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Introduction

1. Feodosy Nikolaevich Krasovsky (1878 – 1948) was the leading Soviet geodesist. I am translating five of his papers, describing some archival information concerning him and adducing four contributions devoted to him. One of them, by V. V. Danilov, begins by dealing with the period before 1917, – essentially, before Krasovsky. That information is useful, and, as a preliminary to all the other materials, I insert a translation of an essay by A. M. Virovetz entirely devoted to the same subject.

Krasovsky's *Selected Works* (1953 – 1956) are the main source for studying his work; Krasovsky's bibliography, containing many defects, but the only one, is at the end of Bagratuni (1959). Many reports describing Krasovsky's life and work were read at a special sitting at MIIGAiK (see *Abbreviations* at end of this Introduction) and published in the same source as [x, xii]. Three of them were devoted to subjects barely touched by the authors included here: cartography, geodetic instruments, photogrammetry. And Krasovsky *headed work on the production of high-precision geodetic instruments* [vol. 6, article *Geod. Instruments*]. See below explanation of such references.

Here, I only add that he was director, then assistant director, science, of TsNIIGAiK and Vice-President and then President of the Baltic Geodetic Commission. In 1940, Izotov (1950) deduced the parameters of the Krasovsky ellipsoid by issuing from his investigations. Soviet geodesy was based on that ellipsoid from 1946, and the figure of the Earth, now generally accepted, does not differ much from it. Together with his former student, the younger great scientist Molodensky, Krasovsky emphasized the need for applying gravimetry in studies of the figure of the Earth.

I have graduated from MIIGAiK in 1951 as an *astronomer geodesist*, attended the lectures of three authors translated below, and V. V. Danilov was in addition the supervisor, or mentor of my diploma. During my student years, F. N. did not read lectures anymore, but his name had been on the lips of our instructors. His nickname, which I also came to know, *Saint Fedos*, only described his scientific prestige.

Krasovsky [iii] and especially Danilov [ix] had highly praised the socialist system which hardly reflected their real feelings. Both had compiled their pieces during horrible times; Numerov, about whom F. N. deservedly held a high opinion [ii, § 9], was then arrested (and shot in 1941). Incidentally, similar eulogies are in Khinchin's paper (1937).

In particular, I note that Danilov called the Bolshevik coup d'état of 1917 (25 October, old style, or 7 November, new style) by its official name, *Great October Socialist Revolution*; I have written *Revolution*. Then, the authors very often applied the adjective *Soviet*; instead, I have almost always written *our*.

2. The Decree of 15 March, 1919 created VGU and became a turning point in the development of geodesy and cartography. Lenin had only signed it. Vol. 38 of his *Complete Works* (1963) covers the period from March to June 1919, lists the decrees which Lenin had at least partly compiled, or, in a special list, edited. However, the Decree of 15 March is only mentioned in a commentary (pp. 520 – 572) on Lenin's day-to-day work during that period. There, on p. 521, he is named as participator in a discussion of its *draft* at a sitting of the Council of People's Commissars (= of Ministers, SNK) whose chairman he had been. But who drafted it? The brothers Bonch-Bruevich, Vladimir and Mikhail Dmitrievich (1873 – 1955 and 1870 – 1956) [vol. 3].

The former finished a land surveying school, studied in MMI and Zurich University, and was, at the time (1919), managing director of the SNK. Mikhail graduated from MMI, participated in the creation of VGU (no details supplied), and became its first director (1919 – 1923). In 1939 – 1949 Mikhail edited the nine volumes of a geodetic encyclopaedia; I ought to add, however, that authors had barely referred to it. Kashin (1979, p. 10) stated that Mikhail was *one of the main organizers and managers of VGU*. Without documenting his account (a feature regrettably common for Soviet literature of the time), Kashin also quoted Mikhail's archival notes: the Technical Council of VGU was obliged to study the most modern methods of work and secure a tight connection of science and practice. He, Mikhail, invited Krasovsky to head that Council,

Having been sure of his knowledge and persistence in successfully completing each assignment. […] *His appointment was an expression of that connection, because at the time he had been almost the only representative of* <u>*great geodesy*</u> *in MMI.*

Here, finally, are two passages from the *Lenin Decree* (*Sobranie* 1919, pp. 139 – 140). The VGU was created

For the topographic study of the territory of RSFSR [see below] *aiming at raising and developing its productive forces and economizing technical efforts and financial means.*

To carry out that aim, VGU

a) Unites and coordinates geodetic activities of all Commissariats and institutions of the Republic;
b) On the national scale, implements and is in charge of main geodetic works (trigonometric, astronomical and precise levelling);
c) Carries out continuous and systematic topographic mapping over all the territory of the Republic;
d) Obviating parallelism, unites and directs surveys of every kind; for compiling and publishing maps of national interest to various scales and for various aims of national economy, it collects and systematizes the results of astronomical, geodetic and topographic works of separate Commissariats and institutions;
e) Works out and approves provisions regulating [geodetic] *activities, and technical instructions and rules establishing unity of the methods of* calculations, *and compilation and publication of maps and plans for various departments;*

f) Organizes cartographic work and publishes maps for separate departments, institutions and individuals, in particular by applying to existing cartographic institutions;

g) Manufactures geodetic instruments and optical apparatuses on the existing factories; supplies them for departments, institutions, and individuals;

h) Organizes scientific work in geodesy, astronomy, optics, cartography, instruments, and surveying in general, and for preparing young scientists; collects, systematizes and keeps maps and other materials of surveys;

i) For internationally harmonizing geodetic activities, contacts geodetic institutions of foreign states.

Signed: Chairman of Council of Peoples' Commissars Ulianov (Lenin); Chairman of Superior Council of National Economy [a future enemy of the people] [A. I.] *Rykov; Managing Director, Council of Peoples' Commissars, V. D. Bonch-Bruevich; Secretary L. Fotieva*

Also mentioned: *Published in Izvestia No. 63, 23 March 1919*

RSFSR (Russian Soviet Federal Socialist Republic) was created in 1918. Federal meant that it included a number of autonomous republics and regions. The Soviet Union was officially established in 1922.

Kashin (1979, p. 9) published allegedly the same two passages (not fully) and, quite in agreement with the contemporary new wave of obscurantism, omitted the last item …

3. Arc measurements have been carried out since the end of the 17th century. They aimed at determining the length of one degree of (any) meridian. The latitudinal amplitude of an arc was astronomically determined and its length indirectly measured by a chain of triangles (the simplest case) with all three angles measured in each and a baseline and astronomical azimuth measured at its end, – actually, at both ends, and an adjustment of the arc was required.

One arc was necessary for determining the radius of a spherical Earth, but, after Newton had proved that the Earth was an oblate ellipsoid of rotation, two became necessary; practically, many more for checking the field work and compensating local irregularities. From the end of the 19th century, baselines became measured by invar wires 24 *m* long whose length almost did not change with the air temperature.

Triangulation chains were also necessary for mapping. They were laid out in quadrilaterals (roughly, squares) called *polygons*, and systems of polygons had to be adjusted as a single whole; *threading* polygons would have led to an accumulation of unavoidable errors. According to Krasovsky, who borrowed his main idea from Helmert, those polygons were adjusted in a few stages. First, each chain was preliminarily adjusted and replaced by a geodetic line connecting its end points. Second, these lines were adjusted. Third, their adjusted values were applied for returning to the chains. Fourth and last, the chains were finally adjusted.

Bearing in mind the required scale of the general mapping of the USSR territory, Krasovsky established the necessary precision of all field measurements and the optimal size of the chains and polygons. Later, for more precisely determining the parameters of the general Earth ellipsoid, gravimetric measurements were added to such arcs. Here also, Krasovsky played an active part, but the main worker in that new direction had been his former student M. S. Molodensky. While being a student geodesist, I heard that Molodensky, then a student at Moscow University, had asked Krasovsky to secure him a job for the summer and that Krasovsky arranged his participation in measuring a baseline.

Especially important for the general development of geodesy was the so-called Struve arc mentioned many times in the sequel. Here, I only provide some little-known information about it. Vasiliy Yakovlevich Struve (1793 – 1864), an astronomer, professor at Dorpat (Tartu), became the first Director of the Pulkovo Observatory (1839 – 1861). His classical *Duga Meridiana* (Arc of the Meridian) was published in Petersburg in 1856 – 1861 and reprinted in Moscow in 1957. A French edition appeared in 1857 – 1860 with vol. 2 preceding vol. 1 and its English translation is now available.

The name of the translator(s) is (are) not given; the place and year of the *Publisher's Introduction* are Copenhagen, 2008, its complete title is

Arc of the Meridian of 25 °20′ between the Danube and the Glacial Sea measured under the direction of

C. De Tenner […], Chr. Hansteen (Director, Roy. Geogr. Dept Norway), N. H. Selander (Director, Roy. Obs. Stockholm), F. G. W. Struve (Director, Central Obs. Nicholas of Russia [Nicholas I, Tsar in 1825 – 1855])

F. G. W. Struve

Struve was indeed the sole author of that publication and he it was who studied and described the theoretical and methodical essence of that vast undertaking. This fact does not diminish the merits of Tenner, see for example Virovetz [i]. A foreign scientist, after coming to Russia, had to change somewhat his name; had Gauss moved to Russia, he would probably be called *Kyril Fedorovich.* Friedrich Georg Wilhelm Struve changed Wilhelm to Vasiliy and selected his patronymic, Yakovlevich, according to his father's name, Jakob.

The author's Introduction makes known that, apart from the base extensions, the arc was comprised of 258 triangles and that Tenner and Struve had measured 11°10′ and 9°38′ of it respectively, leaving 4°32′ for the two other scientists (whose work was likely accomplished under more trying natural conditions).

In 2005, UNESCO inscribed the Struve arc on its Heritage List as a memorable ensemble (Wikipedia).

Karl Ivanovich Tenner (1783 – 1860) was an astronomer, a military geodesist (a general), Honorary Member of the Petersburg Academy of Sciences. Unlike Tenner, Struve did not pay due attention to the laying out of the centres of his signals, and his work was soon lost (Novokshanova 1967, p. 36). She (1957, pp. 85 – 86) also published Struve's letter of 1856 to Tenner. It was really cordial and acknowledged the recipient's merits. And Struve mentioned that Bessel told him that he held a high opinion about Tenner.

In addition, Novokshanova-Sokolovskaia published books on Schubert (1958) and Struve (1964).

Abbreviation applied throughout the collection

AG = astronomical geodetic

GGK = Main Geodetic Commission of VSNKh (of the Superior Council of National Economy)

GUGK = Main (now, Federal) Directorate of Geodesy and Cartography

KVT = Corps of Military Topographers (before 1917)

MGI = Moscow Geodetic Institute

MIIGAiK = Moscow Institute of Geodesy, Aerial Photography and Cartography; now, Moscow State University of Geodesy and Cartography

MMI = Moscow Land Surveying Institute; now, University

TsNIIGAiK = Central Scientific Research Institute of Geodesy, Aerial Photography and Cartography; now, bears Krasovsky's name

VGU = Superior Geodetic Directorate

Krasovsky's works mentioned here and throughout the collection are gathered at its very end. With obvious exceptions, in all the biographies English titles actually describe Russian items. Also throughout the collection references such as [vol. i] are to the English translation (32 vols, 1973 – 1983) of the third (Russian) edition of the *Great Sov. Enc.* (1970 – 1978).

In some of his contributions, Krasovsky estimated the precision of the results obtained. He often applied the term *mean error* apparently bearing in mind the mean square error of the final result. Then, he (and Virovetz) invariably attached a double sign to those errors, which is not done anymore, and in a few cases makes a formal mistake when writing that a square root is also equal to a number with a double sign. Finally, in many cases he introduced the mean square error without justifying its appearance, see my Note 8 in [x].

After Krasovsky's lifetime, geodesy achieved great progress. Geodetic satellites secure links between points separated from each other by up to several thousand kilometres, and electro-optical range finders measure distances of 20 – 25 *km* with an error of 1:400,000. Accordingly, triangulation can be replaced by *trilateration* in which distances rather than angles are measured. And the era of satellite geodesy had begun.

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I

A. M. Virovetz

A brief survey
of the development of basic geodetic work in Russia before 1917

XX Let Sovetskoi Geodesii i Kartografii
(20 Years of Soviet Geodesy and Cartography), vol. 1. Moscow, 1939, pp. 271 – 288

[1] The most important practical aim of basic geodetic work is the construction of a system of control points necessary for topographic and cartographic mapping. Therefore, the development of that work is closely and inseparably linked with cartography, and the history of both should surely be considered at the same time.

That geodetic work for studying our territory was necessary, became felt even in the 15th century. At the time of Ivan the Terrible the first Russian geodetic manual, *A Book Called Geometry Or Geodesy by Means of Number* [radix] *and Compass …* was compiled. In the middle of the 16th century appeared the first general map of European Russia, and, in 1667, the first *drawings* of the Siberian land. Both are considered very important contributions to the contemporary cartography of Russia, and maps of separate regions also became then available. All those maps were, however, very imperfect, their compilation was not based either on astronomical stations or some geometrical constructions. Thus, on a map compiled in 1614, Russia was moved about 3° to the east and its figure became compressed from north to south for about 300 *km* and stretched from west to east for more than 1,500 *km*; the Caspian Sea was utterly distorted and shown as though stretched from east to west rather than from north to south etc.

Only under Peter the Great attention was turned to the need for more thorough geographical study of the country. Special expeditions were being sent for investigating poorly known parts of the state (the Caspian Sea, Kamchatka, the Kuril Islands etc). In 1720, the surveying of the inner lands had begun. A special decree ordered to

Select thirty young men from the Naval Academy sufficiently instructed in geodesy and cartography and send them to various provinces for measuring the land, compiling maps and describing the inner parts of Russia.

The works had been executed according to special directions which indicated that the latitudes of towns and some points along the borders of the districts should be determined by astronomical observations. Compass points were established by astrolabes and distances measured by ropes. These were the first Russian geodetic works carried out for cartography. Surveying was executed over certain districts and the completed maps immediately sent to Petersburg for compiling a general map of the state.

In 1726 the Academy of Sciences invited the French astronomer Deslisle to head these cartographic activities. He proposed to determine latitudes and longitudes of as many as possible most important points and thus to raise the quality of the map. His proposal was accepted, and the Academy of Sciences began to determine, intensively enough, astronomical stations for cartographic applications. At the same time, some astronomers-observers were being trained. The Academy's activity became especially enlivened when the celebrated mathematician Euler was in charge of compiling that general map. In 1745, after almost twenty years of work, the Academy issued the *Atlas of Russia … Compiled according to geographical Rules and Newest Observations*.

That publication was a remarkable event which moved Russian cartography to a foremost position. Euler testified that *The geography of Russia was brought to a much better condition than the geography of the German land.* The compilation of that atlas was very useful for the subsequent development of geodesy and cartography in Russia both in respect to controlling and carrying out surveys and to training geodesists and cartographers. The Academy continued cartographic activities by executing new astronomical determinations and correcting maps according to newest materials. Especially successful were the years 1757 – 1765 when Lomonosov had been in charge of that work. Among the remarkable results of those times we ought to indicate the astronomical work of the geodesist A. D. Krasilnikov. Being attached to the Bering expedition, he determined 11 astronomical stations extending from the Baltic to Kamchatka.

[2] To the end of the 18th century the number of such Russian stations totalled 67 which seems quite insignificant. Actually, and in addition when considering the difficulties of travelling, especially for astronomers with their extremely bulky equipment (quadrants of radius 1.2 *m* and telescopes up to 6.1 *m* long, astronomical clocks etc), it was a serious achievement since no country in Western Europe had so many.

The methods of astronomical observations had yet been very imperfect. Longitudes, for example, were determined by observing occultations of stars by the Moon, solar eclipses, eclipses of Jupiter's satellites, – of rare events which sometimes compelled astronomers to stay at the same station for months on end. Incidentally, those methods required the astronomer to be better instructed than nowadays. Latitudes and longitudes were determined with errors about 5 and 8″. Struve, the celebrated Russian astronomer and geodesist of the 19th century, considered that, for available means and methods of previous work, that precision was quite satisfactory.

In the second half of the 18th century, the surveying connected with establishing the borders of land tenure, the so-called land-surveying, began to develop quite rapidly; surveying had also been executed for military purposes, forestry etc. However, all that work still lacked a firm geodetic control. From the beginning of the 19th century astronomical work had also been rapidly developing. Academician F. I. Schubert had instructed the military personnel and compiled a remarkable treatise on determining astronomical stations; its two Russian and three German editions had appeared in short time.

I mark out the work of Academician V. K. Vishnevsky, who, in 1806 – 1815, determined the latitude and longitude at 225 stations scattered over almost the entire European Russia. He selected 13 main longitudinal stations, determined their longitudes by observing occultations of stars by the Moon, and, for raising the precision of the work, visited several times each. He obtained the longitudes of the other stations by carting his chronometers. According to Struve's estimate, he determined latitudes and longitudes with errors about 5″ and 2^s respectively. Struve concluded: *That precision is worthy of surprise especially taking into account how simple were his instruments*.

After indicating the essential success in the field of cartography and astronomical work, I ought to note that triangulation had not been executed at all. The situation in geodesy at the end of the 17th and beginning of the 18th century was nevertheless marked by most considerable for those times triangulations carried out by French geodesists for determining the figure and size of the Earth. […]

Also then, in 1737, Deslisle began to measure an meridian arc. He measured a base about 14 *km* long on the ice of the Gulf of Finland by wooden bars and attached a few triangles to it. He got no support to his undertaking and abandoned it. That attempt was completely forgotten, and for almost 80 years no essential triangulation had been carried out in Russia.

[3] The interest in such work had, however, awakened in the period of Napoleonic wars when the Military Department began to feel very keenly the need for precise topographic maps. Topographic work in Russia, and, first and foremost, in its frontier regions, had to be reconsidered. The new survey of those regions began in the Vilnius province after the end of the Patriotic War of 1812. It was decided, for the first time in Russia, to control it by triangulation stations.

The outstanding military geodesist Tenner was entrusted with executing the triangulation and began work in 1816. Remarkable by a correct approach to its construction, it was separated into three orders. The sides of the triangulation of the first two of them were 25 and 5 – 10 *km* long in the mean, whereas stations of the third order were mostly determined by intersection. The geodetic principle of passing from the general to the particular had been completely realized.

I ought to note especially Tenner's idea of applying the triangulation of the I order not only for the practical aim of controlling topographic surveying, but also for solving a scientific problem, for measuring a meridian arc. Accordingly, he laid out that triangulation, a chain of triangles accompanied by three bases, along a meridian. Tenner mainly guided himself by the experience of French geodesists who had then attained essential success in carrying out triangulations, but he also extended and developed their practice by adapting it to Russian conditions.

For example, having begun base measurements by a base apparatus manufactured by Russian mechanicians after the type applied by Delambre, he soon discovered a number of its essential constructive imperfections and radically altered it. Tenner thus created a new type of a bar for measuring bases with precision 1:300,000. Then, he strictly kept to a very important condition of having approximately equilateral triangles. In low-lying and woody regions of Western Russia this compelled Tenner to build surveying signals up to 30 *m* high. Their construction was, however, imperfect. They consisted of four vertical posts fastened together at various places by transverse beams and strengthened by stops. They were not rigid or stable which undoubtedly affected the precision of angle measurements[1].

An important point was Tenner's rule of fixing the centres of the surveying signals. Their construction was rather simple, but nevertheless they preserved well enough; most of them were found after many years. That care for preserving the triangulation in nature had been lacking in many later works of the KVT, so that a great deal of work done in the 19th century became lost.

Tenner measured angles by a repeating circle [naturally] applying the method of repetition. Typical for his work was a great number of observations; there were 20 – 50 repetitions at a station of the I order and closures of triangles exceeding 3″ had not been admitted (the appropriate angles were measured anew). The probable error of an angle, estimated by those closures, was ± 0.″62; however[2], bearing in mind those repeated measurements, it is more correct to adopt the value ± 1.″7.

After completing the triangulation in the Vilnius province, Tenner began the same work for controlling surveys in Kurland (Courlande) [part of Latvia], in Grodno and Minsk provinces, keeping to his method of accomplishing it and continuing his meridian arc measurement. His results are distinguished by high quality. Thus, in particular, in 1830, when his triangulation was connected with the Prussian triangulation carried out by the celebrated German astronomer Bessel, the lengths of their common sides did not differ by more than 1:200,000.

Tenner's work served as a specimen for Russian geodesists; their main features are easy to detect in the best triangulations of the 19th century. He was a remarkable person of his epoch, very gifted, a prominent organizer, tirelessly devoted to his duties. Being in charge of field work, he personally participated in its execution. His idea about scientifically applying triangulations proved very fruitful and fostered the improvement of the methods of geodetic work and the raising of its quality.

[4] From 1816 to 1831, simultaneously with Tenner's work, Struve, a professor at Dorpat [Tartu], had been measuring a meridian arc in the Baltic provinces. In organization, methods and results his work belongs to the most precise triangulations, is rightfully considered classical and was greatly important both for scientific and practical geodesy. It was then that Struve had worked out the methods of rounds for measuring angles, drawn up the principles of adjusting triangulation and treating arc measurements, solved the problem of standard measures, improved the methods of astronomical observations, and constructed a base apparatus named after him. It was capable of measuring bases with relative error not exceeding 1:1,000,000, and was applied in Russia until the end of the 19th century.

In 1830, the triangulations of Tenner and Struve were joined and constituted a meridian chain of triangles of latitudinal amplitude exceeding 8°. Tenner communicated its results to Bessel and suggested that he use them together with other available arcs for deriving the sizes of the Earth ellipsoid. Bessel is known to have achieved very important results by determining the deflections of the vertical which allowed him in 1841 to obtain the sizes of the Earth ellipsoid. For a long time they were considered the best and were adopted in many countries including Russia for calculating triangulations.

Tenner continued the Russian arc measurement to the south until the mouth of Danube, and Struve continued it to the north, to the coast of the Arctic Ocean. Swedish and Norwegian scientists measured the northern part of that extension according to Struve's indications, and all the work was accomplished in 1852. And so, after more than 30 years of work, a great meridian arc with latitudinal amplitude 25°20′ was obtained. It was equipped by 10 bases and 13 astronomical stations. The error of its total length, as Struve estimated it, only amounted to ± 12 *m*. Not only Bessel[3], but also the well-known British geodesist Clarcke applied the results of that remarkable work. TsNIIGAiK made use of them even now when deriving the sizes of the ellipsoid best suiting the territory of the USSR.

Latitudes and azimuths, but not longitudes were determined at the 13 astronomical stations of the Struve arc so that it was separated into 12 particular arcs. Observations were made mostly by Struve, Tenner and by invited university professors. Latitudes were mostly determined by a transit instrument set in the plane of the prime vertical, azimuths of a mark, set in the plane of the meridian, were measured by the same instrument. The number of astronomical observations at each station was very considerable. It is not amiss to indicate that in the part, laid out under Struve's direct guidance, the mean error of a measured angle was ± 0.″6 which places his result on the level of best works.

[5] In the 19th century, apart from the Struve meridian arc, Russian military geodesists had laid out two more considerable arc measurements along parallels 47°30′ and 52° which extended the corresponding arc measurements in Western Europe. The first one was connected with the Struve arc at Kishinev; it ended at Astrakhan and its longitudinal amplitude was about 20°. The triangulation had been carried out in 1848 – 1858, but astronomical determinations lagged behind until 1890. The results were unsuccessful since the discrepancies of the base conditions amounted to 1:10,000 and even 1:7,000 although the mean error of a measured angle, calculated by the closures of the triangles, was only ± 0.″9. These measurements were apparently not as thorough as the works of Tenner and Struve, their results were worse in spite of more advanced instruments applied.

The second arc measurement along the parallel 52° ended in the east near Orsk with an amplitude 39° of the Russian part. The work had been carried out in 1861 – 1870 and its results were also of low quality. Indeed, in the arc's west-European part the discrepancies of the base conditions never exceeded 1:60,000 whereas in the Russian part they amounted to 1:5,000. Neither of these two arcs was reliable for deriving the figure or size of the Earth ellipsoid. I note that the longitudes were already determined by telegraph communication.

[6] From 1822, when the KVT was established, topographical geodetic work had been developing very rapidly with the surveys being as a rule controlled by triangulation networks. Following Tenner, triangulation was separated in three orders, but his typical strict pattern of constructing the networks was lacking. There was no general plan of geodetic work, and the triangulation had been executed either for separate provinces or several of them independently from the triangulation in the neighbouring regions.

The military geodesist F. F. Schubert was in charge of their considerable part. Unlike Tenner, he did not wish to take into account the scientific importance of geodetic work and only pursued the practical aim of controlling surveys. He therefore thought it sufficient to measure the angles in the triangulation of the I order with errors not exceeding ± 1″, did not consider it essential to keep to a definite pattern when carrying out the triangulation, and, perhaps because of the same reason, did not necessarily secure the triangulation stations by sufficiently firm centres. Schubert also attempted to make use of as many as possible local objects (towers, bell towers) mostly determining them by intersections as stations of the III order.

In a historical essay on the activities of the KVT issued in 1872[4], Schubert's geodetic work was described as follows:

Carelessness … caused perhaps by the aim of the entire triangulation being too one-sided and exclusively practical, only satisfying the undemanding topographic surveying. It was certainly unable to harm the surveying appreciably, but the extensive work, for which considerable means were provided, proved not as useful as it was possible, in all fairness, to require from them.

This appraisal is certainly correct, and it also concerns many other triangulations carried out for controlling surveys in separate provinces. I also indicate that Schubert stubbornly resisted the development of arc measurements in Russia. In 1826, the French government suggested that Russia participate in measuring an arc along parallel 47°30′, but he exerted every effort, insofar as it depended on him, to shelve that suggestion[5].

That attitude, which was certainly transmitted to some of his collaborators, possibly became the reason for the lowering of the quality (as compared with Tenner and Struve) of many later Russian triangulations although carried out by more advanced instruments. Shubert's deficiencies included his ignoring trigonometric levelling whereas Tenner had shown that, being accurately and correctly applied, it can yield quite good results. It is interesting to note that this type of levelling along the Struve arc from the Baltic to the Black Sea showed the difference of their levels of only 1 *m*[6], which was therefore a result of very high quality. The problem of the precision and methods of that levelling is known to remain topical now also. It is also necessary to mention a very interesting determination of the difference of the levels of the Black and the Caspian Seas by trigonometric levelling according to Struve's indications.

Typical was the measurement of distances by a prototype of the modern parallactic traversing. At the middle of the measured distance, approximately perpendicular to it, a short base ca. 300 *m* long was laid out. It was measured by the so-called *method of twine* rather quickly and precisely (with precision up to 1:30,000). Then the parallactic angles were measured with precision not less than 1″. A traverse about 700 *km* long was thus measured from sea to sea, and Struve estimated its precision as being around 1:28,000. The difference between the sea levels was determined very precisely and wholly corroborated by subsequent work.

[7] The first surveys in the Caucasus, in Siberia and Turkestan were executed either without any control points, or at best controlled by astronomical stations. In the Caucasus, triangulation began in 1847 under the outstanding and tireless military geodesist Hodsko (Chodzko), Tenner's collaborator on his arc measurement. From 1847 to 1853, Hodsko (Chodzko) executed a triangulation of the I order of very high quality. That work was extremely difficult because many stations were situated on summits of mountains covered by perpetual snow, and observers were sometimes compelled to live there for a few weeks awaiting visibility. Especially remarkable was the ascent of the summit of Ararat (5.2 *km* above sea level) where Hodsko (Chodzko) and his team remained for six days.

Triangulation in southern Caucasus began in 1860 and the work was also very difficult because of local conditions. Measurements had to be done on summits of mountains covered by perpetual snow and in the arid steppes. Those triangulations are distinguished by high quality: the probable error of an angle was about ± 0.″7, discrepancy of the base condition in the Transcaucasia not exceeding 1:90,000 and 1:50,000 in the northern Caucasus. After being joined with the triangulations in the Volga region, essential deflections of the vertical, up to 50″ in some places, were discovered. They were mostly quite explainable by the disturbing action of the Caucasian ridge, but in some cases there remained residual deflections.

I also ought to note that about that time the triangulation executed by the KVT revealed a latitudinal deflection of the vertical up to 10″ in the Moscow region. Based on its investigation in the 1860s by Schweizer, a professor at Moscow University, it was assumed that a possible reason of that occasion was a comparatively shallow bedding of insufficient density. Later work completely corroborated Schweizer's [!] conclusion.

In Turkestan triangulation began in 1870. The development of geodetic work was marked there by the same deficiencies as in the European Russia. It was carried out without a general plan, and an appropriate mutual coordination was lacking. Sufficient measures for securing triangulation stations were not taken either. Among such work in Middle Asia I ought to indicate the triangulation of 1910 – 1912 by the KVT for joining Russian and Indian triangulations across the Pamir. Its quality was apparently low since the mean error of the [of the angle measurements in the] joining chain amounted to ± 3.″1.

In Siberia, scientifically justified triangulation began in 1909 when the military geodesist Pavlov had been in charge of executing a chain of I order from Omsk to Ust-Kamenogorsk passing through Pavlodar and Semipalatinsk. Later, in 1932, this chain was included in the polygons of triangulation of the I order carried out in Siberia, but it occurred that for some still unexplained reason it led to inadmissible discrepancies (up to 17 *m*) in the adjacent polygons. Networks of the II order in the Semipalatinsk province and some main chains and networks of the same order were executed in Eastern Siberia and the Far East.

In addition to the KVT, other departments had also participated in that work, but their results were only of local importance. Thus, the Mining Department carried out the well-known [?] Bauman triangulation in the Donbas [Donets coal field], the Land Surveying Department, in the Caucasus, the Resettlement Department, in some Siberian regions, and finally, the Hydrographical Department, along sea coasts.

[8] Along with the development of surveys and triangulations, astronomical work had been carried out. The Pulkovo Observatory became the school for Russian astronomers and geodesists, and this aim remained in the future as well. Several generations of specialists have studied there, there also worked the most prominent Russian astronomers and geodesists, Tsinger, Shchetkin, Vitram, Pavlov, Vitkovsky and others. The Observatory manufactured high-precision astronomical and geodetic instruments and equipment.

Beginning with Struve, astronomical stations have been determined for orienting triangulation and controlling surveys to small scales. When pursuing the first aim, the determinations have been carried out with utmost precision possible to attain with best instruments and methods of work. Special care has been displayed for determining the longitudes of the most important places of the country; chronometers were carted, and for obtaining the best possible results, from 30 to 80 of them have been applied.

From the second half of the 19th century onward, the determination of longitudes by telegraph communication, as it extended, had become customary. The high precision of the new method allowed to abandon completely the transportation of chronometers to any place where that communication was available. Among the most considerable astronomical works of that period I ought to mention those of Scharnhorst and Kulberg. In 1873 – 1876 they determined the longitudes of the most important cities situated along the route from Moscow to Vladivostok. Their difficulties were mostly occasioned by the absence of the later constructed Siberian railway. At the end of the 19th century the mutual longitudinal connection of the most important cities in the European part of Russia was established by telegraph communication and some of the Russian triangulations were joined to those abroad (in Austria).

Even in 1828 – 1830 the KVT began experimentally controlling surveys to small scales by astronomical stations. That method had been applied widely enough in 1849 – 1867, when the KVT together with the Land Surveying Department surveyed eight provinces in

European Russia (the so-called Mende Surveys [see also Danilov ix, § 1]). Astronomical stations, which topographers joined by instrumental traverses, were situated about 50 *km* apart. That method was obviously beneficial in closed woody locations since the construction of high geodetic signals was not needed anymore. In Siberia and Turkestan triangulation began developing considerably later [see § 7]. There, controlling surveys by astronomical stations had been applied exceptionally widely, especially in Turkestan where conditions are conducive for astronomical observations.

From the beginning of the 20th century, gravimetric surveys began to develop little by little. For geodesy, their practical aim was the determination of the flattening of the Earth spheroid. This work was going on very slowly, and up to 1917 the KVT had only determined about 300 stations.

And so, to the end of the 19th century the KVT had covered about 2/3 of European Russia by triangulation networks. However, they were distributed very irregularly. The North completely lacked triangulation, little was done in the eastern regions, and large gaps had even remained in the central regions. But the most essential deficiency was that triangulation had not been secured by centres; [and] not more than 20% from such local objects as towers and bell towers have remained.

KVT had neither a general plan, nor general technical requirements for executing triangulations. They were carried out locally and calculated by issuing from their own bases and astronomical stations adopted as the origin. Moreover, different ellipsoids (as introduced by Walbeck [in 1819], Bessel and Clarcke, and the coordinating ellipsoid) were made use of. Understandably, absolutely inadmissible discrepancies had been occurring at the borders of adjacent triangulations.

It was decided to calculate all networks anew and thus bring them to a common system of coordinates. From 1897 to 1907 this great work had been done under the guidance of the military geodesist Scharnhorst. The Struve meridian arc and the arc measurements along parallels 47°30′ and 52° were adopted as the basis; the Bessel ellipsoid was chosen, and Juriev [Dorpat, Tartu] served as the origin. Adjustment was essentially simplified, and the corrections to the angles reached 20 – 25″. That work mainly aimed at getting rid of the discrepancies mentioned above so that triangulations could be applied for mapping.

Scharnhorst had only time for calculating the triangulations of I order (3236 stations), those of the II and III orders were not dealt with. His work was not really important because the triangulation of I order was destroyed most of all. In the overwhelming majority of cases the old triangulations were lost whereas the need for geodetic and cartographic work was increasing, especially at the beginning of the 20th century. So, in 1909 the KVT compiled a plan for executing a new triangulation of the I order extending over all European Russia, and its realization began in 1910. These are the main features of that plan.

1. Chains of I order to be carried out along meridians and parallels 300 – 500 *km* apart with the perimeters of the polygons [necessarily] being 1,300 – 2,200 *km*.

2. As a rule, the chains to consist of simple triangles; at the intersections, bases should be measured and latitudes, longitudes and azimuths astronomically determined.

3. Calculations to be done on the Bessel ellipsoid with Pulkovo observatory as the origin.

The work had been carried out according to specially compiled directions. World War I prevented that plan from being realized; up to 1917 only four bases were measured, 152 geodetic signals built and angles measured at 129 stations.

[9] In Finland, from 1859, instead of carrying out control triangulation networks, surveys had been controlled by astronomical stations about 50 *km* apart. That practice was not something new anymore; new was that precise traverses were being carried out by a special instrument called level-theodolite. It was thought thus to abandon expensive triangulations. Those traverses are known not to justify hopes, and I am only mentioning them in connection with the attempt to apply the level-theodolite for precisely determining heights. It was constructed similar to the universal instrument, graduated to 10″ and applied with a checking telescope[7]. The only difference was that instead of the vertical circle thus graduated it had two sectors with verniers graduated to 4″. Necessarily attached to the instrument were two rods somewhat longer than 4 *m* each. They were held vertically on special stands; each had four marks with the distance between them precisely measured. They served for measuring distances and the angles of inclination of the marks; horizontal angles between the sides of the traverses were measured in the usual way.

In Finland, the level-theodolite had been successfully applied for surveying[8] and it was therefore contemplated to make use of it for levelling along railways and thus achieving control of heights. However, in 1871 – 1872 trial work showed that it was unfit for that purpose since the mean error of the distances amounted to ± 11 *mm/km*. It was then decided to switch to ordinary levels. However, in those years levels of insufficient power had been applied, and the results were rough; in 1875 – 1877 the mean random and systematic error per kilometre reached ± 6.2 and ± 0.9 *mm* respectively.

From 1881, more powerful levels were introduced (magnification 40), and the rods were thoroughly manufactured and calibrated. Observations were carried out by the well-known Russian-Swiss method [?]. The pertinent levelling is called precise; its probable error did not exceed ± 3 *mm/km*, but even that could not have been satisfactory, for example for determining the difference between sea levels.

In 1913 the KVT had therefore worked out a method of high-precise levelling according to a proposal by the French geodesist Ch. Lallemand adopted by the International Geodetic [and Geophysical] Union. That levelling has random and systematic error per kilometre not exceeding ± 1.5 and ± 0.3 *mm*. The magnification should be 35 – 40 and the level graduated to 2 – 7″. But until 1917 the KVT was only able to carry out such high-precision levelling from Petersburg to Odessa and connect the levels of the Baltic and Black Seas. It occurred that the former was 0.7 *m* higher.

[Back] in the 1870s the KVT had worked out the following programme for precise levelling of the European part:

1. To carry out levelling along the meridians [plural?] connecting the Baltic and Black Seas.
2. To carry out levelling along parallels 47°30′ and 52° so far as the direction of the railways will favour it.
3. To carry out levelling along the Baltic and the Black Sea – the Sea of Azov coasts for connecting the tidal stations there by shortest routes.
4. To carry out levelling along railways directed westwards for connecting it with levelling in Western Europe.

The work according to that plan continued for 13 years and its preliminary results are presented in the well-known Rielke catalogue (1894). It shows 1092 points with the total length of levelling lines being 13 thousand kilometres. Later work during 1893 – 1917 produced 32.5 thousand more so that the total length of precise levelling reached 45.5 thousand kilometres.

That network of precise and high-precise levelling was certainly very sparse, but of essential importance for mapping the country since it allowed to bring all surveying to one common level and gave the opportunity to apply levelling of great volume accomplished by various departments for the aims of hypsometry.

In addition to the mentioned triangulations and levelling, about 3,900 astronomical stations were determined and applied for controlling surveys to small scales. All this was only done by the KVT of the General Staff.

[10] In 1822, military, managerial and economic considerations compelled the government to establish the KVT. It was charged with executing military topographic surveying of the frontier regions and a general topographic surveying of the entire country. Accordingly, it widely extended its activities and during 1822 – 1877 Russian geodetic work had in general been successfully developing. Then also the most important problems concerning the methods of geodetic work (measurement of angles and baselines, astronomical observations etc) had been formulated and solved. With Struve's assistance the Russian school of geodesists and astronomers was established[9].

Nevertheless, serious deficiencies were also present: lack of a planned execution of the triangulations of provinces; of their proper coordination, of common technical requirements and wrong treatment, but first of all, triangulation stations had not been secured by centres. The last mentioned circumstance led to the destruction of the triangulation [see § 8] with the remaining points being at best only suitable for *cartographic purposes*.

In 1877, the development of the state geodetic work sharply changed its course for the worse when the government instructed the KVT to work exclusively for satisfying military requirements. In conformity with that restricted aim, it only appropriated the KVT 250 thousand roubles yearly for all state work. The cartographic study of the country for general aims slipped the government's mind, and from that year onward only the frontier regions were being geodetically and cartographically served with all the rest territory (almost its 90%) remaining unstudied. As a result of this short-sighted policy, Russia became topographically and geodetically backward.

Meanwhile, the need for geodetic and topographic material continued to increase, especially from the beginning of the 20th century, and the KVT initiated some measures for constructing a new state triangulation of the I order and [state] high-precision levelling, but World War I stopped these attempts.

11. Lenin, the genius of mankind [genius indeed!], indicated the new route for the development of geodesy and cartography of our fatherland. In the decree of 15 March 1919 he precisely and clearly established the aims and problems of Soviet geodesy and cartography[10]. The year 1919 was the initial data of the development of Soviet geodesy. Twenty years have passed. In former times no one, even in his dreams, could have imagined the present rapid development of geodetic work. Great work is being done each year, and its volume is ever increasing, corresponding to the long-term plans of the national economy.

The power of geodetic organizations exceptionally increased. Such projects as the Struve arc, whose execution lasted for decades (for more than 30 years), are now being carried out in two or three years. Geodetic work is united in the general state plan and common technical requirements developed on a scientific basis.

Notes

1. See Note 7.

2. No explanation provided.

3. So Bessel did make use of the triangulation of Tenner and Struve, see above.

4. No reference provided. The same remark is true about Deslisle (end of § 2) and about some other cases as well. Novokshanova (1958, p 23) defended Schubert by quoting his report published in the same source of 1872 where he had stressed that triangulation should be carried out first and foremost in those provinces which urgently needed it. Later, however, she (1967, p. 36) largely sided with Virovetz (but only mentioned him on p. 83).

Novokshanova (1967, pp. 24 and 25) contradicts Virovetz (§ 2) on two points: 57 rather than 67 astronomical stations were established by the end of the 18th century, and Vishnevetsky had determined latitude and longitude at 223 rather than 225 stations. I am unable to comment.

5. That arc was measured much later, see beginning of same section.

6. In the beginning of the 20th century that difference was measured once more and amounted to 0.7 *m* (§ 9).

7. Geodetic signals consist of two pyramids or of only one. In the first case, the inner pyramid serves as a base for the instrument and the outer, for the observer (and for a mounting for the sighting target), and they do not touch. In Russian woody locations, the signals were high, and both the instrument and the observer were on the same single pyramid which unfavourably affected the measurements. The checking telescope (which I never saw and am not sure whether it is still applied) with a lesser magnification was directed to a nearby mark, the deviations from which were registered by a second observer and then allowed for.

I remember someone recalling that Krasovsky explained the use of that telescope to a representative of a German firm which agreed to produce a number of instruments to the USSR. That apparently happened at the end of the 1920s or the very beginning of the 1930s.

8. See however the beginning of this section.

9. The Pulkovo Observatory became that school (beginning of § 8).
10. Lenin only signed that decree, see my Introduction, § 2. Bagratuni [xi] began his paper by the same wrong statement. *The genius of mankind* was rather its most vicious enemy. Someone told me an episode showing Virovetz from a most repulsive side.

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II

F. N. Krasovsky

Survey of Soviet scientific work in geodesy during 19 years[1]

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Moscow, 1956, pp. 89 – 100. First published 1936

[1] The greater is the territory, the higher are the necessary demands on its astronomical geodetic (AG) and spirit levelling networks. The combined action of very small systematic errors about whose essence we are often ignorant, appreciably tells on the results of geodetic work and can make them not really reliable. It is for this reason that purely geodetic networks, however thorough are their results obtained, must be fitted out with some astronomically determined elements and thus to become AG. In itself, a programme for geodetic work leading to high precision and homogeneity of its results over a vast territory is therefore a scientific achievement. *Organisation scientifique des réseaux géodésiques*, is how the French properly call a complex of appropriate directions determining the patterns of networks and the programmes and methods of their implementation.

A great and important work on the main problems of the organisation of geodetic work has been carrying on from 1923 to the present day by [several departments and institutions are mentioned]. Only a small number of publications regrettably represent its results.

The size of the polygons of primary triangulation; the frequency of its baselines and the arrangement of Laplace stations; the classification of astronomical stations in primary triangulation, and the ascertaining of methods of their determination; problems of the secondary breakdown; the most advantageous distribution of the weights of measurements in primary baseline networks; checks of azimuth determinations; tolerance in AG and spirit levelling networks; the size of the polygons of precise levelling; directions for AG work and levelling of the I and II order; theoretical investigations of the action of errors in triangulation; comparison of quality of the various forms of primary chains of triangulation and the possibility of their implementation in different regions of the Soviet Union, –

this is the list of the main problems which have been tackled from 1923 to the present day.

It is useful to note that rigorous investigations have been partly fulfilled relatively recently, in 1931 – 1935. Until then, the need to offer leading instructions for the rapidly developing geodetic work compelled us to be content with incomplete and non-rigorous studies complemented by the management's experience. Thus [vol. 26][2], the size of the triangulation polygons, the frequency of baselines and the arrangement of Laplace stations was decided on the basis of incomplete studies in Krasovsky's paper (1928), but a rigorous justification of the solution of these problems only appeared in 1935 in

[Izotov (1936) and Zakatov (1937) – Editors]. Just the same, the need to have bilateral Laplace stations in base extensions was established in 1925, although the appropriate data, ascertaining an essential action of lateral refraction on azimuths and confirming the need to have such stations, was only obtained in 1932.

[2] As a consequence of all that scientific work, we have, first, those main directions for the implementation of the state triangulation of the I and II orders, which have been followed from 1925 and are ensuring a good precision and homogeneity of geodetic results over our vast territory meeting the most various practical needs and scientific aims; second, a number of thoroughly worked out instructions and aids which in essence are good manuals for students; third, several published scientific works essentially important for theoretically justifying geodetic practice and further developing the construction of geodetic networks. In addition to the above-mentioned papers, Urmaev[3] and Durnev (1937) belong here.

This last-mentioned work definitely ascertains those regions where triangulation chains of the I order consisting of braced quadrilaterals are more beneficial. It also provides valuable information about the use of local peculiarities of the geomorphological landscape for planning such quadrilaterals. The need to connect and justify geodetic planning with a study of the territory from the viewpoint of physical geography will be practically important.

I will not dwell on the main instructions regulating geodetic works since they are well known. As compared with all other countries, they are characterized by some relaxation of the rigidity of construction in their [the networks'] purely geodetic part, but by more astronomical work than in all large countries, and still more frequent bases as compared with the USA. As a result, we obtain a construction only less rigid (certainly, to a small extent) than the German triangulation, but allowing us to develop the main AG work at least expenses in the flat regions, very unfavourable for precise geodetic observations.

During the latest years, the rapid development of the national economy and industry has raised the demand for surveying great territories to the large scale of 1:10,000. A tendency for an appreciable enlargement of the scales of the general state mapping had appeared. This led to a presently going on revision of the adopted pattern of constructing the main geodetic networks for covering a larger area by chains of the highest order[4].

[3] Concerning the implementation of geodetic work, we ought to note the construction and outfitting of comparators [vol. 12] at MGI for invar wires 24 *m* long. They very favourably differ from those constructed in pre-revolutionary Russia for the same aim. However, when discussing base measurements, we also ought to mention the great work which is going on at the All-Union Institute of Standards (the former Board of Measures and Weights).

They obtained very interesting results unknown abroad: calm periods in the changes of the lengths of invar rods, even old ones and even old platinum rods, are replaced by periods of comparatively noticeable changes. This is one of the results of their subtle investigations, essential for geodesy. A frequent comparison of the working rods of the comparator with the VIMS[5] standard measures, being so thoroughly studied, ensured a high precision in reducing the lengths of invar wires to the international prototype metre. We are justified in stating that our bases are reduced to the same standard measure as those of Finland, Poland, the Baltic republics, Germany, Denmark and England. This is certainly confirmed by comparing our invar wires with Finnish wires which had been used in measuring baselines in all the Baltic republics, Poland and Germany as well as by measurements, in 1935, near Balashov [between Voronezh and Saratov; see Bonsdorff (1935) and Pesonen (1938)] conducted by the Baltic Geodetic Commission. Our own results only differed from theirs by 3 – 4 *mm* per 10 *km*.

Thus, owing to the scientific justification of our base measurements, we are sure that there will be no systematic discrepancies in the linear dimensions when our new triangulations will be connected with those abroad, and there will be no need to find constant corrections to the lengths of geodetic lines caused by the difference of the standard measures.

