the end of the second review, had agreed with those reviewers. Liagre had published a book (1852), and, until 1864, six papers on the theory of probability and method of least squares. And he indicated:

The method of interpolation described in the supplement [to Mayevski], *if it is entirely new, as I think it is, is worthy of being mentioned since it is a simple and advantageous application. We may regret that the author had not substantiated it*.

Brialmont said nothing about Chebyshev but remarked that

Knowledge and clarity distinguish all the works of that officer [of Mayevski].

In a few years there appeared the contributions of Jouffret (1874) and Schols (1875) who investigated Chebyshev's formulas. The former was not only an artilleryman, but (mostly?) a mathematician and actuary. In 1873 he published a book which I did not see, but at least the 1874 paper actually copied its title, and a mathematical contribution (1903).

Schols (p. 63) remarked that Chebyshev (1859) had proved his formulas for a particular case, but he had not justified this statement, and at the very least it is not seen at once. Schols also remarked that Chebyshev considered the general case in 1855, and that in 1873 – 1874 Jouffret provided a simple, but still a somewhat diffused proof in the *Revue d'Artillerie*, see Jouffret (1874).

And now, somewhat more about the contributions of those two authors.

Schols. If $F(x)$ is known, let

$$U = F\varphi_m = F(A_0^m + A_1^m x + ... + A_{m-1}^m x^m).$$

Otherwise, $F = 1$. It is impossible to assign beforehand the value of m, and after determining the coefficients A the series is continued until it agrees with the observations. Chebyshev took successively $m = 0, 1, 2, \ldots$ Schols had not, however, sufficiently explained that check.

Then he offers another polynomial for the φ_m:

$$U = F\varphi_m = F(K_0\psi_0 + K_1\psi_1 + \ldots + K_m\psi_m),$$

$$\varphi_p = x^p + C_{p-1}^p x^{p-1} + ... + C_1^p x + C_0^p$$

with arbitrary coefficients C and values of K determined by the method of least squares.

Jouffret. On p. 60 he introduces *l'erreur moyenne quadratique de l'unité de poids*. In geodesy, this term appeared later (when?), but both Mayevski and Chebyshev had used it in 1870. That term had apparently been in use among artillerymen. On the same page Jouffret issued from the series

$$Y = f(X)[L_0 + L_1\lambda_1(X) + L_2\lambda_2(X) + \ldots]$$

which is continued until the $(n-1)^{\text{th}}$ term and which *evidently* coincided with the Lagrange series [with the formula of finite increments] up to an *écart moyen* more precisely than any other series of the same order. Now, I do not approve of the use of two different estimators of error.

That series, as Jouffret (p. 61) remarks, had been introduced by Chebyshev in quite another form after a very delicate analysis. I corrected Jouffret's references: he should have mentioned Chebyshev (1855; 1859). In a note on the same page he stated:

The series described by Chebyshev was almost at once connected with the Lagrange series by Hermite (1859), see also Chebyshev (1857).

Then Jouffret derived (1) and, in a simpler way, the Chebyshev formula and proved their identity.

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P. Tchebychef

Formules d'interpolation par la méthode des moindres carrés

Mémoires couronnés et mémoires des savants étrangers.
Acad. Roy. des Sciences, des Lettres et des Beau-Arts de Belg.,
t. 21, 1870, pp. 25 – 33

S'il s'agit de trouver les coefficients *a, b, c, …* dans l'expression de *u* représentée par la formule

$$u = F(x)[a + bx + cx^2 + \ldots],$$

où $F(x)$ est une certaine function de la variable indépendante x et u_1, u_2, u_3, …, u_n, désignent les valeurs données de u qui correspondent aux différentes valuers de $x = x_1, x_2, x_3, \ldots, x_n$, − l'on peut calculer les termes de l'expression u successivement, l'un après l'autre, d'après la série

$$u = F(x)[\mathrm{K}_0\Psi_0(x) + \mathrm{K}_1\Psi_1(x) + \mathrm{K}_2\Psi_2(x)\ldots]$$

et trouver, en même temps, la somme des carrés des erreurs commises dans la représentation des valeurs données de u, en s'arrêtant aux termes 1, 2, 3, …, λ.

Nous donnons les formules définitives pour calculer les membres de la série mentionnée.

Dans ces formules les sommations s'étendent à toutes les valeurs de l'indice i, depuis $i = 1$, jusqu'à $i = n$, et $\sum d_\lambda^2$ désigne la somme des carrés des erreurs dans la représentation des valeurs données de u par la série arrêtée au terme $F(x)\cdot\mathrm{K}_\lambda\Psi_\lambda(x)$, somme d'après laquelle on trouvera l'erreur quadratique moyenne par la formule

$$E = \sqrt{\frac{1}{n}\sum d_\lambda^2}.$$

Formules relatives à la détermination du terme $F(x)\cdot\mathrm{K}_0\Psi_0(x)$

$$(0, 0) = \sum[F(x_i)]^2,\ \mathrm{K}_0 = \frac{\sum F(x_i)u_i}{(0,0)},$$
$$\Psi_o(x) = 1,\ \sum d_0^2 = \sum u_i^2 - (0,0)\mathrm{K}_0^2.$$

Formules relatives à la détermination du terme $F(x)\cdot\mathrm{K}_1\Psi_1(x)$

$$(0, 1) = \sum[F(x_i)]^2 x_i,\ (0, 2) = \sum[F(x_i)]^2 x_i^2$$

$$a_1 = (0, 0),\ b_1 = \frac{(0,1)}{(0,0)},\ (1, 1) = (0, 2) - b_1(0, 1),$$

$$\sum d_1^2 = \sum d_0^2 - (1,1)\mathrm{K}_1^2.$$

$$K_1 = \frac{\sum F(x_i)x_i u_i - (0,1)K_0}{(1,1)},\ \Psi_1(x) = x - b_1.$$

Formules relatives à la détermination du terme $F(x)\cdot K_2\Psi_2(x)$

$$(0, 3) = \sum[F(x_i)]^2 x_i^3,\ (0, 4) = \sum[F(x_i)]^2 x_i^4,$$

$$(1, 2) = (0, 3) - b_1(0, 2),\ (1, 3) = (0, 4) - b_1(0, 3),$$

$$a_2 = \frac{(1,1)}{(0,0)},\ b_2 = \frac{(1,2)}{(1,1)} - \frac{(0,1)}{(0,0)},\ (2, 2) = (1, 3) - b_2(1, 2) - a_2(0, 2),$$

$$K_2 = \frac{\sum F(x_i)x_i^2 u_i - (0,2)K_0 - (1,2)K_1}{(2,2)},$$

$$\Psi_2(x) = (x - b_2)\Psi_1(x) - a_2\Psi_0(x),\ \sum d_2^2 = \sum d_1^2 - (2,2)K_2^2.$$

Formules relatives à la détermination du terme $F(x)\cdot K_3\Psi_3(x)$

$$(0, 5) = \sum[F(x_i)]^2 x_i^5,\ (0, 6) = \sum[F(x_i)]^2 x_i^6,$$

$$(1, 4) = (0, 5) - b_1(0, 4),\ (1, 5) = (0, 6) - b_1(0, 5),$$

$$(2, 3) = (1, 4) - b_2(0, 3) - a_2(1, 3),$$

$$(2, 4) = (1, 5) - b_2(1, 4) - a_2(0, 4),$$

$$a_3 = \frac{(2,2)}{(1,1)},\ b_3 = \frac{(2,3)}{(2,2)} - \frac{(1,2)}{(1,1)},\ (3, 3) = (2, 4) - b_3(2, 5) - a_3(1, 3),$$

$$K_3 = \frac{\sum F(x_i)x_i^3 u_i - (0,3)K_0 - (1,3)K_1 - (2,3)K_2}{(3,3)},$$

$$\Psi_3(x) = (x - b_3)\Psi_2(x) - a_3\Psi_1(x),\ \sum d_3^2 = \sum d_2^2 - (3,3)K_3^2.$$

Formules relatives à la détermination du terme $F(x)\cdot K_\lambda\Psi_\lambda(x)$

$$(0, 2\lambda - 1) = \sum[F(x_i)]^2 x_i^{2\lambda-1},\ (0, 2\lambda) = \sum[F(x_i)]^2 x_i^{2\lambda},$$
$$(0, 2\lambda - 2) = (0, 2\lambda - 1) - b_1(0, 2\lambda - 2),$$
$$(1, 2\lambda - 1) = (0, 2\lambda) - b_1(0, 2\lambda - 1),$$
$$(2, 2\lambda - 3) = (1, 2\lambda - 2) - b_2(1, 2\lambda - 3) - a_2(0, 2\lambda - 3),$$
$$(2, 2\lambda - 2) = (1, 2\lambda - 1) - b_2(1, 2\lambda - 2) - a_2(0, 2\lambda - 2),$$
$$(3, 2\lambda - 4) = (2, 2\lambda - 3) - b_2(2, 2\lambda - 4) - a_3(1, 2\lambda - 4),$$
$$(3, 2) = (2, 2\lambda - 2) - b_3(2, 2\lambda - 3) - a_3(1, 2\lambda - 3),$$

……

$$(\lambda - 1, \lambda) = (\lambda - 2, \lambda + 1) - b_{\lambda-1}((\lambda - 2, \lambda) - a_{\lambda-1}((\lambda - 3, \lambda),$$
$$(\lambda - 1, \lambda + 1) = (\lambda - 2, \lambda + 2) - b_{\lambda-1}((\lambda - 2, \lambda + 1) - a_{\lambda-1}((\lambda - 3, \lambda + 1),$$

$$a_\lambda = \frac{(\lambda-1,\lambda-1)}{(\lambda-2,\lambda-2)},\ b_\lambda = \frac{(\lambda-1,\lambda)}{(\lambda-1,\lambda-1)} - \frac{(\lambda-2,\lambda-1)}{(\lambda-2,\lambda-2)},$$

$$(\lambda, \lambda) = (\lambda - 1, \lambda + 1) - b_\lambda(\lambda - 1, \lambda) - a_\lambda(\lambda - 2, \lambda),$$

$$\mathrm{K}_\lambda = \frac{\sum F(x_i)x_i^\lambda u_i - (0,\lambda)\mathrm{K}_0 - (1,\lambda)\mathrm{K}_1 - (2,\lambda)\mathrm{K}_2 - \ldots - (\lambda-1,\lambda)K_{\lambda-1}}{(\lambda,\lambda)},$$

$$\Psi_\lambda(x) = (x - b_\lambda)\Psi_{\lambda-1}(x) - a_\lambda\Psi_{\lambda-2}(x),$$

$$\sum d_\lambda^2 = \sum d_{\lambda-1}^2 - (\lambda,\lambda)\mathrm{K}_\lambda^2.$$

Appliquons cette méthode d'interpolation aux sept premières données du tableau II pour exprimer les trajets *u* du projectile en function des durées *x* par le polynôme

$$u = ax + bx^2 + cx^3 + \ldots$$

Dans ce cas $F(x) = x$.

[Chebyshev provides the values of $x_1, x_2, \ldots, x_7$ and the corresponding values of u_i.]

En cherchant à exprimer *u* par un seul terme

$$F(x)\cdot\mathrm{K}_0\Psi_0(x) = x\cdot\mathrm{K}_0\Psi_0(x),$$

on prendra

$[F(x_i)]^2 = x_i^2$ [seven numbers are printed below]
$F(x_i)\cdot u_i = x_i\cdot u$ [seven numbers are printed below]

[Calculations follow. The result is

$$F(x)\cdot\mathrm{K}_0\Psi_0(x) = 247{,}85x.$$

La somme des carrés des erreurs avec lesquelles le terme trouvé représente les valeurs données se deduit de
[calculations follow and]

$$\sum d_0^2 = 0{,}07854535.$$

[Similar calculations provide $F(x)\cdot\mathrm{K}_i\Psi_i(x)$ for $i = 1, 2$ and 3.]
On trouve pour l'erreur quadratique moyenne avec laquelle les quatre termes trouvés représentent les valeurs données de *u*

$$E = \sqrt{\frac{\sum d_3^2}{n}} = 0{,}0084.$$

En s’arrêtant aux termes trouvés, on a pour l’expression cherchée de u

$$u = 105{,}36x + 15984x^2 + 25631000\, x^3 - 3546000000\, x^4.$$

Nous nous sommes servi de l’arithmomètre de M. Thomas de Colmar pour calculer les produits, les puissances et des quotients qui entrent dans les formules de l’interpolatiion. Avec cette machine on peut faire facilement et promptement la multiplication de huit chiffres pour huit chiffres, ou de sept par neuf, et la division de seize chiffres par huit chiffres.

FIN

Comment

Calculations above were made with an excessive number of significant digits; the same was true about Mayevski. Moreover, such method of calculation had been universal and even Fisher, about five decades later, followed suit. See discussion in *Science*, vol. 84, 1936, pp. 289 – 290.

Chebyshev later invented his own arithmometer (Youshkevich 1968, p. 303).

V

N. Ya. Vygodsky

Mathematics and its representatives in the Moscow University during the second half of the 19th century; extract

Istoriko-Matematicheskie Issledovania, vol. 1, 1948, pp. 141 – 182;
extract, pp. 175 – 182

Not only during the 1890s, the period of reaction, but even before that Bugaev had never been a political radical, but he loved his native tongue and culture and still kept to outdated views about the development of Russia[1]. And so, as far as I know, he never came out as a champion or advocate of the oppression which the government carried out in every field of the political and cultural life. He never hallowed the autocratic regime by the authority of science, never conducted clerical propaganda[2].

Incidentally, it is usually thought that Bugaev along with the Moscow mathematical school of the 1890s had been militant obscurantists and extremely religious. This opinion is based on the calumnious statements of Pavel Alekseevich Nekrasov, a member of the Moscow Mathematical Society. Let us acquaint ourselves with the activities of that peculiarly remarkable man. He (1853 – 1924) was a son of a priest and studied in an ecclesiastical seminary. In 1878 he graduated from Moscow University and Bugaev left him there to prepare himself for professorship. Nekrasov became *dozent* in 1885 and professor in 1886. His dissertation, *On trinomial equations* of 1882, was noticed in Russia (*Matematicheskiy Sbornik*, vol. 11) and abroad (*Math. Annalen*). During 1883 – 1893 Nekrasov had studied various problems in analysis and theoretical mechanics and compiled many works written on the modern level (twenty were published in the *Mat. Sbornik*).

In the end of 1893 Nekrasov was assigned the rectorate of Moscow University just at the time when the advancing reaction began attacking the universities. The new rector should have then become a police agent. The czarist government [a Soviet expression] was not mistaken: Nekrasov was the useful man. After his rectory had expired, he asked the minister for people's education to be retired. The minister, however, preferred to leave the decision to the czar. Alexsandr III indicated the merits of Nekrasov and commanded him to remain rector. The administrator of the Moscow educational region informed Nekrasov about this command which has been kept in his personal records in the Archive of the University.

So he remained rector four years more, then became administrator of that same educational region, and, finally, member of the scientific council of the Minister [not ministry] of people's education. Soon he stopped studying those mathematical problems which interested him previously and in 1898 began to publish books and articles on the theory of probability. Already in his first article (1898) of that ministerial period of his life Nekrasov adopted the bureaucratic

manner of writing, as though laying down his results without bothering to justify them as necessary:

Later, I will present a detailed derivation of all the abovementioned results if circumstances allow me to put my computations in order suitable for publication.

Markov (1899) at once indicated his mistakes which Nekrasov did not recognize, and their debates lasted more than fifteen years. Their sharpness less depended on the essence of Nekrasov's mathematical mistakes than on his becoming an apologist of autocratic rule and Orthodoxy. To readers wishing to get acquainted with Nekrasov's pseudo-scientific methods, I can recommend his book (1912). It was published by the ministry of people's education headed by the reactionist Kasso, a martinet of science whom Nekrasov had the cheek to thank deeply.

Kasso would have hardly found a better way for the spent money since along with mathematical formulas that book contained *chemical formulas of a normative state* such as (p. 119) his

Constitutional formula ABC: this formula assumes the concentration (representation) in the head of the political body of reasonable forces A, B, and C.

Concentration of the element C is the monarch with his officials

Concentration of the element A is the Patriarch (the Synod) with the council

Concentration of the element B is the State Duma with science and press

These lively central symbols which denote the reasoning of the Christian faith constitute the sovereign <u>*sacred security*</u> […] *ensuring the aspiration to bring the Kingdom of God nearer to our terrestrial Fatherland*.

Markov, who in 1902 [in 1912] requested the Most Holy Synod to be excommunicated from the Russian Orthodox Church, attacked Nekrasov with all the might possible at the time. In 1915, their conflict came to a head. Nekrasov, as the member of that scientific council (see above) established a commission for studying introduction of the elements of probability theory into the curriculum of the secondary school. Issuing from the pseudo-scientific approach to the theory it proposed to inspire the students with Nekrasov's gibberish about that triangle (see above). On Markov's initiative the Academy established a commission consisting, apart from Markov himself, A. M. Liapunov, V. A. Steklov, D. K. Bobylev and A. N. Krylov, which decided (1916) that

For a long time mathematicians have been acquainted with Nekrasov's views, but, until they were only discussed in special mathematical periodicals, they could have been considered harmless. However, the situation changes when they are disseminated by an official organ [the journal of the Ministry of people's education] *which the school teachers cannot help considering an authoritative guide to scientific pedagogic issues*

Therefore, the Academy of Sciences, as the most important scientific estate of the Russian Empire (*Charter*, § 1), *which might enter into everything concerning education* (§ 8) *and is obliged to*

care about the dissemination of education in general and to direct it to the general weal (§ 2b), *− the Academy ought to express its judgement about the main mistakes and the wrong (hence, harmful) ideas spread by Nekrasov who wishes to put them into common school use*.

Nekrasov's mistakes are then dealt with, and the report ends thus:

The Commission believes that the abovementioned delusions and wrong interpretation of the foundations of science and the misuse of mathematics directed at the preconceived aim of transforming pure science into a tool bringing religious and political pressure to bear on the rising generation, will irreparably damage education if penetrated into the school life.

Mathematicians, members of the Academy, have thus appraised Nekrasov's activity, but in Moscow, where, after Berdiav's death, Nekrasov became President of the Moscow Mathematical Society (and administrator of the Moscow educational region), he literally terrorized the mathematical circles. During Berdiav's lifetime, Nekrasov, as far as I know, had never attempted to connect his reactionary views with that Society; after his death, however, being the President of that Society, he, on 16 April 1904, delivered a speech devoted to his memory. There, he based his Black-Hundred propaganda on the views of Bugaev but at the same time, of all the founders of the Society. And he (1904, pp. 23, 70) also mentioned, as though his *likeminded* scientists, Fermat, Descartes, Pascal, Newton, Leibniz and other scientists as well as Pobedonostsev, Chomiakov and the Reverend Antoniy Chrapovitskiy.

It seems that, when reporting his gibberish to a scientific society, Nekrasov felt himself somewhat awkwardly, Anyway, when preparing that report for publication, he thought it necessary to add a preface, and there he wrote

I feel myself obliged to mention the peculiarities of exposition and style of my paper. The usual language is not quite suitable for expressing the mathematical contents of the principles of the structure of the world. The translation of that contents into the usual language is accompanied by almost insurmountable difficulties which compels mathematicians either to withdraw to their own field and refuse forever to describe popularly the important vital metric concepts, or to apply most complicated turns of the scientific, philosophical, political, social and church speech, to repeat their statements, and to use ponderous terms incomprehensible for readers, who are accustomed to the glib and stylish language of philologists-novelists and empirical dialecticians.

Indeed, when randomly opening a page of his paper, we encounter, for example (p. 165), such *most complicated turns of speech*[3]*:*

Moraltriangulations[4] *as given in the fact of a family* (*father + mother + either son or daughter*) *or in the fifth commandment* (*honour thy father and thy mother*) *naturally and artificially develops into a freeconnection of society.*

As stated above, Nekrasov's aim was to represent Bugaev as a military reactionist which was not true at all[5]. Yes, the muddled statements which Bugaev uttered in his old age made that aim easier, but Nekrasov was not yet satisfied and thus exposed himself (p. 239):

The completeness of the Weltanschuung belongs to the entire union [of scientists] *and does not allow the separation of N. B. Bugaev from V. N. Tsinger or F. A. Bredikhin, A. Yu. Davidov, P. L. Chebyshev or all of those mentioned from the other* [scientists].

It was possible to consider Nekrasov's statement as gibberish of someone crazy had they not been extremely purposeful[6]. When necessary, Nekrasov knew how to translate his thoughts into *the usual language*. Incidentally, so it was during his debates with Markov, to which I am returning.

Buniakovsky (1846, p. 326) considered it necessary to warn his readers against the application of his formulas concerning probabilities of testimonies to religious faith:

These formulas are derived under the premise that there exist certain physical laws, but since *facts in the spiritual world do not obey physical laws, all the ill-intentioned sophistications used by the pseudo-philosophers fall to the ground*.

Markov (1900/1913, p. 326) boldly denied Buniakovsky:

Independently from the mathematical theorems, it is clear that we ought to doubt exceedingly stories about incredible events which had allegedly happened in long past times. And we cannot at all agree with Acad. Buniakovsky […] *in that we must separate a certain kind of stories which he considers doubtless. However, we do not dwell on this subject to avoid still severest judges and accusations that I am shaking the foundations*.

After Nekrasov's aspirations to apply science for propagandizing religion had met with a rebuff, he decided to accuse his offender exactly of shaking the foundations. He (1916, p. 12) wrote:

By demolishing the abovementioned principles of Acad. Buniakovsky, Markov thus facilitates the spread of the elements of historical materialism. No <u>better</u> guide than Markov's book is needed for a systematic propaganda of extreme groundless materialism. […] *I can only appeal to the world of scientists and teachers and ask it to discuss who of us transforms pure science in an instrument for harmfully influencing the civil and religious health which serves for educating the rising generation*.

Nekrasov's statements cannot therefore be only considered as a manifestation of a mental breakdown. More regrettable is that the Moscow Mathematical Society had been tolerating as its president a man who defamed a scientific institution. We ought to remark, however, that the Society, terrorized by Nekrasov, only tolerated his unbecoming behaviour but did not share it.

In vol. 25 of the *Matematicheskiy Sbornik* we find the record of the proceedings of that Society of 22 March 1905, and there we find:

The secretary reported about the letter of P. A. Nekrasov[7] in which he announces his intention of publishing in the <u>Sbornik</u> a paper <u>Organic foundations of a state</u>. <u>A moral-arithmetical essay on electors and the elected in their mutual relations and relations to the supreme power</u>. […] *At the same time, Nekrasov asks us to register his announcement and <u>he also asks the opinion of the Society about it since his subject is peculiar</u>* (my emphasis, M. Ya. V.).

Referring to the small number of those present, the Society postponed its decision, but never returned to the matter. And on 20 September Nekrasov gave up his presidency. The Society resolved to express him its *gratitude for his activities of many years*. After having thus observed etiquette, the members of the Society probably took a long breath.

Nekrasov's successor became a most outstanding scientist, N. E. Zhukovsky. Apart from him, B. K. Mlodzeevsky, L. K. Lachtin and I. I. Zhegalkin became members of the presidium. The activity of those scientists both with respect to its essence and the time is beyond my chronological boundaries.

Notes

1. See for example Nekrasov (1904). M. Ya. V.

2. On 21 March 1900 the Moscow Mathematical Society (MMS) celebrated its president on the occasion of the appearance of vol. 20 of the *Matematicheskiy Sbornik*. K. A. Timiriazev spoke on the behalf of the physical and mathematical faculty of Moscow University. He began thus:

Highly respected comrade, Nikolai Vasilievich! Your fruitful service rendered to the Moscow University is continuing for more than 30 years. As its professor, and, recently, as the dean of the faculty, you did everything in your power. How much had the teaching of mathematics extended and developed during that period!

3. Encounter, in his own writing!

4. In a few contributions Nekrasov applied the word *triangulation*, and I believe that he picked it up while doubling at the Moscow Land Surveying Institute. Nothing is known about his work there.

5. Polovinkin (1991, p. 194) quoted Bugaev's son who had stated that, perhaps even in 1894, his father became utterly disappointed with Nekrasov. Unlike Polovinkin, I am inclined to believe that testimony.

6. Mikhailov & Stepanov (1985, p. 225) concluded that Nekrasov became mentally ill, but he continued scientific work and published a mammoth contribution on the central limit theorem. See Soloviev (1997) who definitively proved Nekrasov's failure (which was evident) and explained its reason.

7. At that time, Nekrasov moved to Petersburg, and, beginning with February 1905, did not anymore participate in the sittings of the MMS. M. Ya. V.

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VIa

Editorial

Trudy Russkikh Uchenych za Granitsei, vol. 1. Berlin, 1922, pp. 7 – 9

The emigration of Russian scientists began just when the Bolsheviks seized power in Petersburg [Petrograd] and Moscow. Their economic conditions were not yet critical, it was possible to satisfy at least the most elementary requirements of existence. University autonomy was not yet flagrantly violated, and the chaos in higher educational institutes not yet established which alone would have been sufficient for killing both higher education and further scientific work. And nevertheless Russian scientists began to leave Russia one after the other, although most of those departing abroad had no inkling about their future.

They were driven to foreign strange lands by the atmosphere of arbitrariness which neglected the personality. The first émigrés were firmly sure that the separation from their fatherland and home university will be short. It was necessary to hide from the dominant influence of bolshevism, but then, in a few months, to return to a legal state and resume, even under severest economic conditions, usual scientific and pedagogic activities, the goal of their life.

However, the hope that the Russian people will not desire to reconcile themselves for a long time with the Bolshevik regime did not realize. And with the dragging out of the process of the still inevitable downfall of bolshevism, with the extension of its power, the number of scientists, who were compelled to seek salvation abroad, increased. Horrible economic conditions of life [in Russia] actually prevented scientific activities, teaching had been becoming impossible also since the studying young men were in the same grievous state.

An almost complete destruction of all the auxiliary pedagogic facilities without which the teaching of many disciplines became impossible supplemented the picture of the dying out scholars and the dying science. This picture was the more horrible since it was impossible to say that it had been largely brought about by the evil intent of those who were obliged to take care of Russian science on behalf of the new authorities. Among them were some quite benevolent people, who did not believe in the long existence of the Bolshevik regime as well and were inspired at heart with the comprehension of the eternal significance of scientific truths. To put science, scientists, and educational institutions to sleep for the time being rather than to kill them, − such was the slogan of those representatives of the Bolsheviks in the realm of cultural and educational activities.

But they had only overlooked that, just as you cannot for some years with impunity put to sleep your conscience, the same is true in regard to science and its representatives with no danger of the very impossibility of their restoration. Just like weeds with a free access to a luxuriant garden unavoidably suppress the rapidly running wild

beautiful flowers, science falls into decay and perishes in the atmosphere of disrespect and arbitrariness. Experiments on science and scientists became ever more horrible and the entire educational system gradually turned into a cemetery or a hostel for the sick and the exhausted …

On the other hand, the territorial extension of bolshevism[1], the seizure, after prolonged battles, of new regions strengthened still more that unavoidable crave to go West. Only recently we had seen this forced flight when the last university free from the Bolsheviks, a refuge for all those who were unable to remain in their home universities but did not wish to go abroad, fell into the hands of the Bolsheviks.

And so, the small family of Russian scientists was separated into two groups: into those who remained in Russia and those who ran away. It is absolutely meaningless to compare their strengths; suffice it to say that each group includes most eminent representatives of Russian science and education. Their fate is very different but in spite of the difference in what befell them, the sense of unity, of internal kinship, undoubtedly did not diminish at all. The suffering is hard for both although to a different extent.

Being separated, they are pursuing the same goal: they are retaining that cultural wealth which Russia had acquired during centuries of persistent work of its great people. And now also this people will be able to come out of this disastrous time and restore its previous greatness.