Previously, we had to do that, and, therefore, to solve a very important problem by issuing from diverse data, i. e., without being really sure. It is not amiss to note that the excellent coordination between our standard measure of length and those of Germany, England, Poland and all the Baltic republics is appreciably disturbed with respect to France.

[4] On Finland's initiative, the West European countries, when calibrating their 24 *m* invar wires, are beginning, during the latest decade, to apply interference comparators. Here, we are somewhat behind. Only in 1933 did TsNIIGAiK begin investigating and planning the application of the interference of light waves for calibrating invar wires. At present, the equipment for an 8 *m* comparator is made and tested, and working plans for a 24 and 48 *m* interference comparators are developed. All the important parts of the equipment were created according to the strictest demands. From the work done abroad all this is distinguished in that we had, as previously, a *line* main standard measure rather than an *end* measure[6]. This led to considerable complications which were successfully overcome. Now, this work entered its next stage.

Changes in the lengths of wires essentially influence the results of base measurements. Actually, it is necessary to investigate their lengths during the measurement of each base of the I order. Therefore, it is very important to apply the method of the interference of light waves for precise determinations <u>in the field</u> of some control baseline. This problem is successfully studied by our specialists. A practical realization of interference methods of calibration of base wires both in laboratory and the field will certainly be an important accomplishment[7].

Next in turn is a construction and equipment of an appropriate comparator in Moscow. We ought to point out that regrettably the scientific work concerning the methods themselves of measuring baselines by invar wires is almost non-existent, and until recently the important question about the causes of the changes in the lengths of the wires was not touched either here, or abroad.

TsNIIGAiK is now investigating wires manufactured from Soviet invar and aims at obtaining indications for producing wires meeting the requirements of measurements of the II order. From 1934, TsNIIGAiK together with the Central Radio Laboratory in Leningrad is experimenting on a large scale in the use of the interference of electromagnetic waves for measuring considerable distances of the order of several dozen kilometres. They issue from Academician L. I. Mandelstam's [vol. 15] scientific directions. Essential difficulties are encountered, but the first stage of work, the determination of distances of the order of 30 *km* with a maximal error of 70 – 80 *m*, is already completed. It is impossible to say now what can be expected from those investigations for geodesy, but we are sure that results, important for mapping uninhabited territories, will be obtained.

The main part of scientific work in these experiments falls on physicists, but the participation of geodesists is absolutely necessary. Only they can correctly formulate a number of technical demands on the equipment; they also, by appropriately combining methods based on the interference of electromagnetic waves with usual geodetic methods of determining distances, will ensure the applicability of the new method in such conditions in which it would not have been successful all by itself.

[5] Going over to scientific work on precise angle measurements, it is necessary to mention investigations of such measurements and of parallactic [trig-] traverses [vol. 20]. An important conclusion is that the results of measurements are corrupted by systematic errors. This inference, warning us against applying large Wild instruments for triangulations of the I order, essentially coincides with the conclusions of the British geodesists made after their recent work in India.

This finding induced firma Wild to start improving the design of its theodolites, and, also, served for us as a cause for discovering such methods of angle measurements which will weaken these systematic errors as much as possible. The importance of these latter attempts is evident since Wild theodolites can play a decisive part in the forthcoming geodetic work above the 60° parallel.

Another essential and rather unexpected conclusion is that under some physical geographic conditions, often occurring in the central strip of the European part of the Soviet Union, the influence of refraction can considerably lower the precision of night observations as compared with those made in the evening. This fact compels us to repeat and widen those studies since their results can considerably alter geodetic practice.

It is not amiss to note either, that our equipment and methods of measurement lead to absolutely negligible influences of instrumental errors and errors of experienced observers on the mean results. The whole business is decided by external conditions which <u>systematically</u> corrupt the results. For precise angle measurements, the urge towards short-term accomplishment of work on a given station is in general at least doubtful.

Not less important is Prof. V. V. Danilov's experimental replacement, in appropriate regions, of triangulation of the II order by parallactic traverses (1937) [also see Danilov (1935)]. The geometrical justification of this method of measuring traverses is due to Gast, but to Prof. Danilov certainly belongs its real geodetic learning. The results of his scientific work are considerably important for practice, but for some reason they are not applied in our main geodetic works to the same extent as in the practice of several departments. In a number of regions this method can prove to be very advantageous for constructing networks of the II and III orders.

[6] Our geodesy has formulated a number of demands on practical astronomy as a consequence of the thorough determination of Laplace azimuths. The desired mean error[8] in the determination of the astronomical longitude of a Laplace station should not exceed 0.″2. For securing such a high precision we had to establish an appropriate time service in Moscow. Its exemplary work certainly ought to be counted as a scientific achievement of geodetic practice. In the near future that practice will probably have to take on the investigation of the polar motion.

The development of methods of determining the longitude of geodetic stations and the classification of longitude stations attracted special attention of our geodesists. This occurred because until now the application of transit instruments with impersonal micrometers for determining longitudes is still restricted.

A collective of astronomical geodesists compiled new ephemerides of Tsinger pairs of stars which ensure the possibility of selecting the best pairs for determining time according to his method [vol. 28]. This is certainly one of the measures improving the precision of longitudes determined in our conditions.

[7] Other essential problems of scientific geodesy are those of treating, and, chiefly, adjusting trigonometric networks. Here, a substantial step was our transition to the rectangular Gauss – Krüger coordinates [vol. 6, geod. projections] initiated by Prof. N. G. Kell (1930). The main point of the problem was not its methodical development but an expedient application of the Gauss projection which first of all demanded appropriately compiled manuals and tables. Our geodesists had done this.

Then, we have numerous papers in the *Geodesist* periodical on calculating corrections for the curvature [of the projection], the transition back from rectangular to geodetic coordinates, conversion of coordinates, drawing of the kilometre grid, etc. All possible simplicity and convenience are now secured for our entire geodetic and cartographic work.

A mathematical connection of all the systems of rectangular coordinates applied in the Soviet Union and therefore their actual unity; simplicity of adjusting and calculating the main networks; results, expressed in exactly those coordinates which should be used in all applied work, such as land use, mining etc, – all this followed from the thoroughly worked out introduction of the Gauss – Krüger coordinates which had been carried out since 1930.

[8] A great problem about the methods of adjusting triangulation of the I order was formulated for TsNIIGAiK already in 1929 when the polygons of the I order had spread from our Western borders to Volga. For that set of polygons the problem was solved by my method (*Trudy TsNIIGAiK*, No. 1 [not mentioned by Bagratuni (1959)] which is a modified version of the Helmert method[9]. Under certain conditions it considerably speeds-up the work.

An essential difference of my version is an adjustment of the separate chains of triangulation making up the polygons for triangular, azimuth and base conditions before the joint adjustment of the polygons [of the geodetic lines replacing the chains]. This preliminary adjustment ought to tell favourably on the establishment of the azimuths of those geodetic lines and therefore to influence essentially the size of the polygonal closures and the possible deformations occurring during the adjustment of the polygons. The adjustment of these first nine [eight] polygons had fully corroborated my approach.

Then, according to my estimates, a preliminary determination of geodetic coordinates and azimuths by issuing from the adjusted chains, if only the initial geodetic data in the origin of the triangulation are favourably chosen, relieves us from retaining, during the polygon adjustment, a number of additional unknowns and reduces the polygonal equations to comparatively simple formulas. The drawing up of those equations does not at all demand lengthy eliminations of the additional unknowns which are a feature of the Helmert method.

The further and very speedy development of the triangulation of the I order and especially its reaching Khabarovsk compels us now to look for new methods of adjusting it, and this is actually being done. But the vast size of our network raises a number of other problems: establishment of the initial geodetic data; transition to a new ellipsoid from the Bessel reference ellipsoid; correct reduction of measured triangulation elements to the chosen main surface, etc.

I will return to these problems below, but now I am dwelling on the methods of adjusting triangulations of the II and lower orders. These triangulations have been very considerably extended, so that from 1931 this problem has become extremely urgent. The works of Urmaev are the most important. He (1931) applied the theory of adjustment in two groups according to L. Krüger and his so-called *transformation*, providing a strict method of adjusting chains and networks without intersecting diagonals and situated between sides of a triangulation of a higher order, excellently suited practical requirements. Then, we ought to mention the work of Krasovsky [1930, 1931] devoted to the assimilation in our country of the adjustment of triangulation networks by the method of variation of coordinates.

Turning to traversing, we have to note that during the latest few years it became here a method ensuring the initial geodetic control over large territories. And we have worked out methods of replacing triangulation of the I order by precise traverses. I bear in mind the method of traverses borrowed from the USA, which, however, we had to change essentially. In our conditions, traverses are laid out along newly cut passages through forests and in primordial taiga rather then railroads and highways.

Precise traverses with its parallactic version along with appropriately laid out chains of triangulation will allow us to construct as successfully as possible the main geodetic control to the north of the 60° latitude. Usual traverses are more beneficial there for obtaining such controls of the lower order required for mapping.

[9] As is seen, during the latest 19 years we have essentially advanced both in the field work, in treating its results and in scientific efforts. To a sufficient extent we have strictly constructed vast main networks on our territory, have met the demands of mapping it as well as the requirements of applied large-scale engineering, land use, mining etc. surveying.

I should dwell now on the scientific application of the results of our AG work and first of all on its application for determining the size and flattening of the Earth ellipsoid and studying the figure of the geoid. I begin, however, by listing our arcs measured along meridians and parallels[10].

Four large meridian arcs are contained between longitudes 27 and 43°.

Three arcs along parallels of 46, 48, 50 and 56°, all of them between longitudes 20 and 25°.

Six short meridian arcs with amplitudes of 4 – 9°.

Vast arcs along parallels 52 and 54°, both beginning at the Polish border. The first one ends in Ust-Kamenogorsk [on the Irtysh river, to the East of Karaganda] with a longitudinal amplitude of 55°. The second one reaches Novosibirsk, then lowers until parallel 49° and ends in Khabarovsk with a total amplitude of about 107°.

More than three hundred astronomical stations are already determined in that network with all the three elements (latitude, longitude, azimuth) thoroughly measured at each. This great work is a most prominent achievement. During 19 years we collected perfect data for scientific goals. It exceeds fivefold the European material gathered over 70 years of the 19th century and is almost equal to that collected in the USA during 1860 – 1910.

Our data certainly are of great scientific importance. The four great meridian arcs have a large weight in deducing the equatorial semi-major axis of the general Earth ellipsoid; our arcs along parallels have a large weight in determining the mean flattening and in addition they provide a unique and sound material for studying the longitudinal changes in the values of that flattening.

A very subtle problem of ascertaining the systematic deviations of the geoid from an ellipsoid of rotation will be studied by essentially issuing from our arc measurements. Its solution is considerably important for geophysicists and geologists and in general for earth sciences which investigate the processes of the Earth’s formation and the life of our planet in the past, present and future.

Thus, we have already contributed vastly and most valuably to Earth studies, and each year our contribution noticeably increases. And we also have to turn attention to our great gravimetric work, a general gravimetric survey of our country, which is going on from 1933[11]. The results of gravity determinations are applied in geophysics, geology and geodesy. Until 1933, this work was being planned mostly in accord with the demands of geological prospecting and was not compact. The general survey began in 1933. It will provide us in the near future with new and wide possibilities for scientifically treating AG data and in applying new methods of studying the figure of the Earth.

Great arc measurements can be here applied either geometrically or, when using some data, by allowing for the influence of the irregularities of the distribution of masses above and below the surface of the Earth. The latter should issue from gravimetric results obtained in geodesy and its advantages can not be doubted. It is aptly to mention B. V. Numerov's reports of 1929 on the application of gravimetric data for determining deflections of the vertical as well as Mikhailov's most important investigation (1939) of the same subject. These works have played an essential part in the application of gravimetry in geodesy. And Numerov [Numerov & Chramov (1936)] had recently published theoretical investigations on the methods of determining the general figure of the geoid from measurements of gravity[12].

[10] In 1932 – 1934 a wide study of the deflection of the vertical by gravimetric measurements had been carried out near Moscow in the region of the so-called *local Moscow attraction*. Many scientists had been investigating it from the 1860s because of the discovered large anomalies of gravity existing in spite of the absence of any overground relief either in that region itself or nearby.

[11] These are the main interesting results. In a flat country, with an appropriate density of gravimetric stations around and near a certain point, in a circumference with a radius of 20 – 30 *km,* and pendulum observations situated 30 *km* apart in the zone between radii 30 and 100 *km*, the mean error of the determined deflection of the vertical in that point, without allowing for the influence of more distant zones, will not exceed 0.″5. Those zones should certainly be taken into account on the basis of the general gravimetric survey of the country; for the region near Moscow their estimated influence should be around 0.″8.

Upon receiving these results, TsNIIGAiK accomplished a number of studies in establishing the size, the flattening and the orientation of a Soviet ellipsoid by jointly applying AG and gravimetric materials.

It is not out of place to say here a few words about the problem itself of establishing a Soviet ellipsoid. Until now, we are still reducing geodetic results to the Bessel ellipsoid oriented by the astronomical coordinates in Pulkovo. This ellipsoid has an equatorial [semi-major] axis about 800 *m* shorter than that of the mean Earth ellipsoid. We still do not project strictly our triangulation but somehow develop it onto that unhappily chosen ellipsoid arbitrarily oriented in Pulkovo.

All this is a relic of old times, unscientific, and it is certainly high time to pass on to other procedures. In our conditions, the determination of the size of our Earth ellipsoid is both a purely scientific and practically important problem. On the other hand, bearing in mind the size of our territory, that ellipsoid will certainly approximate the general Earth ellipsoid. The establishment of our ellipsoid can not be solved without investigating the size and the flattening of that general ellipsoid.

A scientific formulation of that problem and the aspiration to be the first in solving it compels us to fulfil the following demands. The size and the flattening of our ellipsoid should coincide with the appropriate parameters of the general Earth ellipsoid reliably determined by issuing from all the contemporary astronomic, geodetic and gravimetric data. It should be oriented by reliably established geodetic coordinates and azimuth and reduced to the general Earth ellipsoid, in the appropriately chosen origin and to the geoidal height in that place relative to the general Earth ellipsoid.

If and when solving this problem as stated, we will induce other nations as well to put an end to the still existing arbitrariness in the choice and determination of ellipsoids. On the other hand, exactly such a solution leads to correct reductions of all directly measured triangulation elements and the treatment itself of triangulation will become strictly scientific with its results sufficiently precise for the final establishment of the Earth ellipsoid.

We ought to note that, as formulated, the solution of that problem demands a combined use of the results of arc measurements and of the general gravimetric survey. Then, we may note with satisfaction that our scientific geodetic work had already largely established the methods of solving the stated problem.

Finally, we should state that the Soviet Union will be the first nation to treat quite scientifically its arc measurements and to establish an ellipsoid for geodetic work. This is made possible by the material which is being provided by the general gravimetric survey, and, above all, by that attention to Soviet science which will secure the accomplishment of a number of important geodetic and gravimetric projects having purely scientific aims in regions in which life does not yet demand precise work.

Such projects are hardly possible abroad but feasible here and on a large scale in accord with our [geodetic] importance. Only in two or three years we will have stations (for example, near Novosibirsk) with gravimetric coverage extending over a territory of radius 2,700 *km*. For such stations, the deflection of the vertical with respect to the general Earth ellipsoid will be determined with an error hardly exceeding 0.″5, and the geoidal height relative to the normal spheroid with an error less than 10 *m*. Together with the necessary appropriate comparisons with a number of other stations and the results of arc measurements this will properly orient the Soviet ellipsoid.

Then, the programme of our arc measurements is essentially supplemented by the demand, already being taken into account, of a sufficiently detailed gravimetric study of a strip 200 – 250 *km* wide along the meridian or parallel of each arc and appropriately continued in its end points. This new programme of our arc measurements should be adopted abroad as well, but its large-scale implementation is not secured there.

This new programme coupled with the results of our general gravimetric survey will enable us, when treating the arc measurements, to allow for the influence of the overground and underground relief and underground deposits on the direction of the vertical in a given station, without introducing any hypotheses about the structure of the earth's crust. In mountainous regions this work will certainly be somewhat more complicated, but scientific studies concerning the Caucasus and Crimea has already begun.

[12] I should add that from 1935 astronomical gravimetric levelling[13] is being made along the arc measurements. It provides profiles of the geoid and allows to reduce all the measured triangulation elements to the surface of some ellipsoid. TsNIIGAiK had worked out the justification and the method of applying such levelling.

AG and gravimetric data already collected and being collected, the scientific methods already having been worked out and applied when those vast and most valuable materials are used, ensure an essential advance in establishing the general Earth ellipsoid. The study of the figure of the geoid is also formulated on a reliable scientific basis and demands an appropriate reorganization of the programmes of collecting necessary data and of the methods of their treatment in all other countries.

The results obtained along with the establishment of the Soviet ellipsoid, – the distribution of the deflections of the vertical and the geoidal heights above a properly determined ellipsoid, – together with properly established anomalies of gravity will certainly provide most valuable material for geologists and geophysicists. It will indicate underground deposits and prolongations of mountain ridges and give some data on the difference of the densities of those ridges, ascertain the picture of the isostatic compensation for a number of our regions and probably corroborate the existence of systematic deviations of the geoid from a normal ellipsoid.

My preliminary treatment of our arc measurements together with those of the USA and Western Europe already shows that the length of the equatorial semi-major axis of the general Earth ellipsoid is about 150 *m* shorter than that of the now adopted (and based on the geodetic work in the USA); that the existence of a triaxial Earth ellipsoid is sufficiently well corroborated for the zone between 30 and 60° north latitudes which includes the USA, Western Europe and our territory until the 90° meridian (Krasnoyarsk). My sketchy study of applying our geodetic data for scientific aims is sufficiently convincing for the following conclusion: In the Soviet state, the collection of the vast geodetic data meeting practical requirements is carried out simultaneously with large-scale work ensuring their application for scientific aims in such a way which can not be done now in other countries. We ensure compactness and strictness of the results obtained. Our methods of treating the data and programmes of work are ahead of those of foreign scientists. The system of life of the Soviet state based on science secures further and essential advances in the work of our scientists in geodesy. In the near future Soviet geodesy will naturally play the most important part in international geodesy.

Notes

1. This essay was likely meant to honour the 20th anniversary of the Bolshevik coup d'état [*the Great October socialist revolution*] of 1917.

2. Recall that I am thus referring to the English edition of the third Russian edition of the *Great Soviet Enc*.

3. Urmaev published quite a few pertinent papers.

4. One of our professors at MIIGAiK told us, his students, that Krasovsky had organized a conference for various users of geodesy to voice their requirements about the scales of mapping.

5. The institute of standards was mentioned above; now, the author correctly abbreviated that All-Union Research Institute of Metrology and Standardization.

6. Bomford (first edition, 1952, § 2.06) stated that end standards *for long tended to be obsolete although convenient for comparison with the wave-length of light*.

7. As a student of MIIGAiK (1946 – 1951), I did not hear about interference comparators. I participated in measuring a few bases in the Ukraine in 1948, when no control measurements were done in the field. I had also worked a few hours calibrating wires on the Moscow comparator in the classical optical mechanic way. However, the National Standards Lab. of the Finnish Geodetic Inst. measures baselines, at least experimentally, by interference methods from 1947, see Google.

8. *Mean error*, here and below, is likely *mean square error of the mean*.

9. Most important was Helmert's introduction of geodesics which, according to the Krasovsky version, replaced chains of triangulation. They were applied in the adjustment of the polygons after the preliminary adjustment of the separate chains, see also below. On Helmert see Wolf (1968, pp. 324, 378) and Sheynin (1995, pp. 80 – 82). I have studied Helmert's contribution which did not connect the adjustment of networks with the choice of a reference ellipsoid etc. He had to treat a medley of triangulation systems.

10. Zakatov (1950, § 90) stated that the Soviet arc measurements had extended over 45 thousand kilometres.

11. Both Danilov [ix, § 12] and Izotov [x, § 9] indicated, that that survey had begun in 1932.

12. The next section is not connected with the previous text.

13. That levelling determines the geoidal heights relative to the chosen reference ellipsoid (the *profile*). Deflections of the vertical are needed, and the influence of their non-linear change between stations is allowed for by gravimetric measurements.

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III

F. N. Krasovsky

Arc measurements: some new directions in compiling their equations, and in the programmes of their execution

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German version: Krasovsky (1936a)

Adequate precision of projecting a vast triangulation on the surface of the reference ellipsoid adopted for its treatment is only attained when the heights of the measured baselines are known not above the level of the oceans, but above that same ellipsoid; or, in other words, when the *geodal heights* above that ellipsoid at the places of these bases are known. Only then the subsequent treatment of the triangulation provides exactly those *geodetic coordinates and azimuths of its stations* which will wholly correspond to the same magnitudes as understood in higher geodesy. This problem is sufficiently described in my report (1935a).

There can also exist an essentially different approach to reducing an executed triangulation to an adopted reference ellipsoid: after reducing the measured baselines on the ocean level, we will simply develop, or, more precisely, lay out the triangles of the triangulation on the surface of that ellipsoid. And, when preserving the lengths, the angles will be distorted, but quite imperceptibly, even in case of a large territory of the order of the USA, and even when the sizes of the reference ellipsoid considerably deviate from those of an ellipsoid best suited for that country.

Actually, exactly that process of *laying out* triangulations on the reference ellipsoid is now applied in each country when treating geodetic materials, – exactly so, rather than *projecting* triangulations on its surface along the normals to it at the appropriate points of the surface of the Earth. As a result, *we do not obtain the geodetic coordinates* as established in higher geodesy; we get some *geodetic* latitudes and longitudes. They cannot be defined geometrically, and their analytic connection with the real geodetic coordinates is lacking.

It is certainly understandable if this shortcoming and vagueness in treating vast geodetic materials is caused by the absence of appropriate data, but the situation is quite different when the described process of *laying out* is defended as being quite normal. Such a defence is being justified, for example, by the insignificant additional discrepancy which corrupted the great polygon circumscribing almost the entire territory of the USA when the triangulation was laid out on the Bessel ellipsoid rather than on the Clarcke ellipsoid of 1866. Proofs of the insignificance of that discrepancy by long calculations and subtle considerations became of course quite superfluous since Gauss had published the results of applying a conformal mapping of an ellipsoid on a sphere in geodesy.

It seems to me that the problem of simplicity and easiness of treatment of geodetic materials should be abandoned if reduced to methods leading to geometrically indefinite and obscure results, especially concerning systems of coordinates lacking geometrical definition.

It ought to be noted that, concerning the components of the deviations of the vertical, ξ and η, included in the free terms of the equations existing in an AG network, calculation of the geodetic latitudes and longitudes should be carried out with the radii of curvature of the reference ellipsoid. If the lengths of the geodetic lines also reduced to the surface of the reference ellipsoid by projecting them along the normals to both their ends have been applied, the equations of the arc measurements should include the terms p_3ds and q_3ds allowing for the difference of those projections on the *sought*, and the reference ellipsoid.

Actually, we introduce in the indicated equations the lengths of geodetic lines reduced to the surface of the ocean (of the geoid) and therefore derive the free terms of the equations of arc measurements by the same method of *laying out*, but without the terms p_3ds' and q_3ds' with ds' understood as the difference of the projection of a geodetic line on the surface of the *sought* ellipsoid from its projection on the geoid.

The mean square value of the height h of any point of the geoid above the mean ellipsoid should be estimated as ± 50 *m*. Therefore, the presently applied method of treating arc measurements, when h is changing very slowly and little over a large (non-mountainous) region, is accompanied by mean square distortions of the order of ± 40 – 50 *m* in the semi-major axis derived from separate arcs extending even for 15 – 20°. These distortions are due to the impossibility of introducing corrections of the kind of p_3ds' and q_3ds'. It remains unknown how do they compensate each other when the number of the arcs increases. In any case, this is the cause which noticeably influences the precision of the figure and size of the Earth ascertained even by vast geodetic materials.

Imagine now that we were somehow able to determine the heights h of the geoid above the reference ellipsoid. Then we certainly have all possibilities of precisely *projecting* the triangulation stations on the surface of the latter by normals to it at those stations. Only then we will obtain a system of geodetic coordinates distinctly and clearly formulated in the geometric sense.

And then, applying the equation in my report

$$h_i = (1 - \frac{\cos\psi}{k_0})\Delta a + \frac{a\sin^2\varphi}{k_0^3}[\cos\psi - \frac{\sin\varphi}{\sin\varphi_0}(2 - \frac{\sin\varphi}{\sin\varphi_0})]\Delta\mu -$$

$$\frac{M_0}{\rho''}\sin\psi\cos\alpha_0\xi_0 - \frac{N_0}{\rho''}\sin\psi\sin\alpha_0\eta_0 + \cos\psi h_0$$

for the height h_i of the sought ellipsoid above the reference ellipsoid, we may express the corrections p_3ds and q_3ds through Δa, $\Delta\mu$, ξ_0 and

η_0. Neglecting the small influences of the angle at the end of the geodetic line between the normals to the reference ellipsoid and the sought ellipsoid, we obtain $ds = (h_i/R)s$. Then the following absolutely distinct method of compiling equations for ξ and η is provided.

a) Geodetic latitudes and longitudes reduced to the reference ellipsoid and included in the free terms of the equations of arc measurements will be determined quite precisely since the bases of the triangulation are reduced to the surface of the reference ellipsoid by taking in consideration the heights h_i of the appropriate points of the geoid.

b) Corrections p_3ds and q_3ds expressed through Δa, $\Delta\mu$, ξ_0 and η_0 are introduced into these equations. No new unknowns except the height h_0 of the sought ellipsoid above the reference ellipsoid at the origin of the triangulation will then appear. Corrections p_3ds' and q_3ds' will not be neglected anymore. Understandably, h_0 will be unreliably determined from the equations for ξ and η. However, the following method of determining that height (and its removal from the unknowns) will probably become feasible in not a distant future.

In the USSR, in a few years the development of the general gravimetric survey will provide such points in its heartland, around which the continuous gravimetric coverage extends for 2,500 – 2,700 *km*. For such points the height N_0 of the geoid above the surface of the normal spheroid will be determined by the Stokes formula with an error generally less than ± 10 *m*. For one of those points it is necessary to derive ξ_0 and η_0 according to the Stokes theory which will probably be possible with a mean error of the result not exceeding ± 0.″6. The derivation of N_0 as well as of ξ_0 and η_0 should certainly be checked by the other points. Assume $h_0 = H_0 - N_0$ where H_0 is the geoidal height in the origin above the reference ellipsoid; in most cases it is zero and anyway we know it. Introduce the gravimetrically established values of ξ_0 and η_0 in the equations of the arc measurement and we will thus compel the sought ellipsoid to touch the normal spheroid in that origin which indeed is necessary in geodesy. And it is certainly quite sufficient to determine h_0 with an error not exceeding ± 10 *m*.

I believe that not so large additions, although embracing some parts of the adjoining oceans, to the already executed gravimetric survey in Western Europe and the USA will enable after some time to obtain central points around which the continuous gravimetric coverage of the territory extends for some 2,500 *km*.

And so, I have outlined the main propositions whose adoption, as I believe, will introduce clarity in the compilation of the equations of arc measurements, and in addition essentially eliminate local influences on the derivation of the size of the ellipsoid, i. e. the influence of applying baselines of triangulations not reduced to the surface of the normal spheroid. Had the gravimetric survey continuously and uniformly covered all the territory of a large state and in addition extended *everywhere* for 600 – 800 *km* beyond its borders, it would be possible to derive sufficiently reliable magnitudes *N* for geodetic stations and then directly determine the corrections p_3ds' and q_3ds'. The so-called *geodetic latitudes and longitudes* in the equations of arc measurements would have to be calculated, as it is done now also, with the lengths of geodetic lines reduced to the surface of the geoid. Geodetic connections of the triangulations of various countries are lacking, and, when jointly treating the arc measurements, differing ellipsoids are derived, all of them of the same size but differently situated in the Earth's body. The adoption of the stated propositions will very noticeably eliminate that difference. Anyway, the still lacking connection of the ellipsoid derived from separate arc measurements with the general earth ellipsoid will be established so far as it depends on determining N_0, ξ_0 and η_0 with a good precision in the origin of the AG networks of different countries.

The practical execution of the method of treating arc measurements as I have proposed demands

a) The determination of the geoidal heights h_i above the reference ellipsoid in the AG networks.

b) A selection in each country of a central point around which a detailed gravimetric survey extending by 150 – 200 *km* should be done; in the zone between radii 200 and 1,000 *km* gravimetric stations 30 – 50 *km* apart should be established, and 90 – 100 *km* apart in the mean in the zone between radii 2,700 and 3,000 *km*.

c) The worldwide gravimetric survey should be reinforced on the oceans, in Africa, in the Arctic and in some parts of Asia adjoining the USSR from the south and India from the northeast.

Item *a* is already being carried out in the USSR. In non-mountainous regions it is sufficient, as it turned out, to accompany the chains of triangulation by astronomically determined stations 70 *km* apart and a gravimetric survey extending over a strip about 250 *km* wide along the triangulation chain. In mountainous regions it will probably be necessary to have astronomical stations 15 – 20 *km* apart and to extend the gravimetric survey over the whole region including the foothills.

According to my proposal and with my participation, in TsNIIGAiK, M. S. Molodensky, a geodetic engineer, developed the method for these works concerning item *a*. His appropriate paper [1937] is being submitted to the ninth conference of the Baltic Geodetic Commission. In 1935, TsNIIGAiK had carried out works according to item *a* along the meridian arc 700 *km* long from Pulkovo to Orsha; in 1936, these works are covering the arc along parallel 54° from Orsha to the Urals between meridians 30 and 61°.

In the USSR, work according to item *b* will probably begin after 4 or 5 years. As to item *c*, international geodetic institutions should apparently exert vigorous efforts to obtain permanent credit [?] for a worldwide gravimetric survey. It should be organized as a separate enterprise among the international scientific undertakings, it is certainly about time for abandoning hopes [of the work being done by] occasional expeditions.

In concluding, I indicate that the introduction of gravimetric coverage of strips about 250 – 300 *km* wide along the triangulation chains of the I order should be recognized compulsory by all countries and carried out to the full. Such gravimetric material will be useful not only for the aims indicated here; it will allow to introduce reliable corrections to astronomical latitudes and longitudes in those places where these observed magnitudes are noticeably corrupted by purely local influences, pending, of course, at least an approximate gravimetric coverage of the zone around them for 1,000 *km*.

IV

F. N. Krasovsky

Arc measurements in the USSR, derivation of the size of the Earth ellipsoid and study of the figure of the geoid

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1. General considerations about deriving the Earth ellipsoid by astronomical geodetic materials

In the previous century, arc measurements were being treated only by applying *the AG material* collected for them; i. e. the sizes of the ellipsoid were being derived by minimizing $\sum(\xi^2 + \eta^2)$. Here, ξ and η were the components of the deflection of the vertical from the normals to the sought ellipsoid at the astronomical stations of the arc measurements. At the times of Struve, these stations were located about 200 *km* apart; triangulations carried out by the British in India were an exception: they decided to obtain the so-called *group* astronomical stations. By the end of the previous, and at the beginning of this century that distance became essentially shortened and equalled 40 – 70 *km*. Recently, British geodesists are determining these stations along the *main* arcs of the arc measurements in India every 16 *km* in the mean. If some arc measurement is separate, the condition

$$\sum(\xi^2 + \eta^2) = \min \qquad (1)$$

only definitely formulates the problem of deriving a *local* ellipsoid best approaching the geoid by its appropriate part along that separate arc. And even that problem becomes indefinite when the arc is measured along a parallel; instead, the radius of a circumference best approaching the geoid's parallel is derived.

We certainly cannot put up with a random choice of those distances. Obviously, we ought to have such a distance between astronomical stations that an *interpolation* of ξ and η for intermediate [AG] stations becomes possible. But then, as the materials concerning the distribution of the deflection of the vertical indicate, the distance between astronomical stations along arc measurements should only be 15 – 25 *km in a flat, and 5 – 10 km in a mountainous region*.

Imagine now that we have several unconnected arc measurements carried out in different countries. This was the main case when the sizes of the Earth ellipsoid had been derived in the previous century, and the situation is still the same, although some changes were made in treating the materials. In that century, Clarcke based his derivation on the largest of all material. He took into account arc measurements in India; the Struve arc, unconnected with them; and the Anglo-French measurement along the prime meridian unconnected with either as well as the old arc measurement in Africa executed by the British and remaining all by itself.

When treating *n* arcs unconnected with each other, condition (1) certainly does not make up for the lack of connections. After treating them together, we obtain *n* ellipsoids of the same sizes but differently located in the Earth body. Their connections both with one another and with the general Earth ellipsoid remain *unknown*. We are only justified in applying condition (1) when supposing that the deviations of the geoid from the general Earth ellipsoid are of the type of small and short waves called forth by purely local causes. Then we may hold that the thus resulting mean errors of the semi-major axis and flattening of the ellipsoid allow to estimate the closeness of the ellipsoid derived from the *n* arcs to the general Earth ellipsoid.

Exactly this assumption had justified the derivations in the 19th century. It is wrong and we appraise those conclusions accordingly. The existence of *general* deviations of the geoid from an ellipsoid, of wide waves reaching heights up to 200 *m* and covering great areas (perhaps to the extent of a quarter of the surface of the Earth), requires a preliminary isolation of the systematic parts of ξ and η corresponding to those general deviations. Failing this step, the solution of the problem leads to establishing *n* ellipsoids, and the derivation of the best ellipsoid for some separate arc is not demanded. Instead, a requirement geometrically indeterminate, or impossible to be expressed geometrically, is introduced: to set common sizes to all those ellipsoids and thus form some complex from the isolated profiles of the geoid obtained from the *n* unconnected arcs without paying any attention to the longitudinal difference of the situation of those profiles. The probability of the result approaching the sizes of the general Earth ellipsoid cannot be established.

The influence of the general deviations of the geoid from an ellipsoid on the derivation of the sizes of the ellipsoid can several times exceed the greatest of those deviations themselves. Cases are even possible in which the increase in the number of arcs will cause the results to worsen rather than to improve. For example, imagine that our arc measurements are carried out along parallels from the western border to the Pacific Ocean, and, along meridians, until parallel 60° and longitudinally separated by approximately equal intervals. Then, if the arc measurements made in India and Indochina are added to those arcs, and if the Earth ellipsoid is triaxial, or if the waves of the geoid in the zone between latitudes 60 and 20° are situated just as they are in case of a triaxial ellipsoid, the size of the equatorial axis of the ellipsoid will be corrupted by about + 100 *m*. In addition, that size of the ellipsoid in India and Indochina best approaching the geoid will be shortened by 200 – 250 *m*.

Had we known the main features and the geographical distribution of the general deviations of the geoid from an ellipsoid, we would have certainly arranged the arc measurements in a manner weakening the influence of those deviations on the derivation of the Earth ellipsoid. However, until now the executed arc measurements have been placed according to the triangulations carried out for practical purposes, without any allowance for the peculiarities of such a complicated surface of the geoid. This forced simplified approach to the problem leads to the ellipsoid (more precisely, to *n* ellipsoids) derived from the modern arc measurements under condition (1) receiving sizes essentially depending on the random choice of the arc measurements which does not at all allow for the change of the main bends of the geoidal surface.

The error of these sizes calculated by the method of least squares only characterizes the influence of the deviations of the geoidal profiles along the applied arc measurements from some ellipsoid sufficiently suited to the complex of those profiles but *differently oriented for each arc*. This mean [?] error cannot represent the extent of the closeness of the sizes of such an ellipsoid to those of the general Earth ellipsoid. I note that the random choice of arc measurements led to a difference of almost 1,000 *m* of the semi-major axis of the ellipsoid in the two well-known derivations by Bessel and Clarcke.

I will dwell now on yet another method of deriving the sizes of an ellipsoid from AG measurements. Suppose that a large country is wholly covered by an AG network of chains of triangulation of the I order executed along meridians and parallels and in some cases along other directions. Assume that these chains are situated 200 – 300 *km* apart forming a system of geodetic polygons and that astronomical stations are determined, in particular without fail at their intersections with such distances between them that allow to construct connected geoidal profiles along the chains. For each astronomical station we can write out the equations only for ξ, or only for η, or for both these magnitudes. Solving all these equations together under condition (1), we will obviously derive a reliable *ellipsoid best suited to the geoidal surface within the boundaries of that country*.

This problem is of course quite definite geometrically, but the connection of that ellipsoid and the general Earth ellipsoid remains unknown. Even if the area of the considered country is of the order of that of the USA or the USSR, the extent of the closeness of those two ellipsoids cannot be reliably established only by AG methods.

At the very beginning of this century, American geodesists applied the theory of isostatic compensation according to Pratt for deriving the sizes and the situation of an ellipsoid from the AG network of the USA. The topographical isostatic reductions D_m and $D_p\sec\varphi$ of the astronomical latitudes and longitudes φ and λ are the corrections of these magnitudes for the summary influence, in a given point, on the direction of the plumb line of the surrounding topographical relief above the sea level and the *compensating* layer within that level and the *isostatic* surface. This latter is one of the equipotential surfaces characterized by a constant pressure of masses located above it.

It is hardly possible to deny the existence of isostatic compensation *in the mean of the entire surface of the Earth*. However, the pattern of this compensation is not established and, in addition, compensation of separate points or entire regions can be incomplete, or entirely failing, or even too strong. That the magnitudes D_m and $D_p\sec\varphi$ little differ when different patterns are applied, is hardly admissible for geodesists because these little differences are definitely systematic and can noticeably influence the study of the figure of the Earth. Introducing those magnitudes as corrections of astronomical latitudes and longitudes leads to the replacement of the real geoid by an *isostatic geoid*. When admitting, according to the theory of isostasy, that all processes which caused the formation of the Earth crust and are still changing its structure, were concentrated in the layer above the surface of isostasy; that below that surface the conditions of hydrostatic equilibrium have invariably been more or less fully satisfied, – if admitting all that, the *isostatic geoid* will, *on the whole*, be close to that ellipsoid of rotation which we call general Earth ellipsoid.

The treatment of materials pertaining to the USA shows that within its territory the *isostatic geoid* is five times closer to that general ellipsoid than the real geoid. Given isostasy and isostatic compensation, the introduction of the topographical isostatic reductions D_m and $D_p$secφ in the astronomical latitudes and longitudes is equivalent to an essential approach, *by and large*, for the entire Earth, of the directions of plumb lines at astronomical stations of arc measurements to the directions normal to the surface of the general Earth ellipsoid at those stations.

This conclusion is doubtless very important for geodesy, but its application to separate arc measurements or even to a whole complex of such measurements can certainly fail to be successful. Arc measurements and the measurement of gravity in the USA show that the application of the hypothesis of isostasy according to Pratt is entirely successful for the depth of compensation of 60 – 114 *km*. Our AG work leads to essentially different inferences. Compensation of the Urals is to a considerable extent incomplete; no compensation is noticeable in the Middle Volga; in the Caucasus, the introduction of topographical isostatic reductions is only felt for the depth of compensation exceeding 250 *km*.

The last-mentioned fact actually thus formulates the problem of corroborating isostasy: in any region, it is possible to select such a depth of compensation for which a good agreement is achieved between the corrected topographical isostatic reductions, astronomical latitudes and longitudes and geodetic latitudes and longitudes of the same stations, corrected in turn for the passage from the reference ellipsoid to the best suited for the appropriate region.

To continue. In India's heartland the isostatic compensation is also essentially incomplete. On the contrary, it seems to be sufficiently well confirmed for Eastern Siberia and the Far East[1]. In many parts of the USSR the situation is this:

1) On a vast flat and low-lying territory substances of low density are deposited on some depth. In this case *zero* topographical isostatic reductions sometimes certainly do not take any account of the influence of that deposit which lead to deflections of the vertical up to 5 – 8″. In a sufficiently vast territory they are to some extent systematic (Middle Volga).

2) Or, on a low-lying plain stretch extensions of mountain ridges and plateaus formerly considerably sunk are submerged now under a thick layer of alluvium. Lack of allowance for such circumstances often results in obviously wrong signs of the topographical isostatic reductions at points situated nearby. The reductions themselves, rather than correcting the appropriate latitude and longitude, appreciably (up to 8 – 10″) corrupt them.

Owing to the presence, in the heartland of India, of a whole system of massive underground mountain ridges, British geodesists refused to apply there the isostatic theory at all. On the contrary, the results of its application to the arc measurements in Western Europe proved to be by and large sufficiently favourable.

So what new points does the application of the theory of isostasy introduce into the programme of arc measurements? The need to confirm isostatic compensation in a given region by comparing AG deflections of the vertical with the values of D_m and $D_p\sec\varphi$. The need to detect and isolate regions where, because of geological causes, the topographical isostatic reductions are not functions of only the heights of the topographic relief above sea level.

All this compels us to have astronomical stations along arc measurements sufficiently often, probably not farther apart than 30 *km*. In addition, as I believe, triangulation should cover a strip of some width, sometimes 200 – 300 *km* wide, along those measurements, and a number of astronomical stations should be established there as well. The reason is, if the application of the isostatic theory for some stations provides doubtful results, the disturbance of the compensation should be ascertained, at least only in the geodetic sense. This certainly demands a study *over an area* of the residual discrepancies between the astronomical corrections for isostasy and the geodetic results, also amended for the corrections of the sizes of the axes of the reference ellipsoid.

After introducing topographical isostatic reductions, the passage to the *isostatic geoid* compels us to replace appropriately the real geoid in each considered above derivation of the sizes of the ellipsoid from a separate arc measurement or from *n* unconnected measurements. We will once more derive *n* ellipsoids of the same sizes but differently situated in the Earth's body. *However, as a whole, we may expect that the sizes are considerably closer to the sizes of the general Earth ellipsoid than those derived from the same material without applying isostasy*.

We may regrettably only judge the success of applying isostasy from an appreciable decrease of the $\sum(\delta\xi^2 + \delta\eta^2)$ with $\delta\xi$ and $\delta\eta$ being the residual discrepancies between the magnitudes $\varphi - D_m$ and $\lambda - D_p\sec\varphi$ and the magnitudes of geodetic latitudes and longitudes, *B* and *L,* reduced to the derived ellipsoid. This is, however, an *indirect* sign; and in addition a systematic component is invariably left in $\delta\xi$ and $\delta\eta$ because of 1) the adoption of a wrong pattern of isostatic compensation; 2) same, of the depth of compensation; 3) the disturbance of the conditions for hydrostatic equilibrium in the layers below the surface of isostasy; 4) an incomplete compensation in some regions.

The random and systematic components of $\delta\xi$ and $\delta\eta$ are inseparable which surely leads to overstating the precision of the conclusions made when applying the isostatic theory. We should obviously get an adequate justification of that application in geodesy from the results of geological and geophysical investigations. However, until now they do not meet the requirements of geodesy and, as I suppose, we cannot therefore be completely sure in the sizes of the Earth ellipsoid derived by applying isostatic theory even to vast materials.

American geodesists suggested at the same time both isostatic theory and the so-called *method of areas*[2]. I have discussed it in connection with deriving an ellipsoid best suited to the geoid within the territory of some large country. The construction on all that territory of an AG network with chains of triangulation along meridians and parallels 200 – 300 *km* apart[3]; the establishment of astronomical stations along those chains 30 – 40 *km* apart in the mean, and, in particular, without fail, at their intersections; and the compilation of equations for ξ and η not along the arcs (of meridians or parallels) but just as it is done by treating an *AG network*, – such are the main features of the *method of areas*.

It is doubtless essential to obtain geoidal profiles along mutually perpendicular directions for studying that complicated surface as also any other. A whole system of such interconnected profiles obtained when applying the method of areas allows to represent the surface of the geoid by its small parts connected in a single whole, geometrically known, each corresponding to one cell of the network. Americans contrast this method and the *method of arcs*, appearing when selecting for arc measurements only a few of considerable length and adequately accompanied by astronomical stations from the chains of triangulation of a given country.

The arc measurements in Western Europe can serve as an example of the *method of arcs*. It is there that we have an arc along the prime meridian passing through England, France, Spain and ending in Algeria; an arc along the 52° parallel passing through Ireland, England, Belgium and Germany whose German part is an AG network extending along the parallel as a strip 300 – 600 *km* wide; the well-known Struve arc measured by Russian geodesists and approximately following the 25° meridian and extending from Nordkap sticking out into the Arctic Ocean to the mouth of Danube. Although interconnected, they do not at all embrace *an area*. England, France and Spain are covered by chains and networks of triangulation of the I order, but neither France, nor Spain have an AG network covering all their territories, and only along the prime meridian arc the astronomical stations are situated sufficiently often. The Struve arc was geodetically connected with our triangulations of the I order, but an adequate number of astronomical stations either in that remarkable arc or in the chains connecting it with our AG network was not established. The passage from the method of arcs to the method of areas for all the territory of Western Europe is certainly an immediate task of the International Geodetic [and Geophysical] Union.

2. A joint application of gravimetric and astronomical geodetic materials

Gravity measurements ensure the possibility of investigating and determining the deviations of the geoid from the *normal* spheroid. The latter's form (but not sizes) is determined by the appropriate formula of *normal gravity*. Gravimetric work allows to determine the height N of any point of the geoid above the normal spheroid and *therefore to connect a geoidal profile obtained from some arc measurement with the latter's surface*. On the other hand, gravimetric results allow to determine the slope of an element of the geoidal surface relative to the corresponding element of the surface of the normal spheroid, – therefore, to derive the deflections of the vertical relative to the normals to the latter's surface.

Since it is admissible to neglect the difference between the normal spheroid and the general Earth ellipsoid (of the same flattening), *the enormous value of an adequate application and collection of gravimetric materials for solving the main problem of geodesy is obvious*.

Gravimetry provides the heights N of a number of points of the geoid which interest us above the surface of the normal spheroid and the components ξ^g and η^g of the deflection of the vertical at the astronomical stations of arc measurements relative to the normals, at the same points, to the surface of the normal spheroid. Therefore, having gravimetric materials, we can a) Reduce the results of all geodetic observations to the surface of the general Earth ellipsoid (more precisely, of the normal spheroid) which is important when applying and treating large AG networks for correctly deriving the sizes of the Earth ellipsoid. b) Introduce corrections $-\xi^g$ and $-\eta^g \sec\varphi$ to the astronomical latitudes φ and longitudes λ of the astronomical stations of arc measurements and thus obtain latitudes B_0 and longitudes L_0 reduced to the general Earth ellipsoid, and becoming the *true* latitudes and longitudes. To be sure, we had a similar method when applying the isostatic theory to treating arc measurements as outlined above. However, the magnitudes D_m and $D_m \sec\varphi$ will be more or less reliable if the distribution of masses in the Earth's crust founded on the adopted pattern of isostatic compensation corresponds to the real distribution. As to the magnitudes ξ^g and η^g, they are obtained from the observations of the distribution of gravity and are *measured*. In one case, when applying maps with contour lines, we *calculate* the corrections D_m and $D_m \sec\varphi$ and replace observations and measurements ascertaining the irregularities in the structure of the Earth's crust by some pattern of the distribution of these irregularities only by and large confirmed by observations. In another case, we determine the influence of those irregularities on the needed magnitudes by observations without introducing any hypotheses.

Regrettably, that picture of applying, and of the importance of gravimetric materials for treating arc measurements, *when being practically carried out, acquires an essentially different appearance*. To derive quite precisely the magnitudes N a coverage of *all the surface of the globe* by gravimetric stations is needed but will not happen so soon.

A reliable derivation of ξ^g and η^g at some point M assumes that a) Near that point gravity is determined sufficiently often, for example, each 10 *km*. b) The ring between radii 30 and 200 *km* from point M is covered by gravimetric stations 30 – 40 *km* apart. c) A sparser coverage by gravimetric stations, for example 60 – 80 *km* apart, of the ring between radii 200 and 800 *km* from M is made. d) Also, a still sparser coverage by gravimetric stations of the ring between radii 800 and 2,000 *km* from M is in existence and an allowance for the influence of further zones up to radius 4,000 – 6,000 *km* from M is made.

And in addition, when applying gravimetric methods of determining N, ξ^g and η^g, it is necessary to have the so-called *worldwide* gravimetric survey. We should have a most precise connection of the initial gravimetric stations of all countries; the formula of the *normal* gravity should be reviewed and derived anew taking into account considerable new gravimetric materials, especially concerning the USSR; and, finally, new scientific investigations of the complicated problem of reducing gravity should be carried out[4].

At present, we have an insignificant number of gravimetric stations in Africa, still less in Australia and South America; determinations of gravity on ocean islands do not provide adequate results, and the territories of the oceans themselves only began to be gravimetrically studied in 1926 owing to Vening-Meinesz' submarine voyages in the Pacific and Atlantic. To these materials are now added those of the Papanin expedition[5] and the data collected by Soviet geodesists on drifting ships.

This condition of the worldwide gravimetric survey essentially hinders the application of gravimetric methods of investigating the figure of the Earth. However, all such difficulties will fall away in due time; in future, the figure of the Earth will doubtless be studied by jointly applying gravimetric and AG materials. At present, gravimetric work has been developed to a greatest extent in the USSR where *they* are also enjoying greatest attention. Sufficiently developed is the determination of gravity in India and western Indochina.

The successful application of the isostatic theory for deriving the sizes of the Earth ellipsoid from the vast AG network of the USA, and for treating arc measurements in Western Europe, was probably conducive to weakening the interest of the appropriate states in gravimetric work for solving the main problem of geodesy. However, the failure of applying the isostatic theory to arc measurements in India induce the British to study the local geoid by gravimetric methods. The same is true concerning our geodesists since the application of the isostatic theory had failed on a considerable part of our territory.

Can we expect success of that combining of AG and gravimetric materials, bearing in mind, as stated above, that the quantitative results of the worldwide gravimetric survey are scanty; that in some countries adjoining the USSR (Turkey, Persia [Iran]) no gravimetric work had been, or is done, or that it is done on a small scale, and not systematically (Rumania, Poland, Austria, Sweden)? Here are the answers.

Imagine that we have an arc measurement along some geodetic line, a chain of triangulation of the I order with latitude and longitude quite precisely measured at astronomical stations 70 – 100 *km* apart. For such stations we can derive the components ξ and η of the *AG* deflections of the vertical with a mean error of about $\pm$ 0.″3 reduced to some *reference ellipsoid*.

Imagine also that along that geodetic line, in a strip about 250 *km* wide, gravity is determined sufficiently often. Throughout exactly that area gravimetric stations should be located about 30 – 40 *km* apart, and somewhat oftener in the central part of the strip about 80 – 100 *km* wide, in some places even every 10 – 15 *km* depending on the locality or on the variability in the anomalies of gravity. Our young scientist, M. S. Molodensky [1937; 1948], has developed a method that, given such gravimetric material, allows to obtain ξ and η with a good precision for any point situated on a straight line between adjacent astronomical stations of the arc measurement. A peculiar method of interpolating those ξ and η at the astronomical stations themselves is there applied.

Thus, gravimetric work as described just above allows to obtain ξ or η for any number of points along the arc measurement and to determine in sufficient detail the geoidal profile there; however, astronomical stations 70 – 100 *km* apart are also necessary.

A most precise determination of astronomical stations 30 *km* apart or less is not needed anymore, but geoidal profiles should be studied in more detail. Actually, the Molodensky method determines not ξ and η but the *heights N′ of stations of the arc measurement* selected sufficiently often, *above the surface of the reference ellipsoid*. The method is based on interpolating AG ξ and η at astronomical stations of arc measurements *which also include the influence of all the remote zones*. This leads to the allowance for the same influence on the interpolated values of ξ^g and η^g, which is the main idea of my method of 1934. Such a collection of gravimetric materials along arc measurements is equivalent to establishing *a new programme noticeably differing from the programmes of arc measurements described above*.

In the USSR, when in the near future all its territory will be gravimetrically surveyed, and, with astronomical stations situated, as now adopted, along chains of triangulation of the I order each 70 – 100 *km*, the additional work for carrying out this *new programme of arc measurements* will only come to *thickening that gravimetric survey* within strips about 100 *km* wide along each chain serving for an arc measurement. Each third chain of the I order should be thus applied; they will therefore be about 660 *km* apart in either direction, along meridians and parallels.

As a result of the derivation of magnitudes *N′* along all those chains, the geoid will be perfectly portrayed relative to the adopted reference ellipsoid, or any other ellipsoid whose connection with the former is established or given. *That determination and study of the geoidal surface provided by the system of heights N′, if carried out on our entire territory, will all by itself be a most valuable contribution,* such as we did not dare dream about even ten years earlier.

In the inhabited parts of our territory the chains of triangulation of the I order are situated 220 *km* apart with precise main chains of the II order accompanied by astronomical stations 100 *km* apart inserted between them. Such insertions will doubtless be practised in the uninhabited parts of the territory as well. It is therefore obvious that, depending on the features of the course of gravity anomalies in some region, we can study in more detail and more precisely the geoidal surface by thickening the general gravimetric survey in the appropriate places along both the chains of the triangulation of the I order situated between arc measurements and the main chains of the II order.

When all the territory of the USSR is triangulated according to our adopted normal pattern, it will become possible to show the geoidal surface in contour lines relative to the reference ellipsoid or other ellipsoid connected with it in a known way. The errors of the method will certainly be felt when applied over such a considerable territory, but we may suppose that, when the heights N' are transferred by a thousand kilometres, their errors will remain within ± 1.5 *m*. Below, I will indicate what was already done for studying the geoid over our territory by applying that method of *astronomical gravimetric* levelling along arc measurements.

Now, I am dwelling on the transition from N' to N, to the distances of the geoid from the normal spheroid. As stated above, a precise derivation of magnitudes N requires a gravimetric survey of the entire globe. At present, apart from the USSR, only 20% is surveyed. The completion of our gravimetric survey in 1948, and the determination of gravity by Soviet researchers during the next ten years over the whole Arctic, the directly adjoining part of the Pacific and Mongolia, will increase that portion up to 40%. It should be expected that during the same period other countries will nevertheless fulfil some gravimetric work in Africa and South America and carry out additional gravimetric voyages to the Pacific, Atlantic and Indian Oceans[6]. In other words, we may with certainty expect that by 1948 the gravimetric coverage of the globe reaches 45 – 50%. For us, it is important that considerable gravimetrically studied areas directly adjoin our territory from various sides, and that, conversely, considerable unstudied areas are very remote from the USSR (the southern part of the Atlantic, southern and south-western parts of Africa, South America).

All this makes our situation especially favourable for applying gravimetric methods of determining some geodetically important magnitudes. A spherical segment with angle $\psi = 60°$ at the centre of the Earth and coordinates of its centre being $\varphi = 50°$ and longitude $L = 65°$ will concentrate on its surface 75% of the gravimetric coverage to be expected by 1948. Unstudied in that segment will remain a part of China, Arabia, Turkey and Greece although those areas will be each surrounded from all sides by considerable and continuously covered areas.

For the time being, let us imagine that gravimetrically that segment is entirely studied. When deriving N for its central point we separate the globe into zones of 20° and calculate the mean anomaly of gravity in each by applying, when necessary, *hypothetical anomalies* provided by the Finnish scientist Hirvonen (1934). In other words, we assume that, for the mentioned segment, the material derived from observing gravity is entirely available; for all other zones we accept the distribution of gravity according to Hirvonen.

This is how he obtained it. For comparatively small regions situated between the well gravimetrically covered, he interpolated gravity anomalies existing on the edges of those latter. For considerable oceanic regions he accepted gravity anomalies numerically equal but contrary in sign to topographical isostatic reductions of gravity. He thus assumed that the isostatic geoid coincided with the normal spheroid and that on its surface gravity was normal. It seems that, until getting a better map of gravity anomalies, we have to apply the results of Hirvonen. His data leads us to the following results for six zones, I – VI, $\psi = 60 - 80°(20°)160 - 180°$:

Mean anomalies Δ_g: – 0.9; 0.7; – 5.4; 0.0; – 3.9; – 3.0
$F(\psi)$ = – 1.03; – 0.89; – 0.27; 0.34; 0.54; 0.26
$\Delta_g F(\psi)$ = 0.93; – 0.62; 1.46; 0.0; – 2.10; – 0.78. Sum – 1.11 mgl[7]

This means that, had the actual distribution of gravity over the globe coincided with Hirvonen's data, the influence of all parts of the globe situated beyond the segment with $\psi = 60°$ on N will only be equal to 2.5 *m*. For separate and even considerable regions that actual distribution will probably appreciably differ from the one derived by Hirvonen. However, due to the isostatic principle, these deviations of the real gravity anomalies from the Hirvonen *hypothetical* anomalies will, *for the Earth as a whole, to a certain extent possess the features of random magnitudes*.

For 32 trapezia of size 5 by 5 degrees situated in the Indian Ocean including the Sunda Islands the real anomalies were established from Vening-Meinesz' observations after Hirvonen had published his *hypothetical* anomalies for the same region. We find that the systematic deviation of the former anomaly from the latter amounted to 14 *mgl* for a trapezium situated on the equator. That region is among those with sharply pronounced irregularities in the structure of the Earth's crust, so that for most of the other regions ± 14 *mgl* should be thought appreciably exaggerated.

We will now adduce further considerations.

1) Errors of systematic nature in the Hirvonen anomalies Δ_g for some region are mostly occasioned by disturbances of isostatic compensation.

2) For the whole Earth, the sum of systematic errors δ_g in the Hirvonen anomalies should be very near to zero.

This latter proposition makes probable a considerable compensation of $\sum\delta_g$ over separate zones rounding up an essential part of the total surface of the globe. If in a given zone the changes of the signs of δ_g alternate in all k of its approximately equal parts, its mean square value in that zone can be assumed to be $\pm 14/\sqrt{k}$ *mgl* or still less if $\sum\delta_g$ is compensated.

We do not know the distribution of δ_g over different regions, but the six zones, I – VI, accounted for in our calculation of ΔN, cover ¾ of the entire surface of the globe, and we are justified in maintaining that for them $\sum\delta_g$ is close to zero. Assuming that for any of them the mean square value of δ_g is equal in the mean to $\pm 14/\sqrt{2}$ *mgl*, we will thus deny both the compensation of the δ_g in each zone and assume that one of their two values corresponds to a half of the zone, and the other value, appreciably differing from the former, covers the second half of the zone[8].

For such large zones as I – III, we should certainly expect, that in each the mean δ_g is appreciably less than ± 14/√2 *mgl*, since the change of signs of those δ_g will occur not once, but 6 – 8 times. Note also that in zone I the observed values of Δ_g comprise 0.5 of all the values, and 0.3 in zone II. The mean error of *N*, with a definite, as it seems, bias towards exaggeration, is

$$m_N^2 = 2.27^2 \frac{196}{2}[0.25 \cdot 1.03^2 + 0.49 \cdot 0.89^2 + 0.27^2 +$$

$$0.34^2 + 0.54^2 + 0.26^2], \; m_N = 2.27(14/\sqrt{2})\sqrt{1.20} = \pm 24.7m.$$

Hirvonen himself assumes that the mean square value of δ_g is even ± 30 *mgl* but he believes that δ_g remains unaltered over an area of about 36 squares 5 by 5 degrees (on the equator). Then for zones I, II and III we ought to assume *k* equal to 8 – 6, for zones IV and V, 6 – 4, and $k = 2$ for zone VI so that calculation provides $m_N = \pm 28$ *m*. This value is probably also exaggerated, and more than ± 24.8 *m* is[9]. According to my reckoning, the influence of the incompleteness of the gravimetric coverage of the same segment is hardly larger than ± 10 *m*.

During the next decade the worldwide gravimetric material will doubtless noticeably increase. We may therefore assume that the entire mean error of *N* for considered points of our territory will be less than ± 25 *m*. It is important for us that for some points of our territory, especially favourably situated with respect to the available gravimetric coverage, the results of gravimetric work in India and Germany, quite unsatisfactory from the viewpoint of a planned worldwide survey, even now allow to establish the magnitudes *N* with mean error less than ± 25 *m*.

In an appropriately chosen origin of our AG network we can gravimetrically derive the magnitudes ξ_0^g and η_0^g. It is necessary for the gravimetric survey to cover the territory all around such a point not less than for 2,000 *km*. More precisely, for deriving those magnitudes with mean error less than ± 1.″0, a considerably detailed survey inside radius 150 *km*, a gravimetric point every 30 – 40 *km* in the ring 150 – 600 *km*, and a much sparser survey in more remote rings are needed. If almost the entire segment with $\psi = 60°$ can be considered, the mean error of ξ_0^g and η_0^g for that point can be reduced to less than ± 0.″5.

For most of the usual astronomical stations of our arc measurements the magnitudes ξ^g and η^g can be determined with mean random error about ± 1.″5 as already established by TsNIIGAiK. Their influence on the semi-major axis of an ellipsoid, *as solely derived by our AG network, will only amount to about 20 m*. However, these ξ^g and η^g will be also corrupted by systematic errors very little and slowly varying along some parallel or meridian arc. Still, exactly they will impart a *local* nature to an ellipsoid thus derived but will hardly result in an error of the semi-major axis of the ellipsoid exceeding ± 30 *m* if the material expected in 1948 (i. e., when all our territory is gravimetrically covered and new gravimetric work is carried out in Asia beyond the USSR as well as in the Arctic) will be used.

After determining ξ_0^g and η_0^g and assuming the flattening of the ellipsoid corresponding to the assumed formula of normal gravity, the equations of our arc measurements will only contain one unknown, the correction of the semi-major axis of the reference ellipsoid. *Under the stated conditions* it will be generally determined with mean error about $\sqrt{20^2+30^2} = \pm\ 36\ m$ if the error of establishing ξ_0^g and η_0^g is disregarded. When allowing for that influence [wherefrom was the 35 below?]

$$m_a = \sqrt{20^2+30^2+35^2} = \pm\ 50\ m.$$

A gravimetric derivation of ξ_0^g, η_0^g and N_0 doubtless ascertains the most correct position of the derived ellipsoid in the Earth body; and, if ξ^g and η^g are applied, that ellipsoid will be closest to the general Earth ellipsoid with respect to the sizes of its axes. These circumstances all by themselves testify in favour of the methods of establishing an ellipsoid for geodetic purposes in the USSR as described in this section. In spite of the still little developed gravimetric work in the Asiatic part of the USSR, these methods should be applied for the derivation of the Soviet ellipsoid[10]. At present, the comparison of the thus derived results with the derivation by the usual AG approach is very interesting and important since we will certainly reveal the possibilities of beneficially combining the still restricted gravimetric material with the AG data.

Applying all the gravimetric material collected in all countries, we can study the geoidal figure by the general methods of the theory of the Earth figure based on expansions in spherical harmonics of the acceleration of gravity, and of the radius vector of a point on the geoidal surface. Those methods should be altered for providing an expedient use of AG materials and data obtained from the nature of geoidal profiles along large meridian and parallel arcs. However, no one had still applied those methods, or, especially, their mentioned alterations, for treating large materials. I am therefore restricting my account by indicating that those methods should be applied for studying the geoid by and large, *along* with those mentioned here and mostly aimed at studying the geoid within the Soviet territory and at establishing an ellipsoid meeting the requirements of our geodetic work.

3. The state of arc measurements and derivation of the sizes of an ellipsoid in the USSR

At the beginning of this century, the Struve arc, measured under his leadership, mostly by Russian astronomers and geodesists in the first half of the previous century, at Russian expense, was geodetically joined with the new triangulation of the I order whose construction began in 1908. We certainly are completely justified in including the Struve arc measurement in proper Soviet arc measurements, although its considerable part is now in Finland, Poland and Rumania. Among the chains of triangulation of the I order carried out on the territory of the USSR I indicate the following as quite meeting the requirements and the programme of arc measurements or being easy to turn into arc measurements.

1. A meridian chain $L = 30°$ from Pulkovo to Nikolaev on the Black Sea, amplitude 13°.
2. A meridian chain $L = 36°$ from Murmansk to Petrozavodsk, then to Jankoi in the Crimea, amplitude 23°.
3. A meridian chain $L = 42°$ from Kostroma on the Volga to Zugdidi in Transcaucasia, amplitude about 16°.
4. A chain somewhat inclined to meridian 48° from Kazan to Astrakhan, amplitude about 10°.
5. A meridian chain $L = 56°$ from Cherdyn to Orenburg, amplitude 9°.
6. A meridian chain $L = 62°$ from Irbit through Cheliabinsk until Verkhne-Tobolsk, amplitude 6°.
7. A meridian chain $L = 79°$ from Novosibirsk to Alma-Ata [present Almaty], amplitude about 12°.
8. A *parallel* chain Shimsk – Kazan – Baikalovo on the river Tobol, amplitude 36°30′, mean latitude 57°30′.
9. A *parallel* chain Orsha – Cheliabinsk – Krasnoiarsk, amplitude 62°30′, mean latitude 55°.
10. A *parallel* chain Krasnoiarsk – Uhlan-Ude – Khabarovsk, amplitude 42°30′, latitude 56 – 48°.
11. Chain Gomel – Orenburg – Ust-Kamenogorsk, mean latitude 51°30′, longitudinal amplitude 51°30′.
12. A *parallel* chain Pereiaslavl (on the Dniepr) – Stalingrad (Volgograd) – Temir, mean latitude 49°, amplitude 26°.
13. A *parallel* chain Tiraspol – Kerch – Astrakhan, mean latitude 46°, longitudinal amplitude 18°.
14. Chain in a direction inclined to the meridian Orenburg – Kazalinsk (Aral Sea [not existing anymore]) – Alma-Ata (now Almaty), latitudinal amplitude 8°20′, longitudinal amplitude 21°30′; latitudes 52 – 43°, longitudes $L = 54 – 75°$.
15. A *parallel* chain Zugdidi – Tbilisi – Baku, mean latitude 42°, longitudinal amplitude about 7°.

I have not included a few meridian arcs with amplitudes 4 – 6°. Total latitudinal amplitude of the meridian chains including the Struve arc is 122°, total longitudinal amplitude of the *parallel* chains reduced to degrees on the equator, is 166°. Extent of all the chains, 288°.

In this vast AG network we have at present more than 480 Laplace stations with all three astronomical elements (latitude, longitude, azimuth) measured quite precisely. They are situated along the chains of the triangulation of the I order about 70 – 100 *km* apart. At the places where the bases and base extensions are situated, there are two Laplace stations at the ends of the lines of departure of the extensions 15 – 30 *km* apart which is certainly essential for checking and heightening the precision of the Laplace azimuths. These Laplace stations are considered absolutely precise when the chains and polygons of the I order are adjusted. In addition, they are also valuable when the equations of arc measurements are being compiled.

Understandably, when deriving the sizes of an ellipsoid it is advisable to replace each such pair of stations by a single fictitious station. Consequently, we ought to reckon somewhat more than 300 astronomical stations, each of which leads to one equation for latitude, one for longitude, and one for azimuth. The material pertaining to the USA, on which Hayford in 1909 had based his ellipsoid later called *international*, consisted of determinations of astronomical latitudes at 381 stations, of astronomical longitudes at 131 stations, and astronomical azimuths at 253 stations with only 32 Laplace stations among them. In other words, 765 astronomical elements of arc measurements in the USA correspond to more than 900 astronomically determined elements in the AG network of the USSR.

In volume and composition of astronomical determinations our arc measurements are already loftier than those pertaining to the arc measurements in the USA and applied in 1909. However, previously the immensity of these American materials surprised very many specialists. Until our new AG work have developed, the joining of the arc measurements made in the Old World to those in North America barely changed the derivation of the sizes of the Hayford ellipsoid, so small was the joined data as compared with the American material.

Now, however, our arc measurements together with those in Europe provide a *somewhat larger* material than applied by Hayford. The Old and the New World already enjoy at least equal rights in deducing an ellipsoid from arc measurements. This is doubtless very important for the study of the figure of the Earth.

I have indicated that our astronomical stations are situated along the chains of triangulation of the I order 70 – 100 *km* apart. As stated in § 1, according to the modern opinion about the programme of arc measurements, that distance is too great. Owing to possible essential local deflections of the vertical, a random placing of astronomical stations does not allow to construct quite reliable geoidal profiles. As to arc measurements, I ought to say the following.

1. An appropriate frequency of astronomical stations is only achieved in *a part* of the arc of the meridian in Western Europe and in two new *main* arc measurements in India. Had we demanded quite strictly the determination of astronomical stations along the arc measurements 30 *km* apart, we should have thrown away sufficiently much European and American material.

2. Our general gravimetric survey, vigorously carried out since 1933, allows us to consider differently the frequency of astronomical stations along our arc measurements, see § 2. Actually, if keeping to the plan of astronomical gravimetric levelling along the meridian and *parallel* chains of triangulation of the I order, 450 *km* apart, astronomical stations less than 70 – 100 *km* apart along arc measurements coinciding with such chains will not be needed. Even for those chains of the I order along which the astronomical gravimetric levelling has not been carried out, the general gravimetric survey in most cases allows to detect sufficiently sure the regions of local attractions and, together with the values of $\varphi - B$ and $(\lambda - L)\cos\varphi$,

to establish the course of the deflections of the vertical between the astronomical stations of the arc measurement and the changes in ξ and η in their vicinity, although with a fairly low precision.

We can thus decide whether one or another astronomical station was appropriately placed along an arc measurement and to say correctly whether, and exactly where, additional gravimetric determinations and astronomical stations are there needed. It seems to me that in a number of cases the materials of the general gravimetric survey, if it is compactly covering the regions intersected by arc measurements and is appropriately thickened in some places, allow to correct and supplement, after necessary calculations, the determinations of AG ξ and η without adding new astronomical stations. We are therefore able to say definitely: the fulfilment of the general gravimetric survey on our territory as formulated and being carried out, and *the joint application of gravimetric and AG materials* allow to consider the existing chains of triangulation of the I order (see their list above) accompanied by astronomical stations in general *sufficient* for being applied as arc measurements. The need for some additional gravimetric and astronomical work along these chains is certainly not excluded.

In 1935 – 1937 TsNIIGAiK fulfilled astronomical gravimetric levelling along the meridian arc Pulkovo – Orsha (on the river Dniepr) – Gomel – Nikolaev (on the Black Sea) and the *parallel* arc of $\varphi = 54°$ from Orsha to Cheliabinsk. During those three summer seasons two teams had been able to fulfil, according to the new scientific programme, two arc measurements of total length 3,400 *km*. Soviet geodesy can take pride in such work.

Our arc measurements should include astronomical gravimetric levelling in their programme. After being fulfilled according to that new, to that Soviet programme, they will provide materials of value and quality unmatched by similar materials pertaining to any other country. Only they will directly rather than indirectly determine geoidal profiles along considerable distances, determine the geoidal figure over a vast territory and provide a most reliable and most valuable additional material for deriving the geoidal heights *N* above the general Earth ellipsoid (see § 2). And on top of all that, the results of this astronomical gravimetric levelling are needed for an appropriately precise treatment of such a vast AG network as being constructed in the USSR[11].

The territory of the European part of the USSR, from the meridian $L = 30°$ in the west to meridian $L = 62°$ in the east, from parallel 58° in the north to parallel 48° in the south, is covered by chains of triangulation of the I order sufficiently uniformly accompanied by astronomical stations. Here we have, in essence, arc measurements carried out according to the method of areas. From that AG network, with appropriately applied rich gravimetric materials, we can very reliably derive an ellipsoid *best suiting the geoid on that territory*. However, to the east of meridian 62° arc measurements are covering an area not so uniformly and only between parallels 56 and 45° until meridian 82° (Novosibirsk). There are no arc measurements at all to the north of parallel 56° eastwards of meridian 62°, and from meridian

82° to Khabarovsk the AG network is represented by a *parallel* arc, at first having mean latitude 54°, then 50°.

Two thirds of the Asiatic part of the USSR is not yet touched with arc measurements, and very considerable AG work is certainly needed there. Given the existing arrangement of our arc measurements, *the problem of deriving an ellipsoid best suiting the geoid over all the territory of the USSR cannot be raised*, – exactly because our Asiatic part is only represented by one profile along the arc Novosibirsk – Irkutsk – Khabarovsk. Our arc measurements, including the Struve arc, are extending from parallel 70 to parallel 44° but a continuous large amplitude only exists along the Struve arc and the arc Murmansk – Jankoi longitudinally situated rather near the former. The maximal amplitude of the other meridian arcs does not exceed 14° and cannot be called considerable.

This not very large latitudinal range and the prevailing large arcs along nearby parallels condition a fairly low reliability of only deriving the sizes of an ellipsoid from our arc measurements: we get the semi-major axis and the flattening with mean errors about ± 100 *m* and ± 2 in the latter's denominator.

Enough was stated in § 1 about the significance of those mean errors obtained when only deriving the sizes of an ellipsoid from AG results. They only acquire a certain geometrical definiteness if some *compact* territory is uniformly covered by arc measurements according to the method of areas. Then they characterize the *possible* precision of selecting an ellipsoidal surface best suiting the geoid over exactly that territory. However, our territory serviced until now by arc measurements is arranged rather intricately. [A detailed description of that arrangement follows.]

We may conclude that joining the *parallel* arc with latitudes 54 and 50°, see above, to the arcs in the well covered area will not result in deriving an ellipsoid best suiting the geoid over either that area or along that arc. […]

And so, we cannot at present raise the problem of establishing an ellipsoid *best suited to the geoid over all our territory*. And, when solving together the collected by now very numerous equations of all our existing arc measurements, we are deriving an ellipsoid that cannot be recognized as the best suited to the geoid over the territory enveloped by these measurements. Its intricate configuration and the mentioned presence of very large geoidal waves prevent such a recognition.

Nevertheless, the treatment of the existing arc measurements by TsNIIGAiK during 1934 – 1938 has a definite importance. *First*, we ought to mention the considerable work on compiling the equations of arc measurements whose number reaches 1,200. Their catalogue will be *extended* with the further development of our arc measurements. At present, it is a most valuable contribution of Soviet geodesy to the future establishment of the general Earth ellipsoid and investigation of the geoidal figure.

Second, gravimetric ξ^g and η^g were derived for a considerable number of astronomical stations (about 200) with gravimetric stations covering the territory surrounding them not less than for 600 *km*. By applying them as corrections to astronomical latitudes and azimuths, and issuing from the semi-major axis and flattening of the ellipsoid derived by Krasovsky in 1935,

a = 6,378,296 m; μ = 1:298.6,

the corrections to the initial azimuth of our AG network and its initial latitude were determined from the latitudinal and azimuth equations of the arc measurements. *To orient appropriately a large AG network is doubtless a very important and very responsible problem.* Applying here gravimetric materials is very essential since it considerably rids the derivation of the initial azimuth of the influence of the small, and partly of the general waves of the geoidal surface. Such an application of gravimetry in geodesy is the first ever made.

Third, the sizes (and flattening) of an ellipsoid were derived a) Only from our AG material. b) From the same material but applying isostasy. Topographical isostatic deflections of the vertical according to the Bonsdorff method were derived for great many stations. c) From the AG material of the USSR and Western Europe, without, and then with applying isostasy. d) From the material of the USSR and USA.

In 1935 – 1936 Krasovsky obtained[12]

1. From the Soviet AG network up to Novosibirsk, without the Kazakhstan chains
a = 6,378, 182 ± 96 m, μ = 1:298.97 ± 2.0 [in denominator];
a = 6, 378,097 m if μ = 1:297.
2. Without allowing for the Earth ellipsoid being triaxial
2.1. From Soviet materials: see first line in previous item.
2.2. From the USSR and Western Europe
a = 6,378, 247 ± 58 m, μ = 1:300.6 ± 1.4;
a = 6,378, 183 m if μ = 1:298.6.
2.3. From Western Europe and the USA
a = 6,378, 373 ± 35 m, μ = 1:298.3 ± 1.1
2.4. From the USSR, Western Europe and the USA
a = 6,378, 338 ± 32 m, μ = 1:299.97 ± 0.8;
a = 6,378, 307 if μ = 1:298.6.
3. Allowing for the Earth ellipsoid being triaxial, assuming mean polar flattening 1:298.6, longitude of longest meridian + 10°, equatorial flattening 1:30,000, four alternatives 2.1 – 2.4 provide:
a = 6,378,165; 6,378,193; 6,378, 235; 6,378,210 m.

When additionally considering materials that have existed at Krasovsky's disposal, the arc measurements in Kazakhstan to the east of meridian 62° south of parallel 56°, and the *parallel* arc measurement Novosibirsk – Khabarovsk, the results above somewhat change; TsNIIGAiK soon publishes these new findings. It is typical that for flattening 1:297 the Soviet materials noticeably shorten the semi-major axis of the Hayford ellipsoid. Different methods of treatment and differing composition of materials (with or without isostasy, arc measurements only to meridian 62° or all of them) by 280 – 130 m. *The joining of the Soviet, American and West European materials leads to a small shortening of that semi-axis, only by 50 – 70* *m, but to an appreciably lesser flattening* (to 1:299.6 – 1:300 from 1:297). For flattening 1:297 the joint materials of the USSR, Western Europe and the USA shorten the larger semi-axis (6,378,388 *m*) of the Hayford ellipsoid by 130 *m*.

Also curious is that agreement between the derivations of the Earth ellipsoid from the Soviet; the Soviet and West European; the Soviet, West European and American materials calculated by Krasovsky who took into account the influence of the ellipsoid being triaxial. The new derivations by TsNIIGAiK will probably little change these conclusions. The provided results lead to very important and interesting inferences about Soviet arc measurements and their treatment:

1. The decision of the International Geodetic [and Geophysical] Union (adopted without Soviet participation), nevertheless only carried out in Finland, to call the Hayford ellipsoid international, *should obviously be revised by taking into account the Soviet arc measurements*.

2. Soviet arc measurements in agreement with gravity determinations reveal, all by themselves as well as by comparing them with materials of arc measurements of the USA, Western Europe and India, *the presence of very large geoidal waves and the need to allow for their influence on conclusions from arc measurements*. The vast arc measurement along parallels 56 – 52° from our border with Poland to the Pacific Ocean is extremely important for ascertaining the position of such waves on our territory. The addition of the European arc measurements along parallel 52° and the geodetic connection in the near future of the AG networks of the USSR and the USA will make it possible to establish those waves along a great profile. But it is essential to carry out astronomical gravimetric levelling along arcs extending as far as possible. Done along parallel 55° from Orsha to Cheliabinsk, it is an example for all other countries. However, having provided this example, we should obviously execute such levelling along a *parallel* arc from Cheliabinsk to Novosibirsk to the Pacific Ocean, and then along the coast of the Sea of Okhotsk until the Bering Straits[13]. Astronomical gravimetric levelling is needed not only for obtaining considerable geoidal profiles, but for precisely adjusting our large triangulation as well.

3. According to considerations expounded in § 1, the mean errors obtained, when solving together the equations of all these arc measurements in the USSR, Western Europe and the USA, cannot reliably estimate how close does the ellipsoid derived from all those arc measurements approach the general Earth ellipsoid. The mean errors of Krasovsky's derivation, ± 32 *m* for the semi-major axis and ± 0.8 for the flattening's denominator, are certainly [only] formal. They would have represented reality had there been no very large geoidal waves, or had their influence been reliably accounted for. An attempt to allow for them made by Krasovsky led to a change in the semi-major axis of almost 100 *m*. In other words, no justification is yet possible for assuming that the derived conclusions based on all modern arc measurements (except those in India) establish the sizes and the flattening of the general Earth ellipsoid. This is all the more true since

those derivations, although possessing more weight than any other calculation, did not apply the extensive Indian materials or the new (not yet published) materials of the large, not yet accomplished arc measured by the British in Africa, from the Cape of Good Hope to Cairo.

Nevertheless, these derivations allow to establish reliably an ellipsoid for treating the materials of Soviet geodetic work and for calculating our AG network.

The presence of large geoidal waves on our territory conditions a possible deviation of the geoidal surface from a most correctly established ellipsoid of 50 *m* or more. A precise treatment of our large AG network will therefore require a reduction of some of its elements to the surface of such an ellipsoid. The essence of that problem will not change at all if the sizes of the semi-axes of the derived ellipsoid differ from those of the best suiting ellipsoid even by 150 *m* (but not by more). *And I think that such permissible variation means that the problem is already solved.*

4. Establishing an ellipsoid for treating our vast AG network requires, apart from selecting the sizes of its semi-axes, its correct orienting. This problem is reduced to establishing the components ξ_0 and η_0 of the deflection of the vertical at the origin of the triangulation and the geoidal height N_0 there above the general Earth ellipsoid. The solution of this problem should be based on applying gravimetric results, see § 2. The derivation of ξ_0 and η_0 by TsNIIGAiK cannot be considered final, but apparently it still allows to establish those magnitudes with mean error not exceeding ± 1″.0 which is also an essential result of our treatment of arc measurements.

* * *

The results of our modern arc measurements should be considered sufficiently important and instructive. During the next years arc measurements will be executed along the large rivers of our Asiatic part, along Ob, Yenisei, Lena, Kolyma, and along the coast of the Sea of Okhotsk (probably until the Bering Straits). They, and the astronomical gravimetric levelling carried out along them, will eliminate the mentioned weak aspect of our modern arc measurements. On the other hand, during the same time the development of the worldwide general gravimetric survey will allow to raise the problems indicated in § 2 whose solution leads to the possibility of establishing the general Earth ellipsoid *only from our* AG and gravimetric materials with mean error of semi-axis [which?] less than ± 50 *m*. Our vast materials and our programmes of their collection and methods of their treatment radically change the statement of the problem of studying the figure of the Earth and its sizes that is still existing in foreign countries.

Notes

1. The text of Krasovsky's contribution, as reprinted in his *Sel. Works*, is accompanied by numbers [1], [2], …, dutifully copied in my translation, and denoting the appropriate Notes which are, however, almost lacking. His original text of 1939 has neither Notes, nor those numbers. With a single exception, the Notes below are my own.

2. My translation back from Russian.

3. Meridian and *parallel* chains may be replaced by chains in arbitrary directions which, however, should separate the territory in cells. F. K.

5. Ivan Papanin headed a few *polar stations*. The best known is the drift-ice research unit, 1937 – 1938.

7. Notation $F(\psi)$ not explained.

8. If the values of δg in a given zone belong to one and the same statistical totality, their arithmetic mean is their best estimator. Otherwise, it is difficult to choose a reasonable estimator, and, anyway, the author did not substantiate his choice.

9. This value is correct although it deviates a bit from ± 24.7 as stated in the formula above.

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V

F. N. Krasovsky

Some considerations about executing the main astronomical geodetic work in the USSR

Izbrannye Sochinenia (Sel. Works), vol. 2.
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[1] The socialist construction in the USSR and the […] third five-year plan […] raise for our geodetic service great and responsible tasks of mapping our territory and of the accompanying execution of the main astronomical geodetic work. In the next years we are faced with the completion of the main geodetic control over the most important economic regions of our European and Asiatic parts […] and with accomplishing a large volume of such work in our eastern and northern regions. I am dwelling on the peculiarities of the organization of that work in the regions where geodetic practitioners did not yet have enough experience. I allow myself to describe briefly our different regions for appraising, at least approximately, those peculiarities.

At first, we select the territory of our European part and all the Asiatic part to the south of parallel 55°. Except for some regions, that territory is already sufficiently studied, its very essential part is covered by AG work. In other words, the new barely familiar regions constitute its small part. However, since that territory is great, its small part is not small at all. The new regions occupy an essential area and they will now attract our attention. Such regions are […].

Complications accompanying the AG work in [some of these regions] are sufficiently known, as I believe, to geodesists. [Other] Alpine regions skirt our state border in a strip 100 – 150 *km* wide and directly adjoin inhabited (on our side) areas. This, together with some, although incomplete, geographical knowledge of them, will essentially simplify geodetic work there.

[A detailed description of the various *new* regions follows.]

It is seen now that the mountainous and Alpine regions of our Asiatic part constitute about 65% of its general area which radically changes the established conception about USSR being a flat low-lying land. The success of carrying out the main AG work on the described territories wholly depends on appropriate preparation which should include

1) Collecting and studying the materials and results of geographical, geological, cartographic and soil-scientific and botanical expeditions as well as of investigation of rivers.

2) Collecting information from experienced practitioners about the conditions of executing topographical geodetic work in various difficult regions.

3) Outlining a draft of the necessary minimal volume of the main triangulation chains and levelling lines. They should be situated in the most advantageous places answering, however, some general geodetic requirements, see below.

4) Executing along some of the selected (see Item 3) chains and lines special preliminary geographical geodetic studies based on the collected cartographic and geographical materials, done either directly, or after preliminary surveying the locality by air photography.

5) Specifying, according to the results achieved in item 4, the placement of the selected chains and lines and establishing the organization and methods of geodetic work.

6) Carrying out a number of scientific and experimental investigations answering the peculiar features of geodetic work in essentially differing geographical conditions.

7) Taking measures ensuring the most possible application of aviation, radio engineering and motorized vehicles.

8) Designing, manufacturing and testing newly constructed instruments and devices suitable for working under conditions existing in difficult regions.

Geographical geodetic investigations provide material for more justifiably outlining the situation of triangulation chains and levelling lines and ought to indicate a correct and efficient way of organizing the observations and to formulate sufficiently exhausting descriptions of the conditions of field work, to substantiate the method of constructing the control network (possible replacement of triangulation by traverses, or turning from *usual* to small triangles, or to braced quadrilaterals etc).

Taking into account the lack of experience and poor knowledge of a number of regions, a programme of such investigations cannot be yet discussed. Just the same, the geographer will not at first understand geodetic requirements, and the geodesist will be unable to formulate them to the geographer. However, mutual understanding will be sufficiently soon reached during the work itself and initial failures of geographical geodetic investigations should not trouble anyone.

In many cases the geodesist, being a member of the investigating team, will have to determine astronomically some most typical and far apart points, obviously with low precision, just to show them on a small-scale map. Indeed, in unstudied regions even substantial rivers are shown on modern maps with mistakes amounting to a few dozen kilometres. Those astronomical observations are also necessary for connecting together all the air photography applied for investigations in difficult regions.

In Alpine, poorly studied regions geodetic work should begin by air surveying, certainly in a simplified version, which will allow to obtain a rough map of the investigated area. In a number of cases this work will possibly be beneficially replaced by [visual] investigation from above. Understandably, investigation at ground level will always remain necessary; however, its programme essentially shortens if done after surveying in either way from above. The study of the nature of frozen ground [of permafrost] will be certainly included in the programme of investigating a number of regions. During such investigations, geodesists should especially ascertain the benefits of combining reconnaissance with construction of geodetic signals; a part of astronomical observations with measurement of bases, and another part of those observations with measurement of angles in triangulation.

[2] Research cannot be torn away from practical requirements. These latter will be specified during the AG work, and the subject-matter of research should develop in the appropriate direction. At present, it is quite safe to outline some subjects, but it is yet impossible to establish their exhausting list. I may indicate the need to study/accomplish the following:

1) Determine the lengths of [invar] wires applied for measuring baselines by issuing from the work itself.

2) Investigate those wires and the accompanying equipment for finding out how to select them for measurements of the I order and for ascertaining a number of errors of those measurements.

3) Establish a system of appropriately equipped control bases.

4) and 5) How to measure baselines at low temperatures, in swamped localities and on ice.

6) How to apply interference of light waves in baseline measurements.

7) How to apply light filters to measurements in triangulation.

8) Ascertain the actual errors of triangulation of the I order measured without sighting on light targets and the conditions under which those errors become as small as possible.

9) Replace light signalling by opaque beacons. The possibility of automatic rather than human light signalling.

10) Test the application of theodolites of lesser weight (18 *cm*) and of the Wild type in observations of the I order.

11) Compile observational programmes for triangulation in regions of poor visibility.

12) Investigate refraction in trigonometric levelling.

13) Ascertain types of centres and methods of their laying, and of geodetic signals in regions with frozen ground [with permafrost?] in all latitudinal zones.

14) Design a type of a geodetic universal theodolite of lesser weight for precise angle measurements in triangulation of the I order in our northern and difficult mountainous regions.

15) Design a type of astronomical universal theodolite of lesser weight or of a vertical circle for determining stations of the III order (see below) in difficult regions.

16) Investigate how to take into account the changes in the personal equation in astronomically determined observations of longitudes.

17) Investigate the determination of time by measuring azimuths in astronomical work between parallels 60 and 75°.

18) Specify the results of determining astronomical azimuths in triangulation.

19) How to place Laplace stations less apart for lowering the requirements of precision in angle measurements.

20) Ascertain the conditions under which the replacement of triangulation by traverses is profitable.

21) Design and construct a central geodetic proving ground with a central time service.

AG work in the regions of Siberia and the Far East will certainly become essentially more successful with the further achievements of aviation. The progress of aviation and radio engineering can substantially tell on the organization of that work, radically changing it, speeding it up and cheapening it. In spite of the great work done until 1939 at constructing triangulation of the I order and of the main chains of the II order, *we are faced with an appreciably larger volume of the AG work*. There are grounds for supposing that in the forthcoming decade the aims of the AG work should be as follows:

1) To complete the construction of the chains of triangulation of the I order and the main chains of the II order in our European part and, with the exception of some regions, on the territory south of parallel 55° in the Asiatic part.

2) To begin the main AG work on the other territory to the north of parallel 56° [55°?] in our Asiatic part and develop them in accordance with a) The increase in economic importance of some regions. This activity should not be late for the beginning of precise topographical surveys; and b) The geodetic and cartographic requirements of the national geodetic control network.

The work demanded by Item 1) means constructing chains of the I order and main chains of the II order extending over 18, and about 25 thousand kilometres. This is not a small amount, but, with the exception of some regions situated to the south of parallel 55°, geodetic work will be carried out either in sufficiently well-known, or comparatively known regions according to established programmes and organization. The excepted regions are essentially peculiar; there, a control network for surveying to scales 1:50,000 and 1:100,000 should be constructed. Therefore, it will be necessary to keep there to the normal pattern of the main triangulation which certainly leads to an increase in the volume of work. However, we should suppose that the work in our European part and the Asiatic part to the south of parallel 55° will, and must be fulfilled in the nearest future along with some work in the Kolyma economically important region.

[3] Item 2) concerns the great territory of our Asiatic part to the north of parallel 55°; we should note here first of all that in any of the eight regions mentioned above [not included in the translation] precise triangulation for compiling maps to the scales of 1:500,000 or 1:200,000 is not needed at all. The control network quite sufficient for those maps ought to consist of a network of astronomical stations of the II and III orders. In other words, the execution of chains of the I order and the main chains of the II order has here an entirely different aim as described below.

First, it is absolutely necessary to carry out a certain system of chains of the I order in Siberia and the Far East. In future, it should ensure a comparatively easy execution of chains of the same order or of main chains of the II order connecting that system with regions of topographic surveying. This surveying will then belong to *a single system of coordinates*, sufficiently well established and easily transferred from one region to another. For achieving this goal, the sides of the polygons of triangulation of the I order can understandably be 600, and in some places even up to 1,000 *km* long instead of the normal length of 220 *km*. However, such sparse chains should more or less uniformly, depending on the economic importance of the appropriate regions, cover the territories of Siberia and the Far East.

Second, we ought to take into account the need for a certain stability of the coordinates of triangulation stations obtained when the triangulation of the I order is treated. The final general treatment of that triangulation will undoubtedly be postponed until all the territory of the USSR is covered by the normal pattern of the triangulation of that order. Until then, we may only adjust and treat those parts of the general triangulation of the I order, which will be ready up to some *intermediate* dates. However, these particular adjustments should provide the coordinates of the stations of the network of the I order whose changes, after subsequent adjustments covering ever more material, and after the final general adjustment, will be practically immaterial even for surveys to the scale 1:25,000 (i. e., changes not exceeding 8 *m* over all our territory).

For ensuring such a degree of stability, it is necessary, first, to observe directions with mean error ± 0″.4 and *observe* Laplacean azimuths with error about ± 0″.5. Second, to include a *sufficiently large part* of the triangulation of the I order into its next adjustment and to carry it out without splitting up the entire material at hand. For satisfying these requirements, we definitely ought to include polygons of the I order, covering about 2/3 of all the territory of the USSR, in the forthcoming (but preceding the final) adjustment and calculation of that (of the entire) triangulation.

Therefore, we ought to have an entirely completed system of polygons of the triangulation of the I order in our European part and to the south of parallel 55° in the Asiatic part and, in addition, to complete all the polygons of the same order at least with sides 600 – 800 km between parallels 55 and 62 – 63° in the Asiatic part and in the Lena – Indigirka and Kolyma regions. Then the territory left without the triangulation of the I order will cover not more than 1/3 of the USSR and be enveloped by that triangulation from three sides. With high-quality observations, this will ensure the necessary stability of the results. Note that, owing to their economic importance, the eastern part of the first mentioned region and the whole second region should be covered by that triangulation in the nearest future.

Third, we should not overlook the possibility of a future wider application of the system of astronomical stations of the II and III orders (see below), – of applying them not only for compiling maps to the scales 1:400,000 [1:500,000?] and 1:200,000, but for mapping a number of regions in the Asiatic part to the scale 1:100,000. Astronomical stations of the III order established by *observing* latitudes with mean error ± 1″.5 and longitudes with error ± 3 – 4″ 60 – 80 *km* apart upon the average quite ensure mapping to the scale 1:500,000 *with no allowance for the influence of the deflection of the vertical being necessary at all*; or, more precisely, only necessary in some regions with essentially and rapidly changing gravity anomalies.

If the astronomical network is only constructed in accordance with those requirements, all its stations, numbering about 1,500, will become useless when the scales of mapping increase. For scale 1:100,000 the mean errors of the control stations should not exceed ± 1″.0 and ± 1″.0 secφ (φ denotes latitude) respectively. *These errors, however, should include the error of allowing for the influence of the* *deflection of the vertical*. Therefore, when the network of astronomically determined stations should be applied for compiling maps to the scale 1:100,000, we ought to require determinations of astronomical latitude and longitude with mean errors ± 0″.6 and ± 0″.6 secφ and mean errors about ± 0″.9 in ξ and η, the latitudinal and prime vertical components of the deflection of the vertical.