Our fatherland is so near to, and at the same time so remote from us. And over there, our comrades, faced with the horrors of existence in hunger and cold, are continuing to stand up for the old cultural positions. The news about each new victim heavily tells upon our hearts. During those years there were so many such victims, and we are sincerely glad when finding out that a part of our comrades, not without excessive sacrifices, continues somehow to drag out a miserable existence. We see, however, how little they are able to save out of the formerly created, partly by them themselves. This circumstance imposes on us, living with dignity[2], a sacred duty to use fully all that available to us cultural wealth for continuing our scientific work and to prepare all that little which can be done by us here, abroad, for the restoration of scientific work in our home atmosphere as soon as the political conditions will allow it. In addition, we ought to help the Russian young people in their drive for education, the pledge of their future fruitful activity for the benefit of their people.

These various problems cannot be solved by uncoordinated efforts of separate scientists. In each cultural centre to which the now homeless Russian scientists have fled, they gathered into friendly circles, not only for mutual help, but even more for lightening the achievement of these grand goals which constitute the meaning of their emigrant existence[3].

Here, in this collection, we provided brief essays of the activity of the first Russian academic groups from Belgrade, Berlin, London and Paris. Numerous later groups from Constantinople, Switzerland,

Finland and Estonia did not have time to send such essays, but we hope that these will appear in our second volume.

One of the manifestations of the collective work of the Russian academic groups is this *Trudy* [in Russian, *Trudy* is plural, O. S.]. It aims at lightening the possibility of continuing their scientific work for Russian scientists abroad by establishing an audience of specialists and the public at large which is so necessary for supporting the energy and encouraging further work under the extremely difficult economic and moral conditions of our existence. That audience is open to any speciality for discussing all the problems under one single condition: that discussion ought to be scientific.

We are now preparing a conference of all the academic groups abroad. We are sure that in such a union we will find strength for wider and more fruitful work, we will create a more favourable atmosphere for that. And we hope that our second volume [which appeared in 1923, O. S.] will be the best proof of all that.

Let us hope that the days of our ordeal abroad are ending, but this ought to prompt us to work especially hard rather then to remain idle and become similar to the unreasonable ten virgins [Matthew 25, O. S.] who *took their lamps and went out to meet the bridegroom but did not take any oil with them*. The virgins *went to the wedding banquet* but heard the answer: *Truly I tell you: I don't know you*.

Notes

1. Rostovsev (1919, end of paper) quoted a Russian newspaper from Irkutsk, a city obviously still in the hands of the white movement: in Siberia and *neighbouring regions* the Bolsheviks closed 8 institutions of higher education and burnt 1; the same about 68 and 6 secondary schools, and smashed up 109 and 13 libraries respectively, destroyed 32 historical monuments and 8 museums.

2. Below, extremely difficult conditions are mentioned which was true for perhaps most of the émigrés.

3. In 1925, Chuprov lived in Prague and published a paper in the leading Russian (Soviet) statistical journal. He wished to keep in touch with Soviet statisticians for whom he remained their beloved leader. In the opinion of many of Russian scholars from the Prague *academic group* (possibly of later origin) he acted outrageously and was therefore financially punished. This quite possibly precipitated his early death (in 1926).

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VIb

An Appeal. Report of the Congress of Russian Academic Bodies. An extract

Trudy Russkikh Uchenych za Granitsei, vol. 2. Berlin, 1923, p. 340

The Congress of Russian Academic Bodies Abroad, in appealing to the scientists of all countries and to the entire civilized world on behalf of more than 400 scholars scattered in sixteen countries, raises its voice against those conditions of existence and work that the Soviet regime of utter arbitrary rule and violation of all the most elementary human rights laid down for our colleagues in Russia.

Never, under any system either somewhere else or in Russia itself, men of intellectual pursuit in general, or academics in particular, had to endure such strained circumstances and such a morally unbearable situation. Especially disgraceful and intolerable is the total lack of personal immunity that at each turn causes unendurable moral torment and threatens [everyone] with bodily destruction.

The execution, or rather the murder of such scientists as Lasarevsky, a specialist in statecraft, and Tichvinskiy, a chemist, cries out to heaven. They were shot, as the Soviet power reported, − the first for compiling projects for reforming the local government and putting in order the money circulation, and the second, for communicating information to the West on the state of the Russian oil industry. And still, these horrible acts are only particular cases [typical] of the brutal political regime denying any and every right and reigning over Soviet Russia.

We would have failed our sacred national and humanitarian duty had we not stated our public protest against that murderous and shameful system to our colleagues and to the entire civilized world.

VII

M. Yu. Sorokina

"It is impossible to keep silent anymore". From the epistolary heritage of Sergei Fedorovich Oldenburg

Voprosy Istorii Estestvoznania i Tekniki, No. 3, 1995, pp. 109 – 111

Oldenburg (1863 – 1934) is a critical personality of the Russian scientific-managerial process at the beginning of the 20th century, an orientalist and the permanent secretary of the Academy of Sciences in 1904 – 1929. However, until now he remains a terra incognita of our national social history of science. On the outside, his life was quite happy. He was described in press, and even after the *Case of the Academy of Sciences* (1929)[1], his name did not disappear from Soviet encyclopaedias or reference books[2]. On many occasions he was mentioned (as a rule, in listings) in monographs devoted to the history of orientalism and the Academy of Sciences.

But that was all. A wall of silence erected around an *unprohibited* name proved so firm, that, after the obituary notices of the 1930s, publications about him had only appeared exactly 50 years since his death. A historiographic splash occurred in the mid-1980s, a centenary after his birth, and included a collection entitled *Sergei Fedorovich Oldenburg* (Moscow, 1986).

The reasons are simple. Quite recently, he seemed *too white*. Indeed, a cadet [constitutional democrat] and the minister of education in the Provisional Government [in 1917]. In 1946, his widow attempted to publish a paper about his years of study in a grammar school [gymnasium] and university, but S. I. Vavilov, the President of the Academy of Sciences, concluded his testimonial [1, lists 101 – 102 reverse] by an unambiguous verdict: *Certainly unfit for publication* and justified his severe judgement by explaining that

The manuscript deals with a chronologically near period very critical in the political sense, and with people which, already before our eyes, had to become later noticeable social and political figures.

This, in spite of his first phrase, his *recognition* that

The memory about Sergei Fedorovich is so dear to me, that I greedily catch any new, even insignificant detail of his life.

Today, Oldenburg seems to many *too red*. He is accused of attempting to find a compromise in his relations with the new power; of betraying the ideals; of having been too loyal. Such an opinion confirms the historical forecast of Evg. Trubetskoi. That philosopher [Evgeniy Nikolaevich, 1863 – 1920, O. S.] understood the Russian *maximalism*, "all or nothing", as a dead-end form of social consciousness, as a devotion to the *purity of the formula* irrespective of its practical result.

His contemporaries, whether close to him or not, variously appreciated Oldenburg. The philologist, academician S. A. Jebelev [2,

lists 1 – 1 reverse] wrote on 1 March 1934, the next day after Oldenburg's death:

He had undoubtedly been the spiritus movens of the Academy. […] *The latest years* [1920s – 1930s, M. S.] *had deprived him of the sympathy of many of all those who more or less knew him and did manifest it. Oldenburg wished to switch over, at first by adapting himself to the new conditions, then completely acknowledging them, but he did not succeed, nor could he have succeeded*. […] *How many he saved; saved those who were destined for destruction. And when that became too late, Oldenburg abruptly turned to the left, although hardly anyone believed or will believe in such a turn. And this was the reason why the sympathy of many had dampened.*

It seems that many of those belonging to the academic milieu shared that opinion, see for example academician A. N. Krylov [3], but nevertheless, each time that the permanent secretary wished to resign, no replacement had been found. Note also that for ten years, until 1927, the Academy of Sciences had apparently remained the only institution in the country which continued to live according to its own, actually pre-revolutionary laws. Until now, historians have not duly appreciated this unparalleled fact.

As though answering his opponents, a close friend of Oldenburg, a historian and literary critic D. I. Shakhovskoi [4, p. 272] wrote:

First and foremost, Sergei was a man of real business, an unbending votary of duty, who appreciated results and genuine business. At the same time, he was very self-confident, and, since he invariably moved in a much lower circle, this quality had expressed itself in some kind of self-importance. The immense problems which he took upon himself and carried out with a surprising skill, completely absorbed all his spiritual power and made him a slave of the difficult and responsible service which he took upon himself.

Academician Oldenburg belonged to the generation of national intelligentsia through which, during the period of his highly conscious life, fully ripe in the spiritual sense, came the History [5, p. 223]. That period included three revolutions, the world, and the civil war. These social cataclysms deeply influenced the evolution of the social and political outlook of the entire generation as well as its ethical views inseparably linked to that outlook.

The idea of a spiritual continuity as the foundation of the existence of any society always remained at the centre of their Weltanschauung. The loss of the intelligentsia, of the bearer of this idea, was interpreted as the destruction of culture accompanied by the destruction of the entire social arrangement. Even in 1906, Oldenburg [6, list 12 reverse] wrote to his son:

We have to acknowledge that the centuries of slavery engendered in the heart of the proletariat a hatred of those, to whom life gave all the boons and benefits, and now the retribution is beginning. This should be understood, and, in addition, we ought to understand that all efforts should be made to save, from that terrible wave of economic materialism, the culture, the ideals, all that which adorns life and which, once lost, is not returnable. This, indeed, is our aim, the aim of those who know the significance of those immaterial boons, to

preserve them for mankind. This aim is neither a party, nor a political problem, it is loftier than either.

It was from this viewpoint that the permanent secretary of the Academy of Sciences understood and appreciated the occurring events. He was one of the few scientific figures who, although in his own way, accepted the revolution as a retribution for *centuries of slavery*. Following N. A. Berdiaev, Oldenburg could have repeated: *I have went through the revolution as through a moment in my own life* (quoted from [7, p. 36]).

The status of permanent secretary compelled Oldenburg to be a truce envoy both within the Academy itself and beyond it, to surmount continually the hostility and the mutual misunderstanding about the relations between scientists and the powers that be, and within the scientific community itself.

The *skirt* intelligentsia, that *accidentally puffed up bubble on the people's body which was torn from it and lost any vivid feeling of the reality*; the cadets, *those slobbering bookworms and Pharisees*, − such characteristics abound in the reminiscences about the events of the autumn of 1917 written by the mathematician and, since 1919 [until 1926, O. S], vice-president of the Academy of Sciences, V. A. Steklov [8, pp. 285, 286 etc.].

A pragmatist, a man of volition and action, an eminent scientific authority, Oldenburg actually headed a group of academicians (mostly natural scientists) who stood up for continuing work under whatever regime. Other sentiments reigned mong academicians-humanists. Indeed, they were mostly those cadets impartially mentioned by Steklov, and for them, the October [1917] events could not have been a *revolution*, they insisted in opposing it.

During those first months of the new regime, Oldenburg's role in establishing contacts between the Academy and the state power was exceptionally important. His longstanding acquaintance with Lenin ensured a maximally mild entrance of the Academy into the formed structure of managing science. Furthermore, it to a large extent assisted in securing the leading position of the Academy among the entire scientific community. Yes, that process led to a serious and long confrontation with the people's commissariat [ministry] of education, with M. N. Pokrovsky. In turn, he aspired to be the *minister* of science. Incidentally, that confrontation was inevitable. Both sides professed almost the same ideology of the organization of scientific activity and necessarily clashed when attempting to play the role of the *real* representative of state interests in the field of scientific policy. Oldenburg's appeal below [on the next pages of the same source] on behalf of the Academy of Sciences to the peoples' commissar [minister] of education, A. V. Lunacharsky[3], clearly shows the level of that claim of the *foremost scientific estate* to have the leading role in the social structure.

The advocates of a strong state power[4] by conviction, most authoritative Russian scientists, V. I. Vernadsky, Oldenburg, A. A. Shachmatov et al, even before 1917 had developed the concept and the programme of a *state* [my italics, M. S.] organization of science. They had thus willy-nilly become the founding fathers of the Soviet

system of that organization. I recall B. Pasternak: *The fool, the hero, the intellectual printed and drew posters showing the joy of his own decline*.

And so it happened that, when forgetting the quite justified emotions accompanying the ever newer factual information about the repressions in science, we ought to agree with many basic propositions of the *pre-perestroika* historiography[5].

Let us return, however, to Oldenburg. Only a few have been guessing how much spiritual strength and nervous energy did his role as the truce envoy cost him the more so since he himself believed that *what you are enduring, you ought to endure alone*. And then, in the same letter of 1923, he [9, lists 7 reverse – 9] acknowledged:

It is necessary […] *to save both the scientific work and the people for that work during* [my] *constant debates, conferences, journeys to Moscow* [from Petrograd, as it was then still called, O. S.], *in writing and when defending in endless reports and finding yourself between rude and commanding people and overstrung intellectuals.* […] *From morning to evening, without a single day of respite, amid searches going on* (*we had experienced six of them*), *arrests, endless efforts in the Cheka* [Special Commission for Combatting Counterrevolution and Sabotage, O. S.], *the tears and the suffering of those who are left, often futile and sometimes successful attempts to save people from the shooting, people who have near relatives; the emotions experienced in prison* [in September 1919, M. S.] *when fellow prisoners were taken out of the cell to be shot* (*I thought then that it was easier to die*). *And this is going on for years.* […] *And at the background for all this are deaths, deaths without end, of people near and remote leaving behind widows and children. Being in a central position of a large enterprise, I had willy-nilly been and am near to all this, and people come to me since out of house I am not considered ice-cold.* […] *I believe that I may still live in spite of everything, not because I am ice-cold, but because I have faith in life and people, in spite of everything love it and them since I feel by all my heart the great boon, the charm and joy of life. Life is so infinitely complicated, difficult, − and wonderful.*

This letter markedly differs from his other letters of those years. As a rule, those other ones are businesslike, and no time was left for *lyricism* or personal letters. Oldenburg personally conducted practically the entire academic correspondence irrespective of the social status of the addressees. What left the academic office on special forms had the signature of the permanent secretary on the copies.

The main part of Oldenburg's correspondence with Gorky is kept in his archive at the academic Institute of World Literature and was published in 1987 [10]. Some other letters are published below [on the next pages of the same source, O. S.]. Oldenburg and Gorky had got acquainted in 1899, but only the events of 1917 drew them personally together. In Gorky, the writer, Oldenburg, saw a harmonious join of the peoples' basis and a spiritual reflection peculiar to the intelligentsia. And not the least important was that, irrespective of all the abrupt changes of the political climate in Petrograd, he, Oldenburg, was a *barin*, a man belonging to the upper strata of

society, whereas Gorky invariably remained *our man*, a *proletarian writer*.

His words were listened to, sometimes they were even responded to. Because of his prestige, a Petrograd commission on the improvement of the life of scientists was established and, consequently, many men of science remained living and continued to work. Shklovsky [11, p. 196] remarked that

For the Russian intelligentsia Gorky had been the Noah. On board the arc [he named three publishing houses, O. S.] *intellectuals saved themselves during the flood, and not for the aims of the counterrevolution, but so that literate people will not disappear in Russia*.

Oldenburg's biography is not yet written down. One of the difficulties consists in that actually, in the sense of activity, the Academy and its permanent secretary had almost been a single entity. One of the keys for understanding his complicated personality is possibly to be found in his numerous necrological essays of a historical scientific nature. There, while describing his late colleagues, Oldenburg seems to have rather meditated about himself. In one of these essays he [12, p. 131] wrote:

When the results of a man's activity are summarized, which usually happens after his death, he can be appraised according to two different viewpoints: either bearing in mind the usefulness which he rendered, or his merits as a man and scientist, and those appraisals do not invariably coincide. It often happens that we have to say that he did not render all the possible given his mind, abilities and knowledge. Why does life turn out in such a way, is always a complicated question, but a certain responsibility invariably rests upon himself.

Notes

1. That case (1929 – 1931) was fabricated by the security organs as a dress rehearsal for the further Stalinist show cases. Its initiator was the mentioned below Mikhail Nikolaevich Pokrovsky (1868 – 1932), the assistant of Lunachsky (also mentioned below) and a horrible diehard communist.

2. Under Stalin and his followers such disappearance was the rule. *Undesirable* books kept in libraries were either destroyed or became unavailable for *usual* readers. Mendeleev certainly remained desirable, but around 1990 the volume of his collected works, devoted to investigations of gunpowder (still a top secret!), was still unavailable for general readers-scientists at the Lenin State Library … No one ever thought of rehabilitating books!

3. Much nonsense had been published about Lunacharsky's alleged liberalism. He certainly was highly knowledgeable, but here is Tunkina, commenting on Rostovstev (2002), on p. 11 of that book:

The school had never been broken apart as impudently, ignorantly and barbarously as during Lunacharsky's enlightened dictatorship.

In 1929, Lunacharsky was removed from his position: Stability and common sense were needed rather than revolutionary utopian enthusiasm (Busev 2007, pp. 92 – 93).

Mikhail Ivanovich Rostovtsev, a most eminent historian of antiquity, emigrated from Russia around 1919. Rostovtsev (1919), not included in that book, quoted a letter of a teacher of a boarding school, apparently in Petrograd. She stated that the mortality of the children amounted to 60 and even 70% [yearly]. Rostovtsev also described other horrible facts.

4. The Russian word, lacking in both the orthographic dictionary of 1969 and in a noteworthy Russian – English dictionary, was *etatists.*

5. This is hardly understandable.

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VIII

N. S. Ermolaeva

On the so-called Leningrad mathematical front

Voprosy Istorii Estestvoznania i Tekhniki, No. 4, 1995, pp. 66 – 74

[1] In 1930, the Leningrad Physical and Mathematical Society, established in 1920 by N. M. Günther, a corresponding member of the Academy of Sciences of the USSR since 1924, ended its existence. The history of this event belongs to the subject *Science and the authorities* whose various aspects attract the attention of many authors, for example, Vilenkin [1]. To comprehend the setting of that event, important for the mathematical life of Leningrad, we ought to start from 1917 and at least touch upon the policy of the state towards the training of the scientific personnel, reorganization of the higher education etc. For people, who had not personally experienced the events of those years, it will be difficult to understand the psychological atmosphere of that period the more so since many facts have not been reflected either in print or archival sources.

In 1918, preventive arrests began in Petrograd and the terror intensified after the assassination of V. Volodarsky (M. M. Goldstein) and M. S. Uritsky. Many professors and eminent specialists of higher military and technical schools were among the victims. Thus, it is known that the professor at the Marine Academy I. G. Bubnov (1872 – 1919) was detained in a prison camp near Novgorod, then freed by happy chance[1].

Just after the revolution the institutes of higher education began admitting all those who desired if older than sixteen. If the number of the places was insufficient, workers and their children, as recommended, were preferred. Semi-literate students began to fill the institutes. Soon, however, this situation was modified [changed]. A special permission was required for entering, the admittance of people of non-proletarian origin was essentially restricted, but a document about graduation from school was required [2].

In the end of the 1920s, along with *purges* of various organizations, began *purges* of students. Even last-year students had been expelled for concealing their social origin. However, for the time being the authorities had to tolerate professors and instructors since no replacement was available. Nevertheless, it was attempted to lessen their influence on the students. In 1921, the chairs were therefore replaced by commissions according to the subjects and equally consisting of scientific workers and students with party members being preferred.

In 1923 a Central Bureau of proletarian students was established at the All-Union Central Committee of Trade Unions. Its aim was the education of students in the direction desired by the authorities. In science, the first place was only allotted to investigations directly serving practical requirements of the national economy and higher

education had only to provide practical skills. In 1925, even O. Yu. Schmidt [an eminent scholar] stated, at the first All-Union Conference of Proletarian Students, that [3, pp. 225 – 226]

An educational institute only aims at providing the possibility of practical knowledge rather than at the development of the personality.

Fundamental sciences were considered outdated and connected with their bearers, university professors, almost all of them thought to be class enemies. Not accidentally, a certain Voronov [4] quoted Engels whose letter of 1891 was published here in 1922:

Technical specialists will be our principled opponents, they will deceive and betray us wherever possible, and we will have to turn to terror and shoot them[2].

Voronov added: *During the first years after October* [after the … revolution, or, rather, coup d'état, O. S.] *we neutrally regarded neutrality, but this attitude cannot continue*.

[2] An essential role in the history of our subject had been played by the Communist Academy established in 1918 to develop the Marxist theory and, until 1923, called Socialist. Its section of natural and exact sciences created in 1916 [??] was headed by Schmidt with professors V. F. Kagan, N. P. Kasterin, V. A. Kostitsin, A. Ya. Khinchin[3] and V. G. Fessenkov as members and a large number of scientists was enlisted. The Academy existed until 1936.

In 1928, a study group of materialists, mathematicians and physicists, worked at the Academy along with similar groups of physicians, biologists, statisticians etc. The reports read out at the mathematical section [subsection, see below] were either philosophical and historical or purely mathematical. I mention some of them scheduled for 1928 – 1929 [9, pp. 47 – 49]:

Kagan: Conventionalism and materialism in mathematics

L. A. Lyusternik: Mutual relations between the categories of quality and quantity in mathematical sciences

L. G. Shnirelman: Evolution of the concept of function

A. O. Gelfond: Evolution of the concept of integral

A. N. Kolmogorov: Intuitionism and the Moscow (Luzin) school

L. M. Lichtenbaum: Locally coherent continuum and the Lyusternik space

Also scheduled were colloquiums devoted to

The theory of groups and its application to analysis and geometry (Schmidt, Lyusternik, V. I. Glivenko, Gelfond)

Qualitative methods of analysis and mechanics (Lichtenbaum, Shnirelman)

and there even was a seminar in applied mathematics headed by Luzin.

It is unclear what exactly was actually realized, but some reports were repeated at the Moscow Mathematical Society, and the periodicals *Estestvoznanie i Marxism* (Natural Science and Marxism) and *Vestnik Kommunisticheskoi Akademii* (Herald of the Comm. Acad.) published papers of Moscow mathematicians.

On 29 Dec. 1928 a mathematical subsection was established at the Scientific Society of Marxism[4]. Its sixteen members included A. D. Drosd, L. A. Leifert, V. V. Lyush, V. I. Milinsky, E. S. Rabinovich.

No one was a scientist, but some of them became professors without defending a dissertation. Their work consisted in

Collecting materials pertaining to Marxist literature concerning mathematics and its methodology, delivering reports on problems of the history of mathematics in the Marxist spirit, propagandizing Marxist ideas in mathematics and in forming the mathematical education according to the viewpoint of propagandizing Marxism.

They understood, however, that by themselves they were unable *to attain their aim, so that it was necessary to enlist such specialists who were not first of all Marxists, the new academicians and professors of higher education institutions* [10].

[3] In Leningrad, the leader of that society became L. A. Leifert, a dozent of the University. Son of a well-off man, who as it seems, was an owner of a book publishing enterprise, he graduated from the Petersburg University in 1909, was baptised while a student. In the 1920s, when social origin became extremely important for a career, Leifert stressed in all possible ways his *revolutionary character* without shunning any means.

Thus, owing to his efforts, some talented graduates of the University, for example S. G. Michlin, were not allowed to continue their education as post-graduates. Here is one more story told me by Professor S. N. Numerov. During study hours at the University, Leifert said: *Look! Here is Numerov*[5]. *His grandfather had turned me out of the gymnasium*. That was a lie, and, moreover, Leifert had graduated from a gymnasium with a silver medal, but some students came to a desired conclusion and, in this case, informed on Numerov.

And so, by 1930 political indifference became for the authorities insufficient. Re-election of professors had begun. A. Ya. Vyshinsky, the then rector of Moscow University, published a letter in *Izvestia* [a leading newspaper, O. S.] in which he fiercely assaulted statements such as *Science is objective* or *Newton would have* [?] *discovered his laws even without Marxism* and called the professors class enemies [11].

Elections in the higher school were replaced by appointments[6], but student organizations were freed from *administrative-pedagogic functions* [12]. It became compulsory to appoint as *rectors* prominent party organizers, staunch, which was the main point, even if less experienced in the life of higher education [13, p. 57]. Incidentally, in 1930 the terms *rector* and *dean* were replaced by *director* and *manager*, just as applied in industry. Indeed, the previous terms denoted the titles of the heads of monastery boards and abbots of monasteries respectively, as Vyshinsky [14] told his readers.

All the society not excluding the higher school had to be permeated by the idea of collectivisation[7], as proclaimed [in 1930] by the 16th congress of the Party. Somewhat later another slogan began to be popularized: plan absolutely everything. Along with the Marxist dialectics all that began to be instilled into mathematics. An editorial in *Leningrad Pravda* stated [15]:

We have already achieved the most important from the viewpoint of construction. Only a little is left: to study the technique and become proficient in science.

[4] In Moscow, E. Ya. Kol'man[8] became the main mathematical ideologist and his papers began to appear one after the other, for example [16 – 24] as well as the editorial [25]: it was easy to guess its author. The essence of all these papers as also of similar papers of other authors was this.

Exactly mathematics, the most politically indifferent on the outside, became the focus of reactionary bourgeois philosophy, of intuitivism and conventionalism in particular. The Moscow mathematical school which declared itself outside of politics is the most reactionary.

So said the Vasilievs, Bogomolovs, Florenskys [names in plural, O. S.] *who published their mystical, double-dyed idealistic mathematical contributions; so stated the Egorovs who hid their mortal hatred of socialist construction; thus explained themselves those mathematicians who avoided signing the appeal to foreign scientists about the sabotage*[9] [25, p. 7].

Among those who refused to sign that appeal was N. N. Luzin whom Kol'man [19 – 22, 25] repeatedly although monotonously stung by his critique. The works of that intuitionist Luzin are *empty abstractions*, a statement which Kol'man justified by the problem on the cooling of a rod (which, incidentally, did not concern Luzin, N. E.). And, as Kol'man [20, p. 34] stated, Luzin

Based his considerations on a continuum from which he throws out all the given rational ordinates of points and thus obtains an absolute discreteness, still less applicable to reality than absolute continuity.

And he declared: The root of all evil is that mathematicians are unable *to understand dialectically the unity of the discrete and the continuous, the statical and the dynamical*. *Mathematics* [in Russian, feminine gender, O. S.], is not the queen of the sciences, it always was a maidservant of physics, mechanics, chemistry [25, p. 6]. Mathematics, as also all the sciences in general, ought to be reconstructed on the Marxist principles whereas the formulation of new problems should occur in a planned order, see [16]. But how to achieve this goal? Here is his answer [21].

First, mathematics should not be torn away from practice (unity of theory and practice). Second, only advanced, and, moreover, unified methods should be left for it. We should abandon formal logic, axiomatization and the theory of probability and create, on the highest basis, a synthesis of arithmetic, algebra and analysis and do away with the gap between discreteness and continuity.

Kol'man offers [other] fundamental advices as well (Ibidem). He suggests both *to summarize series and derive the roots of equations stochastically* [without the lacking theory of probability, O. S.]. And

We ought to develop mathematics by further specifying the qualitative differences in the realm of the quantity itself[10],

whereas mathematicians should assimilate the real connection between the concrete and the abstract. The main path for attaining all that consists [22] *in the study of the history of the development of science itself by the Marx – Engels – Lenin method*[11].

S. A. Yanovskaya [27] was delighted with the textbook [26] in which the Marxist ideology allowed the authors to create a new teaching of the means applicable for the planned economy[12]. In their

publications on mathematical statistics these authors ran down Markov, Chuprov, Slutsky as well as Western statisticians in particular Karl Pearson and his theory of correlation.

Reorganization of science [25, p. 8] *is a most important component of the alteration of millions of people, so clearly formulated by Stalin* [not *so clearly* stated by Kol'man, O. S.]. And Kol'man [20, p. 38] railed at the impossibility of commanding science in spite of his, and of the similar efforts which showed the way and created all the necessary conditions for achieving this aim:

We are prepared to sign agreements about socialist emulation[13] *and state various declarations concerning planned work.*

For the sake of justice it ought to be remarked that Kol'man sometimes expressed himself quite reasonably but in such cases he offered his considerations as his own discoveries which opposed all the other [!] creative mathematicians.

[5] In 1930 – 1931, important changes had occurred in the mathematical life in our country. In Moscow [28, p. 48] the Mathematical Society

Expelled reactionists (*Egorov, Finikov, Appelrot*). *It extended itself by including post-graduates and the previous scientific-pedagogic study group. Now the Society struggles to form a new presidium: Kol'man, Vygodsky, Khotimsky*[14]*, Gelfond* […] *et al.*

The declaration of the initiative group for reorganizing the Society indignantly stated [29, p. 70]:

The Society had not at all responded to the arrest of its chairman (Egorov, N. E.) *and fixed the next ordinary sitting with reports of Finikov, Egorov's nearest companion-in-arms in the Institute* [?] *and Society, and Kurosh, just expelled from the Komsomol*[15].