For attaining this precision it is necessary to have, first, the mentioned precision of astronomical determinations corresponding to the so-called expeditionary stations of the II order; second, *the possibility of applying gravimetric material for deriving* ξ *and* η *with mean error about ± 0″.9. The deflections of the vertical should correspond to the very same* [reference] *ellipsoid to which all the stations of the triangulation of the I order are corresponding*.

In other words, we ought to turn from the astronomical latitude and longitude of any astronomical station to its geodetic latitude and longitude, and to do it with mean errors in latitude ± 0″.9 and longitude ± 0″.9 secφ. This is a sufficiently strict task which requires a gravimetric determination of ξ_0^g and η_0^g for the origin of triangulation with high precision, at first corresponding to the normal spheroid, then a sufficiently precise derivation of ξ^g and η^g for the given station, also corresponding to that spheroid, then transferred to magnitudes ξ and η corresponding to the ellipsoid adopted for the geodetic work.

[4] The determination of ξ_0^g and η_0^g by the normal gravimetric survey requires a coverage of the territory in a circle of radius about 2,000 *km* around the origin of triangulation with its appropriate thickening near that point, and a general gravimetric survey of all the rest of the globe. The determination of ξ^g and η^g with the indicated precision requires a coverage of the territory in a circle of radius about 1,500 *km* around the given astronomical station with its appropriate thickening near that point, and the same survey of the territory of radius about 4 – 6 thousand kilometres.

The present state of the worldwide gravimetric survey and of the normal gravimetric survey of the USSR, and especially of the countries adjoining us in Asia (Turkey, Persia [Iran], Mongolia and China) only ensures the required precision of ξ^g and η^g to a rather low extent. On the other hand, for transferring those ξ^g and η^g to a system of geodetic ξ and η we ought to determine with high precision ξ_0^g and η_0^g and the sizes of the ellipsoid best suiting the normal spheroid, – the size of its semi-major axis with mean error of only ± 20 *m*. The task seems therefore hardly feasible.

But let us imagine that a given region with a number of stations of the astronomical network is restricted from all sides by chains of triangulation of the I order (or by main chains of the II order) accompanied by thoroughly determined astronomical stations with given intervals apart (mean errors of astronomical latitude and longitude being about ± 0″.2 and ± 0″.4). According to our adopted pattern, such astronomical stations of the triangulation of the I order are situated about 80 – 100 *km* apart. Imagine further that the same region is covered by gravimetric surveying allowing to derive somewhat reliably the gravimetric magnitudes ξ^g and η^g for the astronomical stations of the triangulation of the I order (or of the main chains of the II order). We have grounds to believe that, after carrying out the normal or [in other words?] standard gravimetric survey *and its special thickening in some places suggested by maps of gravimetric isoanomalies,* the errors of the derived ξ^g and η^g for an astronomical station of the II order will be composed of 1) A random part amounting to ± 0″.8 and 2) A part, systematic for the given region. It will be mainly caused by poorly allowing for the influence of the remote zones or by insufficient materials being provided by the worldwide gravimetric survey. The mean square values of that systematic part can amount to ± (1″.2 – 1″.5). However, it will change over a given region slowly, regularly, and not much.

For astronomical stations of the I order, taking into account the more reliable derivation of the gravity gradient, we may assume the mean random error of ξ^g and η^g reaching ± (0″.4 – 0″.5). Denote the geodetic and astronomical latitude and longitude of such stations by B_i, L_i and φ_i, λ_i. Then

$\xi_i = \varphi_i - B_i$ with mean error ± 0″.2,
$\eta_i = (\lambda_i - L_i) \cos \varphi_i$ with mean error ± (0″.2 – 0″.3).

We can therefore obtain for that station the *corrections* of ξ_i^g and η_i^g

$$\xi_i - \xi_i^g = \delta\xi_i^g, \ \eta_i - \eta_i^g = \delta\eta_i^g.$$

Their mean errors will evidently equal about ± 0″.5.

We also suppose that our region, bordered by chains of the I order, covers from north to south, and from east to west not more than 800 *km*. After obtaining a number of magnitudes $\delta\xi_i^g$ and $\delta\eta_i^g$ for the astronomical stations of the triangulation of the I order situated along the region's border, and perhaps in its middle as well, we can interpolate those magnitudes and derive them for any astronomical station of the II order in that region. The geodetic coordinates of those latter stations can then be calculated according to the formula

$$B = \varphi - \xi^g - \delta\xi^g, L = \lambda - \eta^g + \delta\eta^g \sec\varphi.$$

Even if the interpolation of the corrections $\delta\xi^g$ and $\delta\eta^g$ is accompanied by a mean error amounting to ± 0″.5, this method will provide ξ and η with mean error

$$\sqrt{(0''.8)^2 + (0''.5)^2 + (0''.5)^2} = \pm\ 1''.07.$$

This does not completely answer our task, but lowers the precision of B and L quite insignificantly.

The described interpolation should be based on some mathematical tool, perhaps on expanding [$\delta\xi_i^g$ and $\delta\eta_i^g$] into spherical functions. TsNIIGAiK ought to work this out; understandably, however, the outlined pattern of applying astronomical stations of the triangulation of the I order can be changed. It is important to note that, in a given region, in spite of the incompleteness of the gravimetric materials for the remote zones and the extremely poor development of the worldwide gravimetric survey, chains of an AG network ensure a sufficiently precise transition from astronomical to geodetic coordinates. Indeed, precise triangulation with astronomically determined stations replaces the allowance of the influence of those zones on the derivation of ξ^g and η^g and essentially lowers the inaccuracy of transferring the magnitudes ξ^g and η^g to the system of magnitudes ξ and η occasioned by the inaccuracy of determining the ellipsoid best representing the normal spheroid.

However, in the strip about 120 *km* wide along triangulation chains we should certainly have the standard gravimetric survey and its sufficient thickening, and in a considerable number of cases, apart from gravity determinations covering the region and at the astronomical station of the II order itself, it will be necessary to have 3 – 4 or even 5 – 6 gravimetric stations not further than 20 *km* from it. After determining astronomical stations of the II order with the indicated precision, having comparatively sparse chains of triangulation of the I order and main chains of the II order properly accompanied by astronomical stations of the I order, and a corresponding gravimetrical survey, we can apply the astronomical stations of the II order *without any additional work* as a control network for future mapping of extensive regions in the Asiatic part to the scale 1:100,000 rather than 1:400,000. We ought to stress especially the importance of developing in advance the triangulation over the Asiatic part now being mapped to the scale 1:500,000.

Fourth, the development of triangulation to the north of parallel 55° in the Asiatic part will provide rather reliable heights of a number of points being determined by trigonometric levelling and situated more or less uniformly over all the territory although considerably apart. Owing to the extremely difficult conditions for spirit levelling in Siberia and the Far East, and the ensuing large perimeters of the polygons of levelling of the II – V orders, the system of heights determined by triangulation will be essentially important for mapping the Asiatic part.

All the considerations above only outline the main aims of laying out the triangulation of the I order and the main chains of the II order even when mapping of the territory to the north of parallel 55° in the Asiatic part is going on to the scale 1:500, 000.

[5] We have to add one more consideration. Simplified triangulation and traverses of the IV and V orders laid out between adjacent astronomical stations of the II order and astronomical and triangulation stations of the I and II [?] order will be applied for treating the materials of air surveys. Angle measurements in such simplified triangulations will be sufficiently checked by triangular conditions, and in traverses, by azimuths measured at both their ends and one or two at intermediate point(s). Checking up the scale of the simplified triangulations and linear measurements in traverses will be achieved by the given distance 60 – 80 *km* between end control points known with mean error not greater than 1:2,000, i. e. about ± (30 – 40) *m*.

It is easily seen that, for the established above precision of deriving the geodetic coordinates of astronomical stations of the II order, such precision is unattainable. When a simplified triangulation or a traverse of the IV or V order is laid out between a triangulation station of the I order (or a station of a main chain of the II order) and an astronomical station of the II order with both these end points situated on the same meridian or parallel, the distance between them will be indeed available with mean error ± 30 *m*. And if the line connecting those end points has azimuth about 45°, that error will be roughly ± 45 *m*. This means that in such cases astronomical stations of the II order are sufficient for mapping as a simplified detailed control network. For the main chains of triangulation of the II order laid out from chains of the I order about 320 *km* apart upon the average, and the sides of the polygons of the I order being about 640 – 700 *km* long, those cases will cover 75% of the area of each polygon.

For the other 25% astronomical stations will not form a reliable control network. It will however be ensured by closed traverses and the intersections of the simplified triangulation existing in any case. In that middle part of the triangulation polygon astronomical stations of the II order will simplify the organization of the work by providing a free choice of the time and place for beginning the air surveying, although to a certain extent their importance as control points still remains.

When designing the chains and polygons of the I order and the main chains of the II order, the economic requirements and economic importance of the regions, their geographical peculiarities, and the four aims of developing work in the Asiatic part to the north of parallel 55° as indicated above, – all this should be taken account of.

Permafrost is a feature of a considerable part of the territory under consideration. Specially constructed centres and benchmarks, a thoroughly thought out method of their laying out and a correct choice of the season for that work are needed. Tubular centres and benchmarks would have essentially complicated the work since they require delivery of bulky equipment and foodstuffs for the team of workers. TsNIIGAiK should inevitably continue its study of the types of centres and benchmarks for the zones of permafrost. Because of the conditions of transportation, manufacturing of a geodetic universal of lesser weight becomes essential for those zones. Its type and construction can be designed by issuing from the American Parkhurst theodolite.

During the mapping of the territory to the north of parallel 55° in the Asiatic part to the scales of 1:400,000 and 1:200,000, minimal triangulation work sufficiently ensuring the fulfilment of the main geodetic and cartographic requirements should be achieved there. This means, first, that only four chains of the I order (along Ob, Yenisei, Lena and the coast of the Okhotsk Sea) will be completed according to the programme wholly meeting the needs of arc measurements as formulated nowadays. Second, places, especially difficult for triangulation of the I order (the tundra regions), should be left out. The connecting chains between those along Ob, Yenisei and Lena could be laid out somewhat to the south of the tundra etc.

[6] The first general treatment of the eight polygons of triangulation of the I order in the European part occurred in 1930. They were adjusted as a single whole, and, as soon as being completed, next polygons had been joined to them without readjusting the first eight of them, by *gradual threading*. We ought to conclude that, therefore, the triangulation of the I order was not yet generally treated. The first such treatment should begin when the compiled material allows to derive results of due stability. For the same reason the catalogues of the state triangulation issued until now should not at all be considered final; the coordinates of triangulation stations and the azimuths of the triangulation sides included there will probably considerably change. Indeed, in 1930 the initial geodetic data were only established by comparatively little material and without the still lacking gravimetric support; in addition, large materials were joined and the influence of the deviations of the geoid from the reference ellipsoid which could have been only ascertained since 1936 was not yet achieved. Understandably, the final theoretical and practical development of the methods of adjusting our great AG network should be accomplished in proper time.

We ought to separate the forthcoming astronomical work into precise determinations of latitude, longitude and azimuths in triangulation of the I order and main chains of the II order, and the establishment of astronomical stations of the II and III orders as control stations for mapping. Not less than 800 stations of the I order are [should be?] situated along the chains of the appropriate triangulation 80 – 100 *km* apart. In addition, a proper determination of longitudes requires a selection of a few stations distributed over all our territory as the main longitudinal stations. They should be established with high precision sufficient for determining the astronomers' personal equations and thus serving as initial regional longitudinal stations for astronomical work of the I, II and III orders. Such main stations are already established in Omsk, Irkutsk and Yakutsk, but their network is needed.

The method and the organization of that considerable and responsible work should still be thoroughly thought out; its results will be important not only for geodesy. In future, those stations will be useful during the so-called worldwide determinations of longitudes ascertaining the general movements of the continents.

[7] Longitudinal determinations at the main stations and stations of the I, II and III orders, and gravity determinations are connected with a reliable organization of a central and local time services. Their work should be exemplary, they should have first-rate radios, clocks and astronomical equipment, also first-rate astronomers-observers, scientific leadership, and, finally, observatories favoured by propitious conditions for work of high precision. The Academy of Sciences is studying the proper organization of time service; [anyway,] the experience gained since 1922 indicates that its work should be not additional and accessory for a large astronomical observatory, but a special item in the system of scientific and technical problems being carried out [to be solved] by the state geodetic service.

As to astronomical stations of the I order, i. e., astronomical work in triangulation of the I order and in the main chains of the II order, the following rule formulated by me already in 1930, should be obeyed for the subsequent geodetic application of the determined longitudes:

The longitudes of two adjacent Laplace stations should be determined by the same astronomer during the same season.

The unwieldy astronomical equipment causes essential difficulties when astronomical stations of the I order are established in the Asiatic part to the north of parallel 55°. On the other hand, the need for determining very precisely the azimuths of the sides of the triangulation and the transition from the Tsinger method [vol. 28], when working to the north of parallel 62 – 63°, to azimuth methods of determining time, compel astronomers to apply bulky universal instruments graduated to 2″. Here also it is certainly possible to find, by scientific and technical strivings and investigations, some ways for simplifying the work. The appropriate joint scientific work of astronomical geodesists and opticians ought to begin in the nearest future.

As to the expeditionary astronomical stations of the II order, I have indicated above their importance and the necessary precision of their determination. In especially difficult regions, or those with unfavourable meteorological conditions, stations of the III order must replace them. Their precision is described by the following mean errors:

latitudinal error ± (1.5 – 2″.0), longitudinal error ± (3 – 4″).

After their astronomical coordinates are transmitted into geodetic coordinates, those stations will only be applied for controlling mapping to the scale 1:400,000, not 1:100,000. They can be determined by instruments of lesser weight than those necessary for stations of the II order. However, I believe that our practitioners should not anymore apply the bulky and very complexly constructed astronomical universal instrument graduated to 5″. They ought either to return to small vertical circles of the type of the small Repsold circle (successfully applied by military geodesists [of the KVT?] in sufficiently difficult regions of Siberia and the Far East), but change the connection of the level and telescope and perhaps somewhat intensify the optics, or apply small zenith telescopes with horizontal circles capable of readings to 1′.

Stations of the II order are situated 60 – 80 *km* apart, those of the III order, 80 – 100 *km* apart. During 1939 – 1948 about 800 of them taken together should be established since the existing stations of the I order can be applied for mapping and about 400 stations of the II order were reliably determined previously. All the territory of the Asiatic part to the north of parallel 55° should be uniformly covered by astronomical stations of the II and III orders, and their network will control all field cartographic work. Our geodesists are certainly quite capable of accomplishing this large work.

A special feature of the determination of a considerable part of astronomical stations of the II and III orders is the need for determining reliably their heights above the sea level. This is not necessary only for stations situated along levelling lines of the II, III, IV and V order. And all astronomical stations should be marked in a way enabling them to be distinctly seen and easily identified on air photos during future air surveying of the territory for mapping to the scales 1:500,000 or 1:200,000.

[8] Above, I indicated the importance of gravimetric materials for mapping regions of the Asiatic part and of the north-east regions of the European part to the scale of 1:100,000. This aim certainly does not embrace all the importance of the general gravimetric survey of the USSR and of some other gravimetric work. Properly establishing the initial geodetic data and executing arc measurements; determining the flattening of the Earth ellipsoid; ascertaining the small and large waves of the geoidal surface, – those are the next applications of gravimetric results in geodesy. And we should not overlook their importance for geology, geological prospecting or geophysics.

Gravimetry should be considered a part of geodesy, the part that may be called *physical* geodesy. Since 1931 our gravimetric work has firmly become the duty of the state geodetic service, and there is no reason for compelling it to turn down the problems connected with studying the physics of the globe or the structure of the upper mantle of the Earth crust. Indeed, their solution, very interesting from the general scientific viewpoint and practically important, cannot be accomplished without AG, gravimetric and topographical materials.

The forthcoming laying out of chains of triangulation of the I order, accompanying astronomical determinations, of such determinations of the II order, and gravity measurements is a very substantial work. Its part, outlined here, which is being accomplished in the nearest future, when field cartographic work on a considerable territory in the Asiatic part will be going on is also imposing. This work will, *first*, lead to the complete coverage of the whole European part and the Asiatic part to the south of parallel 55° by chains and polygons of the triangulation of the I order and main chains of the II order, i. e., of that territory on which topographic surveys to the scales of 1:25,000 and 1:50,000 will begin. *Second*, it will provide a sufficient network of astronomical stations of the I, II and III orders for cartographic field work necessary for the compilation of maps to the scales of 1:500,000 or 1:200,000. Certainly, however, tacheometrical traverses, simplified triangulations, traverses of the IV and V order, phototheodolite chains should be laid out between those control stations.

Third, it will be comparatively easy to include those detailed geodetic networks and precise topographic surveys, which become needed in different places in Siberia and the Far East to the north of parallel 55°, in the general system of state geodetic and topographic work. *Fourth*, the network of astronomical stations and a proper accomplishment and application of gravimetric surveying will make it possible to begin the compilation of the map to the scale of 1:100,000 and thus to ensure considerable progress in mapping the territory of the USSR. *Fifth*, it will essentially simplify, or, more precisely, ensure the necessary further development of the AG work so that in proper time the geodetic control will be sufficient for meeting the requirements, rapidly growing under the conditions of Soviet reality. *Sixth*, it will allow to come close to the proper general treatment of our grand AG network.

In concluding, let me remind readers that the success of the further development of the AG work will largely depend on a skilful use of the achievements of transportation, aviation and communication to which the most serious attention is necessary.

VI

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Constructing the basic geodetic network in the USSR

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In all countries, that network is represented by triangulation of the I order which should be constructed with utmost precision. Indeed, the coordinates of its stations, the lengths and azimuths of its sides are adopted as the initial data, as though not corrupted by any errors, for ensuring a rigid control of all the subsequent geodetic operations.

Actually, the results of the triangulation of the I order are spoiled by unavoidable errors, which, however, should be small as compared with the errors in the triangulations of the following orders resting on it. Already this circumstance explains the high demands on observations and measurements of that triangulation and on treating its results. On the other hand, the basic geodetic network in all countries has always been applied for pursuing scientific aims, for determining the Earth's sizes and figure. Therefore, the programme and the organization of the triangulation of the I order should also bear in mind that aim as well as other scientific goals connected with studying the solid mantle of the globe.

In the Soviet Union, the triangulation of the I order is known to consist of chains 200 – 250 *km* long directed along meridians and parallels 200 – 250 *km* apart. Base extensions are constructed at the chain's ends with latitude, longitude and azimuths precisely determined at two stations, at the ends of the base extensions. More definitely, those latter ends define the borders of the chains.

Intersections of the chains make up polygons whose vertices coincide with the ends of the chains; each side of a polygon is thus a chain of triangulation 200 – 250 *km* long.

Understandably, this *normal* pattern does not extend into the yet uninhabited regions of Siberia and the Far East. There, the pattern is determined, first and foremost, by conditions of transportation. During the initial period of developing such territories, the chains of triangulation of the I order have certainly to be constructed even 800 – 1,000 *km* apart, and in some places they will considerably deviate from meridians and parallels.

Our chains of the I order consist of simple triangles, of braced quadrilaterals in the USA, and, recently, in Germany, of double rows of triangles. In general, geodesists abroad construct the chains of triangulation of the I order out of *complicated figures* whereas we are keeping to *simple figures*. The mean error of a measured angle in our chain is ± 0.″7, so that the mean error of the length of our chain's diagonal amounts to ± (0.6 – 0.7 *m*) as is also the mean transversal shift of the chain's end relative to its beginning.

Those mean errors in the triangulations abroad are not as great mainly because of a lesser error of a measured angle and only to a small extent owing to the chains consisting of complicated figures.

From the viewpoint of the influence of the random errors of measurement the benefit of applying complicated figures is of small importance, but only if the chains are accompanied by bases and Laplacean azimuths as described above. However, complicated figures provide an incomparably better and more reliable check of field measurements and a better guarantee of a lessening influence of the systematic errors of measurement.

We also should therefore attempt to construct our chains of triangulation of the I order from complicated figures and thus to achieve more rigid basic geodetic networks. Our exceptional pre-revolutionary backwardness in that field and the unprecedented requirements of our national economy for such networks after the October revolution necessarily and unavoidably caused us to adapt to those conditions, but in the long run we ought to think about passing on to the more perfect pattern.

It is our custom to construct rhombic extensions of bases whereas geodesists abroad prefer double-rhombic extensions and thus avoid essential angles under 40° and weaken the influence of possible systematic errors in the angles. Our extensions have mean error about 1:500,000 and it seems that nothing better should be wished for. However, here we have some formal approach and, on the other hand, that precision is often attained by lengthening the base and therefore choosing an obviously inappropriate place for its measurement. Our geodesists ought to pay attention to problems of constructing base extensions. Our bases are measured precisely enough, but the important problem of preserving the standards of length remains without due notice.

Astronomical support of a chain of the I order consists of measuring precisely enough latitudes and longitudes and bilateral azimuths at both ends of base extensions, and in addition, latitudes and longitudes at a station situated approximately in the middle of the chain. Bilateral azimuths are of essential importance. They have already showed that the high precision of the measurements themselves is cancelled out by the action of lateral refraction. In most cases the mean result of such bilateral measurements with a proper distribution of the observations should noticeably weaken the corruption of the azimuth by refraction. However, this is only a probable assumption, actually in a number of cases it can be wrong.

Because of the great importance of the Laplacean azimuths for controlling the triangulation and raising its quality, the issue of determining them with high precision is quite topical. Concerning the directions of the sides of the triangulation, Laplacean azimuths take on the same role as the bases with regard to the lengths of those sides. We cannot be satisfied with the approach taking place in our practice; bilateral measurements by themselves do not at all secure an absolute guarantee. It seems desirable to introduce the following measures.

1. To replace the usual astronomical brick posts by pedestals 6 – 8 *m* high.
2. To carry out the bilateral determinations of azimuths in three series separated from each other by a month.
3. When choosing the side along which the azimuth should be measured in both directions, the locality ought to be appraised from the viewpoint of possible refractive influences. If the extended base is not suitable, the azimuths along a side of the chain itself, perhaps somewhat shortening it, should be measured.
4. To include some meteorological part in the programme of azimuth determination.

The adjustment of chains for the azimuth conditions is hampered by the issue of correcting astronomical azimuths and longitudes. For that reason, those corrections are shifted onto the corrections of the angles which is wrong. The pattern of the triangulation of the I order should additionally include the determination of *fundamental* astronomical azimuths 1,000 – 1,200 *km* apart. Their mean error (including the action of refraction) should not exceed ± 0.″3, and during the treatment of the chains they should be considered rigid. The choice of their placement and their measurement ought to be accompanied by meteorological studies.

I should indicate that the issues of precisely measuring Laplacean azimuths is examined abroad as well. They are complicated enough, and neither should we leave them aside and restrict ourselves to bilateral determinations by the book.

Astronomical determinations turn the triangulation of the I order into *an AG network*. Such determinations of azimuths and longitudes are of essential importance since they raise the precision of its construction, eliminate the accumulated systematic influences and check the triangulation. Astronomical longitudes and azimuths lead to the determination of Laplacean azimuths and are [therefore] necessary and important for *purely practical geodetic aims*. On the other hand, they turn the chains of the triangulation of the I order into arc measurements and thus serve for realizing geodetic scientific aims; they are also necessary for obtaining the declinations of the vertical which in turn are needed for correctly treating the triangulation.

All the measurements in triangulation are relative to the plumb lines and the equipotential level of the ocean whereas they ought to be relative to the normals to the reference ellipsoid and its surface. The deflections of the vertical, which we obtain by determining astronomical latitudes and longitudes at a number of triangulation stations, are therefore also needed for practical geodetic aims.

Thus, we should not at all consider that in triangulation the astronomical determinations are a burden added to the practical task and only serve for fulfilling scientific studies and solving scientific problems. No, the astronomical part is an organic portion of modern triangulations of the I order which should inevitably be constructed as an *AG network* rather than as simply a *geodetic network of the I order*. The astronomical provision of our triangulation of the I order is richer than in the USA or Western Europe.

Let us, however, return to the construction of our triangulation. Like everywhere else, it makes up cells, polygons of the I order which ought to be filled in by triangulation of the II and subsequent orders. An inaccuracy in constructing those polygons, their deformations will unavoidably tell on the triangulations of the lower orders. The organization of the triangulation of the I order and of the development of the triangulation of the II order should ensure, first, that the mentioned deformations *will not introduce any practically significant changes in the elements of separate triangles of the II order* and only cause general shifts of separate groups of triangles, little and slowly changing from group to group. Second, those deformations should little influence the discrepancies in the coordinates and azimuths of the triangulation stations of the II order as compared with the influence of the errors of that triangulation itself.

For fulfilling the second condition especially strict claims should be laid on the triangulation of the I order. Note that we assume that the mean error of an angle in that triangulation is equal to ± (0.″7 – 0.″9). The considerable experience of our and American works already gained shows that *in a large country* it is very difficult to attain a mean error of angles less than ± 0.″7. This is partly caused by the conditions of our territory: in the northern woody regions, swamps and abundant moisture are very unfavourable for precise observations over 30 – 35 *km* which is the mean distance of a side of the triangulation of the I order. And in the more southern treeless regions and steppes the great *jumping* of the air near the earth's surface is known to everyone. There are, however, other causes as well, for example the type of our geodetic signals.

But we may still believe that, given those natural conditions, whose role is important, and the requirements caused by the vastness of the territory to be urgently serviced, the attained precision of constructing the basic geodetic networks is *optimal*.

Imagine that all the area inside a polygon of the I order is filled in by triangles of the triangulation of the II order, and that that continuous network is adjusted as a single whole within that polygon. Then the mentioned requirement that the deformations of our polygons only cause general shifts of separate groups of triangles, little and slowly changing from group to group without introducing any noticeable corruptions to the elements of the triangles of the II order, – then, considering the actual deformations of our polygons, that requirement is met.

As to the requirement that the discrepancies in the network of the II order should be mainly caused by errors of that triangulation itself rather than by errors in the triangulation of the I order, it is also sufficiently satisfied, although without an exceptional margin. This is especially the case in which the mean error of a measured angle in the filling is about ± 1.″5, and the sides of its triangles are 18 – 20 *km*.

However, that filling network consists of 150 – 180 triangles and checking such a network by a frame of a polygon undoubtedly becomes weak enough so that chains sufficiently more thoroughly executed should be isolated from that network. They will separate that network in parts to be checked independently from each other. We thus come to the idea which is actually realized, to separate polygons of the I order into four parts *by two main chains of the II order*. They should be carried out by methods approaching those of the I order. And in many cases such partitions of polygons of the I order certainly considerably benefit the organization of the topographical geodetic work.

Nevertheless, we ought to bear in mind that the main chains of the II order ensure a good control of the lengths and the azimuths of the sides of the triangles, but the coordinates of the stations of those chains are corrupted by deformations of the polygons and their own errors. It is therefore expedient to adjust and definitively calculate the main chains of the II order not separately, but *together with all* the triangles of the filling network, although assigning larger weights to their angles.

I will now consider the connection between the construction of the basic geodetic network and gravimetric work. The latter should be separated into the general gravimetric survey of the country and the astronomical gravimetric levelling along those chains of the triangulation of the I order which ought to meet the requirements of arc measurements. That levelling consists of

1. Astronomical determinations of latitude and longitude at triangulation stations 70 – 100 *km* apart. Actually, this requirement is included in the programme and pattern of the triangulation of the I order.

2. A general gravimetric survey of a strip 220 – 240 *km* wide along the chain of the I order. Actually, this requirement is included in the programme of the general gravimetric survey of the country.

3. Thickening of that general survey in a circle of radius 110 – 120 *km* from triangulation stations which should ensure for any of them a precise determination of the part of the *gravimetric* deflection of the vertical caused by gravity anomalies.

The astronomical gravimetric levelling provides the geoidal heights relative to the reference ellipsoid and the deflections of the vertical at *geodetic* stations relative to the normals to that ellipsoid. These data are needed for treating the triangles of the I order with proper precision; they also provide the geoidal profile relative to the reference ellipsoid along the considered triangulation chain, so that we certainly obtain the most valuable materials for studying the Earth figure.

Just as the astronomical gravimetric levelling, the general gravimetric survey together with the astronomical determinations of latitude and longitude at triangulation stations allows to derive the same geoidal heights above the reference ellipsoid and to determine the relative deflections of the vertical, but noticeably less precisely.

It is important to note that the complex of our work on the triangulation of the I order, on astronomical determinations at triangulation stations of that order and on the gravimetric surveying ensures a complete application of the AG network and provides a material unparalleled in volume and contents for studying and investigating the Earth figure. At the same time, astronomical and gravimetric work furnish a proper treatment of our vast triangulation of the I order.

VII

Appendix
An episode in the history of the Baltic Geodetic Commission, 1938

Krasovsky's collaboration with the BGC, about which Bagratuni [iv, § 1] said a few words, ended abruptly. How and why?

I. Bonsdorff
Bericht des Generalsekretärs [of the BGC]

Verh. 10. Tagung Balt. geod. Kommission *1938*. Helsinki, 1938, pp. 42 – 45

[..] Tratt die Konvention [about the establishment of the BGC] den 20. Januar 1937 in Kraft für eine neue Periode […], d. h. bis ultimo 1948. […] Der Präsident der BGK [of BGC], Prof. Th. Krassovsky, teilte […] mit, daß die Regierung von USSR ihn wegen seines Gesundheitszustandes von dem Posten als stimmberechtigtes Mitglied der Kommission entbunden hat und Prof. [of Moscow University, future academician] A. A. Michailov zu dem Posten ernannt hat.

[The representatives of the seven remaining in BGC countries took a vote on that issue and Michailov was approved by six voices for 1938 – 1939. *Michailov dankte für die Ehre*.]

Am 14. März 1938 übermittelte die Gesandtschaft von USSR dem finnischen Auswärtigen Amt folgendes Schreiben:

[…] *Le Gouvernement de* [Soviet Union] *a pris la décision de renoncer à la participation ultérieure* [in BGC]. […] *A partir du 1 janvier 1937 l'Union des R. S. S. est entrée dans la Ligue Internationale Géophysique et Géodésique. Les cercles géodésiques des Soviéts estiment que la participation dans cette Unification internationale rend inutile la participation* […] *dans une Unification qui porte un caractère local et poursuit des buts plus restreints.*

Am 21. März übermittelte das Finnische Auswärtige Amt dem Gesandten von USSR folgende Note:

[The Convention does not envisage] *la dénonciation* […] *avant la terminaison de la validité de celle-ci.* […] *Le Gouvernement de Finlande ne peut pas considérer la dénonciation* […] *comme conforme aux stipulations* […].

[On March 24 A. Michailov informed the General Secretary of the BGC (i. e., Bonsdorff)] *daß ich* (that he) *mein Amt* […] *niederlege*. [A new president was elected instead.]

Here are my comments.

1. Krasovsky himself evidently did not ask to be relieved so that a (patently false) pretext had to be invented. Recall Izotov [x, § 15]: Krasovsky *openly made known his thoughts and views, even in those tricky circumstances when it could have harmed him.*

2. That some mysterious geodetic circles decided that Soviet participation in BGC became superfluous, was a damned lie. First and foremost, Krasovsky would have vigorously objected. He himself and many other Soviet scientists had delivered reports at various sessions of the Commission (especially at the session held in Moscow and Leningrad). Among the better-known were I. M. Gubkin, N. I. Idelson, A. A. Mikhailov and M. S. Molodensky. But strangest of all is that the Soviet Union had only joined the *Ligue Internationale* in 1955 [vol. 6, article *Geod. and Geophys. Union*])! Sapienti sat! (For a clever man this is sufficient.)

3. Danilov [ix, § 16] stated that Krasovsky was elected President in 1936.

4. Krasovsky was unable to attend the sixth session of the BGC held in 1932. Its *Comptes rendus* were published in Helsinki in 1933 and there, on p. 18, the President, E. Kohlschütter, described the letter he received from F. N. This is his quotation:

Die Geodäsie eine Wissenschaft sei, die keine Grenzen kenne, und spricht [F. N.] *die Überzeugung aus, daß die gemeinsamen Arbeiten der Kommission alles beteiligten Ländern großen Nutzen für ihre eigene Arbeiten bringen werden, auch hofft er, daß die Verbindung zwischen den Teilnehmern der Baltischen Kommission immer kräftigen werden möge.*

So why did the Soviet authorities decide to quit the BGC? The cause was certainly political, perhaps connected with the serious deterioration, in 1938 – 1939, of Soviet – Finnish relations (and the ensuing war). A more important cause was apparently the Soviet authorities' efforts to conceal as much as possible the Great Terror of those years, cf. the tragic fate of Numerov (my Introduction, § 1).

Krasovsky' great niece, Tatiana Gennadievna Kusenkova, told me that he was accused of excessively praising a German geodesist which did not bode well for the authorities (or was a pretext). Krasovsky evidently did not repent and was forbidden to leave the country. Being the President of the BGC, he directed its work by telephone …

VIII

Archival Information concerning Krasovsky.
A) Archive, Russian Academy of Sciences, Moscow Branch, 1931 – 1945

1. Sittings of Some Subdivisions of the Communist Academy

Fond 351, Inventory 1, Delo 135(1) – 135 (11)

Explanation

This Academy (1918 – 1936), until 1924 called Socialist, officially aimed at studying the problems of socialism and preparing and uniting the *scientific workers of socialism*. It had scientific institutes (of natural sciences among them), sections and commissions. I read the records of the proceedings of several of its subdivisions pertaining to 1931. Below, I say a few words about one sitting.

Krasovsky Declared a Reactionary

While discussing the mathematical curriculum at Moscow University, apparently for student geodesists, S. G. Sudakov, the future head of GUGK of many years' standing, declared that the planned 750 hours of mathematics was an *act of sabotage along Mikhailov's line*, […] *the idea of the reactionary Krasovsky*. Too much hours!

Mikhailov: apparently A. A. Mikhailov, corresponding (1943) and full member (1964) of the Academy of Sciences. That Krasovsky was a reactionary apparently meant that he did not toe the communist line. Indeed, at a sitting of another subdivision of the Communist Academy Krasovsky was praised for strengthening the teaching of physics and mathematics at MGI.

2. Establishing a Commission on Theoretical Geodesy at the Academy of Sciences

Fond 614, Inventory 4, Delo 49(1) – 49 (4 rev)

Explanation

Baikov and Ioffe mentioned below were then (May 1945) Vice-Presidents of the Academy of Sciences; Baikov, however, had been very ill and quit on 25 May. He was hardly able to do anything about Krasovsky's letter. Academician N. G. Bruevich was Secretary. There were no permanent secretaries after 1937.

An academic Committee (not commission) on geodesy and geophysics was indeed attached to the Branch of Physical and Mathematical Sciences (BPMS), although only in 1955, see *Vestnik Akademii Nauk SSSR* No. 4, 1955, p. 69, *In connection with the USSR entering the International Geodetic and Geophysical Union*. That entering was obviously occasioned by the forthcoming International Geophysical Year.

No, Krasovsky did not envision such a narrow (from the geodetic viewpoint) scope of the work of his proposed commission. At present, attached to the Academy's presidium are, among others, National Committees on the International Geosphere and Biosphere Programme and on the Collection and Appreciation of Numerical Data in Science and Technology (i. e., on statistical activities in that field). In 1992, Russia left the mentioned Union.

Ioffe correctly stated that the material he obtained did not answer the assignment and seized the opportunity for shelving the proposed commission.

Krasovsky. Covering Letter

Dear Aleksandr Aleksandrovich [Baikov]!

In August 1944 I began a campaign at the Academy of Sciences; it took a strange shape which compels me to ask your advice not officially, but turning to a senior comrade, to ask what should I do. If your health will prevent you from devoting some attention to my campaign, please let me know it. I will certainly have no claim on applying your strength to my petty campaign.

Corresponding Member of the Ac. Sciences of the USSR F. N. Krasovsky

Attachment: a short note about establishing a Commission on problems of theoretical geodesy at the Branch of Physical and Mathematical Sciences

Note on establishing a Commission on Problems of Theoretical Statistics at the Branch of Physical and Mathematical Sciences

Privately

In August 1944 I sent a note to the Presidium of the Academy of Sciences justifying the establishment of a geodetic institute at the BPMS. The case was sent there and considered at first at the very end of September (apparently on 27 Sept.). Recognizing the need for establishing a special commission on theoretical problems of geodesy, the Bureau of the BPMS asked me to head a provisional committee: I, Academician [O. Yu.] Schmidt, Major General [N. A.] Urmaev, Professor V. V. Danilov, Docent [A. A.] Izotov, Corr. Member of the USSR Ac. Sciences A. A. Mikhailov.

This prov. commission was assigned to establish the problems of the proposed Commission on Problems of Theoretical Geodesy as well as its structure and statutes. Attention was drawn to the fact that the main aim of the Commission was the coordination of the work of Soviet institutions, both academic or not, on problems of theoretical geodesy. This directive was introduced, as I believe, after the sitting of the BPMS, but it occurred that it was especially important. Having been ill, I was absent at that sitting. After formulating the problems and statutes, the materials of the provisional commission were somewhat belatedly submitted to the Bureau of the BPMS.

Taking into account the academicians' poor knowledge of modern scientific problems of geodesy, I have compiled a special note, *Scientific Problems of Geodesy*. The girl typists had made 16 copies which I distributed to prominent members of the Academy including yourself. Only you informed me that my note is interesting and that you will assist my undertaking.

Only at the end of January (on 24 Jan.) the conference of the Bureau of BPMS took place. There, probably academician Ioffe stated that the provisional commission of geodesists submitted material not answering the given assignment by attempting to establish a geodetic institute with laboratories. *Therefore*, he continued, Krasovsky's undertaking should be returned to the Presidium of the Academy. The Bureau compiled and sent out an appropriate decision. Having been ill, I was absent at that sitting of 24 January as well. From those present I only heard that A. F. Ioffe did not allow any discussions, nor did he hear out any explanations from the members of the provisional commission.

After 24 January my undertaking is shelved. From the beginning of March nothing is done at the Presidium of the Academy with the decision of the BPMS. I happened to hear that such cases, after being returned to the Presidium from the branches, are decided by the so-called *managing Presidium*. I have sent Prof. Danilov and N. G. Bruevich for talks with them, but nothing came out of it.

It seems that, since the Bureau of the BPMS fully recognizes the need to have an attached Commission on geodetic problems, establishing its problems is the duty of the Bureau. And, after that, the case should be sent to the Presidium for being approved. Apparently, however, after sending the case back to the Presidium, the Bureau of the BPMS as though considers its duty fulfilled.

At the end of February my telephone conversation with A. F. Ioffe proved unsuccessful. The telephone was not in quite good repair, and I was overcome with emotion and gasped for breath. Ioffe declared that *laboratories and the Institute* are out of question and that in addition the case depends *on Krasovsky's health*.

I began my campaign almost a year ago, but all the facts apparently indicate that nothing will be done at all. I am asking you first of all to help me find out what will now happen about the establishment of a commission on geodetic problems since the BPMS considers it necessary. And, should not I think, that the Bureau of the BPMS treated me as in general is not done by the Academy in respect to its corresponding members? My disagreement with the Bureau should have been discussed, a compromise is possible, is it not?[1] If, however, the case turns on some essential unavoidable discord, then I do not apparently understand the modern problems of the Academy concerning geodesy, and it ought to recommend me not to poke my nose anymore and regret my meddling.

My position with regard both to Soviet geodesists who set their hopes on me, and to the members of the BPMS is very unenviable, and I cannot even imagine my future dealings with them. Please help me, Aleksandr Aleksandrovich, but certainly only if it will not harmfully tell on your health. My own health improved in Barvikha[2] very little, and meanwhile on 12 May[3] they demanded that I left. Where to? Home? My hope to live there June, July and August was destroyed, but without long daily periods of getting fresh air nothing will come out of my treatment. During 59 days of my stay there, long periods in the open were only possible for 12 days. I think, however, that it is wrong to consider the case depending on my health, as academician Ioffe does.

With greatest respect F. Krasovsky, 11 May 1945

Notes

1. Such a compromise could have included statistics and its application in natural science in the scope of the commission's activities. In 1926, after the death of A. A. Chuprov, the Academy considered the possibility of publishing his works, but it occurred that no member of the Academy could have formulated his pertinent opinion (Sheynin 1990/2011, p. 45). The same year a special sitting of the Leningrad Polytechnic Institute, where Chuprov had been working for many years, was held, and there Professor Ioffe declared that he knew only one other person, Einstein, *inspired by science as much as Chuprov was*.

In 1915 or 1916, in a letter to V. I. Vernadsky, Chuprov (Ibidem, p. 130) stated thet in good time the Academy should establish an institute for the statistical study of Russia (which was never done).

2. A sanatorium of the Academy of Sciences near Moscow.
3. The date was obviously wrong. Indeed; Krasovsky signed his note on 11 May.

3. Krasovsky: Geodetic Science during the Latest 25 Years

Fond 614, Inv. 5, Delo 61(1) – 61 (31rev)

The end of its § 3 proves that the manuscript was written during the war; and at the end of § 5 the author indicates that a certain problem will be solved in the next years after the war ends. In addition, the 25 years, as stated at the very beginning of § 2, meant years 1919 – 1944.

There are no formulas, and not much numerical data. Was not it partly written in Barvikha? Its could have been based on the author's manuscript note mentioned in his letter to Baikov (in both cases, see my § 2). In 1941 and 1947 Krasovsky published papers representing his work at the Academy of Sciences and reprinted in vol. 1 of his *Sel. Works*. They have much in common with the present manuscript which is nevertheless an independent contribution. Its style is slipshod, so perhaps it is only a draft. It is separated into seven unnamed sections following after a short Introduction and its subject is wider than suggested by its title. Krasovsky indicates the merits of Clairaut, Gauss and several later scientists of the 19th century, especially Stokes and Helmert, concerning the application of gravimetry in geodesy, as well as Airy and Pratt. They advanced a correct hypothesis on the isostatic equilibrium of large portions of the Earth crust as though swimming on a solid base but disturbed in many places.

The author calls the investigations made by Sludsky, a professor at Moscow University, which regrettably remained unnoticed, not irreproachable but essential. He pays much attention to later studies of the isostatic theory and attributes the birth of *physical geodesy*, i. e., of studying the Earth mantle and, in particular, of horizontal and vertical movements of the dry land, to 1919. Then, Krasovsky dwells on important practical and scientific geodetic developments and especially describes the investigation of invar wires applied for measuring baselines. When mentioning a few sciences which physical geodesy more or less needs to consider, Krasovsky could have included statistics; a simple example: application of the correlation theory.

Other discussed subjects include the polar motion; time-keeping; establishment of the general Earth ellipsoid and thoughts about its being triaxial. Already in 1942 Krasovsky was awarded the State Prize for his initial investigations of the last-mentioned problem. He obtained his second prize in 1952, posthumously, together with A. A. Izotov, whom he mentions in this manuscript.

Krasovsky especially stresses the importance of the work of the International Geodetic and Geophysical Union (not mentioned in his related published papers) and believes that the Academy of Sciences ought to recognize geodesy as a theoretical science.

B) Russian State Archive of Economics

This Archive is keeping many pertinent materials, see the *Putevoditel* (Guidebook) No. 3, 2001 also available at www.yandex.ru I was only able to see the letters to Krasovsky from three persons, see below. All the letters are labelled 280/1/401.

1. Letter from N. E. Zukovsky 25 Sept. 1910

Zukovsky asks Krasovsky to organize measurements of the heights of airplanes during their (apparently, trial) flights over a certain Moscow district.

2. Letter from Yu. M. Shokalsky (an oceanographer, geographer and cartographer) 7 Jan. 1926

Shokalsky wrote to the organizers of a Krasovsky jubilee at MMI. He had been abroad and was unable to attend or even to send a congratulation. That jubilee could have only been occasioned by the 25 years that passed since Krasovsky had graduated in 1900. He [ix] was rector of that institute (1919 – 1921), then became a leading officer at VGU.

3. Letters from A. A. Izotov (cf. [x]).

Two of them are important. In the first of these (without date, written in the second half of 1945) Izotov describes Hayford's equations of arc measurements published in 1909, his own unsuccessful attempt to introduce correct reductions of geodetic measurements (a problem discussed by Krasovsky in several publications), and dwells on his personal circumstances. These are not really explained and remain unknown. Anyway, it occurs that he had to leave Moscow and live in Kazan. He certainly returned and finally became chair of higher geodesy at MIIGAiK.

In the second letter of 28 Febr. 1948 Izotov asks Krasovsky to approve the appended plan of his doctoral dissertation. Izotov published the text of his dissertation in 1950 (to recall, Krasovsky died in 1948) and successfully defended his work.

Sheynin O. B. (1990, Russian), *A. A. Chuprov. Life, Work, Correspondence*. Göttingen, 2011.

IX

V. V. Danilov

Feodosy Nikolaevich Krasovsky

F. N. Krasovsky, *Izrbannye Sochinenia* (Sel. Works), vol. 1, 1953, pp. 7 – 20

[1] Feodosy Nikolaevich Krasovsky was an outstanding geodesist of our time. He created the Soviet geodetic school and to a large extent contributed to its brilliant successes. For throwing more fully light at the advances of our geodesy and the importance of his work, I briefly describe the state of geodetic activities and their results up to the beginning of the 20th century.

Geodetic work in Russia had begun in the first decades of the 18th century when Peter the Great decided to map the country by instrumental surveying and the first geodetic school was established in Moscow. During that century, his idea had never been forgotten and was embodied in the so-called *Descriptions* of the lands of the Empire controlled by a comparatively sparse network of astronomical stations.

However, a real survey of the country by plane table controlled by a network of appropriately compact and precise triangulation was only achieved in the 19th century. In pre-revolutionary Russia, KVT was the most considerable establishment carrying out geodetic work. It was staffed by geodesists of the Military Topographic School in Petersburg which prepared highly qualified topographers perfectly well mastering plane table surveying, and topographic triangulators graduating after a year of additional education and specializing in constructing a central geodetic network consisting of triangulation of various orders.

The leading personnel of KVT consisted of a small number of military geodesists, graduates of the geodetic department of the Academy of the General Staff, then being specialized mostly in astronomical observations, and, during the last years before the Revolution, in carrying out precise angle and linear measurements for triangulation of the I order. Military geodesists carried out AG work of that order and headed the organization of topographical geodetic and topographical work of the KVT.

KVT based its mapping on instrumental surveys to the scales of 1:20,870 – 1:125,220 controlled by triangulation or astronomical stations. At first, K. I. Tenner[1] had begun surveying in 1819 in the Western frontier zone to the scale of 1:20,870 (Vilnius province) and F. F. Schubert, to the scale of 1:16,710. These surveys were partly instrumental and consisted in laying out survey traverses between triangulation stations by astrolabes or compasses with distances measured by chains, contours drawn by eye and relief shown by hatching. However, in 1844 the slow advance of that work compelled a transition to a smaller scale of 1:41,740. Thus 27 provinces in the European part of Russia, all the territory adjacent to Vistula, a large part of Finland and the Caucasus had been surveyed.

From 1848, specialists under general Mende from the Land Surveying Department of the Ministry of Justice had been drawn in. Together with KVT they had surveyed eight more provinces and their work served for compiling maps to the scales of 1:146,000 and 1:417,400 for the European part of the country (excepting the regions above the 60° latitude) and the Caucasus.

In 1870, the most necessary state requirements were satisfied, and military topographic surveys began on a more precise level by plane tables to the scale of 1: 20, 820, and, from 1907, to 1:41,740, with plane tables and telescopic alidades and only controlled by stations of triangular network. Relief was shown by contour lines. These surveys were executed in the western frontier zone, southern Finland, the Crimea and Caucasus, had a high and quite contemporary precision and served as a perfect material for cartographic purposes. They certainly demanded an extension of the triangular and levelling networks.

The network of triangulation accomplished by KVT in the 19th century was uncoordinated, only covered the European part of the country with large gaps and did not represent [a part of] a single state system. In 1897 – 1907 general Scharnhorst had attempted to put that net in order by adjusting it consecutively, one part after another. KVT, however, finally decided that it was necessary to begin the triangulation of the I order anew, according to the pattern and programme worked out in 1907 – 1909 by a committee under I. I. Pomerantsev. Chains of triangulation consisting of simple triangles along meridians and parallels situated 320 – 370 *km* apart with baselines and astronomically determined azimuths and latitudes (but not Laplace azimuths) measured at their intersections were envisaged, quite contemporary in precision and methods of work.

In 1910 – 1916 chains of triangulation had been laid out along the meridian from Pulkovo to Nikolaev on the Black sea with transversal chains connecting them and the Struve arc. Five polygons were constructed, the southern of which had not been closed because of World War I. Of some importance among the older triangulation, except for that Struve arc, are only the chains of the arc measurement along the parallels of 52° and 47°30′ ending at Orsk and Astrakhan respectively; even so, the predominant majority of their stations are lost. Better preserved are only those of the Caucasus, Soviet Middle Asia and Manchuria.

Somewhat better was the state of the vertical control: up to 1916, KVT had carried out precision and *high precision* levelling along railways and river banks, ca. 45 thousand *km* in all, connecting the tidal stations at the Baltic and Caspian seas. The preliminarily adjusted altitudes of the benchmarks and their measured differences were published in the S. D. Rielke generally known catalogue[2] and its additions with the altitudes reduced to the mean levels of the Baltic and Black seas.

Up to 1917 KVT had compiled the following maps.

To the scale of 1:417,400: the European part of the country, Western Siberia, Russian Middle Asia

Scale 1:669,600: a considerable part of Siberia

Scale 1:4,174,000: all Siberia

Scale 1:208,700: the Caucasus and a considerable part of Russian Middle Asia

Scale 1:125,220: Western part of European Russia

Scales 1:41,740 and 1:83,480: Western and Southern frontier zones including the Caucasus, partly Russian Middle Asia and Manchuria

Some of these maps were compiled by instrumental, but mostly by partly instrumental surveying or only taking the measures by eye. The Land Surveying Department also had a considerable number of geodesists. It filled its need in personnel from the graduates of the MMI (the engineers) and land surveying schools with a three-year period of education. That Department had been legalizing the boundaries of landownership. Such work had begun under Empress Ekaterina II and continued until the Revolution. It consisted of carrying out traverses by astrolabes and compasses, later by theodolites and tapes, fixing their turning points by wooden posts and pits, and compiling land surveying plans for each landowner. These plans showed the situation of the boundaries of the owner's, and of the adjacent owners' land, [country] roads, settlements and the main parcels of land (arable land, pastures etc), but not the relief. They covered almost all Russia, but their material was useless for mapping; attempts to compile maps of provinces had been unsuccessful.

Somewhat better was the surveying done by that same department in the Caucasus by angle measurement and plane tables, controlled by triangulation and showing relief by contour lines.

The Forest, Railway, Resettlement , Hydrographical, Geological and other departments had also been carrying out topographical geodetic work, but it was uncoordinated and could have been barely used for mapping the country.

[2] When discussing the scientific geodetic conceptions in Tsarist Russia, two facts ought to be mentioned. In 1816 – 1855 V.Ya. [F. G. W.] Struve, an astronomer of the Derpt [Dorpat, Tartu] University, had carried out the famous meridian arc measurement (Struve 1856 – 1861) from a cape in the North extremity of Scandinavia to the mouth of the Danube, about 25° of latitudinal difference, along longitude 27°. Attention to his work was turned by high precision of its angle and linear measurements, but mostly owing to the thoroughly worked out methodical problems. His book became classical, and our geodesists are still benefiting from it.

The second fact was the work of F. A. Sludsky, professor at Moscow University, in the 1880s on the theory of the figure of the Earth [see Sludsky (1967)], much ahead of those times. He is known to have been the first to offer the differential equation of the geoid and the idea of jointly applying AG and gravimetric data for determining the form and size of the Earth. He also indicated that on land the geoid was situated below the surface of the normal spheroid, and above, on the seas.

Thus, up to 1917, the [general] state of geodetic work was unsatisfactory. Lacking was a unique system of central geodetic network of sufficient precision, either horizontal or vertical. There was no map of a sufficiently large scale covering all the country; maps to the scale of 1:417,400 only existed for the European part of the country and were dated in many parts; to the scale of 1:208,700 and the topographic maps of larger scales 1:20,870, 1:41,740, 1:83,480 and 1:125,200 were only available for small territories mostly in the frontier zone. Departmental geodetic work was carried out from time to time and absorbed considerable means without providing satisfactory results for mapping. A rapid reform of geodetic activities became urgent and was implemented after the Revolution.

[3] On 15 March 1919 Lenin decreed the creation of VGU liable for arranging geodetic work with the aim of uniting all such activities and organizing them for most fully satisfying the various requirements of the country and mapping it to an acceptable scale in the shortest possible time. The beginning of Professor Krasovsky's geodetic career occurred during those years.

We know very little about Krasovsky's earliest years. He was born 26 September 1878 [new style] in Galich, Kostroma province, into a family of an office employee. After losing his father in early childhood, F. N. had to live in strained circumstances. His primary education took place in the district school in his home town.

The teachers paid attention to the boy's outstanding aptitude and attempted to assist him in every possible way in extending his knowledge. Upon finishing that school, the efforts of his uncle, M. O. Krasovsky, a senior land surveyor, made possible for F. N. to enter the general educational classes of MMI holding a state stipend. There, in that Institute's boarding house, Krasovsky had passed his life until the maturing of his creative power.

The classes provided approximately the same education as the non-classical schools [German, Realschule] but in addition they offered three courses in land surveying and two, in engineering. After successfully finishing them, Krasovsky passed on to the senior special courses [of the Institute] and animatedly began to study higher mathematics, mechanics, geodesy, astronomy and other special disciplines. The lectures read by such brilliant professors as I. A. Iveronov, V. K. Tserassky (astronomy), the future academician S. A. Chaplygin (mechanics), L. K. Lakhtin (mathematics), had been to a large extent conducive to his studies. Students had access to the Institute's unique fundamental library boasting 150,000 volumes and containing classics of general Russian and foreign literature of the 19th century, as well as of geodesy, mathematics and astronomy of the same period in Russian, German and French. There did exist a real source for learning!

Young F. N. gave himself up with ardour to studying the talented works of the founders of geodesy, astronomy and mathematics, Struve, Chebyshev, Grave, Markov, Tsinger, Gauss, Bessel, Lagrange and Laplace[3]. Already then the serious taciturn young man had been enjoying deserved authority among his comrades for his deep knowledge and high ethical qualities.

[4] In 1900, Krasovsky graduated with a gold medal and was retained at the Institute to prepare himself for scientific and educational work. The next two years have passed in intensified studies of mathematics, theoretical mechanics, geodesy and practical astronomy. F. N. conducted practical classes for students and at the same time entered the physical mathematical faculty of Moscow University as a lecture-goer permitted to attend lectures without formally becoming a student. He did attend the lectures of best University professors and filled gaps in his education. With gratitude, he later recollected professors Chaplygin, Tserassky and Iveronov who had stirred in him an unquenchable desire for knowledge and a spirit of a researcher.

During his maturing the studies at the University had left deep traces on the intellect of the young scientist and created him as that tireless champion of science and researcher whom he was, and who is thus left in our memory. For completing his preparation, the Institute sent F. N. to Pulkovo observatory. There he worked in practical astronomy under F. F. Witram and A. P. Sokolov and in geodesy, participating in treating the materials of the Spitsbergen meridian arc with Witram and A. S. Vasiliev.

Some attendant circumstances had regrettably curtailed his scientific trip, and he spent in Pulkovo only 5 1/2 months instead of the scheduled ten. Upon returning to Moscow in 1903, Krasovsky submitted a thorough report (1904) on his work in Pulkovo. From there, we find that, in addition to treating the materials of that arc measurement, F. N. was able to participate in the investigation of the Pulkovo horizontal circle and acquaint himself with the compilation of maps of barely known countries, and with various types of map compilation by KVT. His other activities touched on practical astronomy and mostly consisted in acquainting himself with observing stars and treating the measurements obtained; working with a portable transit instrument with a registering micrometer, studying changes of latitudes, proper motion of stars and reduction of various catalogues to the same epoch.

All this work, as he (1904, p. 110) stated, had widened his scientific horizon and allowed him *to understand the subject in a wider context and to imagine clearer the aims of modern practical astronomy*. The report is very instructive for postgraduates in that it shows how should a scholar regard his work.

[5] The *List of Works of F. N. Krasovsky*[4] shows six writings published up to 1904. In the first one (1901), he turned attention to three of G. N. Shebuev's works. The first two of them (before 1901; possibly 1892 and 1895) had analytically and rigorously considered the geometric properties of an arbitrary surface, and, in a particular case, the author had studied the distances, azimuths and triangles on the surface of a triaxial ellipsoid little differing from a sphere. Applications of that case in geodesy had especially interested Krasovsky since it was directly connected with examining the figure of the Earth.

The third Shebuev's work also interested F. N. because the author was the first to formulate and solve the problem about the influence of the anomalies of the potential of the terrestrial attraction on the discrepancies (closures) of the polygons of levelling both for any surface and a surface little differing from a sphere. As an example, the author applied the method of models[5] which is now in general use. For a closed polygon he provided a formula for calculating its discrepancy caused by those anomalies.

Shebuev's investigations did not regrettably find any practical application which is quite understandable: a gravimetric survey of the country was lacking, but the very appearance of his work secures the priority of Russian scientists in the region of the so-called *geodetic gravimetry* created nowadays mainly by M. S. Molodensky with some participation of F. N. himself. The problem raised by Shebuev had found its theoretical solution (Molodensky 1948) and is being put into practice.

Krasovsky's first original work (1902b) is as though a candidate dissertation[6]. It testifies to his complete maturity as an engineer and young scientist. Interestingly, F. N. issued from the idea about the triaxial terrestrial ellipsoid as most approximating the body of the Earth. This idea runs through all of Krasovsky's subsequent writings for more than forty years and is most clearly expressed in (1936b). There, he convincingly showed that the introduction of a triaxial ellipsoid leads to a much better agreement between the results of various arc measurements. This means that the so-called large waves of the geoid are best represented by the simplest of all the regular geometrical forms, by the surface of a triaxial ellipsoid[7].

It can be thought that the idea of the stated optimal property of that ellipsoid was widely disseminated among geodesists of those times. This circumstance can explain both the appearance of the above-mentioned works of Shebuev and Krasovsky's choice of the subject for his first scientific writing. Its importance certainly consists not in the results obtained, but in its methodical approach: he introduced a supplementary unknown and solved the equations of arc measurements compiled for a simpler surface of an ellipsoid of rotation rather than for the surface of the triaxial ellipsoid sought. This trick, which essentially simplified his problem, has always been applied in the future by him himself and his students.

[6] Upon returning from Pulkovo and entering the teaching staff of MMI, Krasovsky began to busy himself with problems in geodetic education and organize laboratories. It should be borne in mind that in pre-revolutionary Russia the Institute had not been able to promote higher geodesy as a science. The instruction in it and in astronomy had been modest and restricted to satisfying the needs of the Land Surveying and other departments for constructing networks of triangulation of the III order. Accordingly, those disciplines were only taught for two years with a small number of [weekly] hours and one summer training session.

From 1907, F. N., as a junior instructor, began reading his own course of higher geodesy for third- and fourth-year students alternating year after year with Prof. I. A. Iveronov, but in 1912 he became senior instructor and chair of higher geodesy. In 1917, F. N. was already an ordinary professor of the same chair. From 1907 to 1917, Krasovsky had been teaching geodesy as a pluralist at the Moscow Higher Technical School. At the same time, he had read lectures [at MMI] in the theory of the figure of the Earth and conducted practical classes in astronomy; Iveronov delivered lectures in that discipline.

F. N. personally participated in surveying several cities (Kursk, Kazan, Revel [Tallinn], and Moscow) accomplished by the students on behalf of MMI, in applied investigations of land-melioration in the Middle Volga region, and directed the astronomical work of the Resettlement Department in Siberia which provided valuable material for mapping.

However, during those years Krasovsky turned his main attention to educational problems. First of all, he improved the organization of the winter and summer training sessions; established a geodetic laboratory; enlarged the geodetic room by modern precision theodolites for measuring angles in triangulation of the I order; built a tower with four posts on the Institute's roof for exercises in angle measurement; supplemented the triangular network in [the vicinity of] Pererva[8], the venue of the students' summer training sessions in geodesy, by a few new wooden geodetic signals and thus approximated those sessions to actual working conditions.

Being himself interested in the issues of *greater geodesy*, he foresaw the impending essential heightening of the demands from the national economy to higher geodesy after a revolution, whose approach had been clearly felt by the most progressive elements of the society, to whom F. N. also belonged. He specified his aim as preparing his students for accomplishing precise geodetic work on the vast expanses of Russia taking into account its geographic conditions; and for securing for them a clear notion of the general problems of geodesy and of the special conditions existing in backward Tsarist Russia.

Krasovsky tackled his aim from two sides. *First*, it was necessary to work out methods of the field work beginning with the construction of the triangulation of the I order. Relevant experience did exist: the Struve arc and the work of KVT during the 19th century, but mostly the knowledge acquired by the lay-out of the chain of the I order from Pulkovo to Nikolaev and of the adjacent five polygons of the same order accomplished in 1910 – 1914 under I. I. Pomerantsev by quite modern methods and instruments. In addition, there existed the experience of constructing departmental triangulation of the lower orders, and of foreign triangulations.

Krasovsky's generally known publication (1916) appeared as the result of developing those problems and reading lectures. There, for the first time ever, he thoroughly described the methods of practically constructing control geodetic networks (building of signals, baseline and angle measurement, precise levelling) with a part devoted to the treatment and adjustment of trigonometric networks.

Second, it was necessary to develop the mathematical part of all the calculations concerning the treatment of AG networks; to study the main problems of Russian higher geodesy; and to trace the main patterns and methods of their solution by issuing from the specific features of Russian territory and the level of development of Russian geodetic work. All that should have widened the students' mental outlook and prepared them for practically solving the problems demanded by life.

Krasovsky's study of these chapters of higher geodesy resulted in a number of lithographed editions of his lectures and monographs devoted to separate issues characteristic for that period of his scientific work. The typical and original features of his school began to manifest themselves: an exhausting completeness and thoroughness of description *with all the conclusions being carried out to practical results.* F. N. never left any puzzling questions; on the contrary, as though foreseeing such cases, he encountered them himself. Not accidentally did all the practitioners and researchers turn only to him when looking for and finding answers to all the occurring questions.

Krasovsky himself considered that the working out of each problem was concluded and indeed concluded it only after obtaining exhausting and clear answers to all questions either formulated by him himself or those which could have been expressed or will be encountered by his readers or listeners. Here, his considerable pedagogic experience and remarkable features as an outstanding specialist in scientific methods had revealed themselves.

[7] After the Revolution, Krasovsky's activities were displayed especially wide. Being concerned about a correct organization of geodetic education, and becoming, in 1919, the first elected rector of MMI, F. N. began separating it into the geodetic, land surveying, cartographic and engineering land-melioration faculties. This measure allowed a considerable strengthening of the instruction in geodetic, astronomical and cartographic disciplines. Courses in gravimetry, theory of the figure of the Earth, photogrammetry and mathematical cartography were included in the curricula of the geodetic faculty, geodetic and gravimetric rooms were established, a new building for the astronomical observatory was built and a 240 *m* baseline arranged in the Institute's yard.

Of decisive importance for the further development of MMI as an institution of higher geodetic education was the creation, in 1919, of VGU decreed by Lenin. It was liable for conducting all the main AG work, surveying and mapping the country, uniting and directing the geodetic activities of all the departments. This novelty changed the aims of the geodetic faculty of MMI. Its graduates were faced with conducting all the AG and gravimetric work over the entire expanses of a vast country in all its diversity. It was necessary not only to assimilate the existing methods of work and the pertaining arsenal, but in addition to solve a number of new and most complicated problems in geodetic theory and practice following from the size of the country's territory and the immensity of the forthcoming aims of socialist construction.

The preparation of highly qualified specialists in geodesy and cartography became the Institute's main goal and led to its further separation in 1930. It broke up into independent Geodetic and Land Surveying institutes. The land surveying faculty was given over to the latter, and the engineering land-melioration faculty transferred to the Timiriazev Agricultural Academy. The geodetic faculty of MGI was itself separated into the AG and geodetic aerial photography departments with the latter soon becoming an independent faculty. Then, the need to organize national production of instruments compelled the Institute to establish an optical-mechanical faculty, without which, as F. N. thought, the development of the national geodetic school could not have been considered accomplished. Finally, the cartographic faculty was soon separated into the cartographic, polygraphic and cartographical geodetic departments but the latter was then transferred to the geodetic faculty.

Thus, gradually, an institution of education consisting of four faculties had been formed; in 1936, it was renamed MIIGAiK. This evolution occurred first of all as a result of the school's reorganization in connection with the new requirements of life and it testified that the new geodetic institute was full-blooded. Professor Krasovsky was the life and soul of that process. He personally worked out or participated in the development of new curricula, programmes and profiles of courses of the separate faculties and in the compilation of educational aids. He designed new rooms and laboratories; wrote fundamental textbooks in higher geodesy; read lectures; organized and directed practical classes for students; guided the preparation of students' degree theses and led postgraduates; and at the same time actively worked as a scientist.

[8] The created VGU formulated new aims, previously unprecedented in scope and importance, and this occurrence became the turning point in Krasovsky's forming as a geodesist. Until then, he only solved separate particular problems connected with surveying cities, levelling over large areas in Zavolzh'e, with his most considerable practical work being the directing of 1) the astronomical observations of the Resettlement Department in Eastern Siberia (1909 – 1917)[9] and 2) the construction of the Moscow triangulation (1919 – 1921). Now, however, the object of his activities became the vast territory from the country's western frontier to the Pacific shores in all diversity of its natural features and complications of arranging the main geodetic work.

F. N. was one of the first to become a staff member of VGU, and he forever merged all his intentions and aspirations with its activities. All Krasovsky's previous work may be seen as preparation to his future tireless activities of an outstanding geodetic theoretician and practitioner.

During his work as an educator, he developed the methods of accomplishing the field work involved by constructing control networks; established a mathematical basis for their treatment up to and including the calculation of geodetic coordinates on the surface of the adopted reference ellipsoid; fundamentally and methodically solved the issue of the forthcoming determination of the initial geodetic data; traced the approach to scientifically applying the AG work for studying the size and form of the Earth.

The graduates of MMI were therefore theoretically quite prepared for solving the forthcoming great problems of geodetically opening up our vast territory and only lacked practical know-how and required scientific guidance (ensured by F. N. with unsurpassed skill).

The year 1921 can be seen as the beginning of Krasovsky's work at VGU; abandoning the rectorship of MMI, he became at first the inspector of works for the Moscow region, then occupied the post of the head of the scientific and technical council of VGU. From 1924 to 1930 F. N. directed geodetic work as assistant head of VGU. During those years, he had to fulfil a great managerial and scientific and technical work of developing, in essence anew, the AG network of the I order; to prepare personnel; acquire instruments and other equipment; work out the pattern and programme of main geodetic works and arrange them in the field, sometimes personally, as when measuring the Riazan baseline of the I order in 1923; to instruct the leading personnel; compile the main instructions for accomplishing the field and computational work, etc. The difficulties involved had been especially great because those had been the most trying first years when the Soviet government was in the making.

[9] Among Krasovsky's scientific works of that period especially important was one (1928) where he, while developing and extending the experience of VGU, suggested and scientifically justified a new pattern for the state triangulation:

1. The size of the chains of I order were almost halved from 370 to 220 *km*.
2. At the intersection of such chains, the astronomical azimuths and latitudes were replaced by bilateral Laplace azimuths. In addition, such azimuths, only unilateral, were envisaged in the middle of each chain; true, this latter suggestion was not put into practice.
3. For securing mapping to the scale of 1:25,000, the polygons of the I order were filled up by a compact network of triangulation of the II order controlled by two intersecting main chains of the same order with a base and bilateral Laplace azimuth at *their* intersection. In 1939, a special commission of GUGK somewhat supplemented that proposed network of the II order by heightening its precision so that it will also control surveys to the scale of 1:10,000.

The merits of this proposal were especially revealed much later, in 1942 – 1944, when the AG network constructed by then (and covering 2/3 of our territory) was jointly and rigorously adjusted according to the method developed by Krasovsky and improved by D. A. Larin. It turned out that only our AG network was thus adjusted; in all other countries, this was at best done approximately. Indeed, only the Krasovsky pattern allowed a joint rigorous adjustment, a feature which manifested his keen foresight. When working it out, he had to a certain extent presciently seen the method of its adjustment but only much later, in 1931, did F.N. directly approach that issue. Taking into account our remarkable success, some other countries (France, for example) began to alter their AG networks after Krasovsky's pattern.

[10] In 1928 – 1929, the main geodetic work had been going on at full speed. Sufficient control was already established over a considerable territory which allowed VGU, and then GGK [1928 – 1930] to begin a planned mapping of the country. Krasovsky [appropriately] attempted to appraise the necessary scale for the state map of the country. Issuing from the requirements of the departments and national economy, he (1924b)[10] quite justifiably concluded that the scale of 1:100,000 should be aimed at, which had to be ensured by an initial mapping of large tracks to the scale of 1:25,000. However, taking into account the necessity of compiling the map to the desired scale in the shortest possible time, he suggested making use of the rich materials of land and forest surveying by some additional work for connecting the separate wood plots and orienting their boundaries at least by determining astronomical azimuths. For that latter aim F. N. designed a special method[11].

Krasovsky's work in mathematical cartography belongs to the same period. He developed a few new projections best suited to the configuration and latitudinal location of a given country (1925). Another subject under his study was the participation of geographers in the compiling of topographic maps. Together with the geographer A. A. Borzov he created a new direction in cartography expressed in the joint compilation of state maps by cartographers, geographers and geomorphologists.

This novelty essentially enriched the contents of maps and heightened their general scientific value which to a large extent explains the success of the very first fundamental cartographic work (maps to the scales of 1:1,000,000 and 1:5,000,000) of our cartographers. Geographers were also drawn in for collating materials already during topographic surveying done by VGU and for compiling geographical descriptions of separate sheets of the map to the scale of 1:1,000,000.

These ideas and suggestions had been reflected in the curricula of the cartographic faculty of MGI, namely, in strengthening the instruction in geology, geomorphology and geography. Accordingly, the value [the usefulness] of the faculty's graduates was considerably heightened.

[11] By the end of the 1930s the main portion of the polygons of the I order in the European part of the country (to the south of latitude 60°) had already been constructed, and the work extended to the East of the Urals. The severity of the barely populated Siberia and the special conditions of the northern regions of the European part of the country caused new problems and difficulties. It became necessary to review and specify the methods of precise linear and angle measurements and to establish the optimal types of centres and benchmarks for differing and extremely diverse geographic conditions taking place over a vast territory including regions of deep frozen ground and permafrost. Necessary to review the building of geodetic signals; examine the methods of precise levelling; develop methods of terrestrial and aerial photographic surveying without which the mapping of the country was impossible; to work out methods of treating and adjusting AG data; trace the programmes and methods of scientifically applying the appearing rich materials, etc.

[12] An urgent solution of all this complex of complicated scientific problems was required. This compelled Krasovsky to initiate the establishment of a research institute which was indeed done at the end of 1928, and that institute was later called TsNIIGAiK. F. N. became its first director. From 1930, after freeing himself from the duties of assistant head of VGU, he wholly surrendered himself to developing research activities and preparing the personnel needed for that as a director, then the assistant (science) director (1930 – 1937).

In spite of great difficulties (lack of premises, laboratories, sufficient personnel, and transfer from Moscow to Leningrad and back), that institute had developed and by 1937 became a large research institution and acquired a deserved authority both home, in the country as a whole, and abroad. It consisted of geodetic, geodetic aerial photography, cartographic and instrument sections. Main achievements during the first decade of its existence, were:

Establishing the proper methods of angle measurements in triangulation of the I order; **examining** the influence of vertical refraction on the results of precision levelling and determining its methods; fundamentally working out the supplementing of the main triangular chains of the I order by astronomical observations so that we became the only country where these observations had been most effectively applied in geodesy; **developing** a rigorous method of adjusting large AG networks (the methods of Krasovsky and Urmaev); **working out** the methods and arsenal of aerial photographic surveying so that it became the main tool for state surveying up to and including large scales and essentially sped up the compilation of the state topographical map. This circumstance had played a great part in the mapping satisfying the requirements of the socialist economy and the successful accomplishment of the Stalin five-years plans. Then, **instructions** in all the main types of AG work were compiled and provided a robust scientific base for their arrangement.

We should also note that in 1932 the general gravimetric survey of the country had begun. It essentially contributed to the future proper arrangement of all the main geodetic works. [To supplement the previous text, the author adds:] During those same years F. N. established the main approach to the development of arc measurements and derived the first reliable size and form of the Earth ellipsoid (the Krasovsky ellipsoid of 1936)[12].

Those considerable scientific achievements of TsNIIGAiK, having been the fruit of a large collective of talented geodesists, were hardly possible without Krasovsky's directing and often personally participating and applying his great scope of scientific activities and deep and original thinking. These traits indeed explain his indisputable authority and importance.

The brilliant successes of our cartography were also to a considerable extent indebted to his guiding influence. F. N. engendered that sphere of knowledge by the idea of an geomorphological and geographical approach to cartographic materials which greatly enriched our maps and placed them at the head of world cartography.

[13] When, in the beginning of 1937, TsNIIGAiK became firmly established, F. N. quit carrying out the duties of its assistant director and turned his main attention to MIIGAiK, to its chair of higher geodesy. Nevertheless, in TsNIIGAiK he continued to direct the work that interested him, the establishment of the initial geodetic data and the study of the geoid's figure. His student, A. A. Izotov, was directly engaged in that work. In 1940, by making use of the vast arc measurements extending from our western borders to the Novosibirsk meridian, and the materials of such measurements in Europe and the USA, Izotov, working under Krasovsky's general guidance, derived the most trustworthy elements [parameters] of the general Earth ellipsoid,

a = 6,378,245 m, ε = 1/298.3,

later called after Krasovsky, and selected as the foundation of all our AG work instead of the previously applied Bessel ellipsoid.

Later Izotov obtained the elements for orienting that ellipsoid most agreeing with the surface of the geoid over all our territory with the origin at Pulkovo but taking into account the Laplace stations at all the intersections of our AG network.

Thus the initial data for the forthcoming joint rigorous adjustment of all our network of I order were determined. In those same years F. N. suggested and worked out the *method of projection* for treating that network instead of the earlier universally applied method of development. The new method was greatly important: the treatment of the trigonometric network of I order became mathematically wholly rigorous and clear; excluded was the additional corruption of the network peculiar to the previous method and lowering its precision as compared with what is typical for field work, by a factor of several dozen.

At the same time, following Krasovsky's indications and under his guidance, M. S. Molodensky worked out the method of astronomical gravimetric levelling whose application allowed to obtain the geoidal profiles along chains of the triangulation of I order and thus enabled to make use of the method of projection in a future adjustment of our AG network. This joint rigorous adjustment was accomplished in 1942 – 1944 by the Central Computation Department of GUGK under Krasovsky's general guidance and as though completed the first period of constructing the country's AG network. We are now the only country in the world possessing a most precise network of I order thus adjusted (and covering 2/3 of our territory). No other country had yet been able to solve satisfactorily this main problem of geodesy.

[14] During that period F. N. compiled the generally known treatise (1938 – 1939, 1942). Its second part (1942) was awarded the Stalin Prize of the first degree[13]. The treatise has become a fundamental aid for MIIGAiK students, practitioners and researchers. Its first part was devoted to the methods of field work involved in constructing the horizontal and vertical control networks and made use of the entire experience collated by GUGK up to 1937 as well as that accumulated abroad and the results of our laboratory and theoretical investigations. From its first appearance (1926), the exhausting completeness and clarity of exposition led to its becoming a *Handbuch*, in which geodesists had been finding the solution of all the problems they encountered.

The second part covered the geometry of the spheroid, the solution of all problems considered on its surface and the scientific issues connected with the application of AG and gravimetric measurements for studying the size and form of the Earth and the structure of its upper mantle and crust. Being deeply original in contents and description, it concentrated the results of Krasovsky's entire scientific work. And, going far beyond the boundaries of an aid, its importance is outstanding, it is a *leading* contribution. Especially important were chapters 9 – 11. There, F. N. quite clearly and fully elucidated all the main issues connected with the adjustment of vast AG networks, treatment of arc measurements and application of the results for deriving scientific inferences about the size and form of the Earth, i. e., about problems in which Krasovsky had been mainly interested during the last years of his life.

In chapter 9, he described, with appropriate fullness including additions made during those years, his method of rigorously adjusting vast AG networks. Striving to secure as much as possible the independence of the derived lengths and azimuths of the geodesics temporarily replacing separate chains, and thoroughly analyzing the sources of error, Krasovsky concluded that it was necessary to raise the precision of the Laplace azimuths by introducing the so-called *fundamental* azimuths 1,000 – 1,200 *km* apart, chosen and observed especially carefully. A preliminary adjustment of the azimuths situated between them allows to heighten considerably their precision.

Krasovsky deduced rigorous azimuth equations with additional terms, as compared with Helmert's equations, correcting the deformation of the AG network if developed on the surface of the reference ellipsoid. Having analyzed the size of these terms, he concluded that the ensuing corruption of the adjusted Laplace azimuths should not be disregarded since they led to a systematic *twisting* of geodetic chains and thus engendered serious deformations in the AG network as a whole.

[15] Until now, each country usually projected its treated AG networks on the surface of the geoid, then developed them, without correcting the lines or angles, on the surface of the adopted reference ellipsoid. Understandably, this development of the irregular surface of the geoid on the curved surface of an ellipsoid resulted in the deformation of the network. Curiously though, the efforts of West European and especially American geodesists had been directed toward proving that those deformations were insignificant and practically unimportant.

If true for small territories, this is not at all valid for large regions, as Professors' Krasovsky and Danilov appropriate investigations convincingly showed. Suffice it to indicate that, for example, the ensuing error of the mutual location of Khabarovsk and Pulkovo is 30 – 40 times greater than the same magnitude due to errors of the field work and can not be allowed. Then, in chapter 11 F. N. proved that the *method of development* also corrupted the thus derived elements of the reference ellipsoid and its orientation. That method, rather than ensuring rigorous mathematical treatment, resulted in unknown and considerable errors being introduced in the coordinates of the networks' stations.

Even were it possible to tolerate those errors from the standpoint of the country's mapping, in principle, for deriving correct scientific conclusions, such an *arbitrary* treatment of an AG network is inadmissible. In future, the defects of the method of development will be felt especially acutely when the networks of different countries are joined. The coordinates of common stations will diverge to such an extent, that even a *cartographic* contact is impossible.

Because of those considerations F. N. proposed to replace the method of *development* by *projecting* the elements situated on the surface of the geoid on the surface of the reference ellipsoid by normals to the latter. An absolutely rigorous, mathematically strictly treated network, free from any additional corruption or deformation, will result. The method of projection was first applied in 1942 – 1944 for the joint rigorous adjustment of our entire AG network. This required a preliminary establishment of the mutual position of the surfaces of the geoid and reference ellipsoid.

In chapter 9, Krasovsky thoroughly worked out the method of compiling such geoidal *profiles* and of projecting its elements (lines and angles) on the surface of the reference ellipsoid. Concerning the adjustment of AG networks, we see that the scientific thoughts of our geodesists in the person of F. N. and his students had considerably advanced, and ensured for us the first place.

Krasovsky (1902b) devoted his first scientific writing to the derivation of a triaxial ellipsoid from Russian arc measurements. From then onwards, he had always been interested in the issues of arc measurements and in the following derivation of the elements of the Earth ellipsoid and its orientation.

[16] After 1930 F. N. returned to that issue in numerous contributions and thoroughly developed the arrangement of arc measurements and their scientific applications. Carefully analysing the materials and conclusions following from the main measurements, he arrived at a number of essential inferences and proposals:

On the necessity of appropriately **locating** the arcs and their interconnection into a single system with a surface coverage; on many fundamental **defects** and the ensuing weak efficacy of the **isostatic hypothesis**[14] when applied to correct astronomical stations for the deflection of the vertical; on the necessity to allow, when treating arc measurements, for a **triaxial** Earth ellipsoid; on the expediency of abandoning arbitrary presumptions and basing the treatment of arc measurements on data of **gravimetric surveying**.

As a result, F. N. proposed a new programme of arc measurements which advisedly combined AG and gravimetric materials. We had indeed begun to measure our arcs according to this new programme.

Krasovsky's contributions had thus introduced complete clarity into the complicated and subtle methods of arranging and applying arc measurements. In particular, he fully elucidated the part of gravity in arc measurements and worked out methods for applying gravimetric observations. All these issues are described with an exhausting completeness in chapters 9 and 10.

When studying the derivation of the size of the Earth ellipsoid and the elements of its orientation, F. N. provided an original method for solving that problem by issuing from the heights of the surface of the geoid above that of the adopted reference ellipsoid. He described this in his earlier contributions (1936a, b) and chapter 9, pt. 2, of his *Treatise*. This method had not yet been applied, but promises much benefit owing to its simplicity and precision.

The discussion above shows that that *Treatise*, pts 1 and 2, represents an exceptional phenomenon both in our and foreign literature. Being absolutely clear, complete and rich in new ideas, it also provides answers to all the questions raised by our practice. This is not at all surprising since F. N. based his writing on experience accumulated during more than 40 years of teaching, almost ten years of direct practical work as an assistant (science) chief of VGU, and, finally, on another decade of guiding the scientific work of TsNIIGAiK.

While actively working as a scientist and teacher, Krasovsky participated in a number of conferences (1921 – 1929), collaborated with the Geodetic Board of Gosplan [All-Union State Planning Committee] taking part in three of its conferences, participated in the activities of the Baltic Geodetic Commission (in its sixth conference in Warsaw in 1932 and seventh conference in Moscow [and Leningrad] in 1934)[15]. In 1933, he was elected its vice-president, and president in 1936. He compiled and read many reports on the main issues of astronomical geodesy, delivered lectures at the Kuibyshev Military Engineering Academy and mathematical-mechanical faculty of Moscow State University. In 1939 F. N. was appointed member of the Board of GUGK; in 1922, he was expert in the conferment of scientific degrees, first at Glavprofobr[16], then at the pertinent All-Union Academic Board.

It may be said that he participated, directly or obliquely, in each considerable measure, in arranging a continuous **gravimetric survey** of the country and compiling its programme; reforming the higher and secondary **geodetic education**; **arranging** the geodetic work at GUGK and several departments; and **examining** the research programme and separate investigations at TsNIIGAiK. In January 1939, F. N. was elected Corresponding Member of our Academy of Sciences (physical and mathematical department) and from 1941 had been collaborating with its Institute of Theoretical Geophysics.

We ought to indicate especially Krasovsky's membership of the Board of GUGK. Enjoying considerable authority, he was able to influence essentially its decisions concerning the main issues of the arrangement of geodetic work and its methods and thus to continue successfully keeping to his general guidelines traced when he had been mainly working in VGU. F. N. valued this possibility very much and remained until death one of the Board's most active members.

At the Academy of Sciences, F. N. devoted the last decade of his life (1939 – 1948) to examining the main issues of higher geodesy which connect it with such adjacent disciplines as gravimetry, astronomy, geology and geophysics. He continued to develop programmes and methods of arc measurements by additionally involving geological, geophysical and gravimetric data and formulated the pertinent problem of studying the structure of the Earth's upper mantle. These two directions are well represented in his reports (1941; 1947). The first of these directions connected with the working out of a new chapter of higher geodesy, the so-called *geodetic gravimetry*, is presently continued by the well-known contributions of M. S. Molodensky, a Corresponding Member of the Academy of Sciences; the second direction is similarly represented by his student, Prof. V. A. Magnitsky [later also a Corresponding Member of the Academy].

[17] Summarizing almost half a century of Krasovsky's activities, of the most prominent geodesist of our time, we ought to say that his great contribution to science is even difficult to appraise now. His brilliant achievements in arranging vast geodetic work and scientifically applying it had advanced our socialist country so that it became the leader both in geodetic theory and practice. Indeed, our geodesy is essentially indebted to him who ideologically [morally] headed a large collective, consisting mainly of his direct or indirect students.

Even more important was the fact that the entire strong body of geodesists had been soldered together in a single harmonious family by the pathos of Soviet construction […].

It is impossible to separate Krasovsky's name from that of his famous students and companions like the scientists Molodensky, N. A. Urmaev, A. M. Virovets, Izotov, A. S. Chebotarev[17], Magnitsky, A . I. Durnev, O. G. Dietz, K. A. Tsvetkov, D. A. Larin, I. Yu. Pranis-Pranevich, P. S. Zakatov, N. M. Aleksapolsky, B. V. Fefilov, et al, or the names of enthusiastic practitioners like A. N. Baranov, S. G. Sudakov, M. K. Kudriavtsev, A. V. Rytov, V. F. Pavlov, P. I. Povaliaev and many others.

They all, each earnestly working in his place, have contributed to the common aim of our glorious geodesy. F. N. contributed so much […] that, were there no previous renowned culture of the Russian nation, or successes of Russian geodesy in the 19th century[18], no Revolution […], he would have been unable to develop and reveal fully his talent, and we would be lacking that Krasovsky, whom he is for us now. Looking over the main stages of his scientific creative work, we see that all his studied issues were raised by life, dictated by its demands.

A tight connection of our geodesy with life and its demands is peculiar for all our geodesists rather than for him alone. True science can not tear itself from life, otherwise it is separated from that ground which nourishes it by its juices […].

An active connection with life and practice engendered both Krasovsky's scientific work and pedagogic efforts. Exactly for this reason his spoken or printed word is so precise and clear and his work so rich in new ideas and so surprising by the deepness and power of intuition. [A quotation from Stalin follows.]

Now, when F. N. is gone, his contributions acquire an absolutely special importance. Collected in a single edition, they will continue to serve as an inexhaustible source of new thoughts and ideas for contemporary geodesists and future generations.

Krasovsky (1938 – 1939, 1942; 1942, p. 441) perfectly well said about Struve (1856 – 1861):

A conversation through this writing with that man of great intellect, a talented theoretician and practitioner of many years, is really necessary for educating a beginner and useful for an experienced and practically knowledgeable geodesist for verifying himself.

These words are no less applicable to Krasovsky's own contributions.

Notes

1. Danilov mentioned quite a few Russian geodesists of the 19th century. See Belikov & Soloviev (1971), Zakatov (1950, § 93) and Virovetz [i]. I have expressed the scales of the old maps in the metric system.

2. I can indicate Rielke (1894) and *Katalog* (1934).

3. Who compiled this list? Did not Krasovsky study, for instance, Cauchy? On the other hand, although Grave was an eminent scholar, his name hardly belonged there.

4. Where did Danilov find that list? This is an example of an inadmissible faulty documentation, as understood at least nowadays.

5. In the 1960s, I had come across a few publications applying the method of a *corrupted model*, as the workers of TsNIIGAiK called it. Begin with an adjusted chain of triangulation (say), randomly corrupt its elements by errors chosen in accord with an appropriate normal distribution, and adjust the chain anew. The result, as far as I remember, largely meant that 2/3 of the corruptions became smaller, and 1/3, larger, although not exceedingly so. A similarity with the Monte Carlo method suggests itself.

6. The degree of *candidate of science* was conferred on those who successfully defended their candidate dissertation. It corresponds to the doctor of philosophy degree [vol. 11, article candidate of science].

7. The simplest regular form is certainly provided by a sphere.

8. Pererva is, or at least was, the name of settlements in several Russian regions.

9. Izotov [x, § 3] mentioned a slightly different period, 1909 – 1916.

10. The date, 1924, does not agree with the context.

11. Explanation (Krasovsky 1924a). At a station with known geographical coordinates, the angles between the Polar star, a supplementary star with a different declination and the selected terrestrial object (not even mentioned by Krasovsky!) are measured. It then becomes possible to calculate the azimuth of the Polar star at that moment, i. e., to determine the sought azimuth. No clock is needed, but this method is not as precise as those requiring a clock.

Krasovsky had published four pertinent papers in 1924, 1925, 1928 and 1929, the last one, 33 pages long, appeared as a booklet (Moscow). In my Bibliography, I only mention the first paper.

12. Izotov [x, § 12] called that *ellipsoid of 1936* provisional.

13. During the Khrushchev *thaw*, those Stalin prizes were renamed *State Prizes*, and thus called by Izotov [x] and Bagratuni [xi].

14. See Krasovsky's discussion of isostasy in [iv, § 1].

15. Actually, in sessions 5 – 9. Danilov (or someone else) omitted the sessions held beyond the Soviet Union and Poland.

16. *Glavprofobr* likely meant an institution of professional education (*obrazovanie*).

17. In a private conversation with a few students including me, Bagratuni remarked that Idelson (1947) naturally did not refer to Chebotarev. Idelson had compiled the first manual in least squares in a modern way whereas Chebotarev (1958) even later published a mammoth textbook on a pre-Helmertian level. He also was a Honoured Scientist of the Russian Federation (actually, a *Honoured Mastodon*). On one occasion he (1951, pp. 8 – 9) stated that it was not sufficient for a mathematical law to *describe* a phenomenon since Marx had argued that it was necessary to *change* the world! Then, he (1958, p. 579) declared that for fourteen centuries Ptolemy had been keeping mankind in ideological bondage … At the time (1952 – 1953) Chebotarev was extremely influential.

18. In § 3, Danilov also stressed the general unsatisfactory state of Russian geodesy in the 19th century.

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X

A. A. Izotov

Krasovsky's contributions to the development of geodesy and cartography.

Izvesiya Vysshykh Uchebnykh Zavedeniy. Geodesiya i Aerofotos'emka,
No. 2, 1979, pp. 42 – 51

[1] The scientific, pedagogic and social work of the prominent astronomer-geodesist and cartographer Feodosy Nikolaevich Krasovsky began at the outset of this century and continued for almost fifty years. Especially fruitful was the second half of this period which coincided with the unique years of the formation and development of our geodesy and cartography as a branch of scientific knowledge and national economy. The Soviet geodetic school, whose universally recognized leader he remained for many years, was indeed born to a considerable extent as an original direction in the development of scientific thought under the influence of his powerful ideas and basic scientific work.

Krasovsky's activities extending over the wide field of geodesy and cartography had been surprisingly many-sided, purposeful and fruitful. He was involved with preparing engineers and scientific workers, solving the main geodetic scientific and technical problems, developing the vital scientific and methodical principles of the organization[1] of the main AG work and topographic mapping, etc. In all these directions he provided that scientific basis on which our geodesy and cartography had been developing during the second quarter of this century, and whose robustness we are feeling until nowadays.

I had been happy to be one of his closest pupils and, in addition, to collaborate intimately with him during many years. Under his day-to-day guidance I went through a remarkable school of scientific and various practical work. I could have recounted much about him both as an outstanding scientist and an original person, but I have to restrict my paper to only providing the most important information about his life and work.

[2] There are very few documents reflecting Krasovsky's childhood and adolescence. He was born 26 September 1878 [new style] into a family of an office employee in Galich, Kostroma oblast (province). After losing his father in early childhood, he had to live in strained circumstances. His primary education took place in the Galich district school, now the Krasovsky 4th Galich secondary school. Owing to the insistent efforts of his uncle, who noticed Krasovsky's remarkable aptitude, he entered general educational classes of MMI holding a state stipend. This was very important for a young man lacking material security.

After successfully finishing these classes, F. N. passed on to become a student of the same institute and graduated in 1900 with a gold medal. He was left at the institute for preparing himself for scientific and pedagogic work and additionally educated himself at the Pulkovo astronomical observatory and the physical and mathematical faculty of Moscow University. His direct teachers and instructors had been such outstanding scientists as I. A. Iveronov and A. S. Vasiliev (higher geodesy); V. K. Tserassky and A. P. Sokolov (astronomy); L. K. Lakhtin (higher mathematics); and the future academician S. A. Chaplygin (theoretical mechanics). Having been strongly induced by their scientific ideas and views, the young man's thoughts and interests as teacher and scientist had been formed. He was destined to contribute greatly to the development of the science of geodesy and cartography.

[3] In the pre-revolutionary years Krasovsky mainly worked in MMI which is very much obliged to him. From 1907, he was a junior instructor, but read lectures in higher [a word is missing in the text]. In 1911, F. N. became a senior instructor, and in 1917 the academic status of ordinary professor was conferred on him. He continuously held that position until his death on 1 October 1948. From 1907 to 1917 Krasovsky also read lectures on geodesy in its applied direction in the Moscow Higher Technical School.

In the pre-revolutionary years F. N. in addition participated in applied investigation of land-melioration in the Middle Volga region, surveying in towns and astronomical observations in the field. From 1909 to 1916 he directed astronomical expeditions in Eastern Siberia for the Resettlement Department which provided valuable materials for mapping the studied territory. Krasovsky's published reports show that he introduced many improvements in the methods and organization of field astronomical work. He is also known to have worked out a widely used method of determining the azimuth of a terrestrial object, called after him, by measuring the horizontal angle between the Polaris and a subsidiary star.

It might be said that F. N. belonged to those prominent scientists who covered their wide section of science and in addition studied adjacent branches of knowledge. Thus, while working mainly in higher geodesy, he studied practical astronomy, gravimetry, the theory of the Earth figure[2] and cartography. His ideas and investigations in each of these disciplines were marked by formulating and solving fundamental problems of great scientific and practical importance.