Egorov is known to have been arrested and exiled to Kazan where he died in 1931 in a special hospital.

Schmidt, who at that time did not anymore head the mathematical section of the Communist Academy, was accused of *a wrong ideological position in mathematics* [27, p. 39] (his contribution *Algebra* in the [first edition of the] *Great Soviet Encyclopaedia* was meant, N. E.) and, in addition, of *his tacit protection* of many published non-Marxist statements etc. [30, p. 16].

Unlike the situation in Leningrad, in Moscow the initiative group included talented mathematicians who also became members of the editorial staff of the *Matematicheskiy Sbornik*. In Leningrad, the events went on in a somewhat different way although in accordance to a similar scenario. The Leifert group oriented itself towards Kol'man, Yanovskaya and Vygodsky and attempted to deliver reports on the foundations of mathematics, but it was not sufficiently competent in that area and therefore restricted its activity to general phrases on the history of mathematics and teaching from the viewpoint of Marxism [31]. In 1931, teams of various kinds were created (collectivism in work!) including those devoted to picking to pieces the work of Professor S. A. Bogomolov and the methodical propositions of Academician V. A. Steklov.

On 20 May 1931 the first of these two teams had participated in the dispute at the Pedagogic Institute by attacking S. A. Bogolyubov's

[Russian] book *Evolution of the Geometrical Thought* already discussed by Kol'man [24] and accusing him of idealism. It also criticized the entire activity of the Society of the Adherents of Mathematical Education whose chairman, from its origin in 1924, had been Bogomolov. In the resolution carried by that discussion under Leifert as its chairman Bogomolov was asked to acknowledge in writing *his mistakes*. The Society ceased to exist [32, pp. 39 – 48].

The second team which proposed to criticize Steklov apparently did nothing. We can suppose that, first and foremost, it thought of criticizing Steklov's book [33] written in 1920 and published in 1923. When describing a very remote period, Steklov drew a sufficiently transparent parallel between the church authority and the ruling party and came out against *fruitless dialectics*. The attack had not taken place apparently since Steklov, who died in 1926 and had collaborated with the Soviet authorities, should be seen as a specimen of loyalty in the eyes of future generations.

[6] Leifert and his group considered as their main achievement their activity directed to *the stratification of Leningrad mathematicians*. Not unjustifiably they thought that three groups of mathematicians were formed at Leningrad University. The *rightist* group included N. M. Günther, V. I. Smirnov, G. M. Fichtenholz et al, the *leftist*, Leifert, Kulisher, the abovementioned Drosdov et al, and the third, the *intermediate* group, had only two members, I. M. Vinogradov and A. M. Zhuravsky.

All Leningrad mathematicians in general had apparently been classified in the same way. Later the Leifert group became able to attract Vinogradov and other good mathematicians. The campaign against Günther, the chairman of the Leningrad Mathematical Society, began before the 1929 elections to the Academy of Sciences. An alternative candidate from Leningrad organizations [?] was Vinogradov and he was indeed elected [full academician, O. S.]. Already in 1926, when discussing possible candidates to the Academy for replacing the late Steklov, academician Ya. V. Uspensky, the nearest comrade-in-arms of Günther in the Mathematical Society and friendly with him, wrote to Krylov [34, list 1 reverse]:

Among them (among S. N. Bernstein, Luzin, Günther and S. A. Chaplygin, N. E.) *I consider Günther as the most powerful on the scientific side. However, as you know, he has a tendency to damage everything that he writes, his works become difficult to read, and a partly wrong impression is forming about him. In addition, a certain naivety of his social statements, although resulting from best intentions, does not turn out in his favour. It will therefore be difficult to defend his candidature.*

Günther and the Physico-Mathematical Society headed by him was accused of a lack of connections with Soviet public opinion, of the small number of its members, of the refusal to instil dialectical materialism into mathematics and not assisting *the march to technical knowledge* [30, p. 37]. The significance of Günther's scientific work was belittled in every possible way both in disputes and press, certainly without adducing any proof. And the Society was also accused of *keeping silent when those professors near to Günther, Ya.*

D. Tamarkin, A. S. Besikovich and, recently, the former academician Uspensky, who had played a prominent part in the Society, emigrated one after another[16] (Ibidem).

The situation intensified during the First All-Union Congress of Mathematicians (Kharkov, 24 – 29 June 1930) when Moscow and Leningrad mathematicians-materialists demanded to send a greeting to the just then occurring 16th Party congress. Academician Bernstein, corresponding members Egorov, Günther and some other mathematicians thought that that will be inappropriate and useless. After all, the greeting was apparently not sent. Indeed, otherwise the Leifert group would have said so in the booklet [30], mentioned below, would have not restricted it to their struggle for its sending[17]. (Bogolyubov, a corresponding member of the Ukraine Academy of Sciences, made known that after the Party congress Bernstein, afraid of being arrested, fled from Kharkov [for how long? O. S.]).

At the next congress of mathematicians (Leningrad, 1934) all the necessary greetings were sent since it was unthinkable to act otherwise. It became very dangerous for the Leningrad Mathematical Society to continue working as formerly, and in the autumn of 1930, on V. I. Smirnov's advice[18], it quit its activities without any announcement [35, p. 6].

The period of 1930 – 1931, *the year of the great change*[19], continued. The Scientific Society of Marxists merged with the Society of Militant Materialists-Dialecticians, and then, in September 1930, a Society of Mathematicians-Materalists at the Leningrad branch of the Communist Academy separated from it. Leifert became its chairman. The new society received concrete instructions not only from the leadership of the Party, but in addition from Kol'man who came to Leningrad and on 21 March 1931 delivered a report. His second report of 27 April 1931 was published [21]. Yanovskaya and Khotimsky also came to Leningrad and delivered directive reports.

The new society dearly needed to increase the number of participants [of its members] and won them over in a mass scale and by November 1931 it already had 88, mostly post-graduates. Among those known in mathematical circles I name G. M. Goluzin, B. M. Delone, L. V. Kantorovich, N. M. Koyalovich, V. I. Krylov, I. F. Lochin, I. P. Natanson, B. I. Segal, V. A. Tartakovsky, D. K. Faddeev and G. M. Fichtenholz.

We may suppose that young people were partly attracted by a desire of revolutionary change in mathematics, and partly were compelled to enter since it was necessary and all others were entering. Teachers probably justifiably thought that otherwise they will be discharged from work. The causes, however, could have been various.

The new society, as stated above, was interested in ensuring for itself a mass character and the only known to me mathematician, whose preliminary request to be admitted as member, was turned down because of *the applicant's unclear position which he holds at present* [36]. He, a dozent, later a professor of Leningrad University, O. K. Zhitomirsky, wrote his request by pencil: *I desire to participate in the positive work of the Society, and ask to be admitted as member* [37].

On 30 March 1931 *the initiative group* of mathematicians, partly former members of the Leningrad Physical-Mathematical Society, put out a declaration about their active, in the spirit of that time, position and stated that they founded the Leningrad branch of the Mathematical Society of the Russian Federation[20]. It was proposed to create a federal mathematical society, of a *wide society*, as it was called [30, pp. 36 – 38]. Nevertheless, that gigantic organization proved to lack vitality.

The declaration was signed by academician Vinogradov, professors and scientific workers Delone, A. V. Dyman, Kantorovich, D. K. Knol, Kulisher, E. E. Lebedev, Lochin, Lyush, Segal, I. A. Skopin, Tartakovsky, Fichtenholz.

At the end of 1931 there appeared the booklet [31] prepared by the Society of Mathematicians-Materialists. The introductory article was anonymous, but it was clearly written under the influence and perhaps with the participation of Kol'man. The title of the booklet was standard: various fronts appeared everywhere (technical, medical, cultural, economic etc. fronts).

The described situation on the *Leningrad mathematical front* included the history of the subject, determined new aims of the Mathematical Society and adduced some documents: the declaration and the draft of the statutes of the Society of Mathematicians-Materialists, the declaration of the initiative group for reorganizing the Leningrad Physical-Mathematical Society, Günther's letter to the newspaper *Leningrad University* and Bogomolov's letter to the newspaper *Za Kommunisticheskoe Vospitanie* (For the Communist Education). The new society only took into account Günther's letter in which he admitted his *mistakes*: they felt that his alleged repentance concealed that he nevertheless remained his previous self.

[7] Not much time had passed, when, on 21 February 1932, during the sitting of the Party and Komsomol fraction of the Society of Mathematicians-Materialists, those present were surprised to hear that Leifert was dismissed from all his leading positions: he had only *taken refuge in dialectical materialism, superficially criticized the position of the bourgeois mathematicians, blundered in the very interpretation of dialectical materialism* and *no work had been done apart from the appearance of the booklet [30] whose contents are meagre in the dialectical sense*. In addition, he made *leftist deviations* and *the struggle against the bourgeois-reactionary part of the professoriate was declaratory* [38].

It turned out that Leifert was dismissed by the directorate and the Party cell of the Institute of Natural Science at the Leningrad branch of the Communist Academy in which he doubled as well as in the Pedagogical Institute. The directorate also questioned the further existence of the Society of Mathematicians-Materialists. Nevertheless, during its next sitting on 1 March 1932 the fraction, while agreeing with the dismissal of Leifert, decided to continue its activity and elected E. S. Rabinovich as president.

New drafts were prepared, once more the Party principle in the history of mathematics was discussed whereas criticism was levelled at the Moscow mathematical school. However, new instructions had

apparently come, and on 19 May 1932 Rabinovich stated that the Society of Mathematicians-Materialists is merging with the *wide* mathematical society[21].

A few words about the fate of Leifert. He was transferred, or, more correctly, exiled to the university of Rostov-on Don (such measures were then usual) and Leningrad mathematicians took a long breath. We have no further documents about Leifert, only legends are left, and we quote the version of Vilenkin [1, p. 98]:

He then moved from one city to another, and, by the end of the 1930s, he came to the Voronezh University and continued to persecute scientists. Stories which reached me say that Leifert attempted to persecute the eminent geometer N. V. Efimov who was then in Voronezh. However, the night before the siting in which the case of Efimov, a son of an officer, should be heard, Leifert was arrested, and, according to all the information, shot since he was connected with Bucharin's students from the Communist Academy.

Leifert had played his part and the authorities did not need him anymore.

During the Second All-Union Congress of Mathematicians the *necessary* terminology was applied widely enough, but mathematicians, and Kolmogorov in particular, attempted to instil it with reasonable sense. Thus, collectivism in the work is indeed necessary, therefore seminars, conferences, contacts between mathematical schools of various cities etc. are needed.

Thus ends the story of the *Leningrad mathematical front*. As compared with the future events in the life of our country, it perhaps seems rather harmless. Nevertheless, how much strength and nerves had ben spent, how much creative intentions had not been realized, − nothing of that can be measured. We only have the fact: the Mathematical Society in Leningrad ceased to exist and was only reborn in 1959.

[8] To end my account, I quote Steklov [33, pp. 98 – 100]:

Reasonable men realized that even practicably it was better to obey at least tacitly the strict rules of the exact science than dare to come out against them with certainly worthless means since suitable means could not exist, if only all the human beings not lacking common sense were not slaughtered just as the Turks had not so long ago thought of slaughtering all the Armenians [living in Turkey] *since they justifiably saw that measure as the only means against separatism.*

Concerning exact science even that means is unsuitable. Mankind will not be extinguished, new human beings will appear instead of the slaughtered and at least their part will not apparently lack common sense. And the previous story will begin anew and ignorance will at last be done away with.

Many representatives of the Roman clergy unquestionably understood well enough that the doctrine of Galileo was true, and, at heart, completely sided with him. Nevertheless, they were compelled to express themselves against arguments … I will not say arguments of conscience since conscience is a loose concept, but of the mind, since it is impossible to supress its rigorous commands, if as rigorous as in mathematics.

They, those representatives, were required to act in that way by the strictest party discipline of that surprisingly harmonious organization, the party which gradually acquired absolute power and which was called the St. Catholic Church. That party, blinded by its achieved success, decided to bend to its strictest discipline even the lucid genius of pure intellect! And it came to grief. It had not realized that the free mind of a strict investigator and thinker can never be submitted to any beforehand determined and invariably fixed slogan of any party. There was not, is not, or will not be any power which can force him to obey that demand!

For supporting its most important interest the party demands the slogan: The Earth is immobile! By its power it compels the scientist to acknowledge this slogan, it threatens with torture and death, but nothing helps. Some are burned alive since they were unable to refute the opposite slogan whose truth is proved by the mind, others mock at those authorities, sidestep and deceive them like Galileo and many of his friends did, but remain their own selves since it is physiologically impossible to deny the commands of the strict and free thought. And the authorities, when demanding the impossible both physiologically and physically, sooner or later destroy their prestige.

There is no way at all of joking with the damnable science, and freedom, absolute in all respects, ought to be granted to its votaries, and the whole world should keenly listen to its genial revelations which sweep over us from century to century in spite of any conventions adopted at a given historical moment[22].

Notes

1. See also [vi-a].

2. This is a quote from the letter of Engels to A. Bebel of 24 Oct. 1891 as published in the German edition [5, pp. 24 – 26] and in a corrupted Russian translation of 1922 repeated in 1923 [6, pp. 23 – 25]. During the publication of the [translation of the] works of Marx and Engels that translation was specified: *shoot* replaced by *remove* [7, p. 356] and frighten [8, p. 163]. *Terror, shooting* and *removal* are lacking in [5]. N. E.

Here is Engels (1891/1979, p. 189):

Technical specialists … wherever possible, and we will have to use terror (Schrecken) *against them and even dupe/foul* (beschissen) *them*.

It was the state that duped and betrayed the population (and itself!). O. S.

3. See [x, Note 6] about Khinchin.

4. Ermolaeva wrote that that subsection was established in Leningrad and had a branch in Leningrad.

5. The astronomer Boris Vasilievich Numerov was shot in 1941, but was he the grandfather of S. N. Numerov?

6. A bit above Ermolaeva stated that the professors were re-elected.

7. Collectivisation in the Soviet Union meant, first and foremost, the brutally forced collectivisation of the peasantry.

8. Late in life, after escaping under a pretext from the Soviet Union, Kol'man published a repentant book (1982).

9. On *sabotage* in statistics see Sheynin (1997, § 3).

10. In 1954, a notorious statistical conference was held in Moscow (Ibidem, § 5.1). There, and even much later, several statisticians declared that mathematicians, who allegedly only understood numbers, were unable to study social events, whereas the qualitative side of life (read: Marxism) escapes them. Cf., however, Buniakovsky (1866, p. 154): an (applied) mathematician ought to understand the meaning of his numbers. True, he had not thought about Marxism.

11. Stalin was not yet named. Many Soviet authors attributed almost everything to those three (later, four) founding fathers of socialism; here, only Engels could have been mentioned. Even much later, that troglodyte, Maria Smit (1961, p. 294) referred to the *power and vitality of the Marx and Lenin economic teaching*. For a planned economy (Smit's subject), Marx was irrelevant, whereas Lenin was quite adept at destroying the population of Russia, but never invaded economics.

12. Sheer nonsense. Here, in 1959, is Anderson (Sheynin 1997, p. 538), who described a book of 1957 written by Boyarsky, a co-author of the textbook [26]:

Its readers will be unable to comprehend modern literature on mathematical national economy or econometrics.

On the agonizing introduction of econometrics in the Soviet Union see Sheynin (1997, § 6).

13. About 1933 or 1934, being a student of the so-called Anglo-American school in Moscow, I heard our teachers argue that the motive force of capitalism is competition, but, instead, it is socialist emulation (*sorevnovanie*, competition or emulation) in a planned economy. Later, that statement (not so stupid in the 1920s) was abandoned.

14. Khotimsky, a talented statistician, was shot in 1937 (Kol'man 1982, p. 132). At the time, he stated, he thought that you cannot make an omelette without breaking eggs. But it was Stalin and a great part of the duped population who needed that omelette.

15. Tokareva (2007a, pp. 113 – 117) reprinted that declaration, although from another source.

16. Uspensky himself asked to be expelled from the Soviet Academy of Sciences.

17. Ermolaeva was mistaken. Tokareva (2007a, pp. 117 – 118) reprinted the text of that greeting.

18. On Smirnov, Faddeev (see below) and Vinogradov, see the beginning of § 6, and Novikov [x].

19. That great change occurred in 1929: the *seredniaks* (peasants of average means) entered the kolkhozes, as Stalin declared. Actually, they were brutally forced to enter.

20. The Russian Soviet Federative Union Republic, one of the main (Union) republics constituting the Soviet Union. Federative, since it included a number of autonomous republics and regions. The Soviet Union was officially established in 1922.

21. The wide society *lacked vitality*, see above!

22. Instead of Steklov's optimistic *sooner or later* read *much, much later or never*, as perhaps all the later dictators convincingly proved.

Brief Information about Those Mentioned

Günther Nikolai Makarovich (1871 – 1941), mathematician, corresponding member of the USSR Academy of Sciences (1924)

Schmidt Otto Yulyevich (1891 – 1956), versatile scholar, vice-president of the USSR Academy of Sciences 1939 – 1942). Head of polar expedition on an absolutely unsuitable ship sunk in 1934. Passengers and crew saved with greatest efforts, and that result was represented as a great victory. Several explanations of that insane expedition were offered, all of them detrimental both for the regime and Schmidt.

Vygodsky Mark Yakovlevich (1898 – 1965), mathematician, co-creator of the Soviet school of the history of statistics

Vyshinsky Andrey Yanuarievich (1883 – 1954), statesman, state prosecutor, procurator general, Soviet law theorist, diplomatist, Soviet prosecutor at the Nuremberg trial, prosecutor at the show trials in Moscow. His motto: *Confession of the accused is the queen of evidence.*

I happened to read that he took his life in 1953 after being summoned to return from New York (as Soviet representative at the

United Nations) to Moscow (and doubtless to be arrested and tried in accordance with own motto).

Yanovskaya Sofia Aleksandrovna (1896 – 1966), a diehard Communist who ignorantly invaded statistics. Later distinguished mathematician (history and philosophy of mathematics, mathematical logic)

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IXa

L. V. Kantorovich

Discussion [of the report of A. V. Topchiev at the yearly conference of the Academy of Sciences]

Vestnik Akademii Nauk No. 4, vol. 29, 1959, pp. 59 – 61

I think that the means for uplifting the science of economics is its close contact with other sciences, their help. In particular, I believe that the time is ripe for convening a special session of the Academy devoted to the main problems of economic science (pricing, efficacy of capital investments, distribution of the productive forces, long-term planning, statistical economic indicators, systems of stimulation) and to the related problems of other sciences[1].

Thus, concerning *physical and mathematical sciences* we should discuss, in my opinion, such problems as the possibility of applying modern mathematical statistics in economics, various problems about the use of mathematical instruments in economics, its cybernetic aspect, economic problems of the peaceful application of atomic energy. *Concerning chemical sciences*: economic estimation of complex chemical production, methods of comparing the expenses of manufacturing natural and synthetic materials. *Concerning geological and geographical sciences*: the most effective usage of natural resources, registration of the forest rent, economically proper combination of the satisfaction of the current requirements of national economy and nature and climate conservation.

Concerning biological sciences: first and foremost, problems connected with agriculture. For example, what should be selected when developing a system of feeding farm animals: providing record milk yields or an increase in mean milk yields per cow, or just an increase of yield at minimal cost. Numerous economic problems are connected with the growing of agricultural and industrial crops, irrigating and fertilizing the soil, with medicine and physiology (physiology of labour, rational diets). And there are especially many problems of economic nature in the field of *technical sciences*. Without their study neither separate technical decisions can be made, nor the policy of developing industry can be properly determined.

I provide one figure to give an idea about the values which are connected with economic decisions: capital investments during the next seven years should amount to 2 thousand million roubles. And our entire population is interested in that each rouble be applied in the most effective way.

Economists often recognize separate shortcomings but believe that, as a whole, our economic science is not so bad. They justify that opinion by indicating that the rates of the growth of our national economy are really excessive, and had never before occurred in the history of mankind. But was the pertinent role of economists so essential? A definite part was here played by the accumulated practical experience in economics and planning, but it was not

generalized by the economic science at all. This explains, in particular, why some harmful phenomena, for example, rush work, is going on for decades.

In the 42nd year of the existence of a socialist state our economic science does not know precisely what does the law of value mean in a socialist society or how it should be applied. It does not know the meaning of socialist rent or whether in general should the efficacy of capital investments be calculated or how it ought to be done. As the latest discovery in the field of economics we are offered, for example, the proposition that *the law of value does not govern but only influences* or that *means of production are not a usual, but a special kind of commodity* etc.

We know Khrushchev's sharp but correct statements about the quality of economic reckoning. Our science is behindhand and its level is low, as it was repeatedly indicated by Party and government [a standard Soviet expression, O. S.] decisions and reflected in many statements expressed during the 21st Party congress.

That backwardness was especially clearly revealed when the problem of applying computers for planning and economic calculations has been realized. Note that in creating the linear or optimal programming, apparently the most effective mathematical method, Soviet science had forestalled the USA by a whole decade, as is also recognized abroad[2]. During the latest year or two, the branch of economic, philosophical and legal sciences had been propagandizing the application of mathematical methods in economics. However, the fear of mathematics did not disappear at all[3], and as a result we are now much behind foreign countries in the practical application of mathematical methods. Linear programming for planning crop rotation is widely applied in Iowa and North Carolina, but not in Leningrad or Ryazan oblasts [provinces] although that was possible a decade ago.

A wide application of those effective methods in economics is undoubtedly of paramount importance. They can have a part in the solution of the problems raised by the 21st Party congress and our duty consists here in achieving everything possible.

Notes

1. No such session had taken place since the preservation of the purity of Marxism was the first and foremost aim of the Party, see [ix-b].

2. In 1975, Kantorovich himself shared a Nobel prise with T. C. Koopmans for the invention of linear programming.

3. In ca. 1972, a jubilee edition of the Russian translation of *das Kapital* was published with only bibliographic comments, but the Council of Mutual Economic Assistance (1949 – 1991) of the countries of the Soviet bloc had not been working according to Marxism! Much more is contained on this subject in the next item here [ix-b], and I also say that economic planning (and life) in the Soviet Union was often hampered by sudden political decisions.

Around 1973, the Plekhanov Institute for National Economy (Moscow), where I had been working, categorically denied the attempts made in Novosibirsk to introduce mathematics into economics. The situation was not, however, so simple: at the same time, the application of mathematical methods in economics was taught in one of the faculties. Then, another circumstance should be mentioned (and instructive examples provided). It was best described by Truesdell (1981/1984, pp. 115 – 117) with a reference to another author:

Wherever money is abundant, charlatans are brought forth by spontaneous generation.

Money for introducing mathematics! Indeed, at that Plekhanov Institute an unbelievably ignorant and impudent plagiarist, who pleased the rector at least by his unquenchable antisemitism, successfully defended his doctoral thesis devoted to that same subject.

Truesdell C. (1981), *Idiot's Fugitive Essays on Science*. New York. Collected essays.

IXb

L. V. Kantorovich

On the state of economic science and its problems

Ekonomika i Matematicheskie Metody, vol. 26, No. 1, 1990, pp. 5 – 14

Introduction by Editorship

Kantorovich wrote this paper in 1962 for the journal *Kommunist* [and died in 1986, O. S.]. Before that, Campbell (1961) published a paper in which he highly praised the work of Leonid Vitalievich Kantorovich and Viktor Valentinovich Novozhilov and noted that they led to a deviation from the Marxian theory of value, to its revision. Such conclusions were noted by our ideological services [apparently, of the KGB, O. S.], and they asked Kantorovich to *dissociate himself publicly* [from that conclusion, O. S.]. Moreover, Campbell's statement agreed with the opinion of such eminent figures as A. Ya. Boyarsky, A. I. Katz, K. V. Ostrovityanov[1] a. o. mentioned by Kantorovich.

However, Campbell based himself on his bad knowledge of Marx, whereas our economists misunderstood the essence of the investigations of Kantorovich and Novozhilov. Anyway, their conclusions were mainly occasioned by the social [by the Party, O. S.] demand, by the care for t*he purity of Marxism*.

In his manuscript, Kantorovich devoted his main attention to analysing the criticisms of his Soviet opponents and only slightly touched Campbell. He thus did not fulfil the formulated demand, and the publication of his manuscript was not allowed. Publishing it now, the editorship believes that it will be useful in both aspects represented in its title. We cannot expect that a manuscript written almost 30 years ago meets all the actual aims of economic science. Such terms as *equilibrium, inflation, monopoly, market,* were only *allowed* when analysing capitalist economies. It was thought that no pertinent phenomena exist here.

However, the main points of the paper are the notion of equilibrium (although that word is absent) and its relation with problems of cost, economic estimations and norms. An essential part in editing Kantorovich's manuscript is due to A. L. Weinstein, and V. L. Kantorovich compiled the Notes.

The Author's Text

[1] Perhaps no other science so directly and immediately influences all the activities of the state, from its organs down to workshops, kolkhozes and shops, as political economy [economics] does. Thus, the principles of economic analysis influence the quality of such important decisions as: what and where to produce; from where to bring in, and where to bring it; how to find the necessary means; where to direct capital investments; what enterprises to build; which consumer goods to manufacture and how to price them; how to

estimate the work of enterprises and state farms; how to award bonuses for that work, how to pay for the work etc.

The correctness of the theoretical basis is obviously reflected in the quality of economic reckoning which in turn essentially influences the results of practical decisions: whether the required products, and how much of them, will be obtained; how and to what extent will the work of the different elements of the economic organs be conformed; whether to expect profit or losses; how high will the living standard, and the rate of the increase in economics be.

For example, it is difficult to establish all the negative influence of the concept, until recently current among economists, that in a socialist economy *demand ought to invariably outstrip supply*, that that is characteristic of the very essence of a socialist society and is its essential advantage (crises of overproduction are lacking). Only a bit exaggerating, we may say that, consequently, queues and interruptions of supply are unavoidable, are characteristic of the essence of a socialist economy, there is nothing bad in them; on the contrary, the longer the queue, the more clearly and strikingly we see the advantages of socialism. Happily, our economic organs mostly ignored that concept, but it certainly had a definite harmful and demobilizing influence. Well, yes, there are queues and interruptions, but they are *characteristic*, so how can we complain? We ought to reconcile ourselves to them. Why study which commodities does the population need? Since demand outstrips supply, all will be bought whatever manufactured. And thus occurs overstocking.

Ostrovityanov [1, p. 118] recently wrote that it was necessary to wait for the explanation offered in 1961 by Khrushchev[2] for understanding that the absence of queues is better than their existence. Only a caste mutual cover-up, only an absolute indifference to the interests of the people can explain the tranquillity of discussing that ruinous concept.

Was it really proper, in 1960, to nominate and elect L. M. Gatovsky corresponding member of the Academy of Sciences because of such *theories* of Soviet trade in spite of numerous votes against him cast by physicists and mathematicians[3]?

[2] Until now, 45 years since October [of 1917], we have not solved the most essential problems of the economic theory of socialism: what are the principles of valuation; how to reckon demand, assign prices and establish tariffs; to determine correctly the efficiency of capital investments; how better to strike balances, etc. This situation is explained by the generally known protracted and abysmal backwardness of our official economic science concentrated in such places as the academic Institute of Economics and the periodical *Voprosy Ekonomiki*.

To a large extent this situation is connected with the period of the cult of personality [polite expression of Stalin's despotism, O. S.], but it is tragic that in the discussed field its consequences are not yet surmounted. This situation should be explained by the fact that the scientists who had created and supported that cult in the economic science are still holding leading positions and continue to destroy each vivid and bright thought.

In no field of knowledge did the cult of personality manifest itself as strongly and perniciously as here. The best, ablest and creatively independent personnel were discarded. A dogmatic and doctrinal style of work has been firmly rooted in economics. The minds of many of its representatives were directed not towards the search for scientifically prepared solutions beneficial for the people, but aimed at *obliging*, at *hitting the bullseye*. Tailing is also characteristic: instead of contributing to the choice of best economic solutions, economists had restricted their efforts to commenting and unrestrainedly praising already adopted solutions. It would have been better to offer useful corrections and means for their accomplishment.

That situation is intact even now. The only *bold* statement by Gatovsky which I heard during the last years criticised the construction of the Bratsk [city in the Irkusk province, O. S.] hydroelectric power station but it was expressed exactly a week after Khrushchev's similar statement [2].

It is hardly surprising that, when the programme of the work of the committee on value was determined, the study of the problem of pricing was opposed since the government allegedly had no intention of revising the prices. I had to mention Tsiolkovsky who had begun to occupy himself with interplanetary rockets long before the decision to launch a satellite was adopted.

And when, soon afterwards, in June 1960, a plenary session of the Central Committee of the Party charged (!) the Academy of Sciences, along with other institutions, to offer proposals for pricing, the Institute of Economics was unable to suggest anything since they had no scientific background to work from. This *weathercock* method of scientific investigations led to constant vacillations of that *official* science about most principled problems. For example, is the law of value valid under socialism or not? Or, as Ostrovityanov once *discovered, it does not govern but influences*? Does our machinery become obsolescent or not? Is it admissible to take into account the borrowing (zadalzhivanie) of capital investments when calculating their efficiency? Far-fetched occasions happened when the same person defended a certain proposition in his candidate dissertation and the opposite in his doctor's dissertation.

An atmosphere of *petty* local cult of personality was created and is still maintained in the economic science. A tight group of its participants (Ostrovityanov, Gatovsky, Ya. A. Kronrod, K. N. Plotnikov, G. M. Sorokin, I. D. Laptev), barely competent and creatively helpless, is attempting to monopolize all the development of the economic science by holding in their hands such key positions as the Institute of Economics, the journal *Voprosy Ekonomiki*, the editing of the textbook on economics.

This group appropriated the business of *the ambassador plenipotentiary of Marx over the whole world*, of the right to say as an oracle, even without opening a book, whether it is Marxist, or anti-Marxist. Whatever it, that group, prophesied, became at once gospel truth, and, although during three years it could have offered three mutually exclusive opinions, each of them, one after the other, was *gospel truth* not subject either to discussion or criticism.

The differently minded are defamed, labels are pinned on them with all the ensuing consequences, they are banned from publishing their works which are consciously misinterpreted, discredited, distorted and/or ignored. Criticism is suppressed and persecuted.

It is difficult to recall painlessly how, in 1949 – 1950, *Voprosy Ekonomiki* expanded a campaign *exposing* the adherents of calculating the efficiency of capital investments, both theoreticians and workers of planning establishments. As a result, Professor Novozhilov, undoubtedly the most eminent Soviet scholar in the field of economics, had to leave his position in the Polytechnic Institute. Even in 1939 he profoundly justified the need to introduce the norm of efficiency in the socialist economy and formulated the principles of its calculation, and in 1940, the first among the economists, appreciated the significance of linear programming just discovered in the Soviet Union [by Kantorovich, O. S.] and was the first to apply it. A. L. Lourie and other serious scientists had suffered as well.

Not only separate people had suffered; the denial of the need to take into account the norm of the efficiency of capital investment put to sleep large means in unnecessary or ineffective construction which dragged on for many years, damaged the country to the tune of hundreds of thousands of millions roubles and essentially influenced the rate of our growth.

[3] Surprisingly, even now, after the developments achieved by the State Scientific and Technical Committee[4] and the 1958 conference on the efficiency of capital investment, when the Institute of Economics had finally officially recognized the need to take into account the factor of time and the period of recoupment and issued a pertinent methodical instruction, Novozhilov was not mentioned there even in its Introduction. At the same time, however, the theoretical part of that instruction was largely based on Novozhilov's results (although it applied them far from fully). And in spite of their active participation in the discussions neither mentioned were many other speakers at that conference.

Confusion in theoretical problems had reflected on economics, and we will even say, on the entire situation in agriculture: many valuable pertinent decisions were not realized owing to the lack of proper economic stimuli. There previously existed a theory according to which in a socialist society no rent existed at all. Then rent was recognized, but there appeared as many opinions about its essence as there were economists who wrote about it, − even more since some authors provided several different opinions each. Actually, rent is formally recognized but the real scientifically justified principles of its calculation are not applied. At the same time, such calculations, when introducing economic accounting (khozraschyot) or estimating the cost of free, and when establishing compulsory (sdatochnye) purchases, or a system of taxes, would have allowed to get rid of many difficulties and shortcomings. Had the conditions in various natural zones been economically adjusted, and therefore the principle of *equal pay for equal work* ensured, the intensive use of the best earth zone would have been properly stimulated …

When studying problems such as the cost price of agricultural produce, our economists of the Laptev ilk fell as though between two stools. Those authors thought that that problem was hardly soluble since the same produce was partly a commodity and partly consumed in agriculture itself. Here, ignorance of mathematics had hindered those economists: this *insoluble* problem is easily solved by linear algebraic equations. Theoretical mistakes led to an incorrect approach of the role of machine and tractor stations[5] and wrong recommendations about pecuniary stimulation. Had there been no such mistakes, the government would have probably no need to resort to a forced temporary increase in the cost of meat and butter.

The lovers of *labelling* are not yet extinct. It is difficult to characterize Kronrod's [3] escapade otherwise than *hooliganism*: all of a sudden, without any justification, he (p. 50) called that same Novozhilov, a Honoured Science Worker of the Russian Federation, an ideologist of the nepman[6] bourgeoisie. Papers [4 – 7] are full of labels, and Ostrovityanov [1] pays much tribute to labels as well. Any idea stepping out of frame of the concepts of that group was received hostilely and its author was defamed.

[4] That same group firmly controlled the editorship of many journals, of the publishing house of the Academy of Sciences and many other [state owned] publishers which allowed it to check the appearance of the contributions of differently minded authors and sometimes to delay their publications by many years. Those scientists even allow themselves to attack economic publishers who dared to publish objectionable authors. Thus, Ostrovityanov [1] pounced upon the editorship of the *Ekonomicheskaya Gazeta* with reproaches for publishing an article by one of our most eminent and respected mathematician, academician Sobolev, about the application of mathematical methods and their importance for economics.

The same group prepared the elections to the Academy of Sciences and carried them out by secret methods and makes use of them for further strengthening its position. The elections of 1960 were characteristic in this respect. Violating democratic principles, the candidates were considered by the branch [of economists et al] without any discussions, and after objections were raised during the general meeting of the Academy, Ostrovityanov, the president of the commission of experts and vice-president of the Academy [in 1953 – 1962, O. S.], deceived it. Only under such circumstances it became possible to blackball Novozhilov and elect Gatovsky. Incidentally, at the same time the inmost recesses of the hearts of some members of the Academy were slightly opened: although most economists declared themselves advocates of mathematical methods in economics, Novozhilov, the most outstanding specialist in this field, got no positive votes at all!

After 1953 [after the death of Stalin, O. S.] the atmosphere in the economical science somewhat cleared up: publication of special literature increased, problems began to be discussed more extensively and freely, definite advances were made in concrete economics. However, the liquidation of the consequences of the personality cult

went, and is still going on extremely slowly. One of the causes of that are the monopolistic aspirations of that group.

The low level of that science also tells very negatively on the education of economists and the economic training of other specialists. This situation certainly incessantly troubles the Party and government as well as the public, practitioners of economics and scientists working in other fields since it cannot fail to be reflected in the real economic and planning work: such a theory bewilders and disarms practice.

[5] Under such conditions it was certainly difficult to expect that the upsurge of the economic science, the surmounting of its backwardness can be achieved by the internal forces of that same group which ensured the backwardness. It lacked either the necessary force or interest. Creating the idea of the insolubility or minor interest of the economic problems of socialism, it raised propositions about the insolubility of the *quantitative aspect of value*, about *the atrophy of value under socialism*. All this was similar to the introduction, often by the same people, of the harmful idea about the atrophy of statistics in the socialist economy which considerably harmed the country and the statistical science[7].

Since economic science gave nothing, specialists in power engineering, builders, geologists, chemists et al began to occupy themselves with economics. Many [specialists] became convinced that an upsurge of the economic science can only be ensured by the participation of scientists from other fields, mathematicians, technically qualified people, cyberneticists. Since economic problems were important for the entire life of the society, proposals for convening a special general meeting of the Academy of Sciences devoted to the main economic problems of socialism were made[8]. However, they met with a most bitter resistance of that group and sabotage, to say it directly.

In July 1960, in connection with the ensued discussion of the proposals of academicians A. I. Berg, Sobolev, A. N. Kolmogorov et al by the Presidium of the Academy, the branch of economic, philosophical and legal sciences established a commission under the chairmanship of that same Ostrovityanov which did not convene even once during two years!

Because of the obstruction by the same group of economists and the hesitation of the chairman the commission on the calculation of values established in 1959 in accordance with the decision of the Presidium was unable to conclude its work although the government and the planning organs vainly awaited its results.

For two years the decision of the Presidium about reinforcing the editorship of the main economic journals and co-opting mathematicians, specialists in mathematical economics into them, was not realized[9].

It is well known that, along with engineers, mathematicians and mechanicians (academicians M. V. Keldysh, M. A. Lavrentiev, S. A. Khristianovich et al) had played an essential and often even a leading part in the creation of our aviation whose quality was decisive in the Great Patriotic War [of 1941 – 1945, O. S.]. It had never crossed the

mind of anyone to say that the engineers ought to manage themselves without mathematicians even if our planes be twice worse and appear a few years later, but similar statements had been repeatedly made about economics …

It is difficult to believe that after the Soviet man was able to photograph the far side of the Moon some people still declare that it is impossible to calculate the cost price of producing a tonne of grain or an axe. It is outrageous to hear such statements 15 or 20 years after mathematics has enriched itself by such new and extremely effective scientific disciplines as linear and dynamic programming specially adapted for solving most complicated planning and economic problems, and became armed by modern electronic equipment.

[6] The negative attitude of that group to mathematics, a conscious reluctance to understand those immense possibilities and perspectives, which are opened up for its application in economics, essentially influenced the development and application of mathematical methods in that field. At the same time, however, the socialist society is arranging its economics on a scientific planned basis. In principle, this allows us to make planning and economic decisions which ensure the most effective use of resources, best results, optimal solutions.

Exactly for this reason, even before the war, socialist planning had first encountered difficulties when determining optimal solutions of complicated planning and economic problems (to determine the maximal effect with given resources or a fixed effect with minimal resources).

It occurred that classical mathematics was not suited for the case of complicated problems of determining optimums. An analysis of economic problems required special mathematical instruments and principally new methods. They were largely created even in 1939 in the Mathematical Institute of the Leningrad University. Effective algorithms for solving planning and economic problems with the allowance for dozens and hundreds of various factors and numerous and various restricting conditions were developed. The efficacy of these methods increased still more with the appearance of computers.

In applying mathematics to economics, the Soviet Union had outstripped other countries by a decade. In the USA those methods had been discovered anew in the end of the 1940s in connection with planning the work of separate firms and military economic planning and called linear programming. However, after becoming acquainted with the pioneer Soviet work, Western science completely acknowledged Soviet priority in this field of knowledge.

At present, linear programming abroad is one of the most rapidly developing branches of applied mathematics. Dozens of books are published, numerous conferences organized and only in the USA four or five scientific periodicals are almost entirely devoted to this subject. Linear programming is widely used in practice, in industry, transportation and agriculture, mostly in enterprises and firms. Cows grow thin if their ration is compiled without applying the methods of linear programming[10], and those farmers who ignore them soon go bust. In the branches [of industry] monopolized by the state and in

military planning these methods are also applied to some extent and they are widely used in the poorly developed countries.

Methods of linear programming and, in particular, the pertinent Soviet works, are widely known and applied in Hungary, Czechoslovakia, Poland, China, Vietnam and other socialist countries. Scientists and planning workers often come to us for consultation. In the 1940s, the theory of linear programming had been rather successfully developing in the USSR[11]. New results were obtained, and first experimental applications took place in 1949 – 1951. However, the atmosphere of the personality cult and the prejudice of the leading economists against mathematics occasioned by ignorance as well as the application of mathematics abroad coupled by a superficial analysis of their work extremely hampered and slowed down the development and introduction of such methods into practice.

[7] Let us mention some of the most essential directions in this subject along which the work of the mathematical economic department of the Institute of Mathematics which the CO[12] of the Academy of Science is developing.

1. Even in 1949 – 1951 a rational cut of materials calculated by the methods of linear programming was successfully realised by several plants and the gained experience was generalized [8]. That experience is essentially important for machine-building and metallurgy, but it had not yet been widely introduced.

2. Rational transportation by the methods of linear programming is now being calculated, although not everywhere, in all kinds of that activity (by railways, air, sea, road, and within plants) as well as when distributing branches of industry, for example the cement and fuel branches. These methods are described in textbooks and even in agitation and propaganda materials.

3. Industrial enterprises are introducing optimal planning directed at the most intensive and uniform use of machinery and the experience of several plants is being analysed.

4. Investigations carried out at the Institute of Mathematics of the CO of the Academy of Sciences (under my guidance) and in other institutions revealed the exceptional efficiency of the methods of linear programming for economically analysing the most rational set of tractors and agricultural machines, determining a rational structure of the branches of agriculture, distributing areas under crops, selecting the most rational forage etc. It ought to be mentioned that the establishment of optimal decisions at the same time allows the appointment of the related objectively determined valuations of various kinds of production and industrial factors. They can be applied for a scientifically justified calculation of free prices, indications of rent etc. A wide application of these investigations can play an essential part in the problem of the decisive upsurge of the agricultural production as raised by the Party.

5. In fulfilling the task ordered by the State Economic Council, a dynamic pattern of the fuel and energy balance of the country, that is, an optimal pattern of the development of the fuel branches and of the production of energy, is being worked out.

6. Rational and scientifically justified tariffs for various kinds of transportation are being developed. Their introduction will increase the profitability of the transport enterprises and improve their service. A new tariff for the taxi service was introduced on January 1, 1961, in all Union Republics. It essentially increased the revenue and lowered the cost for the population owing to a sharp improvement of the work of the taxis (decrease of the idle time and free-running). Actual and forecasted indications have almost coincided which testifies that economic calculations become an exact science.

The application of the same methods in other kinds of transportation and some services can increase the revenue by about 5 thousand million roubles annually and at the same time lower the tariffs and improve the service for the population. Regrettably, the *interested* establishments are not duly *interested*. Thus, a whole year long the Ministry of Finance did not find time to consider the pertinent memorandum compiled by academician Sobolev and me.

Finally, the development of the methods of scientifically justified planning of the training of scientific personnel had begun in response to the assignment of the State Economic Council and the Committee on Coordination. This as also some other investigations are carried out in cooperation with the recently established group for applying mathematical methods in economics[13] at the Institute of Economics at the CO of the Academy of Sciences. Contrary to the widespread myths about the *unsociability* of mathematicians, such working collaboration is being successfully adjusted with other collectives of economists as well.

During the investigation of the apparatus of linear programming it became possible to develop the principles of its application in the planning of the national economy [9 – 10]. The principles of compiling an optimal plan for the future of the separate branches of the national economy and of the entire national economy were formulated. The detailed development of the necessary methods and of their realisation will certainly require time and collective efforts of scientists and practical workers of various specialities, economists, mathematicians, statisticians, planners.

The offered methods of solving multipliers (of objectively determined valuations) can serve as a model for automatically regulating the work of enterprises on the basis of scientifically justified costs of their production and for improving the introduction of economic accounting (khozraschyot).

These methods can be applied for planning the work not only of separate industrial units (plants, workshops), where they had entirely justified themselves, but of separate branches of natural economy and, after some modification, of the economy as a whole. Certain success was achieved in the organisation of the training of the personnel for a *mixed* mathematical and economic speciality.

[8] At the economic faculty of the Leningrad University, where such work has begun for the first time, four pertinent courses (?) were recently prepared. In 1959 – 1960 a very successful experience of yearly courses for economists, graduates of the University, who will study general mathematical and special disciplines of mathematical

economics, was obtained. Next year the training of specialists in mathematical economics will begin at the mechanical and mathematical and the economic faculties of the young Novosibirsk University. Specialisation in mathematical economics is introduced at Moscow University and in a number of engineering economic institutions of higher education across the country.

And still, in spite of the exceptional possibility of applying mathematical methods in a planned socialist economy and of numerous direct indications of the leading Party and Soviet organs about the need to apply them, the rate of their introduction is yet extremely insufficient. Apart from some inertia of the economic organs, during several years an essential negative role was played by the campaign against mathematical economic methods in general and especially against my contributions by the editorship of the journal *Voprosy Ekonomiki* and Ostrovityanov [1, Chapter 13], who had recently directly joined it.

The debate is going on according to the worst traditions of slating practised during the period of personality cult: unjustified assignment of absurd and reactionary views (like *he is propagandizing backward techniques*), pinning on of labels of revisionism [of Marxism] and anti-Marxism, falsification of facts and obvious misrepresentation. Any scientific worker, for whom each economist-mathematician is just a bourgeois economist, repeats economists who lead in running down mathematics and supports himself by their authority without bothering about any serious justification. Indeed, *leading economists*, Boyarsky et al, see, for example, [11], declared that I am a bourgeois theoretician.

It is known that in spite of a few attempts to prevent it, the first all-union conference on the application of mathematical methods in economic investigations and planning took place in April 1960. According to general opinion, it was a real triumph of mathematical methods, and, in particular, of the concepts shared by Novozhilov and me. Nevertheless, the journal *Voprosy Ekonomiki*[14] represented the results of the conference as a condemnation of those concepts.

The first volume of the proceedings of that conference was recently published [14]. Apart from the reports and my and Novozhilov's concluding remarks, it included the convincing answers to our opponents contained in the reports of academician Kolmogorov, corresponding member of the Academy Markov [Junior], professors A. A. Lyapunov and N. A. Shanin et al.

In connection with the application of mathematical methods in economics and in particular, of the methods of optimal planning, a number of new economic indicators certainly occur: objectively determined valuations, estimation of let-out equipment, rent, norm of efficiency, etc. One of the subjects of future investigations should be the ascertaining of the economic meaning and contents of those indicators, their role and place in the Marxist theory of value and the methods of their calculation.

[9] We ought to point out that the most acute foreign mathematicians-economists recognize that the application of mathematical methods in economic analyses can only be the most

effective in a planned economy which lacks spontaneity. Among those scientists is, for example, an eminent English economist Allen (1956), the author of the course in mathematical economics which was translated into many languages (a Russian translation is being prepared). At the same time, a number of foreign authors attempt to represent the appearance and application of mathematical methods in economics as a departure from the Marxist concepts and theory.

Among them is Campbell who had recently published a paper [12] under an affected title. He mentions the great importance of Soviet contributions on econometrics for the entire science (?) and unconditionally recognizes the priority of Soviet scholars in the discovery of linear programming. Briefly describing the history of the development of mathematical economic methods in the USSR, Campbell then indicates that the level of the polemic articles directed against Kantorovich and Novozhilov in *Voprosy Ekonomiki* is low. For example, the obstinacy of Boyarsky who repeated the same arguments, a convincing answer to which had been provided long ago, is, according to his opinion, *almost schizophrenic*.

Actually, our concept is just the application of the Marxist methods to the economy of a socialist society under the complicated conditions of modern manufacturing. The categories and concepts of political economy are of a historical nature, they cannot be mechanically transferred from the Marxist theory of a capitalist society; they ought to be specially developed in conformity with a socialist economy. In particular, this concerns the notion of value. Capitalist value is based on the expenses incurred in a separate enterprise, but for the socialist manufacturing, social in the direct sense, the expenses should be calculated as the expenses of the entire society, and our modern director ought to take into account the entire expenses, i. e., the influence of his actions and decisions on the expenses of other enterprises of the same society, to *the expenses of the feedback*, in the Novozhilov terminology[15]. Only thus we can achieve an optimum for the society as a whole rather than a local optimum.

Just the same, according to our concepts, the categories of rent, norm of efficiency, in all outward appearances similar to the norm of profit, are existing in a socialist society without opposing in any way the Marxist labour value theory. We consider labour as the only source of value, whereas rent and profit (the let-out estimates), as parts of the product of social labour whose distinct separation is necessary for a rational use of resources.

[10] I summarise. The works of Soviet economists-mathematicians lack any revision of Marxism; such a revision is invented by our *Marx' prophets* as a convenient slogan in the battle for strengthening their monopolistic position, for the preservation of caste and dogmatism in the economic science whose destructive nature chokes all progressive ideas and hinders the development of the national economy. The problem is only the need to revise Ostrovityanov (and his gang), but this is necessary, and we have the right to accomplish it.

Notes

V. L. K. = V. L. Kantorovich

1. Kantorovich has much to say about Ostrovityanov. In addition, I (1998, p. 540) note Ostrovityanov's menacing and ignorant statement of 1954: the same methods cannot be applied in economics and stellar statistics.

2. In 1961, at a plenary session of the Central Committee of the Party, Khrushchev stated that in a socialist economy supply ought to exceed demand. V. L. K.

3. At the time, Gatovsky headed the Institute of Economics and the journal *Voprosy Ekonomiki*. Ostrovityanov, as vice-president of the Academy responsible for social sciences, ensured his election. V. L. K.

4. Later, that Committee became a state organ. V. L. K.

5. That incorrect *approach* was perhaps occasioned by the complicated relations between those state stations and the kolkhozes which did not belong to the state (but had been mercilessly exploited by it).

6. *Nepman*, a word derived from *NEP*, new economic policy (1921 – second half of the 1920s), introduction of goods/money relations (which was hardly reflected in law). Connotation: neuveau riche, upstart.

7. See Sheynin (1998, pp. 535 – 539).

8. See [ix-b, Note 1].

9. How then did the Presidium work at all? In 1944 – 1945, F. N. Krasovsky, the leading Soviet geodesist and corresponding member of the Academy, experienced procrastination and reluctance to consider carefully his important proposal to establish a commission on theoretical geodesy (archival letter, see Sheynin 2012, pp. 90 – 92).

10. Cows only grew thin if insufficiently fed.

11. See Note 2.

12. The abbreviation OC is also mentioned below. I do not understand it.

13. That group was headed by A. G. Aganbegyan [a corresponding member of the Academy]. V. L. K.

14. Cf. that report with other relevant information in the *Uspekhi Matematich. Nauk* and the proceedings and the shorthand account of the conference. V. L. K.

That periodical is being translated as *Russ. Math. Surveys*, but perhaps since a bit later. Here are a few lines from another source (Gerchuk & Minz 1961).

Kantorovich argued that new methods of planning, new economic and statistical indicators, and results in economics, statistics and mathematics were required. Kolmogorov, who participated in the discussion, stated (see p. 254) that the joint work of economists and mathematicians should lead to *an essentially new stage in the development of the economic theory itself*. In another source Kolmogorov (Sheynin 1998, p. 542) even declared that

The main difficult but necessary aim is to express the desired optimal state of affairs in the national economy by a single indicator.

That indicator could have only been expressed in money, so that Kolmogorov indirectly denied Marxism. Note also that both he and Kantorovich attempted to avoid any mention of econometrics whose introduction in the Soviet Union had been extremely difficult (Sheynin 1998, p. 542) and that Kantorovich barely mentioned cybernetics (at the beginning of § 5).

I have not seen the proceedings of that conference (Obschie 1961).

15. Issuing from this viewpoint, O. K. Antonov, the eminent aircraft designer and a talented amateur economist, published an unusually clear and critical analysis of the existing economic indicators [13]. A lively discussion followed. V. L. K.

Brief Information about Those Mentioned

Liapunov Aleksei Andreevich, see [x].

Novozhilov Viktor Valentinovich (1892 – 1970), economist, Honoured Scientific Worker of the Russian Federation, Lenin Prize winner (1965) together with academician Nemchinov and Kantorovich, full academician since 1964

Ostrovityanov Konstantin Vasilievich (1892 – 1969), reactionary economist, vice-president of the Academy of Sciences (1953 – 1962)

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IXc

L. B. Sheynin

A cunning ideology and economic terminology

Nohow [know-how] *Bisnes*, No. 9, 2005

While denouncing capital as the symbol of the oppression of the working people (naturally, of those living abroad), Soviet leaders did not intend to reject it at home as one of the elements of the foundation of economic development. Actually, they highly appreciated capital as the means and instrument of production, as a natural resource and as currency, both foreign or national. Respect for capital sometimes broke out in their statements, and especially when it should have been understood in a sense wider than the Marxian. Generally known became, for example, the fatherly reprimand received by the pilot Chkalov from Stalin: his life is a more precious capital than the new plane which he is testing (the *most* precious capital).

Nevertheless, the word capital had been essentially removed from the Soviet everyday life. The terms *fixed* and *floating capital* were replaced by fixed and floating means. Just the same, ownership and loaned capital became ownership and loaned means. True, specialists retained the term capital investment and capital capacity (of manufacturing) but without any hints at the mutual relations between labour and capital.

Such important economic and financial terms as payments on account of capital, capitalization of the (yearly) income, increment of capital and its taxing, current costs (in some systems of accounting they oppose the so-called operational costs) had disappeared. This fallout impoverished the professional language, impeded and is impeding analyses of the economic activities of enterprises and entire branches of the national economy.

Funds. This obscure word was sometimes substituted for *capital*. Thus, statute capital of an enterprise became its statute asset. In 1965, when payment for the use of the provided production capital became required from state enterprises (until the 1990s), its official name was *charge on funds*. The Soviet leadership had no liking for the word *profit* either, whether used separately or in some set expression. *Rentabelnost* (actually, the same as profitability, and certainly derived from the French *rentabilité*) had appeared instead of profit per rouble of expenses, and *rentabelnost of the funds* instead of profit from capital. Attempts were made to replace *profit* by *socialist accumulation* and for some time these two terms competed with each other, then profit won.

However, the replacement of profit by *socialist accumulation* was not a simple play upon words. Profit is a *secondary* economic category, the result of economic activity, a derived magnitude. As to *socialist accumulation*, this indicator ought to represent the will (the plans) of the country leadership about the sum of the means which it considers necessary to mobilize for fulfilling its economic or other projects. In this sense, *socialist accumulation* is an *initial* indicator of

a financial rather than economic nature. Its magnitude should not necessarily coincide with *economic* profit.

The leadership may therefore require the state enterprises to supply the budget with such amounts of money as demanded for financing new constructions (or other projects) but not necessarily corresponding to economic reality. The government is then compelling the enterprises to increase the prices of their produce and to decrease their expenses in the wrong way (to decrease the wage-rates, refuse to purify the harmful industrial waste, to worsen labour protection). The profit can only thus be put in correspondence with the volitional requirements of the government. And so, the attempt to replace profit by the politically more acceptable *socialist accumulation* was not a harmless substitution of terms.

True, the government financed its investment projects not only at the expense of the profit of the state industry. An important source was the income of the kolkhozes and sovkhozes (state farms), as well as the issue of fresh money into circulation, see tables of the Soviet money circulation (Kronrod 1960; Weinstein 1972).

Another essential source of the state revenue was based on the lowering of the price of the produce of the kolkhozes and sovkhozes provided as state deliveries. This source had already appeared in 1928 in the form of the so-called Ural-Siberian method of state grain purchase. The price assigned for the grain of the (individual) farmers was that which had existed formerly, before inflation discredited it. Undervalued prices had been systematically assigned to the kolkhozes since their origin, and the compulsory delivery of the kolkhoz produce was only abolished in 1993.

In the beginning of the 1960s economists had been actively discussing the proposal of Prof. Liberman from Kharkov to make *profit* the main planned and accountable indicator of the work of government enterprises. (Such proposals had possibly been repeatedly made previously as well, but nothing is known about them.) The Liberman initiative was suppressed, apparently since it was politically unacceptable.

In the field of the *investment of capital* the cunning substitution of terms had led, and is leading to confusion of notions possibly hampering proper economic decisions. This concerns the term *payments on account of capital*. Anonymous dodgers fishing in the area of economic terminology replaced it by the generally known *payback time*, the number of years after which the total profit (as stipulated by the project) ought to reach the initial investments. Payback time seems to be the reciprocal of profit. Thus, if the yearly profit on the invested capital is 20%, the payback time is 5 years.