Krasovsky's first published work indirectly indicated that he had begun scientific studies even during student years. Already then he became deeply interested in determining the figure and the size of the Earth. That problem had been the essence of all his scientific work, and, under the influence of his powerful ideas, its solution became the leading direction in the development of our AG science. One of his first considerable scientific works was indeed devoted to the establishment of the size of a triaxial ellipsoid by applying Russian arc measurements.

[4] After the October revolution Krasovsky's scientific, pedagogic and social activities began to acquire really broad dimensions and to develop in most various directions. In 1919, he was elected rector of MMI which then consisted of two faculties. Together with progressive scientists of those times, F. N. succeeded in establishing quite a few other ones. Greatly important was the formation of the geodetic faculty, later the cradle of our higher geodetic education. MIIGAiK had been gradually developing from that faculty. It prepares engineers and scientific workers in all the contemporary branches of geodesy and cartography.

While working out the curricula and programmes of that faculty, then of the [new] institute, F. N. strengthened the instruction in mathematics, geodesy, higher geodesy and astronomy, and introduced the study of theory of the Earth figure, gravimetry and the fundamentals of geophysics. These curricula and programmes had been repeatedly specified, but their main principles are still valid. Krasovsky also compiled a number of educational manuals and a fundamental treatise on higher geodesy in three [in two] volumes (1938 – 1942). It provided the most complete for that time description of the methods of main geodetic works and mathematical theories of higher geodesy. In 1943, F. N. was awarded the State Prize for its second part.

As a teacher and scientist deeply understanding the main problems of geodetic science and the importance of geodetic theories and methods for solving various scientific and practical problems, Krasovsky powerfully inspired the organization and contents of our higher geodetic education. His views and ideas about that education were partly set forth in some of his published works devoted to that subject, but partly are only kept in the memory of his living closest students and associates.

F. N. had invariably cared that the preparation of geodesists of highest qualification should be based on a deep study of AG theories and physical and mathematical disciplines and be oriented to the solution of scientific problems of geodesy itself and of the [geodetic] technical problems encountered in various fields of human activities.

[5] In 1919, Lenin signed a decree setting up VGU whose successor is now GUGK. This setting up was the turning point and the beginning of a new stage in the progress of our geodesy and cartography. From then onward, F. N. closely linked his many-sided activities with the scientific and practical problems of VGU which was responsible for the state geodetic and cartographic service. In 1921, discontinuing his rectorship at MMI but remaining there the chair of higher geodesy and carrying out serious scientific and pedagogic work, he took a job at VGU. From 1923 until 1930 he was chairman of its scientific-technical council and assistant director being the head of the scientific and technical management of all main geodetic and cartographic work done in the country.

Considerable experience in accomplishing AG work and topographic mapping is known to have been accumulated in pre-revolutionary Russia. And, at the same time, advances in geodetic and cartographic science and technique worthy of attention were also attained on the level of that time. However, from the very beginning of its work, VGU had encountered great scientific and practical requirements not tackled previously either here or abroad. They were concerned, first of all, with the construction of a control geodetic network and organization of topographic mapping, in both cases for the entire country.

It was therefore necessary to solve the main scientific problems connected with the establishment of the Earth figure and size. It was evident that these requirements and problems could not be met/solved without appropriate scientific investigations, and at the end of 1928, on Krasovsky's initiative, a State Institute for Geodesy and Cartography was established. It was later renamed TsNIIGAiK, now bearing Krasovsky's name. Until 1930, F. N. was its director, and, until 1937, its assistant director [but see ii, end of § 8]. Very soon the new institute became the main centre of the development of scientific geodetic concepts.

[6] F. N. proved that for a large country the previous principles of constructing geodetic control nets were useless, and revised them. By 1928, issuing from his theoretical investigations on the action and accumulation of the errors of measurement in triangulation, he worked out a harmonious and scientifically justified pattern and programme for the construction of the state triangulation. He solved the problems about the optimal size of the polygons of the I order and the necessary frequency of baselines and Laplace stations (on which the longitude, the latitude and azimuth are determined by astronomical observations). He also set forth the principles of constructing subsequent lower orders of triangulation laid out within those separate polygons.

Krasovsky's proposals envisaged a construction of an AG network satisfying both the requirements of a topographic study of the country and the aims of solving scientific geodetic problems. In spite of repeated revisions and improvements, their main ideas are still valid. Moreover, they had inspired other countries.

By the end of the 1920s, a considerable, for those times, AG network had been constructed in the European part of the country, and it became necessary to treat and adjust it. Helmert had outlined some pertinent methods, but he connected that problem with determining the size of the Earth ellipsoid and establishing the so-called initial geodetic data. F. N. fundamentally revised his method and formulated his own proposals. First of all, he separated the adjustment proper and the determination of the size of the Earth ellipsoid and its orientation in the Earth's body. He also improved the theory and simplified the drawing up of the condition equations taking place in the polygons. Then, he worked out the problems concerning the application of Laplace azimuths during the adjustment of the vast AG network. His deep ideas are still not exhausted and will for a long time retain guiding scientific importance.

[7] The adjustment of such networks encounters the so-called geodetic reduction problem, the choice of a method for reducing measurements to the surface of an Earth ellipsoid which to some extent characterizes the shape and the size of the Earth. Even in the previous century, the Russian mechanician and geodesist F. A. Sludsky [see Sludsky (1967)] had made known, although not clearly enough, his considerations on the two possibilities or methods of solving that problem.

One of them only admitted the reduction to the [mean] sea level, i. e., only to the surface of the geoid, in spite of the further mathematical treatment of the observations being done on the surface of the chosen reference ellipsoid. The second method envisaged a reduction directly to the surface of that ellipsoid by appropriately correcting the observations. However, neither the features of these methods, nor their essence and consequences of their application were studied at all. Krasovsky's deep investigations rather clearly showed that the first one, universally applied, which he called the *method of development*, actually meant the development of the unknown geoidal surface on the surface of the chosen reference ellipsoid. In addition to the corruptions, due to unavoidable errors of measurement, it led to considerable deformations barely yielding to mathematical analysis.

F. N. also showed that the second *method of projection*, as he named it, which had not been previously applied, consisted in projecting geodetic stations and the measurements made there on the surface of the reference ellipsoid along its normals at those stations and therefore lacked any mathematical deficiencies. After his works the strict method of reducing measurements became quite consciously applied both here and abroad.

The new method requires a determination of the deviations of the geoid from the adopted reference ellipsoid within the network under adjustment. However, for our vast territory the previously known solution by astronomic levelling proved useless since it required frequent AG stations which meant much work and heavy expenses.

[8] While desiring to work out more rational methods for determining the geoidal figure, F. N. formulated the idea about applying AG and gravimetric data together. His idea took shape in the works of one of his former students, a Corresponding Member of our Academy of Sciences, M. S. Molodensky, who developed the now widely known method of astronomical gravimetric levelling. Our contemporary school of theoretical geodesy, generally recognized the world over, had been progressing by basing itself on his, Molodensky's, investigations.

In his studies, Krasovsky also worked out methods of adjusting compact triangulation networks. In particular, he made practically applicable the theory of the now widely used method of adjusting geodetic networks by variation of coordinates (1930, 1931). Now called *parametric*, it proved to be the most convenient for applying computers.

It is very difficult to describe all Krasovsky's ideas and writings concerning the improvement of the methods and programmes of main geodetic works. He also greatly contributed to working out the requirements to, and classification of precise AG and levelling instruments. Then, F. N. had developed many mathematical problems of higher geodesy and methods for solving the direct and reverse geodetic problems on the surface of reference ellipsoids.

We know that in the past it was usual to express the location of control stations in the geographical coordinate system which is not really convenient for topographic mapping and useless for applied surveying. The works of F. N. contributed to the correct and general practical use of plane rectangular coordinates in the Gauss – Krüger projection. He himself and his closest students had so fully ascertained the theory and practice of their application, that no subsequent studies by other authors could have added anything new.

In the 1930s, when civil and industrial building had begun to develop widely, Krasovsky was often asked to consult applied pertinent geodetic work. I am regretfully unable to describe here an episode with his consultations connected with the erection of the planned Palace of the Soviets[3] in Moscow, and therefore to show his exceptional ability to penetrate the essence and the methods of solving complicated engineering problems, remote, as it seems, from his main scientific interests. In general, bearing in mind the application of geodetic methods in various branches of engineering, he urged that the fundamentals of applied geodesy should be developed as a scientific discipline. However, as I imagine, this important problem is still not fully solved.

[9] In 1932, the general gravimetric survey is known to have begun. It was a most important component of our AG work and considerably heightened its scientific importance. Having correctly estimated the value of this scientific enterprise, F. N. first of all had worked out a plan for developing gravimetric work answering the requirements for the solution of geodetic scientific problems. At the same time he set forth the main ideas and considerations on the approach to, and methods of applying the materials of that survey for solving the scientific and practical problems of geodesy and cartography.

When considering and improving the theories and methods of higher geodesy, F. N. had always borne in mind the solution of a wider range of scientific problems of geodesy itself and of other earth sciences. Thus, having worked out a modern organization of spirit levelling, he showed that precise levelling should above all serve for studying the differences between the levels of seas and oceans, vertical movements of the Earth crust etc. His ideas fostered a correct organization of the work of GUGK on precise levelling and powerfully promoted repeated levelling. Already in the 1950s their results are known to have enabled to compile a chart of modern movements of the Earth crust within the boundaries of the European part of the Soviet Union and provided most valuable information for understanding the processes taking place in the Earth entrails.

[10] Strange as it is, Krasovsky's scientific activities in topography and cartography are still very little studied. However, even what is well known, testifies about his considerable merit in the topographical studying and mapping of our vast country. He worked out the conic equidistant projection, the most suitable for representing our country on geographical maps; at the time, it enjoyed wide application. It is remarkable that even in 1923 F. N. advanced the opinion that the compilation of a precise topographic map of the country to the scale of 1:100,000 was the main goal of the state geodetic service. We may note with satisfaction that this important problem was successfully solved long ago. In the 1920s he had proposed a system of scales for topographic mapping and put forward the idea of differentiating our territory from the standpoint of mapping. Later Krasovsky outlined the approaches to, and methods of using the results of land and forest surveys and other applied geodetic work for compiling the state topographic map. In 1938 he returned to problems of mapping, and, drawing on the accumulated scientific and practical experience, offered new solutions concerning the scales and contents of topographic maps.

[11] It is surprising but true, that already by the end of the first decade of the work of VGU either F. N. himself, or [at times] a very small group under his supervision fulfilled a great amount of work on compiling the first directions for the state triangulation, astronomical work, precise levelling, topographic mapping and surveys of towns. All of them were specimens of scientific regulation and unification of programmes, methods and results of AG and topographic works for our territory and had been later repeatedly revised and specified in accord with the new advances of geodetic science and practice.

In the 1920s, F. N. had been a member, then the assistant chairman of the Geodetic Board of Gosplan [ix, § 16], obliged to determine the aims and directions of the progress of our AG and topographic work (see Krasovsky (1931c)). In 1939 he was appointed member of the Board of GUGK and remained in that capacity to the end of his life. It may be said that Krasovsky directly and creatively participated in working and carrying out each scientific, technical and organizational measure in geodesy and cartography.

[12] F. N. had begun his scientific activities by determining the size of an Earth ellipsoid from arc measurements; during all his life, he never forgot this subject. His investigations about constructing geodetic control networks, methods of adjusting AG networks, the programme of gravimetric survey, organization of precision levelling, etc, – had been a continuous development of his ideas about, and approaches to studying the figure of the Earth. However, Krasovsky's broad range of direct work on that subject only dates from the very beginning of the 1930s, when new considerable AG networks satisfying contemporary requirements to arc measurements had already been constructed.

F. N. obtained the first new results simultaneously with the adjustment of the polygons within our European territory. They absolutely clearly showed that the formerly known size of the Earth ellipsoid did not serve as a reliable basis for establishing our system of geodetic coordinates.

Krasovsky above all subordinated the investigations of the shape and size of the Earth to establishing a reference ellipsoid and the initial geodetic data for adjusting the national AG network. In solving this problem, he scientifically justified the requirements for the choice of the size of that ellipsoid and the initial data for AG networks and cartographic work. At the same time, F. N. improved the theories and methods of determining the size of the Earth ellipsoid from arc measurements and justified the application of both AG and gravimetric data for solving this problem.

In 1936, making use of our arc measurements together with those of Western Europe and the USA, Krasovsky published his deduced size of the Earth ellipsoid. However, he thought that his conclusion should be specified and entrusted his students in TsNIIGAiK with further investigations leaving to himself their scientific guidance. In the beginning of 1940, by issuing from more extensive data and, again, those foreign materials, new parameters of the *Krasovsky* ellipsoid were obtained. It is now being applied in our geodetic work and in other socialist [at the time] countries. In 1952, the State Prize was awarded for those investigations to Krasovsky (posthumously) and Izotov.

[13] In 1939 F. N. became Corresponding Member of our Academy of Sciences and began to investigate scientific problems of astronomy and geodesy connected with studying the inner structure of the Earth. In these studies, he urged to link the main geodetic scientific problems to investigations in other earth sciences. Considering the progress of geodesy as of one of these sciences, F. N. indicated in his last work that in the past geodetic methods and the results of AG work had enabled to establish that the Earth was an oblate ellipsoid and in addition to ascertain some main regularities in the inner structure of the Earth and its crust. He had justly pointed out that geodesy thus solved many very important geophysical problems in the years *when there was yet no geophysics*. According to his thoughts, the results of AG and gravimetric work, namely the declinations of the vertical and anomalies of gravity, were very valuable numerical data that can help to ascertain problems on the inner structure of the Earth and especially of its crust[4]. Regrettably, his considerations had not yet been duly developed by contemporary geodesists and are awaiting the efforts of future investigators.

[14] In his versatile activities, F. N. attached special importance to pedagogic work which enabled him to prepare engineers and scientists in the main directions of higher geodesy. Holding the chair of that discipline in MIIGAiK[5], he incessantly advised its members about the methods and contents of their work. He himself read lectures on spheroidal geodesy including subjects which now make up the contents of theoretical geodesy. In my view, his lectures had been attractive and interesting not only by their elegant style and for being easily understandable, but above all because he often reached far beyond the known and expounded his own views on the approaches to, and methods of solving the main geodetic problems.

Without exaggerating, we may say that most of our geodesists, who had begun their engineering or scientific work during the second quarter of this century, were Krasovsky's direct students and became the bearers of the ideas of his school possessing great magnetic force. To that school belonged the leading officers of our state geodetic service and many prominent scientists, who have contributed to the progress of geodesy and cartography. To a large extent it were his students who have been tirelessly putting into practice the scientific ideas of their outstanding teacher and mentor and constructed our precise AG network, created compact networks of triangulation and topographic maps of our country. At present, geodetic and cartographic work continues to develop on a new scientific and technical basis but the influence of Krasovsky's scientific ideas and views on their organization is still felt.

[15] Those, who had met F. N., imagined that he was strict and very demanding. Yes, he was very demanding, above all towards himself and therefore towards all those with whom he had to associate. From students, postgraduates and collaborators he had demanded persistent day-to-day work for acquiring new knowledge and accumulating experience for solving the constantly broadening scientific and technical problems. F. N. had extremely highly valued the ability to work persistently and to wish to contribute to the common aim.

In his life and work Krasovsky always kept to the strict demands of moral fibre and especially cared about the professional behaviour of geodesists. Understanding, that considerable engineering problems and important scientific goals were being solved/attained by issuing from geodetic data, he had demanded professional honesty and awareness. If a geodesist allowed himself sloppy work or showed lack of spirit, and such cases did happen, F. N. achieved his dismissal from geodetic work.

In spite of his apparent severity, Krasovsky was kind and responsive. Anyone finding himself in a difficult situation, could have obtained his good advice and a more substantial support. He was not only strict and demanding, but just in his interrelations with others and honestly served his cause. He openly made known his thoughts and views, even in those tricky circumstances when it could have harmed him.

F. N. had been an indisputable authority for, and enjoyed deep respect of geodesists and cartographers. His scientific, pedagogic and social activities were highly estimated and deservedly recognized. He was awarded the Orders of Lenin and of the Red Banner of Labour, and the International Astronomical Union named a lunar crater after him.

From long ago his name belongs to the history of our AG science, but he is still living in the thankful memories of his living students and associates as a tireless hard worker, outstanding scientist and exacting teacher. Owing to his very large scientific heritage, mainly reproduced in the four volumes of his *Selected Works* (1953 – 1956), he will be living for a long time in the minds of the future generations of our geodesists, as though urging them to solve new scientific and practical problems of geodetic and cartographic science.

Notes

1. According to Krasovsky [ii, § 1], in such contexts *organization* meant the choice of the network's pattern and the programme and methods of its construction.

2. The figure of the Earth hardly belongs to an adjacent branch of knowledge.

3. In 1931, the Cathedral of Christ the Saviour was dynamited to free the necessary space for that Palace. However, underground water prevented the building and an open swimming pool had appeared instead. The Cathedral was a masterpiece of architecture for which great many ordinary Russian citizens had donated money. A new Cathedral was erected in 2000 on the same place, for which again money was donated. It was Kirov, a leading Soviet politician, who proposed to erect the Palace which likely made Stalin jealous. Kirov was assassinated under strange circumstances.

4. Isotov did not mention isostasy (and neither did Bagratuni [xi]). Danilov [ix, § 16] referred to Krasovsky: he discovered that the isostatic hypotheses had *many fundamental defects* and was barely effective *when applied to correct astronomical stations for the deflection of the vertical.* See however Krasovsky himself [iv, § 1].

5. So was Krasovsky still the chairman of the same chair in MMI, as is stated in § 3? In § 4 the author added that MIIGAiK had been developing from the geodetic faculty of MMI, but after 1930, when MIIGAiK was established, did not MMI retain its chair of higher geodesy? Apparently, it did. Yakovlev (1979, p. 31), similarly to Izotov, mentioned that Krasovsky had retained that chair until his death.

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XI

G. V. Bagratuni

F. N. Krasovsky (observing the centenary of his birth)

Izvesiya Vysshykh Uchebnykh Zavedeniy. Geodesiya i Aerofotos'emka,
No. 4, 1978, pp. 150 – 155

[1] 26 September 1978 [new style] will be the centenary of Feodosy Nikolaevich Krasovsky's birth. An entire period in the formation and progress of our geodesy is inseparably connected with his name.

The size of our territory, its physical-geographical and climatic conditions, the great problems concerning national economy and defence, have been the decisive factors defining the formation and progress of the main geodetic and cartographic work. Lenin, in his famous Decree of 1919 creating VGU, most fully and thoroughly defined and appraised the pertinent problems. For solving them, it was necessary to work out, scientifically and practically, the arrangement of geodetic work for the entire country taking into account the features of its territory. This problem was exceptional in complexity and dimensions, and new manpower was needed. At that important moment our geodetic school was indeed born and from its very first days Krasovsky became its leader.

F. N. was an outstanding scientist, a talented teacher of higher geodetic education, and a prominent practitioner as well. His importance for geodesy and cartography is not restricted to our country. He belonged to those like Bessel, Struve et al, who had been developing the world geodetic science after Gauss' initial contribution and Helmert's death[1]. He was one of the organizers of periodic geodetic conferences of Baltic countries and participated there; he had been closely connected with eminent German, Finnish and American geodesists O. Eggert, Grossmann, L. Bonsdorff, J. F. Hayford, W. Bowie et al. He was interested in, and studied all the main branches of geodesy, [practical] astronomy and cartography and to each of them he had originally and fundamentally contributed.

Formerly, the main geodetic work in our country had been carried out on a high scientific and technical level and offered [allowed] essentially new solutions for the development of geodesy. Such was the work of Struve (1856 – 1861) and the contributions of *military topographers* K. I. Tenner, I. I. Hodsko (Chodzko), I. I. Stebnitskiy as well as many other works of KVT. However, no geodetic or cartographic activity had been going on on a national scale and neither did there exist a pertinent department.

[2] Only Lenin's decree formulated problems on a national scale, a topographic study of the entire territory for restoring and developing the country's productive forces. It became therefore necessary to work out a stochastically justified organization[2] of a national AG network. By the mid-1920s Krasovsky solved this great problem (1928). Without exaggerating, we may say that he had stochastically investigated and determined the regularities in the accumulation of observational errors and the pertinent influence of the form of the geodetic figures. F. N. had thus provided a classic; nothing similar is existing in the entire world geodetic literature[3].

Investigations in the same directions have been going on until now, and serious advances were made. We may note, for instance, Tatevian (1967). However, all work of this kind and the results obtained have their issue in that contribution more or less supplementing and developing its main propositions and conclusions. It was also highly appreciated abroad. Fifty years have passed since it was published, but we are still referring to it in studies and teaching.

In our days, the benefit of Krasovsky's pattern and programme are telling upon their admitting a joint adjustment of the great national AG network by computers. The main features of the present pattern of constructing such networks still largely coincide with Krasovsky's conclusions.

[3] F. N. had begun his scientific work by studying the figure of the Earth, and he concluded his life work by solving that problem. It was no mere chance that in 1953 a State Prize was awarded to him (posthumously) for the deduction of the parameters of the Earth ellipsoid now named after him. In 1946, the Council of Ministers adopted them as the Earth's constants for all geodetic, astronomic etc work.

The determination of the figure of the Earth is remaining an important scientific problem of higher geodesy. It had been carried out for a few thousands years, but its history had qualitatively changed in the work of Krasovsky[4]. Helmert (1880 – 1884) is known to have defined geodesy as a science of that figure. [Even] during Newton's time it became clear that the real figure of the Earth did not coincide with any geometric figure, which was the beginning of a new stage. Many new ideas had been formulated in the 19th century, and the 20th century had solved that problem in the first approximation by establishing that it was connected with the inner structure of the Earth. This is what Belousov (1964), an eminent geophysicist and a Corresponding Member of our Academy of Sciences, wrote:

Geodesy, which up to now had only been the science about the external figure of the Earth, is also becoming the science of its inner structure.

Precise data, obtained by geodetic observations of the movements of the Earth's crust and artificial satellites, provide very valuable information about the distribution of masses in the crust and the upper mantle of the Earth. F. N. was naturally unable to pronounce any opinion about the observations of artificial satellites, but our geodesists are applying them when pursuing the aims coinciding with those which Krasovsky had raised for *physical geodesy*. He it was who coined that term; during the latest decades, its scope has widened to such an extent that it is now considered an independent scientific discipline, *cosmic geodesy*.

That considerable scientific work in physical geodesy, which F. N. had initiated at our Academy of Sciences, is now being successfully continued by his students and associates, Corresponding Members of the Academy M. S. Molodensky, Yu. D. Bulange[5] and V. A. Magnitsky.

The results of satellite observations provide the flattening of the Earth ellipsoid, and such calculations had been accomplished here and in the USA. Their results were very close, and, furthermore, they corroborated the value of the flattening according to Krasovsky, 1/298.3. The error of that value is only expressed in the second decimals of its denominator.

Already in the 1940s F. N. thus solved one of the fundamental problems of higher geodesy, the determination of the parameters of the Earth ellipsoid, and so thoroughly that we may still be applying his results both in practical work and theoretical investigations.

[4] Krasovsky is highly meritorious not only because he had elaborated the patterns and programmes for constructing the state geodetic network and arranged precise geodetic observations in the field, but, in addition, in connection with mathematically treating the results of vast geodetic networks. Above all, he had devised a method for projecting the results of observations onto a reference surface instead of the indefinite *method of development* used by such prominent geodesists as Helmert and O. Eggert. Later, Molodensky worked out Krasovsky's idea and suggested a harmonious system of astronomical gravimetric levelling.

By the beginning of the 1930s, a considerable, for that time, AG network consisting of 10 polygons was constructed in the European part of the country, and a scientific and technical problem of working out methods for strictly treating and adjusting such networks had been encountered. F. N. had created such a method, and, after preliminary trials, published his work (1934)[6]. At the same time he had developed methods for calculating geodetic coordinates on an ellipsoidal surface and paid special attention to the problem of transferring them over large distances.

Again at that time, topical became the problem of selecting the most expedient geodetic projection and coordinate system for treating the results of geodetic observations on a plane. After profound and thorough theoretical investigations, Krasovsky had chosen a conformal projection and the Gauss – Krüger coordinates. He also devised supplementary aids such as tables, patterns of calculation and various nomograms for treating geodetic networks. He solved the ensuing problems so thoroughly and expediently that we are often applying his created arsenal almost without changing it.

[5] To this period also belong great scientific and technical problems of mapping our vast territory. Investigations were needed for establishing the scales of topographic surveying, plans and maps. F. N. had studied these problems and published a number of papers dealing with them. One of them (1938) as though summarized his work.

As an assistant chief of GUGK for scientific and technical problems, assistant director of TsNIIGAiK for science and a Board member of GUGK, Krasovsky had been systematically engaged in such studies. He also directly participated in organizing cartographic education in MIIGAiK. According to his proposal, a speciality, engineer-field cartographer was established there. These engineers, as he imagined, will be heading field cartographic work.

[6] At the beginning of the 1930s, construction, and especially hydro-technical projects and town development (for example, the construction of the Moscow underground), had begun on a large scale. New geodetic problems were encountered concerning observations and measurements and Krasovsky had participated in working out the scientific and technical basis for the necessary geodetic work on a large scale.

He was connected with such applications even in the beginning of his career. In the 1920s, he had been teaching geodesy [as a pluralist] at the construction faculty of Moscow Higher Technical School. His lectures, as is seen in an extant manuscript, were clearly directed towards applications, especially construction, and his views are still important. F. N. thought that it was above all necessary to substantiate scientifically the precision of the work, to elaborate the proper methods of measurements and select suitable instruments. He stressed that the preparation of engineers in geodetic institutes should take into account the requirements of the various branches of national economy, and construction in particular.

Incidentally, he had consulted geodesists constructing the Moscow underground; his students A. N. Baranov, A. Sh. Tatevian, M. I. Sinyagina, G. K. Zubakov, M. N. Sokolov, G. D. Onar and others, had been working there. He also consulted the erection of the Palace of the Soviets when its circular foundation began to be laid. Finally, Krasovsky had been supervising the degree work [where?] on orienting mines.

[7] F. N. is greatly meritorious for arranging the higher geodetic education. For more than 25 years he chaired higher geodesy at MIIGAiK; in 1919 – 1921, he was the elected rector of MMI and headed the methodical commission of its geodetic faculty. The rapid flourish of that faculty was connected with his activities.

Geodetic education in pre-revolutionary Russia is known to have remained in difficult circumstances, and F. N. often indicated this fact. This is what he (1934) wrote:

Until 1917, there existed the Land Surveying Institute with no separate faculties. Geodesy and practical astronomy had been treated in a single textbook together with civil law, land surveying laws and other legal subjects destined to preserve private landownership. This curious phenomenon of the Tsarist times, this school called the Land Surveying Institute, should have been utterly reorganized.

Modern higher geodetic education is retaining the features which had been established by the efforts of the professors, geodesists of that institute, under Krasovsky's guidance.

F. N. has great deserts for creating textbooks in higher geodesy. By the mid-1920s, geodetic work had begun to develop rapidly, and the preparation of engineers on a large scale was topical, geodesy was becoming a leading direction in technical education. In 1924 – 1925, widely drawing on his own considerable scientific and technical experience, Krasovsky compiles and publishes, in 1926, the first part of a fundamental *Treatise in Higher Geodesy*. Its second part, written in 1927 – 1929, had only appeared in 1932. A thoroughly revised and supplemented edition of that *Treatise* was published in 1938 – 1939 (its first part) and in 1942, – the second part for which Krasovsky was awarded a State Prize of the first degree. For its time, his work was an exceptional occurrence both in scientific level and completeness of covering its subject.

Krasovsky's main writings including that manual were collected in his *Selected Works*, vols 3 and 4 (1953 – 1956) edited by V. V. Danilov, M. D. Soloviev, A. A. Isotov, P. S. Zakatov, A. I. Durnev and S. G. Sudakov, but regrettably that revised edition was not reissued.

F. N. had been systematically preparing scientists for work in higher education. He selected his candidates by carefully studying their scientific possibilities and the results of their practical work. Most of those chosen had therefore become known scientists and educationists, such as professors Durnev, Zakatov, Isotov, Magnitsky, A. M. Virovets. And such prominent geodesists, astronomers and cartographers as Danilov, N. A. Urmaev, Soloviev, M. K. Tsvetkov, M. N. Sergeev, F. V. Drobyshev, Molodensky, M. S. Zverev had been his associates and collaborators.

[8] And F. N. had also collaborated with those eminent scientists of his time who had created their own schools and directions of work in geodesy, cartography, photogrammetry as well as astronomy and gravimetry; for instance with A. A. Mikhailov, N. G. Kell, A. S. Chebotarev, V. V. Kavraisky. Being highly cultured both generally and scientifically, Krasovsky had always been able to communicate with them during scientific discussions and conferences on the fundamental problems of higher geodesy and the above-mentioned disciplines. In conversations with his postgraduates F. N. often referred to the works and authority of his colleagues.

It is very important to note that all those scientists greatly respected Krasovsky and reckoned with his authority and writings. In 1939, being unanimously supported by the geodetic scientific community, he was elected Corresponding Member of our Academy of Sciences, and in 1943 the title of Honoured Scientist of the Russian Federation was conferred on him. He certainly was our outstanding scientist, and his name may be ranked among those of Gauss, Bessel[7], Struve, A. R. Clarke, Helmert, W. Jordan, and V. V. Vitkovsky.

Notes

1. Bessel had preceded Helmert.

2. For such contexts, I am borrowing that term from Krasovsky [ii, § 1].

3. This is a mistake, see for example Friedrich (1937) and a later contribution written on a high mathematical level, Grafarend & Harland (1973).

4. This statement is not borne out in the sequel.

5. Certainly derived from the French *Boulanger*.

6. I can only refer to Krasovsky (1931a; 1931b).

7. I do not agree with the author: Gauss and Bessel should not have been included.

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XII

L. P. Pellinen

F. N. Krasovsky and the development of astronomical gravimetric levelling in the USSR

Izvestia Vysshikh Uchebnykh Zavedeniy. Geodezia i Aerofotos'emka, No. 2, 1979, pp. 71 – 77

[1] A remarkable feature of our AG network, distinguishing it from all the other extensive continental geodetic constructions, is the wide development of astronomical gravimetric levelling (AGL). Their lines make up a firm basis for a precise determination of quasi-geoidal heights above the Krasovsky ellipsoid over all the territory covered by the AG network. In 1934 Krasovsky had formulated the idea about a joint application of AG and gravimetric data for calculating the geoidal heights[1], and then, vigorously, as was characteristic of him, assisted the development of the AGL whereas M. S. Molodensky worked out the theory of the AGL method and brought it to practical applicability. I am therefore connecting the development of the AGL with both these scientists.

When considering the mathematical treatment of our AG network, Krasovsky showed that it can only be irreproachably accomplished when the heights of the baselines above the adopted reference ellipsoid were determined with an error not exceeding 2 – 3 *m*. Following Helmert's well-known proposals, it is possible to treat an AG network without knowing the geoidal heights; his method of adjusting AG networks envisaged a simultaneous choice of the best suiting reference ellipsoid by issuing from the measurements reduced to the geoidal surface and then, without any changes, *developed* on the surface of the reference ellipsoid. That approach is possible if, after making the mentioned choice, the geoidal deviations ς from it will not exceed 3 *m*.

However, Krasovsky clearly realized that within our vast territory those deviations will be much larger (according to his opinion, not less than 20 *m* in the mean). He resolutely concluded that a strict mathematical approach to treating triangulations requires its projection on the surface of the reference ellipsoid, and that, even if very successfully choosing it, the determination of the ς's was also needed. Krasovsky considered different methods of such determination in addition bearing in mind the study of the Earth figure by applying those heights, ς's. Also by applying them, he worked out a new type of the equations of arc measurements and justly considered it very promising for determining the size and form of the Earth ellipsoid. Bearing in mind those two aims, he (1942) proposed the following pattern of determining the geoidal heights:

1. A more precise transfer of the geoidal heights along chains of triangulation of the I order 600 – 800 *km* apart; he considered those chains as *arc measurements of the new type*.
2. A less precise transfer of those heights along all the other chains of triangulation of the I order for determining them at least at the places of baselines.

Until the mid-1930s, geodesists possessed a restricted experience of precisely transferring geoidal heights by astronomical levelling in Germany, Switzerland and the countries on the Indostan peninsula. After studying that experience, Krasovsky concluded that even in a flat country astronomical stations along the lines of precise astronomical levelling should not be more than 8 – 15 *km* apart, and about 3 – 4 *km* in mountains. Therefore (1942, p. 560),

In our conditions a wide application of astronomical levelling is out of the question. And, in addition, in our vast territory the appropriate expenses will be great, but work will go on very slowly.

[2] Nowadays, we are possible to estimate the expected precision of astronomical levelling by trial calculations along the lines of high-precision AGL if considering them faultless. By applying that approach, TsNIIGAiK had derived an empirical formula for the mean square error m_ς (in metres) of astronomical levelling for distance L in our flat and hilly regions and astronomical stations S kilometres apart:

$$m_\varsigma = 0.00082S\sqrt{L}. \qquad (1)$$

For ensuring the transfer of geoidal heights with precision of 1.25 *m*/2,500 *km* (which corresponds to Krasovsky's proposals) the astronomical stations should be about 30 *km* apart. This means making astronomical observations at each triangulation station. Actually, gravity anomalies occur even in flat regions (for example, in the well-known zone near Moscow) in which the errors of transferring geoidal heights are larger, so that for obtaining a secured precision the distance S should be shorter than 30 *km*. Thus, although Krasovsky's estimates proved somewhat strict, on the whole his conclusion about the inappropriateness for our country of precise astronomical levelling was quite justified.

Instead, he proposed AGL. In his report (1935a), he indicated the need 1) to choose a reference ellipsoid for treating our triangulation sufficiently close to the best suiting ellipsoid over that territory and 2) to deduce the geoidal heights h above that ellipsoid[2]:

Without discussing the method of determining the h's, I only indicate that for that aim we ought, on the one hand, to determine astronomical latitudes and longitudes at a considerable number of stations of the I order besides the Laplace stations 80 – 100 km apart, and, on the other hand, to thicken in appropriate places the gravity determinations of the general gravimetric survey.

Understandably, that additional astronomical and gravimetric work will be of considerable volume. I think, however, that a proper application of gravimetry for geodetic aims will essentially simplify the determination of the h's necessary both for treating triangulation and studying the geoidal figure.

That was Krasovsky's first publication in which he put forth in a general way his considerations about organizing joint AG and gravimetric work for determining the geoidal heights above the reference ellipsoid. At the same session of the Baltic Geodetic Commission I. A. Kazanskiy presented the simplest interpretation of the method of AGL. His report was devoted to the successful practical experience of deriving the declinations of the vertical by issuing from the local gravimetric survey near Moscow:

The gravimetric method of deriving the declinations of the vertical can be applied at the stations of astronomical levelling and between them by determining the local difference between the gravimetric and the AG systems of those declinations between the stations of astronomical levelling as though increasing their number …

When applying that approach to the method of AGL, gravimetric declinations of the vertical should be calculated at astronomical stations and at a considerable number of intermediate stations along the line of astronomical levelling. Therefore, for all the simplicity of description [?], see for example Krasovsky (1942; 1941), that modification of the AGL had not been applied in our country. Only in the 1960s it was scantily made use of abroad, for example, in the USA.

[3] Krasovsky himself (1935b) expounded in a general form another interpretation of the method of AGL which proved more rational:

This is the main idea. 1) A gravimetric survey executed within a sufficiently wide strip, whose long axis is the studied geoidal profile, provides the material for ascertaining it with respect to some normal spheroid of unknown sizes or position. 2) Astronomical determinations at the triangulation stations of the I order along the same profile allow to reduce it by parts to the surface of the reference ellipsoid by coordinating the "absolute" deflections of the vertical, obtained by the gravimetrically determined geoidal profile, between the "relative" deflections determined at the triangulation stations by the AG method.

That modification of the method of AGL applies gravimetric deflections of the vertical along with gravimetric geoidal heights, but as a rule it is only necessary to know both at the astronomical stations. If, for regions with essential anomalies, the geoidal profile between those stations ought to be specified, gravimetric geoidal heights should be determined at intermediate stations. This, however, is easier than obtaining gravimetric deflections of the vertical, and calculations will be necessary for a smaller number of stations as compared with the previously discussed version of the method of AGL.

Krasovsky's considerations were however not yet mathematically shaped. Moreover, if a local gravimetric survey is applied, they are inexact: the calculated gravimetric geoidal heights and deflections of the vertical could not be regarded as absolute magnitudes concerning an unknown spheroid.

[4] The well-known publications of M. S. Molodensky (1937 and later) contain a rigorous theory of the AGL brought to practicality. He justified the necessary radius of a circle to be covered by gravimetric surveying, formulated the requirements which it should satisfy, investigated the main errors of the AGL, detected subtle effects of applying geodetic coordinates derived by the method of development and offered formulas for the appropriate corrections.

Krasovsky highly appraised his work and stressed that it was he who had worked out the idea of the AGL. I remind readers of Molodensky's main formula of AGL between some astronomical stations A and B, S_{AB} apart:

$$\varsigma_B - \varsigma_A = \frac{S_{AB}}{2\rho''}(\theta'_A + \theta'_B) + \frac{S_{AB}}{2\rho''}[\overline{\theta}_A + \overline{\theta}_B + \frac{\overline{\varsigma}_B - \overline{\varsigma}_A}{S_{AB}} 2\rho'']. \qquad (2)$$

θ' are the components of the AG deflections of the vertical along the levelling line after applying the corrections for the curvature of the line of force of the normal gravity field. The dashes above indicate the gravimetric heights of the quasi-geoid and [or] the components of the deflection of the vertical calculated by issuing from the gravimetric survey of some region enveloping A and B.

The first term is the result of the astronomical levelling if only applying the deflection of vertical at stations A and B and assuming that between them those deflections are changing linearly. The second term is the allowance for a non-linear change determined by gravimetric data. Molodensky proposed a convenient bifocal plate for directly calculating the sum in the square brackets without separately determining the magnitudes $\overline{\theta}$ and $\overline{\varsigma}$.

Already in 1935 – 1936, owing to Krasovsky's vigorous assistance, field work and calculations needed for the AGL had been rapidly developing. The experience gained and Molodensky's theoretical investigations corroborated Krasovsky's opinion about the necessary volume of additional gravimetric work. He (1942) wrote:

With a systematic and persistent general gravimetric surveying little and even insignificant additional AGL will be required, and especially so if the thickening of the gravimetric network is achieved by elastic pendulums of the Lejay type and static gravimeters.

He also mentioned the pioneer creation of national elastic pendulums and static gravimeters by TsNIIGAiK, and I note that Molodensky's prototype of the static gravimeter for AGL served as the basis for developing the first national equipment for gravimetric prospecting.

Krasovsky encouraged the development of precise AGL in conjunction with the country's general gravimetric survey and foresaw the great importance of that work for general investigations of the Earth figure. He (1942) wrote that when the gravimetric knowledge of the territory on all sides from any point on the arc measurement coinciding with a line of the AGL will extend for 2,500 – 3,000 *km*, we will arrive at a new, more perfect type of arc measurements. And he added: *The USSR will perhaps approach that type of arcs more rapidly than all the other countries.*

[5] After the war, in 1947, accepting Molodensky's proposal, regular execution of lines of precise AGL was started. Around astronomical stations 40 – 50 *km* apart the gravimetric survey had been thickened in the vicinity of radius 50 *km*. Then also compact gravimetric surveying for geological and geophysical aims had been very widely carried out. It was thus ascertained that the errors of astronomical determinations at triangulation stations rather than the lack of gravimetric data were restricting the precision of the AGL. Calculations showed that in most cases sufficient precision of the AGL (3 *cm/km* of traverse) is attained by applying astronomical stations 70 – 100 *km* apart, as is usual for all the triangulation of the I order.

This fact allowed to apply widely only calculations for carrying out the AGL by issuing from astronomical latitudes and longitudes determined by the general pattern of the development of AG networks and the results of gravimetric prospecting. From the 1960s, most lines of the AGL had been thus executed.

Up to 1971 a very large number of lines of high-precision AGL had been accomplished and the general adjustment of the levelling polygons finished. This allowed to solve Krasovsky's problem of ensuring the transfer of the geoidal heights within the country with precision of 3 *m*. Only in its isolated eastern regions the expected errors of those heights amounted to 6 *m* owing to the indirect influence of the errors of plane coordinates, essential for large distances, on them.

The perimeters of the polygons were 2 – 4 thousand kilometres, also in accord with Krasovsky's proposal. In most cases, the radius of the region of integrating exceeding a thousand kilometres is possible for calculating gravimetric deflections of the vertical and geoidal heights. The influence of more remote zones is now possible to determine with high precision by issuing from modern satellite models of the Earth gravitational field. All the data for compiling the equations of arc measurements of the most perfect type, about which Krasovsky thought, are now available.

As mentioned above, for determining the heights ς of all the baselines of the triangulation above the reference ellipsoid , in addition to the main lines of the AGL along some chains of the triangulation of the I order, secondary work concerning all other chains of the same order is also needed.

In 1939, B. V. Dubovsky had carried out the first wide calculations of the ς's still with respect to the Bessel ellipsoid. He applied the AGL and astronomical levelling if gravimetric data were insufficient. So as to compile the data about geoidal heights for the general adjustment of our AG network, the first map of those heights, this time above the Krasovsky reference ellipsoid, was completed in 1943. Molodensky's essential corrections allowing for the method of development were taken into account.

[6] For Krasovsky, the need for the secondary constructions was evident. He (1942) described his considerations, showing an expedient economic approach to their organization. Bearing in mind the first results of the AGL, he indicated that that method will provide sufficient precision of those constructions if only the general gravimetric survey of the country without additional thickening work and the usual astronomical determinations along the chains of the I order 70 – 100 *km* apart are applied. At the same time, he did not reject the possibility of applying astronomical levelling in cases of astronomical stations 40 – 50 *km* apart, especially for Siberian regions. And for calculations he proposed to apply only the first term of the formula (2). Note that, understanding astronomical levelling only to denote precise work, he simply called its ensuing modification *derivation of geoidal heights by astronomical determinations at triangulation stations*.

In the USSR, the general gravimetric survey usually forestalled the calculations of the transfer of geoidal heights, and our astronomical levelling did not develop widely. However, it can prove beneficial for some developing nations, especially if taking into account the influence of topographic masses on the non-linearity of the changes of the deflection of the vertical. And, when issuing from reckoning according to formula (1), it is possible, as a rule, to make astronomical determinations more than 50 *km* apart.

Our map of geoidal heights has been regularly specified later also. Computers opened up new possibilities which made improvements of the technique of AGL possible. In particular, according to O. M. Ostach' proposal, it proved expedient to represent its main formula as

$$\varsigma_B - \varsigma_A = \Delta\bar{\varsigma}_B - \Delta\bar{\varsigma}_A - \frac{S_{AB}}{2\rho''}(\theta'_A - \overline{\theta'_A} + \theta'_B - \overline{\theta'_B}). \qquad (3)$$

The gravimetric magnitudes $\bar{\theta}$ and $\Delta\bar{\varsigma}$ are obtained by integrating within a *sliding* region, a circular zone of constant radius circumscribed around each applied astronomical station. The second magnitude is analogous to the gravimetric height $\bar{\varsigma}$ and, for a sliding region of integration, corresponds to $\bar{\theta}$. Magnitudes θ and $\Delta\bar{\varsigma}$ are more conveniently defined by computer than $\bar{\theta}$ and $\bar{\varsigma}$ in (2) which correspond to a constant region of integration in points A and B.

In 1971, the Ostach method was applied for compiling the map of the geoid (Pellinen a. o. 1972). And in most cases the execution of lines of the AGL of lower precision was abandoned. As a rule, the AG geoidal heights were indirectly interpolated by issuing from the gravimetric heights calculated in detail by the computer. That somewhat resembles Krasovsky's considerations (1935) mentioned above.

[7] For developing the AGL, after 1971, just as when carrying out planned AG constructions, we began to replace polygons by compact geodetic networks of the II order with numerous astronomical stations in these networks and in the polygons of the I order as well. Also applied was the compact gravimetric survey executed for geological prospecting which actually became a new high-precise general gravimetric survey of our country.

At present, gravimetric deflections of the vertical and the quasi-geoidal heights $\Delta\varsigma$ are calculated at more than two thousand astronomical stations. The high precision of those calculations allows to detect very surely both random and systematic errors of the astronomical determinations. In principle, it is possible to carry out the AGL and, at the same time, to establish corrections to astronomical coordinates.

[8] In concluding, I note that the experience of our AGL corroborates Krasovsky's insight. He actively supported Molodensky's investigations, presented an expedient and mainly realized pattern of developing AGL, indicated ways of applying their results for general investigations of the Earth figure. He vigorously backed the first trial execution of the lines of AGL. Krasovsky did not stop at palliative, least laborious patterns of that levelling, and attempted to create polygons of high precision although obviously not needed in the next future for the general adjustment of the national AG networks. We see now, that his foresight and persistence were completely corroborated.

Notes

1. Here and below I am applying that term instead of a more rigorous term *quasi-geoid*. Understandably, Krasovsky did not apply it in his contributions here quoted. L. P.

The surface of a quasi-geoid is determined by the values of the potential gravity of the Earth's surface. In high mountains, it deviated from the geoid by 2 – 3 *m* [vol. 6, *Geoid*]. O. S.

2. Here and in the next quotation I am retaining Krasovsky's notation *h* for the geoidal height instead of the now adopted ς. L. P.

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Krasovsky. Bibliography

In compiling this Bibliography, I have extensively used the Bibliography included at the end of Bagratuni (1959) which he had published in spite of its many defects. The additions such as [1, pp. …] or [2, pp. …] refer to the first two volumes of Krasovsky's *Selected Works* (1953 – 1956). A few items listed below are not mentioned in the main texts. English titles denote *Russian* contributions.

A special point concerns Krasovsky (1938 – 1939, 1942). As a student, I studied its first part (1938 – 1939) whose authors were Krasovsky and V. V. Danilov, but at least from 1978 Krasovsky somehow became its sole author and Danilov himself [ix, § 13] accordingly mentioned that source. However, both Krasovsky and Danilov are mentioned as authors in [vol. 6, article *Geodetic instruments*] and in Izotov's article *Geodesy* also there.

The *Verhandlungen* of the Baltic Geodetic Commission, where Krasovsky published many of his reports, also had a French title which I do not reproduce below: *C. r. de la Commission géodésique Baltique*. There, Krasovsky mistakenly wrote *USSR* (at first, even *S. S. S. R.*) instead of the correct German *UdSSR*.

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The last two volumes are reprints of the *Treatise* (1938 – 1939; 1942).