Payback time ousted the notion of *yearly profit*, but this change is fraught with mistaken estimation of investment projects. Suppose that a farm buys a tractor (and the necessary accessories) and that its work ensures the farmer an additional yearly profit of 10% on the invested capital. The stipulated period of its work is only 8 years so that only 80% of its cost will be returned under the guise of additional profit. It follows that the purchase of the tractor did not cover its cost.

If assuming that the tractor covers its cost when the total additional profit becomes equal to it, then its purchase should be called mistaken. Actually, however, its cost is returned not by the profit but by the accumulated depreciation. Each year 12.5% of the cost of the tractor should be allotted for its complete restoration. Then, after 8 years, a necessary sum for its replacement will be accumulated. As to the additional profit, it does not participate in the *return* of its cost. It is therefore wrong on principle to discuss payback time by issuing from the accumulated profit.

According to general notions, taxation in the USSR should have essentially differed from taxation in capitalist countries. Taxes should have been levied on industrial enterprises rather than on working people whom they as though should not concern. Income tax was indeed low; in his time, Khrushchev even promised to abolish it. There should not have been indirect taxes either, i. e., taxes on commodities, the excises, as they are called in the financial theory. Actually, however, buyers did pay indirect taxes (for which a terminological disguise was invented).

Over decades, the programmes of many socialist parties, the RSDRP (Russian Social-Democrat Worker Party) included, rejected commodity taxes. It was assumed that they did not conform to the problem of a progressive taxation of the rich. True, during the NEP (New Economic Policy, 1921 – 1929) period this programme idea was abandoned and excises introduced. Then, in the beginning of the 1930s, when NEP was abolished, excises formally, only in theory, disappeared. Excises did exist, and not only on vodka. They concerned many manufactured goods and sometimes even foodstuffs as well. The word excise had however disappeared from financial practice. It was banned from textbooks and special literature to say nothing about the media. Instead, an obscure turnover tax came into existence.

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X

S. P. Novikov

Mathematicians and physicists of the [Soviet] Academy of the 1960s – 1980s

Voprosy Istorii Estestvoznania i Techniki, No. 4, 1995, pp. 55 – 65

Introduction by the Editorial Staff

We are offering our readers the reminiscences of Academician Sergei Petrovich Novikov which he wrote down in 1991 in the style of *Oral History* about some past events of our academic elite and which, strictly speaking, are not yet history. However, during the latest years events in our country have become impetuous, and, when reading his emotional story, you feel that he is describing developments of very remote times. They are still memorable, and many dramatis personae are still living, thank goodness, but they, the developments, seem to have happened in some other life, and commentaries are already needed for younger readers.

Any reminiscences are subjective, and only their author is responsible for their accuracy, and even more so, for his opinions. We stress this circumstance only because we cannot agree with many of his appraisals. However we consider the role of I. M. Vinogradov in the life of our mathematical community, the significance of his mathematical achievements should not be underestimated. We also think that it is impossible to agree unreservedly with his utterly negative appraisal of the moral climate reigning among mathematicians of that period. Rendering due respect to the civil virtue of the leading physicists and recognizing the justice of Novikov's reproaches upon some leaders of the mathematical community of that period, we should not forget that among those who determined the social face of our national mathematics there were such morally good and active personalities as I. G. Petrovsky and V. I. Smirnov[1]. After all, the *Letter of 99* mentioned by Novikov was nevertheless born in the mathematical milieu.

We have provided a few typical examples, but many more could have been cited. The study of the social history of our national mathematics has only begun and the problem about the evolution of the social atmosphere in the Soviet mathematical community seems to belong to central issues. The roots of many events which occurred in the mathematical life after the war [after 1945] should be looked for in the circumstances of the previous *dark* period, the end of the 1920s and the 1930s, marked by the battles of the *Leningrad mathematical front*, persecution of D. F. Egorov and the *Case of Academician N. N. Luzin*[2].

All these events are not so remote, but for their proper historical understanding there are insufficient documents (they had mostly been deliberately destroyed) or testimonies of directly involved participants. They very seldom thought about writing anything down. L. A. Luisternik [1965] lamented that he was slow to realize that

Markov's report made on the occasion of the centenary of Chebyshev's birth which he had listened to with Luzin and which contained most interesting reminiscences about the life and work of that great mathematician should have been written down. No one did, and information most valuable for the history of science was lost[3].

Lost, or are disappearing many such most important testimonies to the history of our culture, and we ought to be especially thankful to our distinguished mathematician who expended efforts and time for writing down his reminiscences about the events, whose participant he was, and submitted them to our journal.

The Author's Text

1. Family Reminiscences

The Leontovich family has been known to me all my life. My parents, Petr Sergeevich Novikov, and Luidmila Vsevolodovna Keldysh, together with Michail Aleksandrovich Leontovich (Min'ka) had been students of the same year at the physical and mathematical faculty of the Moscow State University. They formed a company which lasted all their lives. It also included a number of [later] eminent physicists, students of Leonid Isaacovich Mandelstam: Aleksandr Aleksandrovich Andronov (Shurka) who was married to Leontovich's sister, the Landsberg family, Tamm, a Nobel prize winner and teacher of Sacharov (they called him Igor Evgenievich since he was a few years older), the family of the astronomers Pariysky, whose head, Nikolai Nikolaevich (Kolia), married Lidia Viktorovna (Lida), a fellow student of my mother. She showed a natural aptitude for writing and compiled interesting reminiscences.

Until health allowed it, the members of that group had been leading a simple life and adhered to tourist habits. Even I myself, a boy of 12 or 13 years of age, together with them once or twice went on a boat trip lasting a few days (kayaks were still unknown). Leontovich's little children called my father Petr, and mother, Luidmila, other names they did not know, whereas I did not dare anymore call Michail Aleksandrovich *Min'ka*.

The ladies of the Academy sniffed scornfully upon seeing in Abramtsevo [a holiday home belonging to the Academy] the improper clothing and noticing the simple way of life of the Leontovich family. However, Tatiana Petrovna Sveshnikova, Leontovich's wife and another fellow-student of my mother, gave rise to thoughts about something sectarian and I think that life with her had not been easy.

That company worshiped all-embracing honesty (in life, science and social relations) and willingness to struggle against the dark and stagnant forces which the collective communist intellect had been constantly stirring up against science from 1918. Sometimes extreme necessity compelled the conflicting thoughts of the main leaders of the nation to allow information from beyond which led to the elevation of honest and fair men. They were honoured to confer with inveterate scoundrels (sometimes, especially in the pre-Breznev period, however, not lacking in animate abilities).

So it happened with Andronov who, after enduring a period of persecution, all at once became an academician and, in a short time, a

member of the Presidium of the Supreme Soviet [of the Soviet Union]. And, while remaining there, he died of high blood pressure just after reaching 50. Life near Stalin had apparently been nervous, especially if you desire to do some good for the people.

It seems that after his election to the Academy in 1946 it was also decided to elevate Leontovich, but he himself had prevented it. Father told me that in 1949 Leontovich was charged with reading a salutatory address to [our dear] Comrade Stalin on the occasion of his seventieth anniversary, apparently during a meeting of the physical and mathematical class of the Academy. But he discredited himself when reading this address. While enumerating the toasts to the Führer of the world proletarians, he left out Stalin's newborn title, *coryphaeus of science*. The horror-stricken secretary [presidents of Party cells were never elected] of the Party organization [of the class] encountered Leontovich who explained: *I had not prepared my address and left out that new expression out of surprise*. This episode was hushed up, but the organs of security invariably considered Leontovich as a man able to perpetrate *a hostile act of wreckage* [as a potential enemy of the people]. It was rumoured that Beria ordered to endure him only because of his professional skill necessary for the fulfilment of the thermonuclear project.

I would not know whether these stories, which reached me through direct family channels, were accurate, but they are more truthful than any memoirs or reports published before 1988 whose texts, although signed by worthy scientists or military men, had always played up to present leaders. Already in the 1960s, when the communities of physicists and mathematicians of the Academy have been officially separated from each other, I had often heard the question: Why is the academic community of talented physicists morally higher than that of the mathematicians? Did it follow from the essence of science itself of that period, or from a combination of personal traits? Why had the norms of honesty and fairness in the circle of talented mathematicians deeply decomposed?

I agree with that fact itself. Indeed, after Sergei Natanovich Bernstein had died, the only eminent mathematicians in the Academy for whose invariable unshakeable decency I personally may vouch, were Petrovsky, Leonid Vitalievich Kantorovich and my father. And I think that the influence of Andronov, Leontovich, Tamm and Sacharov essentially determined the level of decency of physicists. Other leading physicists (even more brilliant scientifically like Landau or those who held higher administrative positions like Kurchatov, Arzimovich, Aleksandrov) had been under their moral influence.

An essential role, the highest although difficult to achieve example for scientists, was played by the peculiar and eminent indeed personality of Petr Leonidovich Kapiza. He kept himself aloof from social activity and delicately showed respect to the highest government authorities. However, he stopped with unusual dignity all their attempts to involve him in something doubtful and sometimes he was able to help definite individuals who got into trouble. Some scientists later spread rumours that they had also done the same, but clandestinely rather than openly as Kapiza. Vinogradov, for example,

said that neither did he sign, in 1973, the letter condemning Sacharov. Yes, he did say it, but only after Suslov and Breznev had decided that that letter was a mistake. In addition, no one was able to confirm Vinogradov's statement.

In the moral sense, Vinogradov became very influential among mathematicians. To promote his carrier, he, already in 1929 – 1932, had chosen to oppose intelligentsia and to inform on people and after the war [in 1945] he became an ideologist of anti-Semitism as well. This happened at the beginning of the Stalinist anti-Semitic campaign of the 1940s. Exactly then he spread the fake story about his natural anti-Semitism which contradicted his pre-war image. Although distressing, we have to acknowledge that, because of him and those scientific managers or scientists on which he had exerted much corrupting influence, it was the mathematical community that later became a source of the loathsome fascist spirit. For the sake of justice it ought to be nevertheless noted that some mathematical scientists much more eminent than Vinogradov[4] then or later manifested very doubtful moral qualities of another kind quite independently from Vinogradov, but I will not name them. The honesty and fairness of that generation had rested on physicists whose last large-scale bearer was Sacharov.

2. The Academy: the First Half of the 1970s

From the mid-1950s to the end of the 1960s, I, as it seems, barely met with the elder Leontovich's and only rubbed shoulders with their son Andrei, a student of Arnold at the mechanical and mathematical [formerly, the physical and mathematical] faculty of Moscow State University. Until 1968 my scientific career had advanced quite successively. I managed to fulfil quickly topological works which became famous and in 1966 I was elected corresponding member of the Academy. This happened before I signed the generally known letters in the spirit of the advocates of human rights. For a long time after that my own carrier and the carriers of the other signatories (regrettably having been late to be elected to the Academy) came to a stop.

My parents and Leontovich and such eminent mathematicians as I. M. Gelfand, A. A. Markov [junior], D. E. Men'shov, V. I. Arnold, Ya. G. Sinai as well as physicists and many intellectuals also signed similar letters. From that moment in 1968 a new period of *re-education* of the intelligentsia had begun, the Breznev *cultural revolution*, so to say. Vinogradov's denouncing activity flourished anew as well. Until physicists remained influential (until 1964) he had behaved *decently*: he hindered some talented scientists and contrasted to them those serious scientists whom physicists would have supported (although he should have supported both those groups).

The first disgraceful election of corresponding members of the Academy occurred in 1964, just after it separated physicists and mathematicians. Arnold and Ladyzenskaia were not elected, but success came the way of Vinogradov's former scientific secretary and his assistant in collaboration with KGB, absolutely incompetent in science.

The further, the more of the same, and then began the *cultural revolution*, the epoch of corruption and impudence. Vinogradov helped me during my election to corresponding membership in 1966, but was unable to re-educate me, although he bent over backwards to provoke me into some anti-Semitic deeds or other doubtful actions, to make me *a politically reliable member of society* and told me rather vile stories. I kept silent.

When in 1970 I was awarded the Fields medal, Vinogradov clandestinely sent the Party's local district committee a denouncing reference to me, but publicly stated the opposite as though he actively *defended* me. His activity prevented my journey to the International Congress of Mathematicians where the medal should have been festively presented.

In the autumn of 1970 Isaac Markovich Chalatnikov invited me to collaborate with theoretical physicists from the Landau Institute in Chernogolovka[5]. I had indeed begun doubling there in the beginning of 1971 although Vinogradov had done his damnedest to hinder me. Actually, I began mainly working there.

Many new remarkable mathematical problems had then cropped up in that field, and the physical community craved for assimilating modern mathematical methods (?), − topology, dynamic systems, new ideas in algebra and algebraic geometry. In the beginning of the 1970s I happily left Sodom and Gomorrah and, together with Yasha Sinai entered that community. Officially, I began exclusively working there in 1975.

In the corridors of the *kapichnik* [the Kapiza seminar] I began to meet Leontovich. We talked here and there, and he told me very curious things. I was interested to know that physicists, and more precisely, Leontovich, rather than Lavrentiev, as some mathematicians sometimes describe it, had posed a problem to my father: the inverse problem of restoring the form of a body given its gravitational potential. The problem was prompted by the Kursk magnetic anomaly. About 1940 Lavrentiev generalized it after the appearance, in 1938, of the work of Petr Sergeevich [Novikov].

I heard from Father even previously that Leontovich despises some good mathematicians, in particular, Khinchin[6] and Vinogradov (!), but did not know why, and, on the contrary, that he much respected Kolmogorov[7]. Khinchin, as it seems, attempted to study, not competently enough, the foundations of statistical physics, but everything concerning Vinogradov was much more funny. In the 1930s, when Leontovich had been compiling a textbook on statistical physics, he became interested in the order of the fluctuations of thermodynamic magnitudes during the transition to the limit of infinite volume.

Beginning with a free Fermi-system with periodic border conditions at zero temperature, one soon arrives at the problem of the number of integers in a sphere or on a sphere with an integer radius[8]. Igor Vladimirovich Arnold (father of V. I.) told him that Vinogradov was the best specialist in such problems. Leontovich came to him and began speaking in the language of the eigenvalues of the Laplace operator rather than in the language of integers. Vinogradov did not

know the meaning of eigenvalues or eigenvectors, referred Leontovich to Sobolev and instead suggested to have a drink. Leontovich did not conceal this story and Vinogradov spitefully spoke about him, and, for good measure, began to hate the Arnold family.

However, in the 1970s the authorities, as I mentioned above, lost faith in the circle of distinguished physicists. The activity of Sacharov, the mere number of signatories of the letters of the advocates of human rights, and the per cent of Jewish blood among eminent physicists, − all that had apparently influenced the powers that be. And it became desirable to prove that all their former merits were exaggerated. Anti-Semitism had been ever more becoming the general aim of the Party, and occupied the place only after embezzlement and corruption. Mathematicians, with Vinogradov at the head, prepared to *fawn* before the authorities, began at once to attack the books of the physicists, the splendid textbooks by Leontovich, L. D. Landau and E. M. Lifshits, and Zeldovich, to nag at trifles and to accuse them of incompetence. Even Lev Andreevich Arzimovich, the head of the physicists [at the Academy], became very disturbed. Leontovich even said that that situation contributed to his untimely death (he had a bad heart).

In the beginning of 1972 Aleksandr Danilovich Aleksandrov, Andrei Andreevich Markov [junior] and your most humble servant had sent a letter to the Committee on Lenin prizes [introduced instead of Stalin prizes] which protested against the awarding of that prize to Vinogradov for insignificant, trivial works; after those of the 1930s, for which he was awarded a Stalin prize, he had not provided anything serious. It was intended to conceal our letter, but Petrovsky read it out. Only the brutal pressure in the form of an order exerted by the chairman of the Committee frustrated our attempt to prevent the awarding of the prize, but our letter provoked much talk.

It was this episode after which Leontovich began to be especially interested in me. But *penalties* certainly followed. I was turned out from here and from there to deprive me of my fictitious *influence*, or just became insulted, but all this little disturbed me anymore.

Much more important was the evolution which the Academy had underwent. Not without a powerful help from the mathematicians who had connections with the Political Bureau of the Central Committee of the Party such a personality like Logunov was without much noise elevated on behalf of nuclear physics [physicists?]. He was recommended as an *organizer of science*. In other words, he belonged to those who were able to command a team of soldiers on a building cite[9].

He began his carrier at the end of the Stalinist epoch as a postgraduate of Professor Terletsky, who battled in philosophical literature against the *reactionary Einsteinianism*. In the eyes of the Breznev uppermost echelon Logunov was the ideal of a scientist who will be able to transform physics just as those people wished, to appropriate it. Incidentally, somewhat recently professor Terletsky (who was then about 80 years old) objected to an accusation, which briefly appeared somewhere in print, that in the beginning of the 1950s *Leontovich had* as though *thrown him down from a flight of*

stairs. That, as Terletsky declared, was impossible since he was physically stronger than Leontovich.

The 1960s gave place to silence and dumbness. Leontovich remained the last academician to speak sometimes at general meetings against dubious personalities who had appeared in the Academy in the first half of the 1970s. In the 1960s, there were many such speeches, but they were also (?) initiated by thermonuclear physicists, others only joined in. The elder generation had been disappearing and the situation was changing.

For several years the President of the Academy remained in a grave physical and nervous condition and in 1975 a replacement began to be looked for him. Happily (I do not know the details of the intrigue) it became possible to persuade Breznev to appoint (!) Anatoly Petrovich Aleksandrov, and at the time this was the best possible decision, although later the Breznev regime influenced him as well. It was also ordered (!) to assign Logunov as vice-president. So Logunov presented to the Political Bureau some pseudo-scientific materials coming from *the West* which criticized the programme of the guided thermonuclear synthesis. He aimed at preventing Aleksandrov from becoming President, but Logunov had carried out this plan scientifically incompetently and, anyway, Breznev had already decided.

At the general meeting in the end of 1975 where the presidium was *elected*, Leontovich exposed Logunov's story about those *materials*. He indicated that Logunov was absolutely incompetent in that chapter of physics. Soon after that meeting I myself saw how Logunov, in the lobby of the House of Scientists, warily peeped out from around a corner and waited for Leontovich to be alone for *explaining himself* without any witnesses. He probably had to *report* that he accomplished that feat and that that unpleasant episode was left behind.

Aleksandrov, that *cunning fox*, was assigned the Main Scientist of the leader. In physics, an analogue of the notorious session of the Lenin All-Union Academy of Agricultural Sciences[10] was postponed. He deftly availed himself of M. V. Keldysh's support and it was rumoured that he promised Keldysh the officially canonised version of the *three K*, the *creation of the nuclear – missile shield of the homeland* (Kurchatov – Keldysh – Korolev) nicknamed *the shield of the three ka-ka* [of the three turds]). I would not know whether Aleksandrov gave such a promise, but he did carry it all out for Keldysh. The new President also began to elevate rapidly Rem Viktorovich Chochlov who knew how to please the leaders. Petrovsky had time to recommend him to Breznev as successor to the rectorate of Moscow University. Chochlov was a very worthy man and uncommonly clever. The physical and mathematical sciences had once again obtained chances [of development] in spite of the general degradation of the country. Even among mathematicians a new orientation began to occur: the results of the very first election to the Academy were incomparably better than those previous.

3. We Take the Field together with Leontovich

In 1975, the scandals at the Editorial – Publishing Council (RISO) of the Academy provoked by the Vinogradov team and directed against physicists were at the very height. One of Bogoluibov's students, Boris Valentinovich Medvedev, became indignant: the expediency of reprinting *Quantum Mechanics* by Landau and Lifshits had been discussed by incompetent people (although academicians), and he began to defend it [the book].

All at once, Vinogradov personally ordered Bogoluibov to kick Medvedev out of the Steklov Mathematical Institute. In his capacity as director of that institute he sent and signed a letter to RISO in which he maintained that Medvedev did not represent the opinion of that institute and accordingly recalled him from RISO. Medvedev, however, was soon fixed up in the Institute for Theoretical and Experimental Physics, and, moreover, the class (?) of general physics and astronomy adopted a decision condemning the incompetent *discussion* of those books and named the culprits.

The quarrel between Vinogradov and Bogoluibov intensified and favourably influenced all the next elections of mathematicians to the Academy. My professional and personal relations with such an eminent scholar as Bogoluibov (in spite of all his deficiencies) and his students, workers at the section of quantum field theory of the Mathematical Institute, worthy and intelligent people and serious specialists, became very good. Under the new conditions, Vinogradov supported L. D. Faddeev (whom Bogoluibov did not like), a very talented scientist, and quickly elevated him to the position of head of the Leningrad branch of the Institute and to the Academy. I and Faddeev exerted every effort to remain friendly, but Vinogradov had been able to cause Faddeev quarrel with some very worthy scientists.

In the beginning of 1977 Vinogradov, being 85 years old (and still uncommonly healthy) was re-elected director for the next five years[11]. A. D. Aleksandrov called me from Novosibirsk [from the Academy's branch there] and asked whether we will tolerate this, and whether it is possible to enlist Leontovich and to come out together at the general meeting of the Academy. I asked Leontovich, and he became burning with desire to speak out. However, only a few days before the meeting it was still unclear whether Aleksandrov was ready to participate essentially in some such action.

Leontovich several times asked me about it in an impatiently irritated form, as I would say. Finally, Danilych [Danilovich] arrived in Moscow and confirmed his firm intention to speak out. It was promised that just after his speech the floor will be given to Leontovich. Danilych told me:

You are still a corresponding member, not a full academician. We, Leontovich and I, will speak out, after which all their team will begin to lie. So perhaps you will be able to provide <u>information</u> and refute something since you are in the know about the Institute. For a corresponding member, information is a proper form.

I promised. Leontovich called me in my absence and told my wife:

So that I will not say a lot, let Serezka [*Sergei*] *write theses for me. You see, I began to forget and will not keep from a talk about racism.*

The point is, I myself tried to convince Leontovich not to mention anti-Semitism; even apart from it, Vinogradov made enough dirty tricks. He himself counted on that topic since he was supported by the general feeling reigning among the highest authorities. The day before the meeting (Vinogradov was already re-elected director of the class of mathematics with a few votes against him) I gave Leontovich through Lev Petrovich Pitaevsky (just elected corresponding member) my version of his speech in a sealed up envelope, only a page long. My idea was simple. Provide just one example which I have mentioned above: describe how the book of Landau, a Novel prize winner, was discussed in the RISO and what happened to a modest man, a quantum physicist Medvedev, who, being sincerely indignant by the incompetent discussion, dared to defend that book. This is what happens to a physicist in the Mathematical Institute, this is how number theory [Vinogradov's speciality] governs physics!

That was all. Leontovich read it by heart after somewhat improving my text. His speech was brilliant and sounded like a supplement to the speech of Danilych. I think that even his [whose exactly?] old friend, Anatoly Petrovich, the President of the Academy, was taken aback. He apparently thought that the criticism will be directed against racism and considered some such discussion unfavourable for himself.

Danilych was the first to speak, and for me his subject was unexpected. A brilliant idea is always simple and this is how he began:

Official information about Vinogradov as director of the Mathematical Institute is propagated. It does not conform to the truth. It is said that he has continuously been director since 1934 whereas all of us know that during the war years the director was the eminent mathematician, academician Sobolev. For Sobolev, this is slanderous, an attempt to negate his merits manifested during the difficult war years etc.

Then he mentioned the persecution of scientists in that institute including me. After that spoke Leontovich, then followed, as was expected, many persistent lies expressed by the *team*. Incidentally, one of its members stated that Vinogradov is honest, is as pure as the driven snow, and he supported and elevated me.

Finally, the chairman, V. A. Kotelnikov, declared: *This is the end. No one will have the floor. Everything is clear*. However, it should have been my turn but the chairman did not allow me the floor. Nevertheless, I stood up and came to the tribune:

My name is mentioned as a speaker during discussion, and I ought to provide the information. Yes, Ivan Matveevich [Vinogradov] *had sometimes elevated young men of talent, me for example* …

Derisive laughter and shouting. I pulled myself together and raised my voice: *But he harmed a still larger number of talented scientists, restricted their activities and persecuted them*. As an example, I mentioned Chernavsky, a student of my mother, who was expelled from the Institute soon after he had attained a wonderful topological result which won serious recognition by the international mathematical community. Then I went back to my place but noted that Rem Chochlov, who sat in the presidium, approvingly glanced at me.

Yuri Vasilievich Prokhorov, a nice man and a good mathematician, Vinogradov's assistant at the Institute, took the floor after me, apparently by order, and once more began to extol Vinogradov's good deeds rendered to young scientists. Academician Knunyants, a chemist, shouted from the floor: *And what position do you occupy in that Institute*? Prokhorov was taken aback and sat down. The discussion ended. Later Prokhorov had always supported me and cocked a snook at Vinogradov.

President Aleksandrov looked pensively. I thought that he did not want Vinogradov's failure and postponed the sitting and the voting itself until the next day. That was a barbaric procedure adopted four or five years previously, when a President of the Academy was himself nominated for directing the Institute. It stipulated that either the voting will be by show of hands or by a secrete balloting and that that will be decided by a show of hands. Furthermore, all that had to be carried out if someone required a secret ballot.

In this unique case, the sitting resolved by a show of hands that there will be a secret ballot. The Vinogradov team became greatly frightened, they thought that Vinogradov will fail. This, however, did not happen. More than 1/3 of the votes was *against*, and, as a nominee for election to full, or corresponding membership, he would have failed, but in this case 50% of positive votes was sufficient for election with invalid ballot papers considered positive.

Vinogradov was elected with apparently 74 votes against him out of ca. 180 votes. Academicians are disciplined and many are accustomed to cast their votes without marking them. For such voting the result was nevertheless unique: the nominee was an eminent scholar, already old, whose scientific authority had ben boosted far more extensively than the frog boosted itself in Krylov's fable[12].

The voting set up a clamour and it was rumoured that the President of the Academy charged his subordinates with putting the question about Sobolev, whom he personally knew and respected, in order. It was revealed that there were no necessary documents in the Institute confirming his directorship, so where had they disappeared? The history of our mathematics apparently deserves a criminal investigation into the safe keeping and certainty of documents.

Together with Danilych we went to my place and, while discussing the details of the wrangle, had a well and truly drink. The official personnel of the Institute was embarrassed and the ordinary men chuckled[13]. Never was a nominated director of an institute discussed during a general meeting in such a way either before or after that. In the second half of the 1980s President Marchuk called off the confirmation of directors by general meetings. I think that the reason was his peculiar respect rendered to us and Danilych: he desired to see his friend as the director whereas we had already spoken against him in the lobby.

4. The Shadow of Leontovich[14]

In the summer of 1977 Chochlov, the vice-president owing to A. P. Aleksandrov, had perished absurdly. It was said that to Chochlov he thought to turn over very soon the presidency and that that it was

already *co-ordinated* with the Central Committee of the Party. And once more loomed up the figure of Logunov. Not without a recommendation of mathematicians Breznev appointed him rector of Moscow University. At that time, he, together with Folomeshkin, a worker of the Institute for Physics of High Energies (Logunov was its director), an idealist and a refuter, declared that he was able to refute Einstein's general theory of relativity (GTR)[15].

Such evolutions, a recognition of self as a great scientist, is typical for managers elevated to high administrative positions in science due to their administrative and economic activity. Indeed, when being elevated, elected to the Academy and to high positions there, they were invariably declared to be most eminent scientists. Their positions became more important than those of the really eminent scholars who had assisted their elevation. And the result? Human psychology very often becomes prisoner of that effect of public laudation.

Folomeshkin, the co-author of Logunov, a bit touched with denying the GTR, which he did not understand, represented another type. Incidentally, among average quantum physicists of the post-war generation, cut off from the geometric ideology and lacking geometric education, his viewpoint was not exceptional.

Even many years previously, as a student, Folomeshkin knew well enough that he will refute Einstein and bet his fellow student Leonid Petrovich Grischchuk about it. In the beginning of the 1977, after intrinsically realizing that he refuted the GTR, he, as eyewitnesses told me, went to Ludwig Faddeev in Leningrad holding a manuscript with Faddeev and Folomeschkin stated as co-authors. Indeed, he had to find an academician as a go-getter and was prepared to share with him his discovery. Faddeev, however, laughed off his proposal.

And so, in the autumn of 1977, there appeared a series of papers and reports by Logunov and Folomeshkin (although I do not maintain that Folomeshkin was their sole author). I read the first papers attentively, laughed for a long time and firmly decided to ignore them. Everyone has the right to be crazy in any way (so it seemed to me). At first, their activity had been rather harmless[16], but then Folomeshkin, the idealist, accidentally perished, and co-authors of another type occurred in Logunov's surroundings.

Logunov became irritated at the lack of recognition, began to arrange public reports or rather shows for specially selected incompetent listeners and never acknowledged the mistakes made on previous occasions (and indicated by some physicist) but his co-authors modified his texts and statements.

Interpreting all this in its own way, the Central Committee of the Party obviously supported that activity as the overthrow of *the Jewish idol*. The significance of the occurring events during 1978 – 1982, that special period of the late-Breznev decay, was evident. I ought to say that the Mathematical Institute had heard out Logunov's report and had a good laugh! Vinogradov knew the worth of suchlike attempts and for Pontriagin the level of refutation became at once evident. A. P. Aleksandrov became nervous.

Those on the top, however, quickly elevated Logunov. They only regarded the critical statements made by Lifschits and Zeldovich, both

Jews, as being in favour of Logunov. Faddeev had only revealed the mistakes of Logunov and his group in *Uspekhi Fizicheskikh Nauk*[17], and that gave rise to Logunov's malice. Aleksandrov apparently looked for a kamikaze (?) and, in a word, about 1980 Leontovich called me and proposed to make a joint statement about Logunov's work devoted to the refutation of the GTR. I was a specialist in this field, published a few pertinent papers and cooperated with the main scientific schools, and, from the beginning of the 1970s, actually worked together with the Landau group.

Chalatnikov arouse my interest in the charms of the Einstein gravitation, and I think that a geometer, close to the circle of physicists, cannot avoid this subject. I had not accepted Leontovich's proposal [because of the impending election, see below?] which much distressed him.

Years have passed: In 1981, I became full academician, and several new general secretaries of the Central Committee of the Party and Presidents of the Academy came and went. G. I. Marchuk, the new President of the Academy, elevated by E. K. Ligachev[18], obviously supported Logunov. He won ever new positions: member of the Central Committee of the Party, and the Chebrikov[19] commission, head of a many-milliard programme concerning the physics of high energies, vice-president of the Academy, rector of Moscow University, head of the publishing activities of the Academy, a great scientist, a refuter of Einstein, etc., one of the highest protégé of the *partocracy* and the KGB, member of the nomenclature[20] of the highest level …

Suchlike people are assigned all positions at once, and, since simultaneous work everywhere is impossible, they may sit back, create a deficit and as though *hand-feed* the scientific community and have the work done by *dark horses*[21]. For most members of the nomenclature this is the usual pattern of action which ensures the support of a number of scientists. However, those *horses* proved especially incompetent and it became clear that the Logunov leadership will definitively sink the University. My native mathematical and mechanical faculty, still the best in the whole world in the 1960s, steadily degraded[22]. It was impossible to tolerate this situation.

The refutation of the GTR had been acquiring grotesque and comical traits. Logunov began to accuse in print the entire physical and mathematical community of primitive mistakes of the students' level which had been repeated for 70 years by everyone including the greatest scientists, both mathematicians and physicists. This had apparently reflected his level, his opinion about the entire scientific community, the opinion of one of the highest managers of science. Readers! Do not be surprised by the low practical level of those who, in August 1991, attempted to carry out a putsch[23]. For a long time, THEY had thus already led science.

In the spring of 1988 elections to the 19th Party conference[24] were held and the state of public opinion at Moscow University assured me that not everything was yet lost. Anticipating the general meeting of

the Academy scheduled for October 1988, I called Sagdeev to seek his advice:

The Leontovich shadow appeared before me. If I manage to take the floor of the general meeting, I shall recall his statement of 1975 about Logunov's incompetent interference into the work done in other fields. Will anyone from the academicians of the Kurchatov Institute confirm that fact if someone denies my statement?

Sagdeev promised to help and offered a few concrete advices. Following one of them, I registered beforehand certainly to have the floor. In a few days Sagdeev called me:

The Leontovich shadow appeared before me as well. I intend to propose Sacharov as member of the Presidium. Marchuk is intriguing, wishes to prevent me from becoming a member, and he will achieve his aim. Well, I will propose that, only please do not disclose it. It should happen suddenly. Ludwig Faddeev promised to keep an eye on the permission to speak out in proper time by asking Velikhov, a member of the Presidium, about that.

After some discussion we both, Sagdeev and I, began to realize our programme. My speech about Logunov, the Einstein gravitation and Moscow University was published in the *Vestnik Akademii Nauk SSSR* No. 2, 1989. Sagdeev acted wonderfully, but I do not describe the details of that meeting. You should have seen the faces of some of the members of the Presidium when Sagdeev proposed Sacharov! One of the *counsellors*, Kirillin, as it seems, uttered something like a squeal and proposed to remove, under some pretext, Sagdeev from the tribune.

Sacharov was elected. After the last sitting, academicians were driven home, three or four to a car. I rode with Sacharov and Faddeev and congratulated Sacharov with his election to the Presidium, and he answered: *Your speech pleased me much*[25].

5. [Appendix.] The Speech of Academician S. P. Novikov at the General Meeting of the USSR Academy of Sciences 25 April 1990. The Discussion of the Candidates for Vice-Presidents of the Academy

I am thankful to G. I. Marchuk who had read out my constructive proposal. I still believe that the problem of higher education should not be *generalized* and charged to one single vice-president. A vice-president and some members of the Presidium ought to be engaged with education for each science or group of related sciences and be responsible for it.

Concerning the business of publication and its technical re-equipment, I would have confidentially stated 20 years ago that it was the proper place for A. A. Logunov although under the condition that he will really occupy himself with that duty without being busy with a great number of other leading functions. Some of our vice-presidents, not only Anatoly Alekseevich [Logunov], have much surpassed Gaius Julius Caesar, the genius of ancient history, with respect of the matters pursued at the same time: he was only able to busy himself with three of them.

In 1988 I presented the general meeting my scientific estimation of the ten-years' results of the Logunov group directed at the refutation of the Einstein gravitation. It was published in the *Vestnik Akademii Nauk SSSR*, No. 2, 1989. Allow me to cite Academician Sacharov's opinion about the work of that group directed at the creation of the relativistic theory of gravitation, RTG (*Priroda*, April 1988, pp. 26 – 27); I only read out two quotations, both from p. 26:

Zeldovich and Grischchuk convincingly proved that the matter actually concerned an equivalent formulation of the equations of the Einstein theory rather than a new theory.

The authors of the RTG wrongly state that the conclusions of the GTR are ambiguous.

You see now that Sacharov's as well as my own opinion is this: the own theory of the Logunov group is not new and their refutation of the Einstein gravitation is mistaken. I understand that here, in our Academy, the opinions of Academician Sacharov are not fondly listened to. They were not taken into account even when he warned about the awful threat posed by the situation in which incompetent apparatchiks hold energy of immense power whereas scientists are kowtowing to them and guessing which expert opinion better suits them. And what followed? A strike against their own people[26].

It is bad if a man who refutes the scientific community heads it as well. In 1988 I had already informed the general meeting about the consequences of such a situation for the Moscow University (*Vestnik* No. 2, 1989).

One more remark about our activity in publication. Here is a [Russian] monograph. Author: A. A. Logunov. Title: On the works of Henri Poincaré, On the Dynamics of the Electron. Second page: *Annotation. The monograph of A. A. Logunov, On the Dynamics of the Electron, published by the Academic Institute of Nuclear Research, had drawn wide attention of specialists. Responding to the numerous requests made by the readers, the typography of Moscow University had printed this second run.*

I turn the attention of the meeting that Logunov is the rector of Moscow University.

Third page: Poincare's dedication. Fourth page: Contents. Introduction, p. 5. Poincaré, On the dynamics of the electron (5 July 1905); Same title (23 July 1905), pp. 11 and 21. Pages 5 and 6 [not p. 5 as stated above]: Introduction. I will only read out the last paragraph.

This is a new edition of two papers by Poincare. For the benefit of readers modern notation is used and short commentaries compiled with the assistance of Professor V. A. Matveev […] *are provided*. Academician A. A. Logunov

And so, the Logunov monograph ends on p. 6, then follow the Poincaré papers. I like Poincaré very much. It will be nice to publish the same as his works with commentaries by Logunov and Matveev. Tell me please, what idea has the head of our publication activity about the essence of a monograph?

Logunov's candidature was supported [seconded?] by academicians Markov, Bogoluibov, Baldin, Fridliander, Zefirov and corresponding

members Jelepov and Gerstein. Academician Roumiantsev [a mechanician][27] asked: *Are there really no valuable monographs in the natural sciences on two pages*?

And now the voting. Voters: 268 academicians (total number, 306, excluding those who were ill and abroad on official journeys). Participated: 234. For election, it was necessary to gain more than 50% of the 268 votes, i. e., 135 or more. Positive votes: 169. Logunov was elected vice-president responsible for higher education and publishing activities[28].

Notes

The first three notes concern the Introduction

1. Novikov (2002, p. 339) did not consider Smirnov a worthy enough mathematician, and here, in 1995, he did mention Petrovsky very favourably. In several places he also acknowledged Vinogradov's merits (see a bit above), mostly those of much earlier times. Novikov *did not mention* the *Letter of 99* (see just below) which appeared in 1968, although co-signed it. It defended a cruelly persecuted eminent mathematician, Esenin-Vol'pin. Later (?), however, Novikov decided that the *Letter* was specially provoked by the KGB (to reveal potential *saboteurs*?). Kolmogorov compiled and sent a similar separate letter. (Russian Wikipedia.)

2. See Ermolaeva [viii]; Tokareva (2007); and Youshkevich (1989) and Demidov & Ford (1996), respectively, On pp. 113 – 117 Tokareva reprinted a declaration which was published in 1931, signed by most eminent mathematicians (Luisternik, Shnirelman, Gelfond, Nekrasov and Pontriagin, later a notorious anti-Semite) but contained most dirty, fantastic, ultra-communist accusations.

3. I (2006) discovered and published a draft of Markov's report compiled by him himself.

4. I do not have Shafarevich in mind. I think that he does not fit that description. S. P. N. Here is his book (2006, pp. 399 − 401): *Jews lack creative principles and a scientific vacuum is reigning in Israel*. His book is crammed with suchlike ridiculous anti-Semitic statements. Vinogradov had been director of the academic Mathematical Institute (indeed stated by Novikov although much below). O. S.

5. The academic Landau Institute of Theoretical Physics was founded in 1965 by a group of eminent theoreticians of physics, students of Landau, I. M. Chalatnikov, L. B. Gor'kov, I. E. Dsialoshinsky and A. A. Abrikosov. In the 1980s, that Institute had been considered the best in its field in the whole world. S. P. N.

6. See my general comment. I also note that Khinchin (1932) published a paper extoling, contrary to common knowledge, the scientific atmosphere in the Soviet Union. Possibly he was somehow compelled to disgrace himself. Cf. Note 2.

7. In § 1 Novikov mentioned three mathematicians *of unshakeable decency*, but passed Kolmogorov over in silence. It seems that Kolmogorov had positively although indirectly influenced Soviet scientists. It became known that he was gay (a criminal offence!) which the powers that be had chosen to tolerate but which certainly compelled him to abstain from *undesirable* activity. Nevertheless, he came out in defence of an eminent mathematician (Note 1).

8. This description is not definite enough.

9. It was usual (especially during the Breznev epoch) to describe scientific managers as most eminent scholars, therefore to have them elected to the Academy. The consequences were bad. Experts and managers should not be confused. S. P. N.

Still, managers should not be likened to superintendents on building cites. O. S.

10. As desired by the Party leadership (Lysenko's statement), this session (1948) had virtually done away with Soviet genetics. Novikov had thus indicated that a similar fate was awaiting Soviet physicists, but that at the last moment those responsible realized that physics was too important for allowing Soviet philosophers to destroy it.

11. Not definitively, see below.

12. Krylov [xiii, Note 7] published a fable, *The Frog and the Bullock*, in which the Frog attempted to puff itself up as large as the Bullock but got burst. Novikov's

comparison was unfortunate: Vinogradov did not burst, and, anyway, it was impossible to puff him up still more intensively than the perished Frog puffed itself up.

13. Later the situation in the Institute very considerably improved. S. P. N.

14. Leontovich died in March 1981.

15. This field was very remote from those areas of physics with which Logunov dealt before that. Bogoluibov told me that Logunov did not known that field at all. S. P. N.

16. Harmless? Total Party and government (and everyday) anti-Semitism and the attitude of the *average quantum physicists* towards the refuters allows me to doubt it.

17. From 1958 that journal is being translated into English but its English title changed a few times. Nowadays, it is called *Physics-Uspekhi*, is prepared in Moscow but published in London.

18. In 1985 – 1990, Ligachev, a diehard communist, was member of the Political Bureau.

19. In 1982 – 1988 Chebrikov was head of the KGB. However, I do not know anything about his commission.

20. Special lists (nomenclatures) of Soviet and Party officials (of *nomenclature workers*) had been separately compiled for every level of authority.

21. Why hand-fed?

22. The situation in Moscow University is somewhat better now, but the mathematical and mechanical faculty had improved least of all. S. P. N.

23. The unsuccessful putsch aimed at preventing the imminent break-up of the Soviet Union which occurred in December 1991. Gorbachev was then (wisely?) resting in the Crimea.

24. That conference took place in June 1988 and heralded the beginning of the perestroika.

25. Sacharov also indicated in print that that refutation of the GTR was mistaken. S. P. N.

26. The Chernobyl catastrophe occurred in April 1986 and the radioactive wave had *struck* quite a few other countries as well. The highest Party and government apparatchiki did their damnedest to conceal the danger rather than to save the affected population and the blame was levelled against switchmen, as the Russian saying goes. Much later the Patriarch of Moscow and all Russia declared that God did not stay the hand of the operator because of human sins (why not … *directed the hand* …?). What else could have he said?

27. An obvious hint for Logunov's benefit.

28. Was there a secret ballot or not?

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XI

S. P. Novikov

Mathematicians and history

Priroda, No. 2, 1997, pp. 70 – 74

1. The first steps of the Morosov pseudo-discovery

Many years ago, almost each Sunday, I and my brother Andrei went to the Liapunov family. We gathered there, boys and girls from a small and friendly circle of intelligent mathematical families. Sometimes came Vladimir Igorevich (Dima) Arnold, a year older than me. Wildly enthusiastic, unusually handsome and belonging to one of the most high-born Russian gentry, Aleksei Andreevich Liapunov (my father's student) organized a children scientific society and acquainted us with the elements of various sciences, especially biology. Around 1950 – 1951 he told us about the attempt of the celebrated revolutionary, narodnik (populist) N. A. Morosov who had done 20 years in Schlisselburg.

Issuing from astronomical phenomena, clearly or allegorically (in his opinion) described in the Bible and other ancient books, he attempted to date truly the historical events. And it turned out that the wars which had happened during the time of the classical Hellada, the Peloponnesian War, for example, occurred not in the end of the fifth century BC, but in the thirteenth century AD, when the Franks, the crusaders, possessed Greece, etc. In a word, that the entire ancient history was just rubbish. My parents became mightily irritated with Liapunov for accustoming us to that nonsense, as they thought.

However, just at that time there appeared the method of radiocarbon dating which largely confirmed the standard chronology, whereas Aleksei Andreevich never returned to history. He essentially defended biology and biologists in their struggle against Lysenko, and his daughters, who are my old friends, and the members of their families became biologists.

For many years I have not recalled Morosov and thought that his considerations had sunk into oblivion[1]. Nothing of the sort! In 1967, as a young corresponding member of the Academy of Sciences (I was elected at age 28) just returned from my first scientific journey to the USA (before that, I was not allowed to go abroad without a good cause, and later I was banned for ten years from such travels for signing letters which defended the dissidents, in particular, Esenin-Volpin) and almost at once went to Novosibirsk to attend a topological conference. It was there that my former mentor, Mikhail Mikhailovich Postnikov, reported, in July 1967, about the historical discoveries of Morosov.

I did not hear him, but came to know about his report from friends and colleagues. From 1956, I studied topology in the seminar headed by Postnikov. Then, he was very competent in that field, but only two years later we understood that he (only 30 years old) was no brilliant scientist anymore. As far as I know, he had convinced himself in that

the radiocarbon dating was groundless: much material is needed for its statistical appraisal which, in his opinion, was never available. And it was really ascertained that that dating should be corrected owing the changes in the natural radiation background of the Earth. It seems that for events which occurred 3000 years ago, the corrections do not exceed 500 years. However, if you desire very much to believe in anything, you will be able to conclude something worth a lot of money out of events worth a few pennies ... I had a laugh and forgot about the Morosov pseudo-discovery for ten years more.

By the middle of the 1970s my best students in topology, ripe and recognized by the world science, were B. M. Buchstaber, A. S. Mishchenko and (partly) A. T. Fomenko who also was the student of P. K. Rashevsky, then our chair and an honest and intelligent man. They had already been invited to report to some sections of the International Congresses of Mathematicians. By that time I began to work in mathematical physics, and Fomenko essentially helped me, when, in 1971 – 1975, I took upon myself the arrangement of a course on Riemann geometry on the mathematical and mechanical faculty (it encountered resistance).

As a human being, Fomenko was unusually nice, and I, and many others liked him as an author of quite good paintings as well. However, those paintings should have been attentively looked at: in my opinion, they compelled the viewers to become thoughtful about the mental peculiarities of his personality. And I have also begun to note some oddity in his mathematical activities. In 1977 – 1981 Mishchenko and Fomenko wrote a number of insipid, as I thought, papers about integrable systems and I informed them about my opinion, but they had not agreed with me. It seems that after leaving topology they were unable to assimilate the main point in that field, which was new to them, to comprehend what was interesting in the theory of those systems, and what was not interesting. My opinion coincided with the views of other professionals. It seems indicative to me: if a mathematician is unable to assimilate a neighbouring field of his science, it will unquestionably be more difficult for him to penetrate into the essence of other sciences alien to mathematics, such as history.

2. The faith in the Morosov pseudo-discovery strengthened among mathematicians

In the second half of the 1970s, Postnikov attained great success. He won over to Morosov a group of talented young men from the mechanical and mathematical faculty of Moscow University. Among them were my students, Fomenko and Mishchenko; others had sided with them, but soon became disinterested. At first, Tolia Fomenko attempted to convince me, he thought that as soon as I see the astronomical arguments dating back to Morosov, but specified by him, Fomenko, I will at once pass over to his faith. He came to me with maps and drafts.

Let us consider the Peloponnesian War between Athens and Sparta. Thucydides described the eclipses, two of them solar with an interval of seven ears, then, eleven years later, a lunar eclipse. When

ascertaining the dates of such three eclipses to within a hundred years, it is possible to indicate them precisely. This argument was already taken into account in the 17th century, and the exact date of the beginning of that war was determined as ca. 430 BC.

Fomenko (Morosov) objected: Thucydides had described those three eclipses as full, but at the end of the fifth century BC one of them, as seen on the pertinent territory, was incomplete; a full eclipse can be found fifteen centuries later, during the period of the Franks the crusaders. Having heard Fomenko out, I asked him how was it possible to make conclusions by issuing from such an inexact, in its essence, material. You are not considering mathematical logic; do you know the level of precision in ancient descriptions? Did Thucydides witness these eclipses, did he attach any significance to the difference between a full and an incomplete eclipse? In a word, such conclusions are obviously absurd. Fomenko was grieved.

His and Mishchenko's propaganda became very persistent. And when they began to make reports for historians and carving the way for publishing those ideas, I stated that their activities can harm the scientific authority of the chair of differential geometry of the University. They were offended and the more so because of my bad appraisal of their mathematical work of that period.

The 75-years-old academician S. M. Nikolsky seems to become mightily attracted by the new theory and communicated their work [which one? O. S.] (I. M. Vinogradov refused to communicate it). However, a quarrel soon occurred between Fomenko, Mishchenko and Postnikov. It was Fomenko alone who had really been working but Postnikov wished to be the *leader*. In addition, a response of the most prominent historians was about to happen, and Fomenko began to soften the sharpest angles, to renounce his refutation of history and attempt to present everything as a statistical analysis of the sources without making any far-reaching conclusions.

Profiting from Fomenko's retreat, Postnikov published a paper in *Tekhnika Molodiezhi* in which he clearly declared that the ancient history did not exist, attributed all the *discoveries* to himself and named his adherents who were specifying the details. As a response, three historians, academicians of a considerable Party and ideological weight, B. A. Rybakov, Yu. V. Bromlei and the third one, whose name I forgot, had sent a strongly-worded letter to the Central Committee of the Party. They urged to do away with the Morosov pseudo-discoveries by communist methods and to forbid teaching to Fomenko and Mishchenko.

Fomenko came running to the Central Committee. He told me that a prominent official from the department of science and education of that Committee had friendly remarked: *It is absolutely indifferent to me when exactly was Gaius Julius Caesar killed*[2]. That official, as Fomenko told me, called *Tekhnika Molodiezhi* and *advised* them to publish Fomenko's refutation of Postnikov's paper.

From that moment his behaviour changed. He began stating that he broke off his relations with history, and, indeed, in 1984 – 1990 he resumed the active study of mathematics, of three-dimensional topology. His contribution to the theory of three-dimensional

manifolds seems to me very useful. He also proved himself a skilful organiser of numerical topological calculations. In any case, I decided that Fomenko had *recovered* and began to support him once more. My warm and, as I thought, trusting relations with him had resumed. Incidentally, in the second half of the 1980s I noticed that V. A. Sadovnichy, the then first pro-rector of Moscow University, had a very high opinion on Fomenko. He told me this himself after, in ca. 1987, Fomenko had reported to the Moscow Mathematical Society about his joint computer-topological research with Matveev. I remarked that that research was good.

In the end of the 1980s and beginning of the 1990s I started hoping that I will be able to hand over to him both my chair and the presidency of the Moscow Mathematical Society (I had been president in 1985 – September 1996).

Only after 1992 it became absolutely clear that, although N. N. Bogolyubov supported me, I could not keep my promise given sometime to Kolmogorov to restore to life and head the department of mathematics of the mechanical and mathematic faculty: in 1985 – 1987 A. A. Logunov and Sadovnichy had been sharply resisting my efforts. It became clear to me that the faculty is dying although separate organs can possibly still exist for a long time[3]. I began to support the activities of younger good mathematicians aimed at the establishment of a Free University [in the Soviet Union! O. S.]; Arnold started to do the same even before that.

3. The Morosov pseudo-discovery is triumphing

In the end of 1990 Fomenko was elected (not without my help) corresponding member of the Academy of Sciences. Before the elections I had arranged a questioning of the members of the Moscow Mathematical Society: whom of our mathematicians not yet elected to the Academy do they consider the best? The same, with respect to their own narrow field of mathematics?

Keeping to political correctness, we (?) did not ask their opinion about the members of the Academy. Yu. I. Manin and Ya. G. Sinai, then not yet even corresponding members, broke far away from the others, and, out of the rest, Fomenko was among the first three after B. E. Margulis and D. V. Anosov. I certainly understood that his popularity was due to skilful advertising and sympathy of our (and foreign) mathematicians for his pictures. I supported his election after it became clear that Sinai will not pass whereas Manin and Anosov were already elected. Nikolsky actively supported Fomenko.

The world began to change rapidly[4] and I spent a part of each year abroad. In 1992, when being in Maryland during the spring term I found out that Fomenko, in agreement with Logunov and Sadovnichy split up my chair. Before my departure he had kept silent about his intentions and it soon became clear that he needed a chair as a base for a new round of a large-scale attack on history.

Also then, in the spring of 1992, I had for the last time supported (very weakly) Fomenko during the new elections, already to full membership. I had sent an e-mail from the USA supporting Adyan, Anosov, Ulyanov and Fomenko but he was not then elected.

Upon returning home in 1992, a month after the election, I found out two sudden circumstances. First, the destruction of Fomenko's book on multivariate calculus of variations in the scientific literature. I bear in mind the review published by a celebrated American mathematician Almgren in the *Bull. Amer. Math. Soc*. He had indicated gross mistakes to Fomenko even before that book was published. M. Gromov, one of the best geometers in the whole world, publicly stated in Maryland that Fomenko had very deftly compiled his books: the beauty of presenting his results in introductions as well as in his public lectures has nothing in common with the abstract contents of his mathematical theorems of little interest.

The second novelty which I encountered in the summer of 1992 was that the publishing house of Moscow University had recently put out Fomenko's [Russian] book *Methods of Statistical Analysis of Narrative Texts and Their Application to Chronology*. It contained all the Morosov rubbish. The University acted carefully: it was stated that that book was being published at the author's expense.

The book opened by a comment written by the President of the *International Bernoulli Society*, Professor A. N. Shiryaev, who mentioned the author's achievements in mathematical statistics. Western probabilists to whom I had already shown the *History according to Fomenko* were surprised by that comment: they knew Shiryaev as a clever and reasonable man. Shiryaev himself told me that he had sent the book [its text] to three Western specialists, members of that Society, who provided positive references. During a long conversation, Shiryaev, however, admitted that he had actually sent them not the text of the book, but its English abstract in which the author had written about a certain statistical criterion and promised its application to history. Refutation of history was not mentioned. And now allow me to keep silent about Shiryaev's role[5].

During the latest 25 years I had many times encountered proofs of some rubbish (for example, that we directly assimilate nitrogen from the air, to say nothing about the various ridiculous theories in astrophysics). Each time some *near* mathematician provided a scientific basis by means of a *clever* statistical terminology. Regrettably, twice, as I recall, they were my colleagues, probabilists and statisticians from the Steklov Mathematical Institute. Statistics is such a science that it allows you to obtain the desired, you only need to insert the properly treated data[6].

Buchstaber is one of my best students in topology and an applied mathematician. When answering the request of really serious applied statisticians from the seminar jointly headed by himself and Aivasian, he came to them [as to members of that seminar] a few years ago with Fomenko and his theory and data, but, being Fomenko's friend, did not personally participate [in the ensuing discussion]. Fomenko had reported beautifully, but they asked him the data, studied them, then shrugged their shoulders: there was nothing to discuss.

In December 1993, during a conference on geometry in Tel Aviv, I staid with Lenya Makarov-Limanov, a mathematician of our school, and offered him a bet: thrice open at random Fomenko's book mentioned above, and each time I will find there something fantastic.

The host's son, a student, had that book, and the bet was on. Each time I indicated some identification: at the first instance, of ancient Assyria with Germany; then, of the Babylonian captivity of Jews and the Avignon captivity of the popes; and, finally, of a German chief Odoacer, who, in 476, killed the last West Roman emperor and the German chief [the Roman-German Kaiser] Otto I, the emperor of the German empire, who lived a few centuries later. I won the bet.

During the next years Fomenko published a current of books devoted to the refutation of history, and in some cases he continued to insert the Shiryaev introduction. His books began to appear as published by the University itself, by its mechanical and mathematical faculty or its educational and scientific centre, etc. History was refuted by mathematics! It was now his work rather than a hobby.

Fomenko successfully refuted Russian history as well. There was no Kievan Rus, no Mongolian yoke. Batu Khan was a Russian ataman Batia as well as Ivan Kalita. Dmitry Donskoi was also Tokhtamysh and the Kulikovo battle actually took place in Moscow with Mamai's headquarters in the Taganka street[7]. There were four Ivan the Terribles since it was impossible for one man to have eight wives, and one of them became Vasily the Blessed.

Fomenko gave many interviews about those discoveries to many newspapers. In the autumn of 1995 he revealed to *Komsomol'skaya Pravda* that, in particular, Isaac Newton had held the same views about the trustworthiness of history and repeated himself on other occasions as well. How can that be? In special works Newton studied whether Jesus was born exactly in year zero or a few years before that[8]. It seems that, according to Morosov – Fomenko, we should discuss the confusion between names and between dates amounting to about a millennium. Newton was doubtful about the truthfulness of the history of Egypt more than three thousand years ago[9]. (Englishmen are now ashamed to mention this; but recall, it was the beginning of the 18th century, when only the Bible had been considered truthful.) Even in a horrible dream Newton would have never thought of doubting the main events of the latest three thousand years.