Part 2. Oscar Sheynin: Unpublished Papers

I

Statistics. Its essence

1. Introduction

1. The Willcox collection and the scope of this paper. The word *statistics* was introduced into English not later than in 1770 by W. Hooper in a translation of a German book (Bielfeld 1770, vol. 3, p. 269): *Statistics teaches us what is the political arrangement of all the modern states*. Whichever of its (later) definitions is accepted, one of its main goals is analysis of numerical data rather than a complete enumeration of the objects studied, cf. Kendall (1960/1970, p. 46) who studied developments in Italy in the 15th century:

We are … still short of a statistical approach. Counting was by complete enumeration and still tended to be a record of a situation rather than a basis for estimation or prediction …

Except for some obvious cases, I use the word *statistics* in the sense of mathematical (or theoretical, see § 8.3) statistics. The first who had attached modern meaning to that word was likely John Sinclair in 1798 (Pearson 1978, p. 2). A much wider issue is revealed here. In Germany, in the mid-17th century, the Staatswissenschaft (or University statistics, or statecraft) was born, and a century later G. Achenwall established the Göttingen school. It studied most various aspects of a given state, barely using numbers. Achenwall advised state measures fostering multiplication of the population and recommended censuses without which a probable estimate of the population could still be obtained. He (1752/1756, Intro.) also left an indirect definition of statistics:

Statistics is not a subject that can be understood at once by an empty pate. It belongs to a well digested philosophy, demands a thorough knowledge of European state and natural history taken together with a multitude of concepts and principles, and an ability to comprehend fairly well very different articles of the constitutions of present-day kingdoms.

His follower Schlözer (1804, p. 86) put into circulation a pithy saying: *History is statistics flowing, and statistics is history standing still*. Unlike he himself, later authors adapted it as a definition of statistics, a discipline which therefore left aside studies of causes and effects. And here is Obodovsky's paraphrase (1839, p. 48): *History is to statistics as poetry is to painting*. Poetry, however, is not a science.

Another line of thought, political arithmetic, largely opposing Staatswissenschaft, had also appeared in the mid-17th century (Petty, Graunt). It used numbers rather than words, applied elementary stochastic considerations for describing a given nation and studied causes and effects. Graunt (1662/1939, pp. 78 – 79) argued that

The Foundation, or Elements of this … policy [of the *Art of Governing*] *is to understand the Land, and the hands of the territory to be governed. … It was necessary to know how many People there be of each Sex, State, Age, Religion, Trade, Rank or Degree etc, by the knowledge whereof Trade and Government may be made more certain, and Regular*.

It is instructive that Graunt (p. 79) doubted whether his findings were necessary for anyone excepting *the Sovereign and his chief Ministers*.

Humboldt was a resolute proponent of numbers. In 1838 he (Knies 1850, p. 145) declared that *Sind die Zahlen immer das Entscheidende; sie sind die letzten unerbittlichen Richter*.

During its long history, the scope of the Staatswissenschaft narrowed with the birth (or rebirth) of new disciplines or sciences; the study of climate is an obvious example. The battle between the two streams of thought continued, however, for many decades. Staatswissenschaft still exists, at least in Germany, although in another sense. It is still taught in the universities, certainly includes numerical data and studies causes and effects. It thus is partly the application of the statistical method to various disciplines and a given state. The previous preference of describing its subject by words rather than numbers is easily explained by the restricted possibilities of mathematics in general (which was gaining new ground with every generation).

My aim is to define statistics considering also its various aspects and accounting for its history. I am therefore making use of the list of 115 direct or implicit definitions of that term, published from 1749 onward and collected by Willcox (1935) (who had borrowed 68 of them from Böckh (see below), but I also added other important definitions made by eminent scholars either before 1935 or after that.

Here is what Willcox indicates:

Nearly a generation later [than 1884], in an appendix to his eight-page "Outline …" [of *Grundriss der Vorlesungen über Theorie der Statistik*, as he adds on his next page], *Böckh printed 68 definitions. To that ephemeral publication, which he kindly sent me when it appeared, I have added nearly as many, largely from English and American writers*.

I had not found that *Grundriss*, but discovered (or knew before) several statistical contributions published by a Richard Böckh since 1875. In many cases Böckh provided incomplete bibliographic information and/or did not indicate page numbers. In some instances Willcox, as he stated on p. 388, had inserted them himself. There also exists a similar collection compiled by Nikitina et al (1972). Having seen it many years ago, I remember that the definitions were only provided in Russian translations and that the biographic information was also incomplete.

2. Experimental design and exploratory data analysis.

Experimental design (Cochran 1978) originated in the 1920s with Fisher's study on the planning of agricultural experiments. It investigates the effects of various changes (treatments) imposed on the experimental unit concerned, though its methods are foreign to the planning of geodetic (say) observations. I attribute one of the branches of error theory to that design perhaps even widening somewhat the usual scope of this new discipline. Finney 1960/1970) held that it did not entirely belong there, and I agree with that opinion.

Exploratory data analysis (Andrews 1978) aims at making data more comprehensible by discovering their possible structures or anomalies. Its role had greatly increased with the increase, in many areas of research, in the information available. In particular, systematic influences clearly determine an important feature (or structure) of observational errors (§ 9.2).

Introduction of contour lines into geophysics (Halley in 1701; see Chapman 1941, p. 5 or Sheynin 1984b, pp. 68 – 69) provides a good example. Halley published a North Atlantic chart of the lines of equal declination for epoch 1700, and later Humboldt (1817) plotted isotherms on a world map and referred to Halley (Humboldt 1858, p. 59).

The goal of that analysis is largely achieved by informal methods, as in the examples above, and Tukey (1962/1986, p. 397) aptly concluded that

Data analysis, and the parts of statistics which adhere to it must … take on the characteristics of a science rather than those of mathematics.

Its similarity, in this respect, with experimental design is obvious. See also Tukey (1977). An important analysis made in the mid-19th century was Snow's discovery (1855/1965, pp. 58 – 59 and 74 – 86) that the spread of cholera was occasioned by the impure drinking water. It occurred that mortality from cholera per 10,000 houses in a certain metropolitan district amounted to 315 deaths for those who drank water containing *the sewage of London*, and only to 37 deaths for those whose water supply was *quite free* [certainly not in the modern sense] *from such impurity*.

At the beginning of the 19th century discussion of causes was restricted in France. Delambre (1819, p. LXVII), see also Sheynin (1986, pp. 282 – 283) maintained that statistics *exclut presque toujours les discussions et les conjectures*, and Fourier (*Recherches* 1821, pp. iv – v) stated that

L'esprit de dissertation et de conjectures est, en général, opposé aux véritables progrès de la statistique, qui est surtout une science d'observation.

In addition, Delambre (p. LXX) listed the main objects of statistics naming geodetic and meteorological observations, the study of diseases, and even *l'exposition des procédés des arts* and *les descriptions minéralogiques*. At the same time, in spite of Laplace's classical research in demography, the first ever sample estimation of the population (of France) with an estimate of its error, he did not mention population.

It seems remarkable that Laplace's astronomical achievements did not influence the attitude of Delambre or Fourier. Thus, observations appeared to indicate a particular lunar inequality (Laplace 1812/1886, p. 361) *avec une probabilité si forte*, that he felt himself compelled to discover its cause. Laplace was concerned with treating observations rather than with studying statistical returns, but the difference does not seem to be here essential.

Numerical description of phenomena without studying causes and effects had also been (unsuccessfully) introduced by the London (later, the Royal) Statistical Society established in 1834. It declared that all conclusions *shall admit of mathematical demonstrations* (which was too difficult to achieve) and stipulated that statistics did not discuss causes and effects (which proved impossible to enforce), see Anonymous (1839).

The practice of such restrictions was not as fruitless as it might seem. After all, Fourier's *Recherches* was a fundamental reference book, and many scholars compiled collections of statistical data pertaining to various branches of natural sciences. And in the 1820s – 1830s the so-called numerical method came into vogue among French physicians (Armitage 1983). Its partisans denied stochastic considerations believing that sufficient data was all they needed.

Neither did all these valuable but restricted efforts hinder the development of the statistical method in the broader sense[1]. By mid-19th century and even earlier the climate of opinion in France had changed owing to such statisticians as Bienaymé (Heyde & Seneta 1977, pp. 21 – 28) and to the emerging medical statistics. The most important pertinent contribution was Gavarret (1840), who took to medicine after graduating from the Ecole Polytechnique. The author attempted to substantiate the application of probability to this science and sincerely acknowledged Poisson's (indirect) influence.

Poisson himself (1837, pp. 213 – 227 and 288 – 297), see also Sheynin (1978, § 5.2), derived formulas for stochastically checking the significance of empirical discrepancies (e. g., of a difference between two probabilities) and in one case he, together with three other scholars, expressed his general views on statistics (Double et al 1835, p. 174):

La statistique mise en pratique, qui est toujours en définitive le mécanisme fonctionnant du calcul des probabilités, appelle nécessairement des masses infinies, un nombre illimité de faits non-seulement en vue d'approcher le plus près possible de la vérité, mais aussi affin d'arriver à faire disparâitre, à éliminer, autant qu'il est possible … les nombreuses sources d'erreurs … [2]

At about the same time Poisson, together with other scientists (Libri-Carrucci et al 1834, p. 535), favourably reported on the benefits accruing from the use of *haute statistique* and on the need to be aided by the calculus of probability.

Now, both Poisson and Gavarret invariably thought and demanded a large number of observations[3]. The German physician Liebermeister (ca. 1877, pp. 935 – 940) resolutely opposed that condition since in therapeutics it could not be met. He argued for a transition from almost complete certainty to reasonable probability which is now the usual aim of medical (and not only medical) statistics.

3. Probability

3.1. Mass observations. From Graunt onward statisticians apparently realized that their findings should be based on a large number of observations, but they did not all at once mention this fact in defining statistics. The Willcox collection includes a pertinent definition (Rümelin 1863 – 1864/1875, p. 222) whereas I can refer to Cournot's earlier but indirect statement (1843, § 103). Here are the two passages arranged chronologically.

Statistics is a science *qui a pour objet de recueillir et de coordonner des faits nombreux dans chaque espèce, de manière à obtenir des rapports numériques sensiblement indépendants des anomalies du hazard, et qui dénotent l'existence des causes régulières dont l'action s'est combinée avec celle des causes fortuites*[4].

Die Statistik ermittelt die Merkmale menschlicher Gemeinschaften auf Grundlage methodischer Massenbeobachtung und Zählung ihrer gleichartigen Erscheinungen.

I adduce here Pearson's definition of statistics *as a science* (1978, p. 3): This, he argued, is *The application of mathematical theory to the interpretation of mass observations*.

3.2. Probability. That statistics stands in need of probability was evident from the beginning of the 18th century. De Moivre, Daniel Bernoulli, Laplace and Poisson justified their statistical inquiries by stochastic methods (and developed probability theory), also see § 2.2. On the other hand, for all his declarations in favour of the theory of probability, Quetelet did not really apply probability in his work (and did not mention it in his definition of statistics, see below). He never mentioned the Poisson form of the law of large numbers, which could have justified his studies in moral statistics, or the central limit theorem, a later term and Laplace's tool *par excellence*. While compiling and systematizing meteorological data and applying the statistical method in anthropometry, Quetelet (1846, p. 275) reasonably contended that meteorology is alien to statistics. He did not, however, foresee that that method will penetrate ever new branches of science.

His vague definition of statistics (1848, p. XI) was restricted to sociology and did not mention probability:

La statistique est une science nouvelle qui a pour objet d'étudier l'homme dans ses divers degrés d'agrégation.

By coincidence or otherwise, influential German statisticians began challenging Quetelet's ideas just after his death in 1874. For example, they anathematized the very notion of mean inclination to crime and denied his celebrated statement on the constancy of this phenomenon (Quetelet 1836, t. 1, p. 10). Incidentally, Quetelet's declaration indirectly presupposed invariable social conditions and was not therefore refuted.

Deciding that the evil was rooted in probability theory, the new generation of statisticians attempted to divorce statistics from it. Bortkiewicz (1904) had to oppose this approach and to repeat his arguments time and time again.

Sampling provides another connection of statistics with probability since (one cause) the sample data are as a rule corrupted by errors, and random errors should be treated by stochastic methods. Simpson (1740, Problem 6) had solved a problem highly relevant to the then yet unknown sample control of quality of production. Laplace was the first to estimate the error of sampling but sample surveys did not become current until the turn of the 19th century (You Poh Seng 1951). Without referring to this innovation, Kapteyn (1906) initiated an international plan for a sample survey of the stellar universe.

Until 1835 (§ 2.2), direct definitions of statistics apparently did not mention its connection with probability. Much later Edgeworth (1885, pp. 181 – 182/1996, vol. 2, p. 25) alleged that statistics was *the science of means in general (including physical observations)* and Merz (1903/1912, p. 567) echoed: *statistics is a science of large numbers and of averages.* Indeed, Edgeworth likely though about large numbers as well.

To sum up: statistics, either theoretical or applied (§ 8.3), requires all the help it can get from probability.

4. Sociology and Natural Science. According to Quetelet's definition of statistics (§ 3.2), this discipline studies sociological problems (certainly including those pertaining to demography). However, during his lifetime statistics had been also applied in natural science (in particular, by him himself). I therefore devote my § 4.2 to natural science whereas my § 4.1 describes the relevant changes which took place in sociology since the advent of political arithmetic.

4.1. Sociology. Graunt (§ 1) argued that it was necessary to know the sex, the state, … of the population. He concluded his statement by greatly restricting the number of those to whom it was necessary. The situation had since essentially changed. Much was achieved in medical and population statistics during the period from Graunt to Quetelet although some findings were not directly needed, for example the ratio of male and female births. True, the related stochastic studies occasioned the discovery of the De Moivre – Laplace theorem (the first version of the central limit theorem).

The scope of statistics in sociology at large has also widened. In the 19th century, criminal statistics had gradually become indispensable. I mentioned Quetelet in this connection (§ 3.2), but he had also discussed other topics. He (1869, t. 1, p. 422; t. 2, p. 173) noticed that cuts in postal charges made both in England and Belgium had led to rises in the numbers of letters exchanged and in the profits received. Obviously, he drew on statistical data. He (t. 1, p. 419) also recommended to study (no doubt, statistically) the changes brought about in society by the construction of telegraph lines and railroads.

In the 20^{th} century, several leading nations had implemented at least some features of the Welfare State and new and important statistical problems have therefore arisen. Bartholomew (1995, p. 9), who discussed the situation, listed some new phenomena, viz., inequality, poverty and mobility (geographical and professional). Accordingly, he mentioned two new aims of statistics, the study of dynamic systems (he did not elaborate) and decision making based in part on personal judgement and non-numerical information *of uncertain quality*. Statisticians, as Bartholomew reasonably believed, can help even in this important field of governmental activity.

4.2. Natural science. During the 19^{th} century, a number of new disciplines pertaining to natural science but inseparably linked with statistics have been originated and developed: epidemiology, public hygiene (largely the forerunner of ecology), geography of plants, zoogeography, anthropometry, biometry, climatology, stellar statistics and the kinetic theory of gases. In addition, many fundamental problems in natural science were studied statistically, as for example the influence of solar activity on terrestrial phenomena. Statistics had long ago became indispensable for science, see my five papers published in 1980 – 1985 in the *Archive for History of Exact Sciences* and covering biology, medicine, astronomy, meteorology and physics. Their résumé is my article (1990b).

Chuprov (1914/1981) was possibly the first to discuss the penetration of statistics into natural science in some detail, and he stated (pp. 165 – 166) that *Only in the middle of the 19^{th} century did their victorious march over the whole field of contemporary science begin* . *Their* meant statistical forms of knowledge, and *only* was connected with Chuprov's reasoning on the difficulties experienced by scholars when introducing statistics into science.

For my part, I distinguish three stages in the application of statistics to natural science. At first, statements were based on general impression which conformed to the qualitative essence of ancient science. During the second stage which began with Graunt (and Tycho Brahe in astronomy) statistical data were available. The third stage dates back to the end of the 19^{th} century when inferences had been first checked by stochastic tests.

4.3. Statistics and statistical method. The former term has been first and foremost restricted to describing the socio-economic situation of a given society, whereas the latter was understood as studying the application of statistics to some branch of natural science. Chuprov (1896, pp. 86 and 88), also see Sheynin (1990a/2011, pp. 117 – 118), was perhaps the first to distinguish directly those expressions. Statistics, as he maintained, studies mass phenomena, becomes a branch of logic and *retains its domination only over the theory of probability*, whereas the statistical method is

The totality of the (inductive) analytical investigations of stochastic causal ties. … [It] *studies mass phenomena admitting a more or less accurate numerical characteristic.*

These statements are contained in Chuprov's diploma work, and I am not sure that probability theory was then taught at the University.

Later Chuprov (1909/1959, p. 130), see also Sheynin (1990a/2011, pp. 123 – 124), stated once more that the statistical method differs from statistics and connected these notions with nomographic and idiographic studies of reality. These latter concepts are not applied in modern writings. They were introduced by the philosophers Wildebrand and Rickert and denoted sciences of regularities and of separate facts respectively. More precisely, Chuprov replaced their *ontological* by *idiographic*. I reject them since separate facts do not constitute any science. Chuprov himself (p. 75) called their confrontation *definitely obsolete*!

The correspondence of Markov and Chuprov began in 1910 (Ondar 1977) and oriented the latter towards probability and mathematics. A few years later Chuprov (1914/1981) possibly dropped the term *statistical method* altogether using instead two neutral expressions, *statistical point of view, of knowledge*.

5. Some new definitions

5.1. The main definition. It goes back to the beginning of the 19th century (Butte 1808, p. XI): *Statistik ist die Wissenschaft der Kunst statistische Data zu erkennen und zu würdigen, solche zu sammeln und zu ordnen*. Then, the aim of this science is (Alph. DeCandolle 1833, p. 334)

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

Chaddock (1925, p. 26) essentially repeated this statement: statistics is *the body of methods and principles which governs the collection, analysis, comparison, presentation and interpretation of numerical data*.

Pearson (§ 3.1) offered a like definition connecting statistics with mass observations. Published in 1978, it was formulated about half a century earlier.

Finally, Kolmogorov & Prokhorov (1990, p. 138) had more to say. First, they defined mathematical statistics calling it

The branch of mathematics devoted to the study of mathematical methods for the organization[5], *processing and utilization of statistical data for scientific and practical conclusions. … By statistical data is meant information on a number* [on the number] *of objects in some more or less extensive collection, which have some specific properties*[6]. … [And on p. 139:] *The method of research characterized as the discussion of statistical data … is called statistical.* [It] *can be applied in very diverse areas of knowledge*. … [However,] *it would be meaningless to unify, for example, socio-economic statistics, physical statistics, … into one science.*

The common features of the statistical method in various areas of knowledge come down to the calculation of the number of objects in *some group or other … This formal mathematical side of statistical research methods is indifferent to the specific nature of the objects being studied and comprises the topic of mathematical statistics.*

Discussion of data is perhaps an elegant variation for *organization* (see Note 15) and *processing.* It follows that statistical method is another such variation for *statistics as a science*, cf. § 4.3.

Would the authors exclude the exploratory data analysis from statistics? Or, would they agree that statistics can participate in governmental decision making (§ 3)? To my mind, their definition is sufficiently broad and allows them to answer negatively in the first case and otherwise in the other instance.

A related definition is due to Kruskal (1978, p. 1072): Theoretical statistics is

the formal study of the process leading from observations to inference, decision, or whatever be the end point, insofar as the process can be abstracted from special empirical contexts.

This statement differs from the previous in that, without attributing theoretical statistics to any area of knowledge, Kruskal believes that it is *not, strictly speaking, a branch of mathematics.* I disagree, see § 7.

5.2. Some competing definitions. Understandably restricting his attention to physics, Maxwell (1871, p. 253; 1877, p. 242) maintained that the statistical method consisted in *estimating the average condition of a group of atoms*; in studying *the probable number of bodies*[7] *in each group under investigation.*

Taking into account other applications of statistics, I infer that these definitions are too narrow but noteworthy since the study of frequencies had then just begun.

5.2.1. Studying statistical data. Bartholomew (1995, pp. 2 – 3) quoted several new definitions of statistics including those of Egon Pearson (apparently unpublished) and Kendall (1950). They stated that statistics was

1) *The study of collective characters of populations*;

2) *The science of collectives and group properties.*

These statements likely mean that statistics studies statistical data. They can be regarded as supplementing the opinion of Kolmogorov & Prokhorov (§ 5.1).

5.2.2. Measuring uncertainty. Some authors believe that the aim of statistics consists in measuring uncertainties. Thus, statistics is

1) *The art of precisely determining the extent … of ignorance* (Chuprov 1896, p. 254). Se also Sheynin (1990/2011, p. 118).

2) *The study of uncertainty* (Lindley 1984, p. 360).

3) *A logic and methodology for the measurement of uncertainty and for an examination of* [its] *consequences* (Stigler 1986, p. 1).

Uncertainty (or ignorance) belongs, first and foremost, to the statistical data. But why should we omit organization (or systematization) or utilization of data? And why restrict processing to measuring uncertainly? Nevertheless, a new dimension had appeared here (Chernoff & Moses 1959, p. 1):

Years ago a statistician might have claimed that statistics deals with the processing of data. … Today's statistician will be more likely to say that statistics is concerned with decision making in the face of uncertainty.

5.3. A word on applied statistics. Even the modern authors did not thus directly mention either the exploratory data analysis or experimental design, possibly because both these new disciplines exceed the bounds of pure mathematics. However, it is perhaps permissible to include them, at least partly, to applied statistics.

Kruskal (1978, p. 1072) offered a successful even if obvious definition of the last term: *This is at least in principle … the informed application* of theoretically investigated methods. On the other hand, I do not agree with Mahalanobis (Rao 1993, p. 339) who believed that statistics in its entirety was an applied discipline:

Statistical theory is not a branch of mathematics. … Like engineering, [it] *requires all the help it can receive from mathematics; but … mathematical statistics as a separate discipline cannot simply exist*.

The aim of statistics, as he argued, was to make decisions *on a probabilistic basis*[8].

Queerly enough, Rao apparently saw no contradiction between this statement and his own declaration (p. 337): Mahalanobis, as he remarked, was

one of the pioneers who, along with Karl Pearson, R. A. Fisher, J. Neyman and A. Wald, laid the foundations of statistics as a separate discipline.

The three last-mentioned scholars *laid the foundations* of mathematical statistics! And some mention of the Continental direction of statistics was necessary.

6. Statistics as a scientific method. Many authors thought (and think?) that statistics is a method. Thus, Fox (1860, p. 331) maintained that it

can hardly be said to be a science at all. … Its great and inestimable value is, that is a method *for the prosecution of other sciences*.

He then compared statistics with the microscope. Alph. DeCandolle (1911/1921, p. 12) stated that *Die Statistik nicht eine Wissenschaft ist, sondern eine Methode*. Contrasting it with mathematics, he remarked that

Der Mathematiker beendet seine Arbeit mit einem sicheren Schlusse aus willkürlichen Ausgangspunkten; in der Statistik gelangt man zu wahrscheinlichen Resultaten …

Later he (§ 5.1) expressed quite another opinion. DeCandolle obviously excluded mathematical statistics as well as probability theory from mathematics, cf. § 8.1. Miklhashevky (1901, p. 476) called theoretical statistics a methodological science arguing that it

Is a science studying the methods of systematic observation of the mass phenomena of social life and of compiling and scientifically treating the numerical descriptions of these phenomena. … Thus, theoretical statistics is a methodological science and, therefore, when applied to other sciences, plays an auxiliary and subsidiary role.

Much nearer to our time, Kendall (1950, p. 128) declared, concisely and definitely, that *Like mathematics, it* [the statistical method] *is a scientific method*. And, on p. 135: *Statistics, like all progressive sciences, is experimental*. I comment on the relation of statistics and mathematics in § 8.1 and on mathematics in § 7. Kendall here indirectly distinguished between statistics and statistical method, and I think that his second pronouncement is not understandable.

7. Mathematics. Alph. DeCandolle (§ 6), Mahalanobis (§ 5.3) and Kendall (§ 5.1) held that statistics did not belong to mathematics, and Kruskal (§ 6) maintained that mathematics is a method[9]. Perhaps it is, but only to a philosopher, or, more precisely, as far as the theory of knowledge is concerned: mathematics is a method of determinate deductive reasoning, also see below.

According to Bourbaki, mathematics is a system, or hierarchy of structures. Almost agreeing with this proposition, and remarking, in particular, that is it not altogether definite, Youshkevich & Rosenfeld (1972, pp. 475 – 476) diplomatically concluded:

Is it possible, or necessary, to offer a rigid and frozen definition of a science which is in a state of permanent lively development and dialectic interrelation with the entire complex of other areas of knowledge?

Another modern author (Bochner 1987, p. 522) reasonably remarked that mathematics *is a realm of knowledge entirely unto itself.* Kolmogorov (1990, p. 148), following Engels, whom he quoted, maintained that mathematics *is the science of the quantitative relations and spatial forms of the real world* but that, during its long history, mathematics became more and more abstract.

It may be well argued that, at least conversely, quantitative relations and spatial forms of the real world are studied by mathematics. An important corollary concerning statistics is in § 8.1. Also note that Kendall's opinion (§ 6) apparently does not allow mathematics to be connected either with reality or abstract structures.

Karl Pearson's celebrated maxim (1892, p. 15) certainly comes to mind: *The unity of all science consists alone in its method, not in its material*. I think that he was at least partially wrong; thus, stellar statistics can hardly be combined with medical statistics, say. A more interesting related question is this: Is mathematics united by its method(s), or by its subject-matter? At the very least, I quote I. M. Gelfand (Shiryaev et al 1991, p. 316) who declared, in 1953, that *The fact that mathematics is viewed as a unified discipline, is due to a large extent to Kolmogorov.* I understand the *unified discipline* as single subject-matter.

8. Statistics Is a scientific discipline

8.1. Its subject. The subject of mathematical statistics is *the formal mathematical side* of statistical research (§.5.1). Much earlier, Kolmogorov (1954) argued that

Mathematical statistics is a mathematical science. It cannot be abolished, and it cannot be made [be considered as] *an applied theory of probability* [either]. *Not all of it is based on this theory.*

See also §§ 8.3 and 8.4. I ought to add that Kolmogorov's viewpoint was in accord with his later definition of the goals of mathematics (§ 7). Indeed, he continued:

In general, it is mathematics that busies itself with the study of quantitative relations of the real world in their pure form. Therefore, all that, which is common to the statistical methodology of natural and social sciences, which is here indifferent to the specific character of natural or social phenomena, belongs to a branch of mathematics, viz., to mathematical statistics.

Mahalanobis' statement (§ 5.3) that mathematical statistics cannot exist as a separate entity is at least considerably weakened. The same can be said about the opinion of several authors mentioned in the beginning of § 7. In § 8.4 I am discussing the actual contents of statistics rather than mathematical statistics in the 19th century.

8.2. Relation between statistics and philosophy. Kruskal (1978, p. 1082) thus described it:

Statistics has long had a neighbourly relation with philosophy of science in the epistemiological city, although statistics has usually been more modest in scope and more pragmatic in outlook. In a strict sense, statistics is part of philosophy of science, but in fact the two areas are usually studied separately.

I would say: For a philosopher, statistics is a method of stochastic inductive reasoning. More precisely: the determination of statistical probabilities (and other magnitudes, means for example) constitutes the inductive part of statistics, whereas the subsequent statistical analysis belonging to probability theory (or mathematical statistics) is, however, deductive.

8.3. Choosing the adjective. Although Laplace obviously did not use the expression *mathematical* (or *theoretical*) *statistics*, and hardly mentioned *statistics*, it was this former that he was studying and developing. The birth of mathematical statistics in the 1920s – 1930s owed much to the Biometric school and, to some extent, to the Continental direction of statistics, as well as to the penetration of the statistical method into a number of ever new branches of science and its applications in industrial production.

The date when the term itself originated is unknown, but in any case Wittstein (1867), see § 8.4, and Zeuner (1869) included it in the title of their books. Zeuner devoted his contribution to studying mortality and insurance. In his Introduction, he wrote *mathematical statistics* in inverted commas.

The term *theoretical statistics* is likely older and was possibly derived from *Theory of statistics* which (in German) was the title of Schlözer (1804).

Some statisticians have recently denied the term *mathematical statistics*. Kendall (1978, p. 1093) declared that *theory of statistics* [is] *an expression much to be preferred*. Ansombe (1967, p. 3n) called mathematical statistics a *grotesque phenomenon*[10]. Kolmogorov (1954), however, expressly denied the existence of any all-embracing general theory of statistics. This, as he held, was in essence reduced to mathematical statistics and *some technical methods of collecting and treating statistical data* so that mathematical statistics should not be regarded as a part of *the general theory of statistics*. But the rapid development of the exploratory data analysis led to the denial of his statement.

I believe that both adjectives are needed, although theoretical statistics is wider in scope. Advances in the theory of statistics as well as their description in terms of contemporary mathematics should be called mathematical. The work of Gosset (Student) or, in a large part, Fisher, for example, belonged and still belongs to mathematical statistics; just the same, Wilks properly called his sophisticated book of 1962 *Mathematical Statistics*[11]. One of the sections of the International Statistical Institute is called *Bernoulli Society for Mathematical Statistics and Probability*. The Mathematical Subject Classification of *Mathematical Reviews* and *Zentralblatt MATH* tactfully called its § 62 *Statistics* without any adjectives.

8.4. The Actual Contents of Statistics in the 19th Century. Several points can be mentioned.

1) Public opinion was not yet studied, nor was the quality of mass production checked by statistical methods.

2) Sampling had been considered doubtful. Cournot (1843) passed it over in silence and Laplace's sample determination of the population of France in 1786 and repeated in 1812 was largely forgotten. Quetelet opposed sampling. Much later Bortkiewicz (1904, p. 825) and Czuber (1921, p. 13) called sampling *conjectural calculation* although already the beginning of the century witnessed *legions* of new data (Lueder 1812, p. 9) and the tendency to amass sometimes useless or unreliable data revealed itself in various branches of natural sciences.

3) The development of the correlation theory began at the end of the 19th century, but even much later Kaufman (1922, p. 152) declared that *the so-called method of correlation adds nothing essential to the results of elementary analysis*.

4) Variance began to be applied in statistics only after Lexis, but even later Bortkiewicz (1894 – 1896, Bd. 10, pp. 353 – 354) stated that the study of precision was a luxury, and that the statistical flair was much more

important. This opinion had perhaps been caused by the presence of large systematic corruptions in the initial materials.

5) Preliminary data analysis (generally recognized only a few decades ago) is necessary, and should be the beginning of the statistician's work. Halley, in 1701**,** drew lines of equal magnetic declinations over North Atlantic (§ 2), which was a splendid example of such analysis.

6) Econometrics originated only in the 1930s.

I list now the difficulties, real and imaginary, of applying the theory of probability to statistics.

7) The absence of *equally possible* cases whose existence is necessary for understanding the classical notion of probability. Statisticians repeatedly mentioned this cause.

8) Disturbance of the constancy of the probability of the studied event and/or of the independence of trials. Before Lexis statisticians had only recognized the Bernoulli trials; and even much later, again Kaufman (1922, pp. 103 – 104), declared that the theory of probability was applicable only to these trials, and, for that matter, only in the presence of equally possible cases.

9) The abstract nature of the (not yet axiomatized) theory of probability. The history of mathematics testifies that the more abstract it became, the wider had been the range of its applicability. Nevertheless, statisticians had not expected any help from the theory of probability. Block (1878/1886, p. 134) thought that it was too abstract and should not be applied *too often*, and Knapp (1872, p. 115) called it difficult and hardly useful beyond the sphere of games of chance and insurance. In 1911, G. von Mayr declared that mathematical formulas were not needed in statistics and privately told Bortkiewicz that he was unable to bear mathematics (Bortkevich & Chuprov 2005, Letter 109 of 1911).

Statisticians did not trust mathematics. They never mentioned Daniel Bernoulli who published important statistical memoirs, almost forgot insurance, barely understood the treatment of observations, did not notice Quetelet's mistakes or his inclinations to crime and to marriage.

Two circumstances explained the situation. First, mathematicians often did not show how to apply their findings in practice. Poisson (1837) is a good example; his student Gavarret (1840) simplified his formulas, but still insisted that conclusions should be based on a large number of observations which was often impossible. Second, student-statisticians barely studied mathematics and, after graduation, did not trust it.

It is not amiss to mention here the pioneer attempt to create mathematical statistics (Wittstein 1867). He compared the situation in statistics with the *childhood* of astronomy and stressed that statistics (and especially population statistics) needed a Tycho and a Kepler to proceed from reliable observations to regularities. Specifically, he noted that statisticians did not understand the essence of probability theory and never estimated the precision of the results obtained.

I should mention an earlier contribution (Corbeax 1833) as well. He (p. xiii) noted that statistics of population became *the vogue*, but, *failing certain conditions* [failing reliable and reliably treated observations] *sinks into a science of deception*. It is necessary, he added, *to attach the due measure of probability to any conclusion*. But how? He himself (p. xii) allegedly *investigated* some problems *from incontestable data*, but where did he find them?

Corbeax adduced mortality tables. On pp. 170 – 172 his table separated the sexes. Quetelet & Smits (1832, p. 33) remarked that such practice *only recently* came into being, and on pp. 36 – 40 they published a separated table for Belgians. Corbeax, however, went further: the table for each sex was presented in five columns, for perfect lives, annuitants, those who met the *conditions for being admitted to life insurance*, *indiscriminate population* and *inferior lives*. He did not explain how he defined all those classes (except the second one), nor did he say anything about his sources. Other unfavourable points can be mentioned as well, but at least Corbeax had formulated a sound opinion and attempted to improve the situation.

8.5. Addendum: a Statistical Conference (Moscow, 1954) and the Soviet Cul-de-sac. The Conference (Anonymous 1954) discussed the essence of statistics and its relations with mathematics and economics. Some pronouncements made there were quite reasonable; thus (p. 44), it was stated that statistics should not be subordinated to other branches of knowledge. However, the prevailing declarations were grotesque and frightening. Indeed, *Only the revolutionary Marxist theory* is *the basis* for developing statistics as a social science (p. 41). Statistics does not study mass random phenomena (p. 61) which (p. 74) anyway do not possess any special features. The law of large numbers is based *on the principle of causality* and is not a mathematical proposition (p. 64). Probability is not the necessary basis of statistics; the theory of stability [of statistical series] is *a bourgeois theory*; and even honest *representatives of the bourgeois statistics* are compelled to violate their professional duty (p. 46).

Finally, the vice-president of the Academy of Sciences (!) Ostrovitianov (p. 82) reminded his listeners that

Lenin had completely subordinated [adapted] *the statistical methods of research … to the problem of the class analysis of the rural population.* [A certain participant], *however, declares that the same methods of research are used in studying economic groups and the brightness of star groups. This can be only said when utterly contradicting Lenin's works.*

Kolmogorov's address (1954) was made at the same conference but hardly anyone referred to it. Without repeating my citations (§§ 8.1 and 8.3), I add that Kolmogorov also declared that it was necessary

To repulse sharply the abuse of mathematics, so typical for the bourgeois science in studying social phenomena.

Thus, he continued, stationarity and stability of time series are assumed without justification. Then, without elaborating, Kolmogorov stated that *some* [apparently, Soviet] *statisticians orient themselves on spontaneous processes and phenomena*. He did not mention the new discipline, econometry, born in the 1930s, without any Soviet participation.

Elsewhere, I (1998, pp. 536 and 533; 2011b, pp. 159 – 160) have described other bizarre statements of Soviet authors. I quote Maria Smit, a Corresponding Member of the Academy of Sciences since 1939, from the first source. In 1930: *Pearson does not want to subdue the real world as ferociously as it was attempted by* […] *Gaus* [her spelling] … And in 1931, literal translation: *The crowds of arrested saboteurs are full of statisticians.* She likely helped to achieve that.

9. The theory of errors

9.1. The term and its essence. Lambert (1765, § 321) coined the term *Theorie der Fehler*. He also devoted much attention to the *Theorie der Folgen* (1765, § 340 – 426), to the consequences of errors of given magnitudes, and determined the most advantageous forms of geodetic figures by applying the differential calculus. That latter theory constitutes the determinate branch of the error theory. Pertinent investigations can be now attributed to experimental design (§ 2).

The theory of errors applies the term *true* (or *real*) *value* of the measured constant. Several scientists including Lambert had indirectly explained it, but it was Fourier who formally introduced it as the limit of the arithmetic mean of those measurements, see Sheynin (2007). Nothing better was ever suggested although the unavoidable residual systematic error is included in that mean.

9.2. The theory of errors and statistics. Statisticians mostly discuss the estimation of the parameters of distribution functions, but on occasion they also apply the old term without explaining it. Here, however, I describe how statisticians regard the theory of errors.

According to modern belief (Bolshev 1978 and later, e. g., 1989), *The theory of errors is only concerned with the study of gross and random errors*. However, it is the practitioner who has to ascertain whether his observations are (unduly) corrupted by systematic errors, and to try to eliminate them as much as possible (also by applying the determinate error theory). Bolshev believes that systematic errors are treated beyond the error theory, but this is not so at all. It follows that the error theory is a separate scientific discipline belonging to the statistical method as applied to treating observations. But the determinate branch of that theory, being at least related to exploratory data analysis and experimental design, rather belongs to the same method in its applied version.

Acknowledgement. I have previously treated much the same subject (1999/2006; 2011) and it was impossible to compile this piece quite independently.

Notes

1. Witness Cournot (1843/1984, § 106) and Cauchy (1845/1896, p. 242):

Le but essentiel de statisticien, comme de tout autre observateur, est de pénétrer autant que possible, dans la connaissance de la chose en soi.

La statistique offre un moyen en quelque sorte infaillible de juger si une doctrine est vrai ou fausse, saine ou dépravée, si une institution est utile ou nuisible aux intérêts d'un people et à son bonheur.

2. The same source contained a discussion on the application of the statistical method in medicine, and there, on pp. 280 – 281, that same Double reasonably remarked:

La méthode éminemment propre aux progrès de [thérapeutique appliquée] *c'est l'analyse logique et non point l'analyse numérique*.

3. Indeed, Graunt's conclusion (§ 1) had to do with the population of England (and its *Land*). Here is Willcox (Graunt 1662/1939, p. xiii):

Graunt is memorable mainly because he discovered … the uniformity and predictability of many biological phenomena taken in the mass.

Halley (1694/1942, p. 5) directly attributed irregularities in the *series of age* to chance and stated that in a larger number of observations they *would rectify themselves*. Such irregularities, however, can well be occasioned by systematic influences.

4. Cf. Laplace (1814/1886, p. XLVIII), who did not mention statistics:

Dans une série d'événements indéfiniment prolongée, l'action des causes régulières et constants doit l'emporter à la longue sur celle des causes irrégulières.

5. The original Russian word was *systematization*.

6. D. P. Zhuravsky (1810 – 1856), a Russian statistician, formulated a similar statement writing *statistics* instead of *statistical data* (or *arrangement of data*). He (Chuprov 1906, p. 692) thought that statistics was a *calculus of categories*, it distributed objects among categories counting their number in each of these.

7. It is possible that Maxwell actually thought about the mean number of bodies. Such widespread confusion was dated already then.

8. Those declarations were made in 1950.

9. I return to this opinion in § 8.1. Only Kruskal (1978, p. 1072) elaborated his statement: theoretical statistics does not belong to mathematics, since

Some [of its] *important areas may be discussed and advanced without recondite mathematics, and much notable work in statistics has been done by men with modest mathematical training*.

In spite of Kolmogorov (§ 8.3), this possibly means that theoretical statistics is somewhat wider than mathematical statistics (even not counting experimental design and exploratory data analysis), but I hold that it still belongs to mathematics.

10. Wilks acknowledged that his discussions with Anscombe *have been especially useful*, see Introduction of 1961 to his *Mathematical statistics* (edition of 1982, p. ix)!

11. The authors of the articles on all three of these scholars in the *International Encyclopedia* (Krusal & Tanur 1978) appropriately discuss their work in terms of mathematical statistics.

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Abbreviation: JNÖS = *Jahrbücher f. Nationalökonomie u. Statistik*

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II

Statistics and the error theory: a debate

Abstract

A debate on the theory of errors between Romanovsky, a statistician, and Chebotarev, an error theorist of the old school, took place in 1951 – 1953. Defending the classical viewpoint, the latter sensibly maintained that the stochastic theory of errors, although akin to statistics, was nevertheless a separate *technological* discipline. And, toeing the Soviet line, according to which statistics was actually subordinated to Marxist ideology and Lexis, Pearson and many other eminent statisticians were regarded as enemies of socialism, Chebotarev also accused his opponent of adhering to the bourgeois statistical school.

1. The Theory of Errors

I (1999) have discussed the theory of errors and its relation to experimental design. Now, I am adding an important point: the notion of *real value* of a measured constant (Sheynin 2007). Fourier (1826) defined it as the limit of the appropriate arithmetical mean, and many authors have introduced the same formula independently of both the forgotten initial definition and each other. An inescapable corollary is (Eisenhart) that the residual systematic error is included in that real value. In spite of the opinion of some statisticians, the same notion is applied now and then in statistics as well. In my opinion, the theory of errors is the application of the statistical method to the treatment of observations[1].

Statistics borrowed the principles of maximum likelihood and least squares from the theory of errors and proved that the least squares estimators possess certain statistical properties, see Petrov (1954). At the same time, statisticians, possibly even now, are insufficiently acquainted with Gauss' second and definitive formulation of the method. Fisher (1925/1990, p. 260) unfortunately stated that the method of least squares was a special application of the method of maximum likelihood and thus actually referred to Gauss' early thoughts, and Eisenhart (1964, p. 24) remarked that only students of advanced mathematical statistics knew the work of Gauss sufficiently well.

The situation in Russia was different: Markov's resolute support of Gauss' mature thoughts (1899) was generally known, especially to geodesists. See however Sheynin (2006/2009) where I emphasize that Markov had undermined his own support by stating that the method was not optimal in any sense; why then defend it?

2. The Ideological Dimension

In the Soviet Union, everything concerning statistics was connected with ideology, see Anderson (1959), Kotz (1965) and Sheynin (1998). The chief culprits of the *bourgeois* statistics were Lexis and Pearson, but even Quetelet and Süssmilch (!) were regarded as ideological enemies. Quetelet, Lexis, Bortkiewicz and Chuprov (!) allegedly *attempted to prove the invariability and eternity of the capitalist order and the stability of its laws* (Starovsky 1933, p. 280)[2]. Actually Quetelet only stated that many phenomena in social life were stable and indirectly added: under invariable conditions. Then, Lexis, later statisticians and Markov attempted to study numerically the stability of ideologically harmless statistical series aiming to distinguish between the stable and the unstable.

Pearson was the main target of obedient Soviet statisticians because Lenin (1909/1961, pp. 190, 274) had called him *a conscientious and honest enemy of materialism* and *one of the most coherent and clear Machian*[3].

Starovsky only exemplified the prevailing attitude, the downright nonsense pronounced even later, in 1954, at a high-level statistical conference (Sheynin 1998, § 5). Again, in 1958 the late Chuprov (Sheynin 1990/2011, p. 160) was called an ideological enemy since he had held that statisticians were only able to provide estimates of the unknowable real magnitudes …

The official role of statistics was to defend the qualitative Marxist economics and philosophy against modern advances. In 1948, Soviet genetics was uprooted, apparently because it did not yield immediate practical results and believed that conditions of life did not influence heredity, a presumption that had hardly fitted in with that philosophy. True, yet another cause was apparently the desire to quench the ties with foreign scientists.

Also in 1948, a conference on mathematical statistics denounced the (rash) action of a scientist (Nemchinov) who dared to speak out in favour of genetics (Sheynin 1998, § 7). Even in 1976 Riabushkin (1976/1980) argued that the quantitative description of social life should be *inseparably bound* to life's *qualitative content*. Then, however, he (1980) stated that, according to another definition, that condition had been dropped and the closeness of statistics and mathematics acknowledged. A resolute denial of submission to Marxism began in the 1990s (Orlov 1990).

Some accusations made in 1948 against Western statisticians were true. Thus, Andreski (1972, p. 16) had much to say about social science in general (he hardly mentioned statistics). He (p. 194) explained the situation as *an endemic bureaucratic disease leading to safe mediocrity*. Truesdell (1981/1984, pp. 115 – 117) even invented two appropriate terms *plebiscience*, characterizing the contemporary state of affairs, and *prolescience* of the future, whose function will be to

Confirm and comfort the proletariat in all that will by then have been ordered to believe. Of course, that will be mainly social science.

Truesdell did not mention Soviet statistics which had become prolescience by the end of the 1920s.

3. The Debate

3.1. More or less reasonable discussion. Romanovsky (1879 – 1954), a prominent mathematician and statistician, the head of the Tashkent statistical school, dared to publish a booklet (1947a) on the theory of errors without sufficiently knowing that subject.

He got his due from Chebotarev (1881 – 1969), a geodesist and the most eminent figure of the old Soviet error-theoretic school. His life's glory was a monstrous treatise on the theory of errors including the method of least squares written on a pre-Helmertian level with some elementary information on mathematical statistics (1958). There, Chebotarev (pp. 374 and 380) unnecessary mentioned Lenin and did not forget Marx either (see my § 4.2). He (p. 579) alleged that *for 14 centuries the Ptolemaic system of the world had held humanity in intellectual bondage*, and (p. 3) expressed his regret, apparently in all modesty, that, having been restricted by the limits of a text book (only 606 pages long!), he was unable to discuss the relativity and the quantum theory[4].

In his first paper, Chebotarev (1951) listed the unsolved problems of the theory of errors and criticized statisticians who discussed it defectively. On p. 9 he did not fail to notice that Romanovsky (1947a) had restricted his description to the normal law and did not consider systematic errors, and he (pp. 11 and 16) declared that Romanovsky (1947a, 1947b) and Idelson[5] (1947) had attempted to revise the error theory, but that its fundamentals nevertheless had persisted. Chebotarev did not mention the statistical methods of estimating the precision of observations although Kolmogorov (1946, p. 57) had stated that

The popular literature on the method of least squares suffers from one essential defect: it contains no instruction for the use of the Student and the χ^2 distributions in estimating the plausibility of the results obtained. […] *When the number of observations is small, the application of the Gauss law instead of those mentioned leads to a very large, and, practically speaking, highly noticeable exaggeration of this plausibility.*

Romanovsky himself (1953, p. 19)[6] later argued, that the fundamentals of the error theory had indeed changed: *prior distributions of the measured magnitudes had disappeared* and *new approaches* to estimating the precision by means of the t and χ^2 distributions became known. His first statement was wrong: the only prior distribution known in the error theory ever really applied was the uniform law which Gauss had initially (in 1809) introduced for the sake of a Bayesian argument, but which he left out (together with the resulting normal law) of the definitive justification of least squares[7].