And all that nonsense, as Fomenko declared, was proved by mathematics! In 1994, being actively supported by I. R. Shafarevich[10], he was elected full academician, and, in 1996, assistant academician-secretary of the mathematical class of the Russian Academy of Sciences [as it is now called]. During the discussion, his historical investigations were supported by academician V. P. Maslov. Now tell me please, why did his activity begin to help Fomenko's administrative-scientific career?

Notes

1. Contradiction in terms.

2. But what abut the consequences of his death? Oh, come on! The real history of mankind only began with Marx and Engels.

3. Later, in 1995, when the publishing house of Moscow University had published a current of Fomenko's books, which were devoted to the refutation of ancient history, he became, by order of the rector, the head of the mathematical department of the mechanical and mathematical faculty. Historical activity was included in the

curricula of that faculty. Will not the mathematical department become the laughingstock in the eyes of international science? S. N.

That rector was Logunov, a scientific swindler, a pigmy who attempted to refute Einstein, *the Jewish idol* as seen by the Party [x, § 4], by *the honour, mind and conscience of our epoch* … O. S.

4. Or, rather, Russia began to change rapidly.

5. In 2001, the yearly journal *Istoriko-Matematicheskie Issledovania* published a paper by Yu. V. Chaikovsky who invented the Jakob Bernoulli – Cardano law of large numbers without even providing a reference to the unsuspecting Cardano. The Editor explained to me that (that same) Shiryaev recommended (not communicated) that paper. I was a member of the Editorial Council, knew nothing beforehand and demanded to be struck off the list of those members. So I was, although two years later, no doubt to conceal any connection between my demand and the new discovery …

6. Many authors stated the same, but here is a warning (Chuprov 1922, p. 143):

Mathematicians playing with statistics can only be overwhelmed by statisticians armed with mathematics.

7. The *yoke* in *Mongolian yoke* is now questioned. The discussion of the place of that battle had nothing to do with chronology.

8. Indeed, it is now believed that Jesus was born 4 – 6 years before year zero.

9. Concerning Newton's chronological reasoning see Sheynin (1971, pp. 220 – 221). One of his commentators (Manuel 1963, p. 35) also implied that the astronomical method in chronology was largely due to him.

10. A pseudo-scientific Prince of Russian anti-Semites. Just imagine: As a nation, Jews lack creative power and Israel is a scientific emptiness (Shafarevich's *Triechtysyacheletnaya Zagadka* (A Puzzle Three Thousand Years Old). Moscow, 2006, pp. 399 – 401). And here is his mortal and apparently scientifically empty enemy: *Integrity is just as important as scientific merits.* Einstein, letter of 1933 to Emil Julius Gumbel, a German statistician (Einstein Archives, Hebrew Univ. of Jerusalem, 38615; perhaps published by now).

Brief Information about Those Mentioned

Almgren Frederick (1933 – 1997), mathematician

Gromov Mikhail Leonidovich (born 1963), a French mathematician of Russian extraction

Liapunov Aleksei Andreevich (1911 – 1973), mathematician, also worked in applications of mathematics to biology, a near relation of A. M. Liapunov. Two brothers Liapunov (end of the 16th and beginning of the 17th century) are known to have belonged to old gentry

Morosov Nikolai Aleksandrovich (1854 – 1946), revolutionary, scientist (worked in chemistry and astronomy). In 1882 – 1905 (for more than 20 years) did time as a convict in a political prison, the notorious Schlisselburg. His theories contradict historical facts (*Great Soviet Enc*., 3rd edition, vol. 16, 1974)

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XII

A. V. Byalko

Will we destroy the entire ancient world?

Priroda, No. 2, 1997, pp. 75 – 76

On 27 December 1996 a seminar *On the chronology of the ancient world and objective data* was held in the Russian scientific centre *Kurchatov Institute*. In his introductory address, its head, V. I. Kohan, Doctor of physical and mathematical sciences (Doctor phys., math.), reported about the unsuccessful attempts to invite academician A. T. Fomenko for a *symmetrisation* of opinions. Then reports were made by representatives of humanities and natural sciences who have to deal with the dating of events which happened hundreds and thousands of years ago.

I. S. Sventsitskaya, Doctor of historical sciences from the Open Pedagogical University, explained why historians do not consider that the publications of Fomenko on the revision of the chronology of the ancient world are scientific. G. S. Knabbe, Doctor of philosophical sciences from the Russian State University of Humanities, analysed the attempts to revise the generally accepted chronology as a social phenomenon. Yu. N. Efremov, Doctor phys., math. from the State P. K. Sternberg Astronomical Institute of Moscow University, showed that the longitudes of the stars in the *Almagest* catalogue were absolutely incompatible with the conclusions made by Fomenko. Apart from that, modern astronomical data on the proper motion of stars lead to the generally accepted date of that star catalogue by Ptolemy (the first century BC with probable error of about 300 years).

Yu. A. Zavenyagin, Candidate phys., math., described the absolute dating of the numerous occultations of stars by the planets and the Moon registered on the clay tablets in ancient Babylonia and the *Almagest*. A. K. Dambis, Candidate phys., math. from the Sternberg Institute provided the results of the analysis of the present places of 504 (not of 8, as Fomenko did) stars of the Ptolemy catalogue. Finally, in a short report I mentioned the latest works in the radiocarbon dating (see *News of Science* on pp. 32 – 33 of this issue).

The attitude of astronomers to the Fomenko attempts at shifting the generally adopted chronology by several hundred years was already discussed here (Efremov, in No. 7, 1991, p. 94), but no response of historians to the revision of their science by Fomenko was reflected either in our (!) journal or in the more popular press[1]. Why? A fragment of the record made here will explain it.

From the report of I. S. Sventsitskaya

In essence, historians have until now ignored the contributions of Fomenko, since we have no basis for a debate. The reason is that he dismisses not only chronology, but the entire ancient tradition and considers it a fantasy. Instead of the methods of working with historical sources accepted the world over, Fomenko attempts to create his own history.

When he only deals with chronology, astronomers are still able to find out whether his logic is correct. But how should historians react when Fomenko declares that the ancient cities Rome, Constantinople, Troy and Jerusalem were one and the same city? We would have to oppose Fomenko by that immense number of sources from those cities with which history is dealing, and this is senseless[2]. However, after his book (textbook?) was published by Moscow University and positive comments appeared in such a reliable newspaper as *Literaturnaya Gazeta*, we cannot just keep silent. And we, historians, wish to say that Fomenko's constructions are based on ignorance, pure and simple.

History is built not only on the chronology of kings or chronicles, but on an immense mass of sources which correlate with each other[3]. A man is living not only during the reign of some emperor, he is living in a house among domestic things, is using them, worshiping the images of deities. Historians take all this into consideration, they are familiar with the continuous evolution of the types of these things.

There are tens of thousands of inscriptions on stone. We may suppose that someone, in pursuing political aims, falsifies literary works, but it is impossible to imagine that such inscriptions can be substituted. And they are written in the language spoken at the time. Linguistics is a precise science which developed a system of the evolution of languages. The language of Herodotus very little resembles the language of Byzantine Greece which in turn had evolved into the modern Greek language. No gaps in this process existed whereas Fomenko maintains that the tradition had a gap in the 12th century when, in his opinion, Christianity had originated in Rome. However, the East Roman Empire preserved the chronicle of events beginning at least from the sixth century.

Furthermore, all local events are connected by the history of the world. We cannot deny the traditional European chronology since it is confirmed by other sources. Here are a few examples.

Fomenko does not deny the [former] existence of Aleksander the Great, but arbitrarily dates his campaigns a few centuries later than they actually occurred. There exist, however, Indian sources of the third century BC, dated inscriptions on stone, as well as letters of the still respected Indian king Ashoka addressed to Aleksander's successors and written in the Greek language of the time of Aleksander, in the language of the memorials of exactly that period.

Consider the connections of India and Rome. In the first and second centuries BC an animated trade began between them. A large number of Roman coins had been found, buried in India, which are dated by Indian parallel materials. We know the portraits of all the Roman emperors, we see their identical images on busts and coins scattered all over the great Roman Empire from east to its western provinces. It is impossible to falsify them.

Debates with Fomenko on a level of scientific proof are unthinkable and the international historical science does not perceive his reconstructions in earnest.

The general inference of the seminar was unanimous and left no doubts: the attempt to reshape the chronology of the latest two millennia has nothing in common with science.

Notes

1. Fomenko (Novikov [xi, § 2]) described how the complaint of three eminent historians was given the cold shoulder by the top echelon of the Party.

2. For opposing Fomenko it was quite enough to cite a few of his outrageous examples rather than providing thousands of documents. See also the highly relevant opinion of Gauss [xiii, note 5].

3. Correlation can be negative.

XIII

I. Grekova

Methodological peculiarities of modern applied mathematics

Voprosy Filosofii, No. 6, 1976, pp. 104 – 114

[1] Our time is usually called the epoch of the scientific and technical revolution and we are so strongly accustomed to that expression that barely become thoughtful about its sense. So it often occurs with words: they firmly weld together into some blocks which we perceive as an item and call forth associations and emotions rather than reflections. Nevertheless, it is sometimes useful to think attentively about the peculiarity of our epoch and ask ourselves what it requires from science. We intend to express some thoughts about the modern period in the development of applied mathematics, about the reformation of its methods and methodology as demanded by our time. To a large extent my considerations are debatable and represent my own point of view with which far from everyone agrees even among mathematicians who work in applications.

First of all, the term *applied mathematics*. Many specialists call in question its very right to exist, and argue that there is only one single mathematics, both *pure* and *applied*. Indeed, some branch of mathematics, often being applied for solving a practical problem, remains as it was in pure mathematics rather than transfers to its applied part. This point of view can seem convincing, but in essence it is wrong.

There certainly is no subject called *applied mathematics*[1], but there undoubtedly exist *applied mathematicians* who are occupied by applying mathematical methods to solve concrete practical problems. They partly spontaneously and partly consciously form an ideology of applied mathematics, its peculiar methodology, or, if you wish, its philosophy. When applying mathematics to solve a practical problem, a specialist is willy-nilly compelled to reconstruct his methods, methodological principles, methods of consideration and deduction, otherwise he just will not get things moving. During the latest decades this process of methodological reformation is going ahead especially intensively.

In our time, we are observing a universal mathematisation of all the branches of knowledge. Mathematical methods are ever wider introduced into practice; governing algorithms and the computers, which generate them, literary become a productive force. Technology, organization and planning are today unthinkable if lacking mathematics.

Once mathematics was a standard of abstraction. A literary image of a mathematician had been formed, of someone who does not care about anything taking place on our sinful Earth. We can at least recall the *Hymn to the scientist* of Maiakovsky[2]:

The red-eared [men in the street] *came* [in from the cold]

But he is unconcerned that they are becoming stupid and obedient.
Indeed, every instant he can extract a square root.

In our time, *extraction of roots* is not anymore a problem for humans (still less for mathematicians). Computers can *every instant* perform millions of arithmetical operations. But the psychology of *those who extract roots* is not yet definitely extinct. Thus, the French mathematician Diedonné, one of the leaders of the known Bourbaki group, wrote:

As a principle, modern mathematics in its essence has no utilitarian goal, it is an intellectual discipline whose practical use is reduced to naught … Mathematics is nothing but a luxury which the civilization can permit itself. …

(Sawyer 1966; quoted from its Russian translation of 1972, p. 18.)

Happily, this viewpoint is not often encountered, at least not in such outspoken manner[3]. Professional mathematicians (even representatives of, so to say, *sterile-pure* science) acknowledge that without practical requirements many sections of modern mathematics (linear and dynamic programming, theory of information, queuing theory, etc.) would not have appeared. However, once having been created, they are forming a vast field for the development of mathematical methods which often become seriously significant in the theoretical sense rather than remaining narrowly practical.

[2] Blechman et al (1976) properly considered the peculiarity, methodology and ideology of applied mathematics. They provided a detailed analysis and a comparative investigation of the most important features of the applied and the so-called *pure* mathematics whose traditions have reared many generations of the university mathematicians. The differences between those features are so serious that, for working in the applied field, a *pure* mathematician ought to re-educate himself. Habitual ideas about mathematical *rigour*; the sense of the notions of *existence*, *proof*, *definition*, *convergence*, *infinity*, etc., − all this should be considered and interpreted anew, and, so to say, deformalized, translated to a simpler, clear and wordly level. They indicated that the application of the so-called *rational* (or, otherwise, *likely*) considerations; the use of *eroded* rather than clearly defined concepts and of neither purely qualitative, nor clearly quantitative categories is necessary. The license to confirm theories by numerical calculations (by the so-called machine tests) is unavoidable. Their book described all these (and many other) peculiarities of applied mathematics in a lively, free and easy and often humorous manner.

Here is one of their expressions which apparently belongs to scientific folklore rather than to a certain author:

Pure mathematics achieves the possible by required methods, applied mathematics achieves the required by possible methods.

The features of applied mathematics as indicated above (apparently except the machine tests) have always been peculiar to it when mathematical methods were applied under any circumstances for the solution of practical problems. In our time, however, these features became intensified and now applied mathematics so sharply differs

from its *classical* version that this fact ought to be specially considered.

Its methods are so new and unusual that they often shock professional mathematicians. It is easy to declare, and is often actually declared, that the so-called *heuristic methods of solving problems, expert estimates, scales of preference* and many other suchlike terms and expressions are *located beyond mathematics*. However, to announce some method unacceptable but suggest nothing instead is not the best way out of the situation. Willy-nilly we have to apply all the currently available methods including such which would have turned our ancestral mathematicians in their graves.

[3] Let us trace the causes which exactly today gave rise to such a total profanation of the *sacred mathematical truths*. Mathematics is now advancing everywhere and invading all the branches of science. Apart from the traditional realm of its applications (physics, mechanics, technology [and astronomy]), practically all the other sciences (economics, sociology, psychology, linguistics, biology, medicine, criminology) are now becoming its customers. It is indeed difficult to mention a science which still does not apply mathematics. If such curiosity does exist, it will very soon probably experience the same fate.

Mathematical models are everywhere constructed and analysed, experiments are planned and treated by mathematical methods. Mathematics begins to occupy itself with such phenomena which have from time immemorial been only studied on the level of humanities. Game theory, for example, studies of conflicting situations; information theory, problems concerning the value and richness of content; the theory of statistical decisions, those concerning reasonable behaviour under indefinite conditions, quantitative description of risks; etc. A special chapter of mathematical statistics, the factor analysis, is applied for preliminary studies of complicated, vague situations with undiscerned structures of the appropriate causal relations.

Mathematical models of human collectives and the relations between their members and hierarchical structures etc. are constructed. In a word, mathematics with its apparatus, technology and methodology penetrates everywhere. Accordingly, the border between the exact sciences and the humanities is eroding and becomes hardly perceptible. For a long time their contradistinction, the demarcation between their methodology and the spheres of their influence had been habitual and the difference between them was clearly seen.

Indeed, which features had traditionally been peculiar for the so-called *exact* sciences? Distinctly formulated problems; quantitative conclusions; logical (more precisely, formally logical) considerations; use of clearly defined terms; wide application of the mathematical apparatus, and, consequently, some *unquestionable* deductions. A deduction is correct if the mathematical transformations leading to it were correct.

The traditional features of the so-called humanities had been different. For them, peculiar were verbal methods of investigation; a

wide application of analogies and convincing considerations; use of terms without a formal definition of their precise meaning; polemics; appeal to feelings and imagination.

However, before our eyes this traditional contradistinction is being ruined. The border between the exact sciences and the humanities is obliterated, the difference between them becomes vague and even partly disappeared. These branches of knowledge penetrate and enrich each other. Often (too often!) this process is regarded one-sidedly, as a pure and all-triumphant mathematisation of all branches of knowledge. Mathematics with its deductive constructions, axiomatics and formal apparatus is considered as some ideal specimen which all the other sciences ought to emulate. Mathematicians often feel themselves as conquerors: *Just wait awhile, we had been too busy until now, but we'll show you what's what*.

That conqueror agrees to consider any other science as such only to the extent of its being provided with formulas and expressed in the mathematical language, whereas all the rest, in his opinion, is just empty words, *vibration of the air*. But there is nothing more harmful and fruitless than that viewpoint. A forceful mathematisation of anything was never useful, it should occur naturally when required by the development of the appropriate science. In addition, and this is especially important, it does not occur unilaterally since this process is a mutual penetration of the two branches of knowledge. Mathematics penetrates the previously alien realms, *conquers* them, but it itself becomes transformed, less formal, less rigorous, changes its methodological features and to a certain extent approaches humanities.

[4] Let us question ourselves: wherefrom and why had the difference between the methodologies of the exact sciences and humanities appeared? Why had the formal mathematical apparatus been very early applied in the exact sciences but only quite recently (and only as an auxiliary) entered humanities? Was it because the specialists in humanities had been more *stupid* than those in the exact sciences?

No, not at all. Actually, the phenomena constituting the subjects of humanities are immeasurably more complex than those of the exact sciences, and it is much more difficult if at all possible to formalize them. Each kind of phenomena studied by the humanities has a much wider range of causes. Paradoxical as it is, the verbal method of investigation is there more precise than the formal logical way. But still, in many cases we are simply obliged to construct mathematical models here also; approximate or precise, for orientation in a phenomenon if not for achieving a definite answer to the formulated question.

Indeed, in our time (in the epoch of scientific and technical revolution) scientific studies of *organization and management* are insistently needed. Immense operations are planned and carried out; their scope, cost and possible consequences exceed everything seen previously. Great masses of machines, men and resources are set into motion. All these measures ought to be reasonably governed which is vitally necessary since they interest and influence the fate of both a separate country and mankind as a whole. Less admissible than at any

previous time are arbitrary or so-called *volitional* decisions. Bunglers, unreasonable and unconscionable people existed before our time as well, but, previously harmful, they are now dreadful.

For avoiding blunders and their serious after-effects it is vitally necessary to develop scientific methods of organization and management. A science of managing technical devices, the theory of automatic control, exists for rather a long time and undoubtedly belongs to the family of exact sciences. But where is the place for a science managing more complicated systems which include not only masses of technical devices, but human collectives and means of communication and information as well? Will it belong to exact sciences or humanities? Neither alternative will do. More precisely, its place will be in both these branches of science. The so-called operations research, the science of a preliminary justification of reasonable decisions in the entire realm of purposeful human activities, is also situated in a peculiar intermediate position between exact sciences, humanities and experimental sciences[4]. It is widely applying the mathematical apparatus, but is not reduced to it at all.

The rule, *Look before you leap*! is nowhere as justified as it is when large-scale, responsible decisions are meant. Mathematical models are an invaluable means for *looking before leaping*, for estimating the reasonableness and efficiency of decisions. They allow us to replace (at least partly) a laborious, expensive and rather dangerous experiment by *mathematical experimentation*.

However, for mathematical methods to become a fully-fledged means beyond traditional fields as well, mutual influence and enrichment [between those methods and the new fields] rather than their one-sided advance is needed. Applied mathematics, when entering a new field, should re-organize itself, draw up a new and more flexible tactic, formulate a new ideology. And this is indeed occurring before our eyes, although neither always nor everywhere and not obviously for everyone.

Along with specimens of really creative activity in the realm of applied mathematics we often encounter pseudo-applied work in which the traditional and sometimes very intricate and delicate mathematical apparatus is running idle. An applied problem is there only an occasion for showing fanciful mathematical exercises.

[5] So which features separate the real modern effective applied mathematics from the appropriate traditional classical science? First of all, the changed methodology, a new set of methods, a new pattern of approaching phenomena ought to be mentioned. Indeed, how was a *classical* investigation by mathematical methods carried out? A clear formulation of the problem was chosen, assumptions formulated and that problem was solved by irreproachable and rigorous formal mathematical transformations. If debates occurred, they had to do with either the correctness of the computations (if these are wrong, the work is ridiculed) or with the choice of the applied mathematical method. Arbitrariness, unavoidable in the formulation of the problem (if its precisely formulated conditions are fulfilled), is admissible only here and not discussed.

A typical example (a known pattern of a problem in mathematical statistics): Once assigned (note: arbitrarily), the confidence level, i. e., the probability of an event which allows to consider it as certain, is not discussed or complained about. Once we agree to consider an event with probability 0.99, say, as practically certain, all the following computations are carried out perfectly precisely and rigorously but it is even improper to ask wherefrom did that level appear. Roughly speaking, the tune of the reasoning is this. Let someone (an outsider) assign that level. And it is not our business to find out the why or the wherefrom; we only ought to answer the question: Given that level, does a certain hypothesis contradict the experimental data or not?

Another example. It is required to determine optimal management. Some parameter is chosen as the indicator of the efficiency (of the target function). Then, by absolutely rigorous methods the version of management that maximises (minimises) the target function is determined by absolutely rigorous methods. But on what grounds and who had assigned that target function? Not our business! It was assigned, and that'l do.

This classical pattern of investigation which separates the *customer* and the *executor* is dating before our eyes. For a modern applied mathematician another pattern is typical: *a personal union of both.* A modern applied mathematician (or a group of such specialists) who solves practical problems should invariably participate in their formulation, but not only in their solution. And not only in constructing models, but also in the choice of the target function, organization of the computations, in comprehending the result and formulating recommendations. In a word, applied mathematicians should not be afraid of *soiling their hands,* otherwise they are not needed by anyone.

The distinctive features of a real applied mathematician are an attentive attitude to the needs of practice; the readiness to penetrate into the details of a real situation and to gain its understanding. How exactly is he given a problem by a practical worker who needs help? Verbally and most often by a vague description. Suppose that an engineer from a workshop turns to him: hitches and bottlenecks occur there and they should be got rid of; I leave aside a rather often case when the practical worker just needs to defend a thesis.

How to manage the available resources, which string should the specialist pull? The practical worker has only vague indefinite complaints and resembles an ill person who quite naturally does not know what happened to him. We certainly do not expect him to come to his doctor with a ready diagnosis.

Nevertheless, pure mathematicians of the classical school often require from practical workers a prepared and clearly formulated problem. It is not their business to pose problems, but rather to solve them as formulated. This is an extremely faulty point of view. An applied mathematician is an *applied* scientist exactly because he ought to *solve* problems and to pose them all by himself. In applied fields a properly formulated problem is tantamount to solving it by more than a half, the rest is more or less achieved by transformations or calculations.

A real applied scientist should be able to discern the main points in a real situation, separate them from the collateral, the minor, to detach the mathematical skeleton from the living reality, to find out from the practical worker what actually does he need, sometimes even to explain to him what exactly is essential for him. While being constantly connected with him, to construct a mathematical model, direct the pertinent calculations, personally participate in analysing the results and formulating recommendations. In a word, to work with rolled up sleeves and forget about his *class honour*. A man not prepared to penetrate into the essence and details of real processes cannot and should not engage in applied mathematics. *Those who live in glass houses shouldn't throw stones*!

Here is one more essential difference between classical and modern applied mathematics. The former usually chooses a single mathematical model and only once, in the very beginning of the investigation, formulates the assumptions whereas all the rest is deduced by formal transformations. Beyond traditional fields this is not so. For perceiving a complicated phenomenon it should be considered from various sides, under different viewpoints, and the results should be compared and debated[5]. It is often beneficial to return to the model, correct it after the first round of calculations is completed. Moreover, it is often beneficial to compare models, that is, to describe one and the same plan by a few models either one after another, or simultaneously.

It is extremely important to reveal the *stability* of the results (recommendations) with respect to the model. If the conclusions do not change or change only insignificantly after the models or the methods of investigation have changed, we have a forceful argument for their objectivity. Such tricks are still regrettably unusual. Science has developed the notion of stability with respect to small perturbations or disturbances, but, as far as I know, not with respect to viewpoints.

So what shall we do when being unable to find a duly stable solution? This can mean that the problem is not yet ripe for being scientifically solved, or that the available information is not sufficient for formulating it. But even then a comparison of results and recommendations obtained by different methods can assist in perceiving the situation and in ensuring, after debates, an acceptable compromise point of view.

[6] The methodology of scientific debates (*debates beget truth; du choc des opinions jaillit la verité*) previously completely alien to mathematics, is peculiar indeed to modern applied mathematics. Participants of seminars and conferences on applied mathematical problems barely debate over the methods of solution, but almost always about *formulations* of problems, and rather often they conclude by a rapprochement between points of views. Debates often occur over the understanding of *optimal solutions*. Classical mathematics is also familiar with optimisation problems, but only under ideally clear formulations when the solution ought to maximise (minimize) one single scalar magnitude (the target function).

This ideal pattern is extremely rarely encountered in real, and at least in sufficiently complicated problems. Almost all of them are multicriterion problems with vector target functions. We desire to maximise one criterion and minimise another one (for example, maximise gross output, minimise the wage fund and maximise profits). As a rule, these requirements are contradictory and there exist no solutions satisfying all of them at once. Attempts to unite several criterions into a single generalized criterion are usually ineffective, often even harmful since they create an illusion of a non-existing scientific substantiation. Here, we have to look for a reasonable compromise, to act as though bringing various viewpoints into agreement, like *running with the hare and hunting with the hounds*.

In such situations mathematical methods of optimization, as perfect and refined as they are, provide but little help. As yet, there does not exist any mathematical *theory of compromises* of full value. Some attempts have been made in the theory of statistical decisions, but with respect to viewpoints they usually provide strongly unstable results. Until now, practically speaking, compromise solutions are only speedily and successfully provided by the human mind, by the so-called common sense. A human being is yet the matchless master of compromises and without him we are still unable to choose any solution of a multicriterion problem (be it not optimal according to any single criterion but admissible under all of them taken together).

Modern mathematics can only perform with notions *larger – smaller – equal*, but not with *admissible*, *of practically of the same value*, etc. which are typical for a human mind. Not every *better – worse* can apparently be reduced to *larger – smaller* (or, if that be possible, we are often unable to carry it out). When choosing a decision, a man, without going into unnecessary details, glances over the situation as a whole and rests on an admissible alternative. On such occasions the duty of mathematicians, however, is to help the man rather than to choose a definite decision, to provide him with the maximal amount of information displayed in an expressive and easily grasped form; to show him the consequences of each possible version of a solution according to various criterions but to discard beforehand those which are non-competitive.

In new, unstudied situations or when dealing with untried measures, such mathematical modelling can often substitute for the insufficient human experience. Furthermore, experience can be *transmitted* from a man (or collective) skilled in making decisions to a machine, to an automaton capable of gradually elaborating a formal algorithm for selecting a decision (the so-called adaptive or learning algorithm). Any means quite remote from the mathematical tradition (for example, expert estimates, voting machines etc.) can be applied although without idolising them, without declaring that the answer is gospel truth. The problems are alive and kicking, and so are their solutions; they are modified, they reject one another just as it should be.

Note one more circumstance. In traditional mathematics [but not in astronomy], once the problem is formulated and the assumptions listed, its solution is always sought on the maximally attainable level

of rigour. On the contrary, for the modern applied mathematics it is usual to require the same robustness of all the sought elements. The precision of the apparatus should conform to that with which we can obtain the initial data. If, when computing according to a certain model, we ought to insert some parameters and functions which will remain unknown in the visible future, we should reject that model, replace it with another one, let it be less precise but based on available information.

[7] And what about the information supposed to be known in a mathematical model? This is one of the sore spots on mathematical works which pretend to be applied but are actually nothing but abstract exercises. Such investigations begin with a classical expression: Some parameters (they are listed) are supposed to *be given*. By whom? Wherefrom? No such question is even posed. Given! Then models are constructed, but they may only be called *informationally deformed*. Take for example the classical model of a conflicting situation, a two-person antagonistic game. It is supposed that in such games each participant knows perfectly well all the strategies (all the methods of behaviour) which his opponent can apply although it remains unknown which of them he will select in each round. Yes, certainly, the thus appearing mathematical theory is elegant, it allows to recommend the proportion of the strategies which each participant should apply to ensure maximal gain. But let us ask: how do we happen to know the entire set of the possible strategies? This hardly ever occurs in practice, and, as a rule, in a conflicting situation it is reasonable to go beyond the strategies which are known to the opponent rather than to change them according to intricately deduced proportions. Is this not the reason why the game models, which many excitingly snatched at, proved comparatively poor in real situations?

Another example. The known problem in mathematical statistics: to determine the confidence interval when only a small number of trials were made. A rather delicate method was worked out. It was assumed that the law of distribution of the observed random variable is known and is normal. And once more the same question: Wherefrom do we know it? And how precisely? And, in addition: what is the practical value of that interval? A small number of trials means little information, and we are in a quagmire. It is unimportant whether the confidence interval is somewhat longer or shorter, the less so since the confidence level was assigned arbitrarily. Nevertheless, much attention is undeservedly devoted to this problem. Here we have an obvious disparity between a crudely formulated problem, conclusions of small value and a subtle apparatus. In general, a misuse of the formal aspect of the probability theory at the expense of common sense is the bane of many pseudo-applied work in which the mathematical apparatus is not the means, but the aim. An interesting though not indisputable booklet Tutubalin (1972) offers pertinent considerations[6].

The application of the probability theory when statistical stability takes place and necessary information is available, is quite justified. Not so when there is no information, In such cases, problems

(choosing a decision under total indefiniteness [ignorance]) belong to the theory of statistical decisions. We do not completely deny its usefulness since it allows us to make some guesstimates, but its possibilities should not be exaggerated. When information is lacking, the solution is invariably *bad* and we ought to try to obtain the necessary data rather than sweat over its substantiation[1], especially since in some cases a successful choice of a decision requires a rather restricted amount of information (Diner 1972).

Indeed, we should never forget that for an investigator lack of information is trouble rather than advantage, although exactly in such cases he has the occasion to show off most refined methods. Reasonably formulated problems should be solved comparatively easily, and it is mournful to see how mathematics sometimes begins to stun common sense. When choosing between *mathematics lacking common sense* and *common sense lacking mathematics*, we certainly ought to choose the latter, although best of all if both mathematics and common sense are present, when common sense invariably checks mathematical computations.

That, however, does not happen always at all. The mathematical apparatus possesses some hypnotic power and investigators often tend to trust unreservedly their computations, to trust the more intensely, the stronger was the flowering of the applied apparatus, the more time (their own, and machine time) had been spent , the more paper used up. At present, when mathematics is fashionable, when information is provided in formulas and flows freely, it is very difficult to distinguish the real from the apparent, real science from its fake.

[8] Too often the application of mathematical methods is understood as a pure and absolute boon. It is thought that any mathematisation is a step forward and the more so when accompanied by automation. Take for example the celebrated automatic system of management, the ASM. These words and the associated notions are already intertwined and formed a single stable block with a large *plus* above it. ([In the Soviet Union] the block *cybernetics* had formerly been estimated by a large *minus* then frantically replaced by a *plus*.)

In a burst of unrestrained enthusiasm the ASM's have been all but idolised, considered as some panacea for any troubles, for mismanagement, improvidence or stupidity pure and simple. And note: the main attention is drawn to the letter A of that block, of the ASM, to automatization. It is thought that a computer inserted into the process of management is a blessing in itself, the modern *technical grace* that replaced the dated *grace of God*. The creation of an ASM usually begins by acquiring a computer and recruiting a serving personnel, but how about the rest? Oh, the rest will appear and the main problem is already solved! Another tune will be heard, forests and mountains will join the dance[7]! So what happens? The computer is installed, the programmers are working, rolls of paper are expended, but, alas! The forests and mountains remain motionless ...

We ought to face the facts and recognise that, until a phenomenon is not assimilated on the level of humanities, the application of mathematical methods is harmful rather than useful. Harmful, since they divert our attention from the essential to the minor, they pave the

way for eyewash. The greedy attention paid to the letter A (see above), is the result of foolishness and haste; in itself, it is not needed by anyone, only needed for the M. Nevertheless, many people think that the main point of the problem of management is the collection and treatment of the information. Its amount is large, so this task should be assigned to the computer.

Often this, in essence an auxiliary procedure, is pushed to the forefront, regarded as an absolute truth. The main question, what kind of information should be collected and treated, is thrown overboard. It is decided beforehand that any information is a boon and that the main goal of an ASM is to achieve the possibility of retrieving it from the computer and displaying it at a moment's notice. Exceptions are rare.

Collection and treatment of information is another such block with a large *plus* over it. But is this so indisputable? Should we collect, treat, store any information? Certainly not. A human mind is incapable of grasping and comprehending at once a large amount of information. It should be first prepared, the essential separated from the secondary, the needed from the unneeded, and the former ought to be presented in the most expressive and easily digested form. All this is a task of applied mathematics as well, but this time the task borders psychology and sociology.

Much is said and written now about the so-called *large systems*. The meaning of this term is not precisely known and sometimes a tautological definition appears: It is *a system which consists of a large number of elements mutually acting one on another*. In itself, a lack of a clear definition is not yet a serious evil, and a specific *yearning for definitions* often seen in scientific investigations is only a tribute of respect to the deductively constructed classical mathematics in which each notion is either rigorously defined by other concepts or introduced as an axiom (without definition). For humanities and related sciences (to which, as I noted, belongs applied mathematics) typical is the use of vague, eroded notions. They are introduced by a series of talks apropos, which throw light from various viewpoints on the studied, rather than by one single, clear definition.

And so, when discussing *large systems*, we may offer one more definition (neither the only possible, nor the definitive): *The managing centre of a large system does not need complete information about all its links; moreover, it is harmful.*

It is about time to stop praying to information of all kinds. It can be needful and useful or unnecessary, jamming and thus complicating management. We should decisively cut off the unnecessary, parasitic information and only deal everywhere with the unquestionably needed. This most important informational side of management ought to be investigated (and until then it is too early to discuss the creation of ASM's). Here, mathematical models can once more be very useful by comparing the quality and rapidity of management in a cumbersome system overburdened by information and a simpler system only dealing with useful information.

[9] One more important circumstance. When having a large system, we should not forget that they usually include human beings and their collectives, and we ought to take into account the specifics of

experiments on people. Here we observe something akin to the Heisenberg *uncertainty principle*: the experiment itself unavoidably influences the course of the studied phenomenon. Similar peculiarities also accompany all possible experiments on people and their collectives.

Pure experiments are here impossible in principle since the experiment all by itself influences the studied process, and biased conclusions do occur. As an example, I can at least mention experiments on new methods of teaching (programmed teaching, application of technical devices etc.). Until such teaching remains an amusing novelty, the students are curious and the success of the experiment is evident. However, once the learning becomes dull, its efficiency disappears. Another example: sociological testing. A *typical* group of subjects is rarely correctly selected and the questions which they are asked can often influence their state.

Those accustomed to the methodology of exact sciences often uncritically transfer the methods of arranging and treating experiments as developed in that field onto experiments on people. In particular, they pay much attention to a *correct* application of the apparatus of mathematical statistics. Actually, however, important is not the apparatus (which can be elementary) but a sensible and sober discussion (on a good level of humanities) of the methodology of the experiments as well as an impartial and careful comprehension of the results obtained. The mathematician participating in the investigation should not remain remote from these problems.

[10] Modern applied mathematics is a science of a special kind located on the border between the exact sciences, humanities and experimental sciences. It boldly uses efficient methods and tricks developed in each of these types of science. It can only be of such nature and perform in that way if it desires to interfere actively with life rather to contemplate abstract notions.

Notes

1. This is a strange statement. Grekova devoted her paper to this non-existing subject, and in §§ 4 and 10 called it a special science situated on the border between the exact sciences, humanities and experimental science.

2. Vladimir Vladimirovich Maiakovsky (1893 – 1930) was an eminent and extremely unusual poet. My school fellow, the late Dorian Rottenberg, had translated selected verses and poems of that poet (*Poems*. Moscow, 1972).

3. However, the viewpoint of the Bourbaki group is probably still alive. Pontriagin (1980) justly and strongly criticized Kolmogorov's introduction of abstract notions in the school curriculum. See also Novikov (2002, pp. 326, 334 – 335 and 347).

4. Grekova mentioned experimental science here and in § 10, but it deserves more attention: such *pure* sciences as astronomy (which only I myself mentioned a few times in square brackets), geodesy and even physics have most important experimental parts. An experiment refuting some physical theory would have compelled physicists to abandon or restrict it. Then, Grekova's statement (§ 6) about maximal rigour does not at all apply to experimental science.

5. Here is the pertinent opinion of Gauss (*Werke*, Bd. 12, pp. 401 – 404) as described by W. W. Weber in a letter of 1841: Applications of the theory of probability can be greatly mistaken if the essence of the studied object is not taken into account.

I had not found any definition of statistical stability (cf. a few lines below). It apparently means that the errors of the pertinent observations obey a single law of distribution.

6. Note a wrong conclusion (Herschel 1817/1912, p. 579): the size of a randomly chosen star will not much differ from the mean size of them all. He knew nothing about either the sizes or the different spectral classes of stars. And *ex nihilo nihil fut*! The sizes of the stars differ tremendously and their mean is a purely abstract notion.

7. Grekova is quoting Ivan Andreevich Krylov's (1768 or 1769 – 1844) fable *The Quartet* expressed in verse. The four musicians were: a pretty monkey, an ass, a he-goat and a pigeon-toed bear.

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XIV

A. Orlov

On the perestroika of the statistical science and its application

Vestnik Statistiki No. 1, 1990, pp. 65 – 71

The discussion about applied statistics in this periodical [1] showed that some of its participants were ignorant of elementary facts. Sheremet wrongly mentioned a renown journal *Annals of Statistics* (before 1973, *Annals of Math. Statistics*). Commenting on that discussion, *American Statistician*, an organ of the American Statistical Association, justly admonished *Vestnik Statistiki* for a lack of editorial correction [2][1].

We cannot anymore endure the state of our statistics caused by the heritage of the personality cult. The divide in statistics, the lack of necessary knowledge suffered by many specialists leads to an ever greater lagging behind the advanced countries with respect to mass application of modern statistical methods. A radical turn of the statistical work is necessary, and for justifying this proposition I begin by studying which professional groups are applying the term *statistics*.

1. How many *statistics* are there in the Soviet Union?

We ought to name, *first of all, the departmental science of the State Committee on Statistics* (SCS)[2] with its developed infrastructure: this journal, a speciality included in the list of economic sciences compiled by the Higher Attestation Commission (HAC)[3], courses and chairs of statistics in higher economic educational institutions. Regrettably, beginning with the 1920s, the scientific level of all that essentially lowered. Thus, the paper of Smirnov [3], a corresponding member of the Academy of Sciences, which was published in 1929, would have been rejected by that periodical in the 1980s owing to its excessive mathematical complication. When examples of a wrong application of statistics are needed, I usually look for them in the textbooks on the general theory of statistics [4] whose ignorant authors are training new ignoramuses to replace themselves.

Ignorance certainly depreciates practical work as well. Is there any point in complete registrations, general censuses[4], when it is well known [5] that there will be mistakes, that the economic/statistical indicators can only be determined up to 5 – 10%? Note that the [estimates of the] errors in the reports of the State Committee on Statistics (SCS) are not provided owing to the low scientific culture of their compilers. It can be expected that a rejection of those complete registrations and a

transfer to sampling will essentially decrease the expenses and heighten the precision of the conclusions. Then, in the field of statistics, the Soviet Union will catch up with India. Industry had long ago realized this [6]: the quality of the production is checked by sampling rather than complete registration.

The *second statistics*: mathematical statistics. According to the HAC list, it, along with the theory of probability, belongs to mathematical specialities. There are pertinent scientific units and chairs in higher educational institutions, the journal *Teoriya Veroyatnostei i Ee Primeneniya*[5] and other periodicals. This is the only internationally competitive national *statistics* which is situated on the modern scientific level. Its competitiveness was indicated by the First International Congress of the Bernoulli Society of Mathematical Statistics and Theory of Probability[6] since it was held in 1986 in the USSR (in Tashkent) [7].

We have several hundred qualified specialists in mathematical statistics, but their work is concentrated in internal mathematical problems and does not yet essentially influence other fields of knowledge.

The *third statistic* is applied. It is a methodical discipline which includes applied mathematical statistics (directed at the application of the part of mathematical statistics seen from *beyond*, by specialists in applied science, rather than from *within*, from the point of view of mathematics itself), with its computer programmes and methodology of application [8].

During the recent years applied statistics has been actively developing in spite of difficulties (lack of periodicals, of an institute, of a speciality in the HAC list etc.). Its methods provide a large technological/economic effect [9], but its scope is several times less extensive than that in the developed countries.

It is strange that the authors of some papers in this periodical are barely acquainted with the term *applied statistics*. This goes to show our alienation from the international science. There are at least five periodicals abroad [10] whose titles include that term. An index [11] of the journals devoted to the theory of probability, mathematical statistics and their application contains 345 items in the Roman, and 25 items of serial items, in the Cyrillic alphabet, and papers on applied statistics are published in most of them. There also exist textbooks, reference books [13] and a scientific literature [14] devoted to that same discipline. But to which discipline is the applied statistics *applied*? Certainly, to the theoretical [15], i. e., mathematical statistics[7].

The *fourth*, very much neglected *statistics* is the application of the statistical methods in industry, medicine, biology, geology, social/economic fields and other branches of the national economy and science [to astronomy and meteorology as well]. As a rule, the level of its infrastructure is low: there are no pertinent specialities in the HAC list, and the

professional life and education are badly recognized. Strange names are therefore used; thus, the application of the statistical method in medicine is often screened by *medical cybernetics* and, dissertations on applications to industry are defended as belonging to standardization and *management of quality*. The only journal, which regularly publishes papers on applied statistics, is *Zavodskaya* (Factory*) Laboratoriya* which has a section on *Mathematical methods of Investigation*.

Managers and specialists are often ignorant of the significance of statistical methods. I have encountered an obvious shortage of works on general problems of the application of modern statistical methods which compelled me to publish myself [16]. The scientific level of many workers pursuing applied statistics is also low. Mistakes even in the state standards on statistical methods were revealed [17].

Primitive tricks [18] often hamper the introduction of modern methods. For example, a comparison of two groups of patients is traditionally been accomplished by the Student test which prevents the introduction of non-parametric methods [19][8]. In general, the scope of our [statistical] work is about ten times narrower than in the USA (an expert estimate).

It is also natural to include here the departmental science of the SCS with respect to the application of statistical methods to economics or *economic statistics*. In the beginning of this paper, I considered it separately [?] since, owing to departmental narrowmindedness, many readers of this periodical had not even suspected the existence of other *statistics*.

And so, I believe that the set of statistical disciplines consists of theoretical (mathematical) statistics, applied statistics and application of statistical methods in various braches of knowledge (industry, medicine, biology, geology, sociology, economics, psychology, history, etc. [astronomy, meteorology]). I consider the applied statistics as the centre of this set, as justly stated by Kotz & Smith [20] who summarized my views [?].

The wrong attribution of statistics to social science essentially harmed the development of our national economy. An obstacle was thus placed between modern theoretical (mathematical) statistics and the organs of the Central Statistical Directorate, later the SCS, whose activity had almost been reduced to registration. Specialists in statistical methods in industry, medicine and all other branches of knowledge even found themselves *in the underground* whereas in the American Statistical Association they constitute a majority [21]. It is about time to correct that mistake.

2. The causes of the backwardness of our statistics

The main cause is obvious: for a commanding administrative system a wide and trustworthy statistics is not necessary; moreover, it is harmful. Even if collected, it is often

labelled *secret*. That was the method chosen from the end of the 1920s for covering economic and political failures. Only in the years of the perestroika had that cover of secrecy begun to open slightly, and at once a number of instruments applied for the falsification of the statistical data became revealed. They allowed the creation of an illusion of prosperity; periodic editions are [now] filled with such revelations [22], but secrecy does not restrict the topic.

In West and East (for example, in Japan), industrial firms usually very actively study the market by statistical methods, these methods are applied everywhere for managing the quality of [mass] production [23]. (During the latest years the Taguchi methods [24] had been propagating explosively.)

Why, however, a Soviet enterprise should study the market since the plan of its work was sent out from above? Why heighten the quality since it reports in the gross? In general, in spite of year-long entreats/appeals to raise the quality of production, in general it remained on an extremely low level. Why comply, when shortages are compelling us to be satisfied?

And now I come over from the economic to methodological (ideological) causes. For many decades the students had been taught determinism (*science is the enemy of chance*). The ignorance revealed in the section on *necessity and randomness* of philosophical textbooks causes specialists in the theory of probability to *laugh healthily*, but they had been doing their black deed: they maimed the students' minds (just like the mistakes in the textbooks in the general theory of statistics did). Indeed, if a man is *a tiny screw*, what can be random in his behaviour? Any randomness ought to be suppressed as being socially dangerous. Let us recall the slogans of the 1940s and 1950s:

Cybernetics is a bourgeois pseudo-science; Genetics is a venal strumpet of imperialism.

Both cybernetics and genetics actively apply statistical methods, a risky kinship for those times!

Monopolism and departmental restrictions dangerously oppose the introduction of modern statistical methods. If some institute is leading in a certain subject, why should it introduce the modern methods of the design of experiments? It will do with ancient elaborations and suppress possible competitors by its authority. If the SCS monopolized the collection of statistical data, why should it introduce modern methods of their analysis? It reports to officials for whom tables of the 1889 type [usual a century ago] are more easily understandable.

The role of the physical and moral terror against statisticians was great. Suffice it to recall the arrest and death of those who in 1937 had carried out the all-union census of the population and the persecution of academician V. S. Nemchinov, an economist and statistician, who was dismissed from his

rectorate of the Timiryasev agricultural academy because of his support, in 1948, of geneticists. The scope and the consequences of the terror are not yet studied.

And so, there are quite enough causes for the catastrophic backwardness of our statistics as compared with the international level. It is characteristic that a more or less normal position is now only observed with respect to mathematical statistics. Mathematics has traditionally been *foolproof*; administrators usually had enough common sense for refusing to dream of putting that so specialized field in their own order. Academic freedom had been traditionally ensured, including the right of creative competition. It is impossible to ask, why are you pursuing subject X whereas the institute is working at subject Y? However, such questions are usual in other professional regions. Freedom, however, is paid for by lack of financing; most works in mathematical statistics is carried out voluntarily since their authors are paid for other duties, as a rule, for teaching or applied work. And there had been no stimuli for introducing mathematical results.

One of the essential causes of the backwardness of our statistics is the lack of probability theory and statistics in the school curriculums[9]. For more than 30 years, in Japan, the USA and other countries the elements of those sciences are being taught to the school students [25]. Here, those elements are being only taught to students [of institutions of higher education] of some specialities. *For them*, statistical methods are habitual, *for us*, exotic.

3. The perspectives

The rejection of the commanding administrative methods, the policy on economic independence of enterprises, on the elimination of the budgetary deficit, announced at the 19th all-union Party conference, raise hopes for the future economic need to introduce widely modern statistical methods. Only hopes! Nowadays, we have too few specialists in national economy who are able to understand what *statistical methods* mean.

Only after introducing the subject *statistical methods* in the secondary school curriculum, as it is done in Japan and the USA[10], we may reckon on their wide sensible application. Not without reason those methods are applied in Japan, mostly in working *groups of quality*, as a principal means for raising quality. Their elements ought to be taught in school rather than in workshops, and only then we will succeed in applying meaningfully the international standards of the systems of the quality of products [26].

The fixed image of determinism ought to be done away with and the elements of the *science of the accidental*, i. e., of probability theory, should become generally known. Such problems take decades to solve, and, according to an expert estimation concerning the knowledge of those methods, the

Soviet society as a whole is in the same position as the American society was in 1959.

Computers give hope of an accelerated development. Sets of programmes, including teaching programmes, and future expert statistical systems (will) provide the users technologically supported possibilities of rapid and sensible treatment of statistical data. Formerly, it was labour-intensiveness of calculation that impeded the extension of the scope of the application of statistical methods.

Nowadays, we can enlist under contract highly qualified specialists for compiling methodical instructions and programmes. This will partly overcome the mathematicians' lack of interest in the practical realization of their ideas and ensure a high scientific and technical level of the production. Thus, on the instructions of the All-Union Research Institute of Standards, the joint Soviet-French-Italian enterprise Interquadro developed a vanguard plan for sets of methodical documents and programmes for statistical methods of standardizing and managing quality. That plan was compiled by a collective of 23 specialists working under contract, among them 9 were doctors, and 14, candidates of science. They were workers of 11 leading scientific institutions, and the reviewers were doctors of science. This allows us to hope that the new *generation* of documents, unlike the former [27], will be free from mistakes. This plan also indicates the possibility provided by abandoning the previous pattern when some suitable institution all by itself develops a certain product. At present, when, in particular, there is no institute of applied statistics, a leap can only be ensured, in our opinion, by a flexible system of provisional creative collectives working under contract.

Theoretically, Soviet specialists in mathematical statistics are on the forefront of international science. For realizing the theoretical potential and catching up with the USA and Japan with respect to the practical use of statistical methods, those powerful instruments of an engineer, scientist and economist, we need to take active managerial and economic measures.

4. The priority tasks

Perestroika of statistics is a part of the perestroika in general, and its requirements are the same: glasnost (publicity) and democracy[11]. Its first period consists of a wide and deep discussion of the problem, and it had already begun by the round-table discussion of *Statistics and the Perestroika* which took place on 2 March 1989 by the editorships of the journal *Ekonomika i Matematicheskie Metody* and the serial edition *Ucheniye Zapiski* [Scient. Trans.) *po Statistike*. The aims of the discussion were to reveal the actual situation in statistics, the general and the special features in the statisticians' viewpoints on the expediency of concrete measures and to select preliminarily creative collectives for some projects, see that journal, No. 5, 1989.

The above makes it clear that I reject the decisions of the all-union statistical conference of 1954 since they hamper the perestroika. To repeat (end of § 1), in particular, statistics is not a social science. It is necessary to establish an analogue of the American Statistical Association which, even in 1970, had more than ten thousand members [28]. That analogue can be a Soviet statistical association (similar to the Soviet Sociological Association) or an all-union statistical society, a member of the Council of the Soviet Scientific and Engineering Societies, or a development of the existing Soviet Committee of the Bernoulli Society which is a part of the International Bernoulli Society of Mathematical Statistics and Probability Theory, etc.

Unlike scientific or technological societies of the period of stagnation, a unification of temporary creative collectives dealing with statistical plans (cf. above), can become a real economic force. However, centralized decisions and financing is needed, for example, to introduce a course in statistical methods in the curriculum of the secondary school. It is necessary to prepare such decisions.

One of the previous misfortunes was the self-isolation of mathematical and applied statisticians from scientists of other specialities. Along with the development of science, the solution of professional problems demands ever more labour and specialisation and their discussion in a professional circle is expedient. That circle is sufficiently wide, and there are little stimuli for stepping beyond it. Apart from the publications of Gnedenko [29], Tutubalin [30] and some others (in particular, of the excellent popular book [31]), there are hardly any contributions on the general problems of modern statistical methods.

An attempt to estimate the economic effect of applying statistical methods was only made by Gnedenko and me [32], and this is a symptom of self-isolation [?]. Agitation for applying them is necessary.

It is possible to indicate additionally a number of necessary measures, but I will not predetermine the results of *a wide and deep discussion* of the first period of the perestroika (beginning of § 4) of the entire statistics.

According to my estimate, the application of statistical methods can provide 20 – 30 thousand million roubles annually. At present, the expenses for the statistical analysis of data are estimated as 2 thousand million roubles annually [33].

5. A centre of statistical methods and informatics [and an all-union statistical federation]

In February 1989, on the basis of the collective of the project Interquadro (§ 3), a centre of statistical methods and informatics was established. The main aim of this public institution is the introduction of modern statistical methods by sets of programmes. In 1989 – 1990 the Centre has been disseminating sets of programmes for applied statistics,

statistical quality control, statistical regulation of technological processes, in particular by control charts [34], for the design of experiments (including a dialogic teaching system based on a known textbook [35]), reliability and testing of production, and application of statistical methods in medicine.

The production of the Centre is oriented towards non-professional users. A number of the latest elaborations of the Soviet school of probability and statistics is included in those sets which are means for the work of engineers, medical men, economists and scientists and allow the treatment of the results of observations (measurements, checks, trials, analyses) on the modern scientific and technological level and the derivation of trustworthy scientific and practical conclusions.

For ensuring a wide usage of the sets of programmes for modern statistical methods, it is necessary to create a ramified net of centres for training, introducing, accompanying, consulting. The need to create a service of statistical consultations was proposed almost twenty years ago [36].

The general meeting of the Centre which took place on 31 March 1989 decided that a creation of an all-union statistical federation was necessary. Its main aim for the nearest future will be to overcome the catastrophic backwardness of the applications of statistical methods in various branches of national economy and science as compared with the developed countries (Japan, the USA).

To fulfil this aim a unification of the efforts of the statisticians working in various ministries and departments is needed. Members of that federation can be statisticians working in theoretical (mathematical) statistics, applied statistics, in applications of statistical methods in industry, medicine, geology, biology, economics (including the SCS), sociology, psychology, and other branches of national economy and in various fields of fundamental and applied science.

Central and regional organs of the federation as well as its associations of various branches of science should develop and introduce modern statistical methods based on the relevant programmes. That work should include instruction, consultation, introduction and escort, establishment of standing seminars, regional, all-union and international conferences, symposia, issue of methodical, scientific and popular editions, certification of programmes for statistical methods, attestation of specialists.

Since the Soviet Union occupies one of the first places in the world with regard to theory of probability and mathematical statistics, the federation ought to ensure a relevant elevation of the scientific and technological level of applied statistical works in various branches of national economy and science. The constituent conference of the federation will take place in 1990.

The attempts to tear Soviet statistics away from international science are doomed to fail. It is about time for the enemies of the perestroika to think about the country's and their own future. On 25 April 1989, in his concluding speech at the plenary session of the Central Committee of the Party, Gorbachev said:

You cannot look at the current processes by yesterday's eyes, cannot only appraise them one-sidedly. An honest and principled political analysis is necessary. The only correct approach is political, an approach from the viewpoint of the interests of socialism and demands of the population. Only such, and none other approach will lead us to correct conclusions. And correct conclusions are the basis for the perestroika of our work.

These words are entirely relevant to Soviet statistics.

Notes

1. This is not really serious.

2. Now, Russian Federal State Statistical Service.

3. In particular, the HAC confirms (or rejects) doctor dissertations successfully defended at scientific councils of scientific institutions and approves the right of such councils to consider those dissertations.

4. General censuses of population are being regularly carried out by all developed countries from the end of the 18^{th} – beginning of the 19^{th} century (although in Russia, only from 1897).

5. English edition of that journal, *Theory of Probability and Its Applications*, is being issued from about the 1970s.

6. The Bernoulli society is a branch of the International Statistical Institute. It never specified the *Bernoulli*: in statistics and probability, Daniel Bernoulli along with De Moivre was a main predecessor of Laplace.

7. Orlov many times identifies mathematical and theoretical statistics. Actually, theoretical statistics, unlike the former, includes the collection and preliminary investigation of statistical data.

8. It is hardly proper to call Student's proposals primitive.

9. In 1901, when answering a questionnaire of the Imperial Free Economic Society, Chuprov voiced his negative opinion about the introduction of statistics in the secondary school. He mostly based it on the lack of qualified teachers, and I doubt that in 1990 the situation was much better. Nevertheless, Chuprov approved the teaching of some chapters of statistics. Then, in 1914 Nekrasov made a similar proposal. Markov, his main opponent, protested against Nekrasov's obvious intention to link probability with ethical, political and religious considerations. In any case, nothing could have been done in time of war. See Chuprov (2004, pp. 25 – 27) and Sheynin (2009, pp. 246 – 247 and 256 – 257).

Anyway, Orlov had not elaborated the experience gained abroad and Pushkin (a story in prose) remarked that *Russian bread does* [Russian cereals do] *not grow in an alien manner*. It seems that the curriculum of the Russian secondary schools now includes probability theory and statistics.

10. Other countries (see end of § 2) are now not mentioned.

11. Cf. Schlözer (1804, p. 51): *Statistics and despotism are incompatible*.

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Except obvious cases, all items are in Russian

Abbreviation: SiK = *Standarty i Kachestvo*

VS = *Vestnik Statistiki*

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