Later Chebotarev (1958, p. 373; 1959, pp. 14 – 15; 1961, p. 25) came to formulate a proposition, in which he apparently believed from the very beginning: to maintain that the theory of errors was akin to, but differed from statistics. He did not, however, mention the essential circumstances which indeed separate the two disciplines (§ 1); instead, he referred to the less important difference between ascertaining the real value of a measured constant and estimating, say, the mean price of bread. Several authors including Quetelet (1846, pp. 63 and 65) regrettably stressed this difference; true, Davidov (1857, p. 16) argued that it was only significant insofar as it concerned the deviations from the means [as it concerned whether or not the phenomenon studied was random], but his opinion remained barely known. Once more, see Sheynin (2007).

Chebotarev (1958, p. 371) also maintained that the theory of errors was a *technological* discipline, but he never called it, as I did (§ 1), an applied branch of statistics. At the same time, he (1959, p. 21; 1960, p. 69; 1961, p. 28) repeatedly urged geodesists to make use of the correlation theory and the analysis of variance.

3.2. The ideological attack. Chebotarev devoted the greatest part of his paper (1951) to ideological problems. a) He denied the *bourgeois* statistics in general and paid special attention to Romanovsky's sins; and b) he artificially connected some purely mathematical points with ideology.

a) Chebotarev (p. 7) singled out Romanovsky's favourable opinion of Pearson and quoted Lenin's statement (§ 2). He concluded that *an implacable foe of scientific socialism* was incapable of *honestly developing theoretical or technical problems* of a particular science.

For good measure he declared Idelson (1947) guilty of discussing the work of Laplace, Gauss, Legendre, Cauchy, Bienaymé, Pearson, Student, Fisher, Jeffreys, *and many more* foreign authors and of disparaging Russian (including Soviet) scholars[8]. At the time, the first charge was not ridiculous at all, although the inclusion of Laplace and Gauss was sheer stupidity, and the second one, no less dangerous, was entirely fabricated.

Romanovsky (1953) argued, however, that Pearson's statistical research was not connected with his philosophical outlook; that one of the aims of his book (1938a) was to underpin Pearson's empirical constructions by probability theory as developed by Chebyshev, Markov and Liapunov[9]; and that in 1937, when he had compiled this book, it was indeed proper to call Pearson the leader of the modern mathematical statistics, although the situation had since changed[10].

Not being satisfied, Chebotarev (1953, p. 24) stated that Pearson's philosophy did influence his work (which was likely true) and he also noted that Sarymsakov (1948, p. 222) had criticized Romanovsly in a similar way. Yes, Sarymsakov indeed stated, that

In choosing the subject of research and the problems to solve, Romanovsky had punctiliously followed the Anglo-American tendency in the field of mathematical statistics.

Chebotarev could have also quoted the resolution (Anonymous 1948) of the Conference where Sarymsakov had made his report. This queer document was crammed with the usual Soviet clichés of the time. It denounced *servility and kow-towing to foreign ideas* and worryingly noted that *sometimes the methods of bourgeois statistics were popularized and applied*. There also, on the same page 314, we read that Romanovsky acknowledged his ideological mistakes made in some of his previous work.

On the other hand, Chebotarev modestly passed over in silence many other statements. Thus, Sarymsakov (l. c.) added that

A considerable part of Romanovsky's work was devoted to concrete applications and made extensive use of the methods developed by the Chebyshev school

and that Kolmogorov (1948, p. 220), in a report to the same conference, mentioned the *enormous work on mathematical statistics done by Romanovsky and his school*. His earlier reference (1947, p. 63) to Romanovsky was also favourable indeed:

Besides his own interesting results in the area studied by the British school, Romanovsky published an extensive course of mathematical statistics [1938] *collecting, with an exceptional completeness, the new statistical achievements that are most essential for applications*.

b) I also discuss two additional points which Chebotarev (1951, p. 8) artificially connected with ideology. He was not satisfied with Romanovsky's definition of a random event. To say, as Romanovsky did, that the outcome of such an event is unpredictable, means that he understands randomness in a subjective way; where then, Chebotarev ominously asked, will he lead us?

Romanovsky (1953, p. 17) reasonably remarked that the impossibility of predicting the outcome of a single event does not preclude studies of mass random phenomena. No, Chebotarev (1953, p. 21) did not agree with this explanation. Such definitions, he preached, were *usual for scientists from the capitalist countries*.

Chebotarev (1951, pp. 8 – 9; 1953, p. 24) also took Romanovsky (1938) to task for using such expressions as *the probability* […] *is described by the law* […] and for following Pearson, that faithful adherent of Mach, for whom the aim of science was to *describe* phenomena. Without noticing that the former expression was quite usual for mathematicians in general and specialists in probability in particular, Chebotarev[11] continued: descriptions are not sufficient since Marx had insisted that *Philosophers have only variously explained the world, whereas it should be changed*.

Chebotarev had defended all this nonsense and continued in the same vein (1958, p. 371) by remarking that Romanovsky (1947a, p. 5), in listing the aims of the error theory, had failed to mention the design of geodetic networks, i. e., to offer a suitable example of the way to change the world … Apparently, Chebotarev was unable to state, in plain words, that Romanovsky had forgotten the determinate branch of the error theory.

4. Some Comment

As stated in § 1, I believe that the stochastic theory of errors is the application of the statistical method to the treatment of observations but Chebotarev (§ 3.1) not quite correctly maintained that the theory was a *technological* discipline. He also formulated some sensible criticisms of Romanovsky (§ 3.1) more or less applicable to contemporaneous statisticians in general. However, he (§ 3.2) toed the Soviet ideological line and made quite a few unreasonable claims. I think that his behaviour may be partly understood by the blind alley in which the old school of error theory found itself at the time as well as his remaining a non-party man but chairing (*lower*) *geodesy* (as it was then called) at the Moscow Geodetic Institute.

The error-theoretic literature has gradually changed since the 1950s, and statistical ideas find ever more understanding. Readers will be well advised to assess the new situation by comparing a survey of all the pertinent geodetic contributions up to the end of the 1960s (Wolf 1968) and a review of the literature published during 1976 – 1984 (Markuse 1985). This latter testified to a much higher mathematical and statistical level recently achieved by geodesists; nowadays, geodesists would not have shirked reading Romanovsky (1947b), as they probably did several decades earlier, or even Linnik (1958).

At the same time, Markuse's review showed that such topics as Bayesian inference or significant tests were (yet?) hardly treated. And no exposition of the MLSq is convincing enough. What is needed, is a book expounding both the classical stochastic theory of errors and the statistical approach and soberly comparing the old and the new with a Gaussian eye for the practitioner. Bearing in mind my reasoning in § 1, I think that even if that happens, the two disciplines will still remain somewhat distinct.

Notes

1. Mathematicians regrettably wrongly defined the theory of errors. Thus, Romanovsky (1955) and Bolshev (1975) thought that it belonged to mathematical statistics and did not study systematic errors. Nikulin & Polyshchuk (1999) agreed with the former statement and restricted the theory to the case of normally distributed observational errors. See also the statements of Fisher and Eisenhart below.

Especially poorly known is the existence of the determinate branch of error theory which nowadays should be seen as the precursor of experimental design. It discussed the entire process of measurement without applying stochastic consideration. Its classical problems are the designing of suitable geodetic networks and compilation of observational programmes best suited for minimizing the influence of both random and systematic errors. To attribute the study of systematic errors to any other discipline (Bolshev) is at least practically impossible.

2. Much later Starovsky (1960, p. 15) somewhat changed his attitude and only mentioned the *antiscientific essence of the* <u>*theories*</u> *of Lexis and Pearson*. In 1948 – 1975 he was head of the state Central Statistical Establishment.

Yes, Stalin died in 1953 and the situation changed for the better. Sarymsakov (1955), in his obituary of Romanovsky, did not find anything wrong with the latter's work, and later Chebotarev (1958, pp. 571 and 578) favourably mentioned his late opponent, and, on p. 524, indifferently referred to Charlier through Idelson! Still later, he (1960, p. 63) called Romanovsky a very well-known Soviet mathematician [and statistician]. Oppression, however, never ceased for a long time, and 1968 saw the beginning of its new wave.

3. Lenin discussed and thus condemned Pearson's *Grammar of Science* (1892) that greatly influenced the scientific community (Sheynin 2010, § 2) and apparently became the reason for electing its author to the Royal Society. In particular, I noted that Mach (1897) had highly appraised it. Now, I additionally mention Newcomb (Newcomb & Pearson 1880 – 1901) who persistently (but unsuccessfully) invited Pearson to deliver a report at a high-ranking international congress.

4. At the end of the 19th century Pearson also studied most serious philosophical problems whereas Chebotarev (1951, pp. 11 – 13) touched them quite unnecessarily and without imparting any useful thoughts. Incidentally, Pearson's study had not yet been discussed.

5. An eminent astronomer and historian of astronomy, who died in 1951, possibly even before Chebotarev's article (1951) appeared in print. On p. 10 the latter remarked that his criticism of Romanovsky also largely applied to Idelson.

6. This note had been barely known. Much later Bogoliubov & Matvievskaia (1997) included it in their bibliography of Romanovsky's works.

7. Kolmogorov (1942, pp. 4 – 5) had outlined the *classical* method of solving the standard problems of the stochastic error theory by introducing prior distributions, but he did not supply any references. According to the context of his paper, he possibly restricted his attention to artillery firing.

8. For that matter, he (1947) did not mention Helmert, and he (pp. 21, 74 and especially 14) overestimated Markov's achievements, but did not refer to Chebotarev's earlier writings or to other backward geodetic contributions, whether Soviet or foreign. For his part, Romanovsky (1939, p. 56; 1947a, p. 66; 1954, p. 62) had mistakenly remarked that Markov was the first to formulate the principle of least squares *with complete rigor*, whereas Chebotarev (1961, p. 27) wrongly maintained that Markov had developed the correlation theory. See Sheynin (2006).

Romanovsky (1939 – 1954; 1955) was more kind to Chebotarev than Idelson in that he referred to Chebotarev's appropriate contribution, the previously published edition of (1958). He had also mentioned Idelson (1947), although only once (1939 – 1954, 1954), and failed to repeat his reference in his next writing (1955).

9. Cf. Chuprov's letter of 1923 to his former student, N. S. Chetverikov (Sheynin 1990/2011, p. 71): Romanovsky

Aims at the same goal as I do: at converting the British literature, – in particular, the teaching concerning the methods of investigating corr. connections, – into the language of the precise theory of probability and at purging it of <u>*all dross*</u>*. And in many respects our paths amusingly adjoin each other.*

10. Elsewhere Romanovsky (1938b) had referred to Pearson in a similar way, but he also respectfully mentioned Fisher. Much more (Sheynin 2008): he published five papers in *Biometrika*, and corresponded with Pearson (1924 – 1925); in 1928 – 1938 he also exchanged letters with Fisher. Chebotarev luckily failed to mention the first fact, and could not have known about that correspondence.

11. I myself had graduated from MIIGAiK in 1951 and attended Chebotarev's allegedly *advanced* course in the theory of errors, but did not understand then what was going on around me.

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Abbreviation: MIIGAiK = *Moscow Inst. Engineers Geodesy, Air Survey, Cartography*

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III

Walther Mann

Letter to Oscar Sheynin

Darmstadt, 23 Oct. 2005

Lieber Herr Sheynin, Ich danke für Ihr Fax vor einigen Tagen, dem ich entnehme, daß Sie überlegen. ob Sie die Gedenkrede meines Großvaters Dr. Alois Schindler aus dem Jahr 1902 zu Ehren von Mendel zur Veröffentlichung geben sollen.

Vorerst mochte ich Ihnen sagen, daß ich mich gefreut habe, wieder von Ihnen zu hören. Ich denke gerne an den freundlichen Empfang […]. Ich komme kaum mehr nach Berlin. Aus Altersgründen […].

Doch nun zu Ihrem Fax. Ich habe natürlich nichts dagegen, wenn Sie die Gedenkrede meines Großvaters veröffentlichen wollen. Ich weiß allerdings nicht, ob sie von öffentlichem Interesse ist. Ich nehme auch an, daß sie in den Instituten und Museen, die sich mit Mendel beschäftigen, vorhanden ist. Ich würde Ihnen, falls es zu Veröffentlichung kommt, gerne helfen, möchte aber selbst keinen Beitrag verfassen. Fachlich kann ich ohnedies nichts beitragen. Und meinen Großvater kenne ich nur aus der Erzählungen meiner Mutter, da er bereits 1930, also vor ein Jahr vor meiner Geburt, starb.

Meine Mutter schilderte meinen Großvater stets als sehr beliebten, aktiven und tüchtigen Mann. Er war Stadtarzt in Zuckmantel, einer kleinen Stadt am Fuß des Altvater-Gebirges im ehemaligen Österreichisch-Schlesien, heute CR [Czech Republik]. Er war einer der drei Neffen, die Mendel in Brünn studieren ließ. (Siehe Seite 4 der Rede und Anmerkung 3 und 12 auf Seite 15 und 16.) Die Mutter dieser 3 Neffen Theresia Schindler geborene Mendel, als meine Urgroßmutter, war die jüngere Schwester Mendels und hatte stets ein sehr gutes Verhältnis zu ihrem Bruder. Sie hatte ihm während seiner Schulzeit einen Teil ihres Erbgutes zur Verfügung gestellt, als die Familie wegen der Erkrankung des Vaters in wirtschaftliche Not geriet. So konnte Mendel mit ihre Hilfe seine Schulbildung abschließen. Es war sicher ein schönes Zeichen des Dankes, daß Mendel später für die Söhne seiner Schwester, also die 3 Neffen, in Brünn sorgte und sie zum Studium brachte.

Die Gedenkrede 1902 entstand, als eine Gedenktafel am kleinen Gebäude für die Feuerwehrspritzen in Heinzendorf angebracht wurde. Diese Dorf war der Geburtsort von Mendel und von Schindler. Mendel hat sich stets diesem Dorf und seiner Familie verbunden gefühlt. Wie ich bei einem besuch vor einigen Jahren feststellen konnte, existiert sowohl das Gebäude als auch die Gedenktafel heute noch. Es liegt nur wenige Schritte von Mendels Geburtshaus entfernt. (Mein Geburtsort Odrau, heute Odry, ist übrigens auch in der Nähe, etwa 5 km von Heinzendorf entfernt. Als Kind vor unserer Vertreibung war ich oft da, da wir noch Verwandte in Heinzendorf hatten, die aber auch alle 1946 in den Westen vertrieben wurden.)

Nun wünsche ich Ihnen […]. Ihr Walther Mann

Walther Mann

Letter to Oscar Sheynin

Darmstadt, 23 Oct. 2005

Dear Mr. Sheynin, I am thankful for your recent fax message from which I concluded that you are thinking about publishing the memorial speech of 1902 honouring Mendel, of my grandfather, Dr. Alois Schindler.

First of all, I would like to tell you that I am glad to hear from you once again. I thank you gladly for the kind reception […]. Because of old age […] I will hardly come to Berlin again. But to return to your fax. Naturally, I have nothing against your wish to publish the memorial speech of my grandfather. However, I do not know is it of public interest. I also assume that institutions and museums which have to do with Mendel are keeping its copies. I would have gladly helped you if the speech is indeed being published, but can not draw up any contribution. Anyway, I can not contribute anything professionally, and I only know my grandfather from my mother's stories. He died in 1930, a year before I was born.

My mother always described my grandfather as a very popular, active and competent man. He was the city physician in Zuckmantel, a small city at the foot of the Altvater mountains in the former Austrian Silesia, now in the Czech Republic. He was one of the three nephews whom Mendel had helped to study in Brünn. […] Their mother, Theresia Schindler, née Mendel, my great-grandmother, was the younger sister of Mendel and invariably had a very good relationship with her brother. During his school years, when the family became impoverished due to the illness of Father, she put at his disposal a part of her inheritance. With that help Mendel was able to conclude his school education. It certainly was a good token of gratitude that Mendel later, in Brünn, took care of the sons of his sister, the three nephews, and made their studies possible.

The memorial speech took place in 1902, when a plaque was mounted in Heinzendorf, on a small building where fire pumps were being kept. That village was Mendel's and Schindler's place of birth and Mendel always felt himself connected with it and with his family. A few years ago I visited it and found out that both that building and the plaque still exist. The building is situated only a few steps from the house in which Mendel was born. Incidentally, my own place of birth, Odrau, today Odry, is also nearby, about five kilometres from Heinzendorf where I still had relatives. As a child, before our expulsion, I often came there. However, in 1946 all of them were also driven out to the West.

Schindler's memorial speech of 1902 was soon privately printed (and W. Mann gave me its copy). Later, Krizenecky (1965, pp. 77 – 100) published it. Without stressing it at all, Schindler mentioned that Mendel was German, and Mann, in a conversation, told me, that in his younger years Mendel did not speak Czech.

Schindler made known many details about Mendel, hardly found elsewhere. He also mentioned letters sent by many scholars to Mendel and likely lost, and named authors who were the first to acknowledge the importance of Mendel's work. Understandably, he did not know that the first was Schmalhausen (1874).

Many applications of the statistical method to natural sciences made up to the mid-19th century can be cited (Sheynin 2009, §§ 10.9 – 10.10). Some employed more or less reasonably chosen functions with empirically determined coefficients. Thus, Lambert, law of mortality, in 1772; Quetelet, law of the squared sums of the mean daily temperatures for the appearance of leaves, flower and fruits of a given species, in 1846; models of spatial arrangement of stars, mid-19th century, etc.

Some other and most important discoveries were made simply by comparing two statistical populations. The best example concerned the proof that the spread of cholera was occasioned by non-purified drinking water (Snow, in 1855).

For his part, Mendel laid the mathematical foundation of genetics by justifying the existence of discrete hereditary factors and discovering the principles of their random separation and recombination. He achieved this breakthrough by most elementary mathematical reasoning (a rare case indeed!) coupled with attentive experimentation carried out over a long period of time.

The trustworthiness of Mendel's experiments (although not of his final conclusions) had been questioned. Doubts were expressed about his subjective honesty but dispelled the more so since his meteorological work testified that he had most accurately recorded his observations. See Orel (1996, pp. 199 f.) for a detailed discussion; in particular, he stressed Weiling's defence of Mendel (1975).

Again, the pattern of his statistical research had been studied in order to perceive whether Mendel made unsubstantiated decisions (for example, concerning an ending of an experiment), but once more he finally remained unscathed.

In later terminology, if the possible genetic compositions (genotypes) of seeds *AA, Aa* and *aa* are produced by crossing of two genes, *A* and *a*, with the appearance (the phenotype) of seeds *AA* and *Aa* being identical, then, according to the development of the binomial $(A + a)^2$, there occur three different genotypes with only two phenotypes in the ratio 3:1. In a more general case, the initial genetic material can be represented as

$$(A + a)^2(B + b)^2$$

and Mendel had the patience to consider seven such factors. For a good description of this subsection see De Beer (1966).

Individuals of the same species usually differ in their sets of genes, so that their offspring are intraspecific hybrids. For this reason, Mendel's discovery was extremely important for the study of heredity. Nevertheless, it remained dormant until the beginning of the 20th century, and even then its fate in England had been complicated. Following Galton, biometricians were rather interested in measuring correlations between parent and offspring. Pearson seems to have been hesitating, but in 1909 he suggested a synthesis of biometry and Mendelism (Magnello 1998). Even more important were the findings of Fisher (De Beer 1966) and Bernstein's study (1924). The latter proved that under wide assumptions the Galton law of inheritance of quantitative features was a corollary of the Mendelian laws (Kolmogorov 1938, § 1).

By 1935, the Soviet Union became a leading centre of Mendelian research. Then, however (Sheynin 1998, pp. 543 – 545), genetics was called an idealistic science opposed to dialectical materialism, and, with the beginning of the cold war in 1947, all international contacts (which certainly existed among geneticists) were quenched. The showdown occurred in 1948 with Lysenko, that notorious and illiterate humbug, most certainly backed by the nation's political leaders, playing the main role. Fisher (1948/1974, p. 61) stated that

under the impulsion of his [Lysenko's] *attacks many Russian geneticists, and those among the most distinguished, have been put to death* … [back in 1940 – 1941].

Even Kolmogorov came under a mild attack (Gnedenko 1950, pp. 7 – 8) for his *new brilliant* [statistical] *confirmation of Mendel's laws* (1940, p. 37). The situation did not change until the 1960s.

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IV

Randomness and determinism: Why are the planetary orbits elliptical?

Abstract

I trace the history of the notion of randomness in natural science, its interpretation from ancient times to our day, note the rather newly developments but leave aside the mathematical attempts to define randomness of finite or infinite number sequences. I also stress the difference between randomness in the general sense and its stochastic specification, a subject of statistics. Then, I discuss the main actual or direct explanations of the ellipticity of the planetary orbits by randomness (Kepler, Kant, Laplace); in spite of Newton's discovery, randomness is not excluded from the system of the world. No similar historical studies are known to me. The notion of randomness is important for statisticians, hence for a great number of applied statisticians working in various branches of science as well. The history of randomness had never been studied in spite of the obvious importance of that notion. My other novelties are: a qualitative explanation of chaos and discovery of Laplace's mistaken opinion about the eccentricity of planetary orbits.

Randomness is a fundamental notion. It is being actively studied by mathematicians who are trying to find out how to define a random finite or infinite number sequence. For a serious popular discussion of the pertinent efforts see Chaitin (1975).

1. Randomness: General Information

Aristotle and other early scientists and philosophers attempted to define, or at least to throw light upon randomness. His examples of random events are a sudden meeting of two acquaintances (*Phys*. 196b30) and a sudden unearthing of a buried treasure (*Metaphys*. 1025a). In both cases the event occurred without being aimed at (§ 1.3) and also, together with other cases, belongs to Poincarè's pattern of § 1.1.

I have discussed Aristotle earlier (Sheynin 1974, § 2.2) but did not correctly interpret his explanation of the birth of female offspring. Concerning Kepler's explanation of the eccentricity of planetary orbits (§ 2 below), see also Sheynin (1974, § 8.1.1). I am referring to vol. 2 of Aristotle's *Works* edited by D. Ross (vols 1– 12. Oxford, 1908 – 1954). Below, I refer to another of his contribution from vol. 8 of the same edition. There also exists an edition of Aristotle's *Complete Works* (vols 1 – 2. Princeton, 1984) whose composition is slightly different; the order of the contributions also differs, and the numbering of the pages and lines is therefore different.

1.1. Intersection of chains and Poincaré's first explanation. Many ancient authors had been repeating Aristotle's first example and Cournot (1843/1984, § 40, p. 55) revived it:

Events occurring as a combination or meeting of phenomena which apparently belong to independent series [but] *happening as ordered by causality, are called fortuitous, or results of hazard*.

[Ces événements amenés par la combinaison ou la rencontre de phénomènes qui appartiennent à des séries indépendantes, dans l'ordre de la causalité, sont ce qu'on nomme des événements *fortuits* ou de résultats du hazard.]

Both examples actually illustrate one of Poincaré's explanations (interpretations) of randomness (1907), then incorporated in his popular book (1908) and in his treatise: if equilibrium is unstable,

A very small cause which escapes us determines a considerable effect […] *and we say that that effect is due to chance*.
[Une cause très petite, qui nous échappe, détermine un effet considérable […] et alors nous disons que cet effet est dû au hazard] Poincaré (1912/1987, p. 4).

Indeed, an insignificant delay of one of the two acquaintances (Aristotle) means that their meeting does not take place.

Poincaré could have just as well cited a coin toss. His deliberations (also see below) heralded the beginning of the modern period of studying randomness. However, Poincaré certainly had predecessors who only failed to mention directly randomness (Sheynin 1991, § 8). Among them was the ancient physician Galen (1951, p. 202): I*n old men even the slightest causes produce the greatest change*; Pascal (1963, p. 549): H*ad Cleopatra's nose been shorter, the whole face of the Earth would have changed*; and Maxwell (1873a, p. 364) who referred to the unstable refraction of rays within biaxial crystals. Elsewhere he (1859/1927, p. 295 – 296) left a most interesting statement (cf. § 1.7):

There is a very general and very important problem in Dynamics. [...] *It is this: Having found a particular solution of the equations of motion of any material system, to determine whether a slight disturbance of the motion indicated by the solution would cause a small periodic variation, or a total derangement of the motion*.

1.2. Corruption of/deviation from laws of nature. Aristotle, whom we must continue to discuss, also explained the appearance of monsters (*Phys*. 199b1; *De generatione anim*. 767b5) as mistakes *in the operation of nature*; he says that the first

Departure from the type is that the offspring should become female instead of male; […] *as it is possible for the male* [for the father] *sometimes not to prevail over the female* [the mother] […].

In my context, I should also mention Thomas Aquinas (Sheynin 1974, §2.4). His general goal was to unite faith and reason and to adapt pagan Aristotle to Christianity. He repeated the Philosopher's thoughts and mentioned *some hindering cause* (some corruption of law) bringing about the *production of females*.

Given a large number of births, regularities of mass random events will, however, certainly reveal themselves. Aristotle did not connect such events with randomness, a circumstance which his commentators had hardly indicated; moreover, he (*De Caelo* 283b1 and in other places) stated that *the products of chance and fortune are opposed to what is, or comes to be, always or usually*. Nevertheless, we are fully justified in calling them random: *corruption of,* or *deviation from laws of nature* also means randomness, and this idea can be traced at least until Lamarck who stated that the deviations from the divine lay-out of the tree of animal life had been occasioned by a *cause accidentelle* (Lamarck 1815, p. 133).

There also, on p. 173, he indicated that the spontaneous generation of organisms was caused by a *très-irrégulière* force but did not mention randomness,

1.3. Lack of laws or purpose. When considering the state of the atmosphere, Lamarck (an 8, 1800, p. 76) stated that it was disturbed by two kinds of causes, including *variables, inconstantes et irrégulières*. Again, no mention of randomness, but then he (1810 – 1814/1959, p. 632) denied it: no *part of nature* disobeys *invariable laws*, therefore *that, which is called chance*, does not exist. Louis Pasteur definitively disproved spontaneous generation, but I stress that until then it was apparently always considered random.

Witness indeed Harvey (1651/1952, p. 338):

Creatures that arise spontaneously are called automatic […] *because they have their origin from accident, the spontaneous act of nature*.

Harvey did not say anything about the essence of accidents, but it seems that he thought them aimless, identified them with lack of law. Many other scientists denied randomness as Lamarck did, see § 2.

I will now mention Laplace (1814/1995, p. 9, my paraphrase) who stated that the arrangement of printed letters in the word *Constantinople is not due to chance*; all arrangements are equally unlikely, but that word has a meaning and it is *incomparably more probable* that someone had written it on purpose. He equated randomness with lack of purpose. This example shows that human judgement is needed for supplementing mathematical reasoning about randomness.

1.4. Separation of law and randomness. In his main contribution to probability, the celebrated *Doctrine of Chances*, De Moivre (1756/1967, p. 329) considered as its main achievement the establishment of *certain rules for estimating how far some sort of Events may rather be owing to Design than Chance*. This is a quotation from the reprint of his Dedication of the first edition of the *Doctrine of Chances* to Newton. De Moivre also stated there that he should think himself

Very happy if having given […] *a method of calculating the Effects of Chance* […] *and thereby fixing certain Rules, for estimating how far some sorts of Events may rather be owing to Design than Chance, I could* […] *excite in others a desire* […] *of* […] *learning from your* [Newton's] *Philosophy how to collect* […] *the Evidences of exquisite Wisdom and Design, which appear in the Phenomena of Nature* […].

De Moivre did not define chance, but it seems to follow that if design (aim of nature) exists, then chance is its corruption; true, design is lacking in games of chance (which he studied), and its corruption is out of question: there, it was lack of any law.

I would say that all this testifies that for De Moivre the main goal of the emerging theory of probability was to study the deviations from the Divine laws of nature. In 1733, his derivation of the normal law of distribution (the first version of the central limit theorem) was occasioned by a study of the sex ratio at birth. For him, the initial binomial distribution of those births was a designed deterministic law of nature, the first statistical regularity of nature (with its parameter only approximately known) and only the actual deviations from it were random in the mathematical sense. See the final version of that derivation (De Moivre 1756/1967, pp. 252 – 253). This is indeed interesting for the history of probability theory.

Poincaré also formulated a dialectical statement about determinism and randomness much broader than the one following from *deviation from laws of nature*: it legitimizes randomness and indirectly defines it but does not say anything about regularities of mass random events:

In no field [of science] *do exact laws decide everything, they only trace the boundaries within which randomness is permitted to move. According to this understanding, the word randomness has a precise and objective meaning*.

[Dans chaque domaine, les lois précises ne décidaient de tout, elles traçaient seulement les limites entre lesquelles il était permis au hazard de se mouvoir. Dans cette conception, le mot hazard avait un sens précis, objectif] (Poincaré 1896/1912, p. 1).

Poincaré's pronouncement restricted the action of his pattern *small cause – considerable effect* (§ 1.1). Exact laws tolerate randomness. Indeed, here is Newton (1704/1931, Query 31):

Blind fate could never make all the planets move one and the same way in orbs concentrick, some inconsiderable irregularities excepted, which may have risen from the mutual actions of comets and planets upon one another, and which will be apt to increase, till this system must be allowed the effect of choice.

Perturbations have appeared here just as errors of observations did in Poincaré's reasoning. Thus, Newton actually recognized randomness, although this time only in its *uniform* version as witnessed by the expression *blind fate*. Whether in English, or in equivalent French and German terms, scientists of the 17th and 18th centuries, if discussing randomness, mostly understood it in this sense (Sheynin 1991). For example, Arbuthnot (1712), unlike De Moivre, only compared Design with a discrete uniform distribution of the sexes of the newly born.

Newton (Sheynin 1971), however, considered throws of an irregular die. In this case and in a separate thought experiment he suggested an embryo of the Monte Carlo method. I briefly add that he also introduced geometric probability, cf. § 1.9.

1.5. Formidable effect of a large number of small causes. I return to Poincaré, to his statement first pronounced in 1907: he (1912/1987, p. 10) attributed accidental errors of observation to chance since

Their causes are too complicated and too numerous. Here again we only have small causes each of them [now, contrary to his previous definition,] *only producing a small effect; it is because of their combination and their number that their effect becomes formidable*.

[Nous les attribuons au hazard, parce que leurs causes sont trop compliquées et trop nombreuses […] nous n'avons que de petites causes, mais chacune d'elles ne produit qu'un petit effet; c'est par leur union et par leur nombre que leurs effets deviennent redoutables.].

Here, variations between individuals of a given species, or once more coin tosses could have been cited.

1.6. Laplace: impossible condition for lack of randomness. Laplace (1814/1995, p. 2) stated that, for a mind, able to *comprehend* all the natural forces, and to *submit these data to analysis*, there would exist no randomness *and the future, like the past, would be open* to it. My example: the outcome of a coin toss will then be predicted, cf. Poincaré's statement (§ 1.5) about errors of observation.

Nowadays, this opinion cannot be upheld because of the recently discovered phenomenon of chaos, see below. Other remarks are also in order. Such a mind does not exist (so that Laplace's statement was purely academic) and there are unstable movements, sensitive to small changes of initial conditions. And I also note that already previous scholars, for example, Maupertuis (1756, p. 300) and Boscovich (1966, §385), kept to the "Laplacean determinism". Both mentioned calculations of past and future (*to infinity on either side*, as Boscovich maintained) but both disclaimed any such possibility.

The main pertinent point is, however, that Laplace had actually recognized randomness. Without applying stochastic methods he would have not been engaged in studying and furthering the theory of probability, and neither would have he been able to achieve brilliant success in astronomy. Here is an example (regrettably the only direct confirmation of the above): a certain astronomical magnitude

Although indicated by observations, was neglected by most astronomers because, as it seemed, it did not follow from the theory of universal attraction. Nevertheless, subjecting [the probability of] *its existence to the Calculus of Probabilities, I determined that its probability was very high, and considered myself obliged to study its cause*.

[… quoique indiquée par les observations, était négligée par le plus nombre des astronomes, parce qu'elle ne paraissait pas résulter de la théorie de la pesanteur universelle. Mais, ayant soumis son existence au Calcul des Probabilités, elle me parut indiqués avec une probabilité si forte, que je crus devoir en rechercher la cause] Laplace (1812/1886, p. 361).

1.7. Chaotic processes. A chaotic process engendered by a small corruption of the initial conditions of motion can lead to its exponential deviation. Only in a sense this may be understood as an extension of Poincaré's pattern *small cause – considerable effect* (§ 1.1). However complicated and protracted is a coin toss, it has a constant number of outcomes whose probabilities persist, whereas chaotic motions imply rapid increase of their instability with time and countless positions of their possible paths. Their importance in mechanics and physics is unquestionable.

My explanation of the comparatively new concept is only qualitative, but still much better than those, offered by previous authors. Thus, Ekeland (2006, p. 125) unfortunately *likened* that process with a game of chance whereas the main point is, to *separate* these notions.

1.8. Random variables in natural sciences and in statistics. This subsection seems necessary for completing the discussion of randomness. In statistics, a random variable should be statistically stable, but in natural science this restriction is not necessary. An approach to that distinction was due to Poincaré (1896/1923, p. 3):

Among the phenomena whose causes are unknown to us, we ought to distinguish random phenomena, about which we initially find out by the calculus of probability, and non-random, about which we cannot say anything.

[*Parmi les phénomènes dont nous ignorons les causes, nous devions distinguer les phénomènes fortuits, sur lesquels le calcul des probabilités nous renseignera provisoirement, et ceux qui ne sont pas fortuits et sur lesquels nous ne pouvons rien dire*.]

Lamarck (see end of § 1.2) provided a good example of the latter phenomena: the deviations from the divine lay-out of the tree of animal life.

Without mentioning Poincaré Kolmogorov (1983/1992, p. 515) agreed with him:

We should distinguish between randomness in the wider sense (absence of any regularity) and stochastic random events (which are the subject of probability theory).

There seems to be no quantitative criteria of statistical stability, and, anyway, practice often has to work in its absence; example: sampling estimation of the content of the useful component in a deposit. However, choose other sample points, and it will be unclear whether they possess the same statistical properties (Tutubalin 1972/2011, §

1.2). But, according to scientific folklore, pure science achieves the possible by rigorous methods, applications manage the necessary by possible means ... *Statistical stability* apparently characterizes phenomena which can be studied by observations belonging to a single law of distribution, to a single population.

Bayes is known to have introduced a very special type of randomness. He regarded an unknown constant as a random variable with a uniform distribution and his approach persisted in spite of previous prolonged fierce opposition. Obviously, his pertinent trials were statistically stable.

I provide now an example of a false conclusion caused by lack of statistical stability of the considered deviations. William Herschel (1817/1912, p. 579), who certainly knew nothing either about the size of stars or of their belonging to different spectral classes, decided that the size of a randomly chosen star will not much differ from the mean size of all of them. The sizes of stars are enormously different and their mean size is a purely abstract notion. There are stars whose radii are greater than the distance between the Sun and the Earth.

Earlier, De Moivre (1733/1756, pp. 251 – 252) refused to admit randomness in the wide sense in mathematical considerations:

Absurdity follows, if we should suppose the Event not to happen according to any Law, but in a manner altogether desultory and uncertain; for then the Event would converge to no fixt Ratio at all.

1.9. Geometric probability and the random chord

I am briefly repeating the contents of my paper (2003) and adding an important conclusion.

Many scholars had been time and time again unconscientiously introducing geometric probability. Thus, in 1743 De Moivre described the probability of *life's failing* during a given time by the ratio of certain segments. John Michell, in 1767, discussed the probability that two stars out of their multitude scattered over the sky *by mere chance* were close to each other. In 1868 Boltzmann defined the probability that the velocity of a molecule was contained in an infinitesimal interval as the ratio of the time during which this took place to the total time of observation (the *time average* probability). Buffon, in 1777, forcefully introduced geometrical probability intending *to put geometry in possession of its rights in the science of chance*. His *needle problem* became the talk of the town.

One upshot of the developments in the 19th century was Bertrand's discovery (1888, pp. 4 – 5) that the notion of uniform randomness (uniform density of the appropriate probabilities) was not specific enough and allowed numerous interpretations. What was the probability (p), he asked, that a randomly drawn chord of a given circle with radius r was longer than the side of an equilateral triangle inscribed in that circle.

He considered three natural ways of specifying the chord (e. g., its direction was fixed) and arrived at three different answers. Poincaré (1896, p. 97/1912, p. 119) showed that Bertrand had actually considered different problems. Thus, he chose the centre of the circle as the origin of a system of polar coordinates, and one of its diameters as the polar axis, denoted the coordinates of the centre of the chord by θ and ρ (also see below) and arrived at $p = 1/2$.

Prokhorov (1988), although without providing any value for p, decided that the most natural assumption for solving the Bertrand paradox was to choose those same coordinates, independent and uniformly distributed.

Many other commentators also tackled that problem. In 1908 Czuber discovered its three more natural versions. Otto Schmidt, in a Russian paper of 1929, stipulated that the probability sought should persist under translation and rotation of the coordinate system (reflection is now also included) and found that $p = 1/2$.

De Montessus (1903), although making an elementary mathematical mistake, broke new ground. Suppose, as he did, that a point, through which the chord is passing, is moving along a diameter of the circle. Denote its distance from the centre of the circle by x. A certain probability will correspond to each point of interval $r/2 \le x < \infty$ and its mean value will be 1/2; indeed, at large values of x that probability approximates 1/2. Note that the set of possible probabilities is here uncountable.

Various authors thus opted for $p = 1/2$. However, according to the theory of information (Brillouin 1956, p. 1 of main text, somewhat obliquely) that value of probability is tantamount to complete ignorance; *Ex nihilo nihil fit*! Here is an example (Poisson 1837, p. 47). An urn contains a finite number of white and black balls in an unknown proportion. The subjective (!) probability of extracting a white ball was 1/2, and his reasoning could have been applied to the probability of the outcome of a coin toss. End of discussion!

2. Kepler

Kepler only formally denied randomness:

What is, however, randomness? Indeed, the most disgusting idol, nothing but an insult to God, Sovereign and Almighty, as well as to the most perfect world that He created.

[Was aber ist Zufall? Wahrlich, er ist ein höchst abscheulicher Götze und nichts anderes als eine Beschimpfung des höchsten und allmächtigen Gottes und der höchst vollkommenen Welt, der er schuf] (Kepler 1606/2006, p. 163).

Kepler was neither the first, nor the last to deny randomness. Aristotle banished it from science by stating that "None of the traditional sciences busies itself about the accidental […] but only sophistry" (*Metaphysica* 1064b15). He was wide of the mark: the theory of probability "busies itself" not about the accidental, but about its laws. Then, Laplace (1776/1891, p. 145) stated that chance *has no reality in itself* (n'a aucune réalité en lui-même), it only signified our ignorance. And Darwin (1859/1964, Chapt. 5, p. 131) thought that variations in his theory were not at all *due to chance*, that such an expression only acknowledged *our ignorance* of the proper causes. Even Boltzmann hesitated to acknowledge randomness.

In astrology, Kepler considered himself the founder of its scientific direction, of studies of the qualitative correlation between heavenly forces and events occurring on the Earth. Leaving aside his predecessors (for example, Ptolemy and Tycho Brahe), I quote his typical statement:

An astrologer who only sees the sky but […] *does not know anything about intermediate causes can only forecast probably* […] *which means a bit better than not at all*.

[Ein Astrologus, der nur den Himmel sihet und von […] zwischenursachen nicht weiss, nur allein probabiliter […] das ist, ein klein wenig mehr dann nichts] […] Kepler (1610/1941, p. 217).

Probably is not definite enough, but the main point is that Kepler actually recognized randomness as corruption of law (§ 1.2).

I (Sheynin 1974, § 7) treated Kepler's astrology in much more detail, but now I turn to astronomy, and namely to the problem of eccentricities of the planetary orbits. At first, Kepler understood eccentricity as the preordained eccentric position of the Sun as measured from the centre of the circular orbit of a given planet. He then changed his (actually, ancient) definition and stated that eccentricity depended on the combination of external forces, see below.

Kepler (1596/1963) first encountered those eccentricities when attempting to construct a model of the solar system by inserting the five regular solids between the spheres of the then six known planets: they, the eccentricities, and, for that matter, unequal one to another, much worried him: *The causes of the eccentricities are not yet studied, and neither are their differences* (Die Ursache der Excentrizitäten wie auch ihrer Unterschiede noch nicht erforscht ist; Chapter 18, p. 111).

In Chapter 17, p. 108, he formulated the problem for those interested: To discover these causes by issuing from the regular solids. God, he added, did not assign the eccentricities accidentally. In the second edition of that contribution Kepler provided Notes to almost each chapter, and we find there that that problem was not solved [by his predecessors] (p. 117) but that he had investigated it, *and look, I have* [he had] *revealed the main* (vorzüglichsten) *causes* (p. 118 with a reference to Book 5 of his *Harmony* (1619)).

Here is the title of one of the chapters of that contribution:

The origin of the eccentricities of the individual planets [is] *in the arranging of the harmonies between their motions* (Kepler 1619/1997, title of Chapter 9 of Book 5 on p. 451).

On that same page he explained that God had combined the planetary motions with the five regular solids and thus created the only most perfect prototype of the heaven.

Again in the same chapter, in Proposition 5, on p. 454, he indirectly mentioned in this connection his second law of planetary motion; for that matter, he could have referred to it in his *Epitome* (1618 – 1621). Even admitting his theory of solids, which definitively fell down after the discovery of the seventh planet (Uranus), we see, however, that Kepler did not explain the values of those eccentricities. In other words, randomness persisted in spite of his efforts, and its cause was left obscure.

In his main work, Kepler indicated that

Examples of natural things, and the kinship of celestial things for these terrestrial ones […], *cry out that* […] *the variables, if any (such as, in the motion of the planets, the varying distance from the sun, or the eccentricity* [which explains why do the distances vary] *arise from the concurrence of extrinsic causes* (Kepler 1609/1992, Chapter 38, pp. 404 – 405).

On the same page 405 he illustrated his opinion by obstacles which prevent rivers from descending *towards the centre of the earth*, and finally, on the next page, he concluded that *other causes are conjoined with the motive power from the sun* [affect their motion], cf. deviation from laws of nature (§ 1.2).

Kepler (1618 – 1621, 1620/1952, Book 4, pt. 3, § 1, p. 932) voiced his main statement in a later contribution:

If the celestial movements were the work of mind, as the ancients believed, then the conclusion that the routes of the planets are perfectly circular would be plausible. […] *But the celestial movements are* […] *the work of* […] *nature* […] *and this is not proved by anything more validly than by observation of the astronomers, who* […] *find that the elliptical figure of revolution is left in the real and very true movement of the planet.* […] *Because in addition to mind there was then need of natural and animal faculties* [which] *followed their own bent* […] [and] *did many things from material necessity. So it is not surprising if those faculties, which are mingled together, could not attain perfection completely. The ancients themselves admit that the routes of the planets are eccentric, which seems to be a much greater deformity than the ellipse.*

Or, more subtly: attempts to obey laws of nature which are, however, too complicated to follow, involve those same deviations.

3. Kant and Laplace

3.1. Kant. I do not know if or to what extent had Kant borrowed from Kepler, but in any case he held to external influences, – again to deviations or complications preventing obedience to laws of nature (§ 1.2):

The multitude of circumstances that participate in creating each natural situation, does not allow the preordained regularity to occur.

[Die Vielheit der Umstände, die an jeglicher Naturbeschaffenheit Anteil nehmen, eine abgemessene Regelmäßigkeit nicht verstattet] (Kant 1755/1910, 1. Hauptstück, p. 269).

Why are their [the planets'] *paths not perfectly circular? Is it not seen clearly enough, that the cause that established the paths of* *celestial bodies* […] *had been unable to achieve completely its goal?* […] *Do we not perceive here the usual method of nature, the invariable deflection of events from the preordained aim by various additional causes*?

[Woher sind ihre Umläufe nicht vollkommen zirkelrund? […] Ist es nicht klar einzusehen, dass diejenige Ursache welche die Laufbahne der Himmelkörper gestellet hat, […] es nicht völlig hat ausrichten können […]. Ist nicht das gewöhnliche Verfahren der Natur hieran zu erkennen, welches durch die Dazwischenkunst der verschiedenen Mitwirkungen allemal von der ganz abgemessenen Bestimmung abweichend gemacht wird?] (Kant 1755/1910, 8. Hauptstück, p. 337).

3.2. And now I turn to Laplace:

Had the Solar system been formed perfectly orderly, the orbits of the bodies composing it would have been circles whose planes coincide with the plane of the Solar equator. We can perceive however that the countless variations that should have existed in the temperatures and densities of the diverse parts of these grand masses gave rise to the eccentricities of their orbits and the deviations of their movement from the plane of that equator.

[Si le système solaire s'était formé avec une parfaite régularité, les orbites des corps qui le composent seraient des cercles, dont les plans, ainsi que ceux des divers équateurs et des anneaux, coïncideraient avec le plan de l'équateur solaire. Mais on conçoit que les variétés sans nombre qui ont dû exister dans la température et la densité des diverses parties de ces grandes masses ont produit les excentricités de leurs orbites, et les déviations de leurs mouvements du plan de cet équateur] (Laplace 1835/1884, Note 7, p. 504).

The causes mentioned by Laplace could have hardly be called external, but one of the main relevant explanations of randomness, deviation from the laws of nature (§ 1.2), persisted.

4. Newton

Newton theoretically proved that the Keplerian laws of planetary motion resulted from his law of universal gravitation. In my context, it is necessary to stress: it is generally known that he also established that the eccentricity of the orbit of a given planet was determined by the planet's initial velocity. For some greater values of that velocity the orbit will become parabolic (with its eccentricity ε equal to unity, not less than unity as in the case of ellipses), for other still greater values, hyperbolic (with $\varepsilon > 1$). And for a certain value of that velocity an elliptic orbit will become circular. And it is difficult to imagine that such changes do not occur gradually, that, consequently, the eccentricity does not vary continuously with the velocity. This discovery certainly does not contradict Newton's statement about perturbations (§ 1.4).

All these findings, as Newton proved, persisted for planets (not material points) having a regularly variable density. I believe that irregular variations of densities (but hardly temperatures) peculiar to a given planet (Laplace) could have only somewhat corrupted the eccentricity caused by its initial velocity and in any case Laplace did not provide any calculations.

5. Discussion

In spite of his formal denial of randomness, Kepler had at least sometimes actually acknowledged it. Whatever he could have thought, his laws did not explain the values of the eccentricities. But it really seems that Laplace (and Kant) were mistaken (Kepler was obviously ignorant of the law of universal gravitation). I am not sure that Kant had studied Newton attentively enough, but Laplace certainly did (only after 1776? And even after 1813?).

Witness finally Fourier's comment (1829, p. 379) on Laplace's *Exposition*: *it is an ingenious epitome of the principal discoveries*. And on the same page, discussing Laplace's *historical works* (to whose province the *Exposition* belonged):

If he writes the history of great astronomical discoveries, he becomes a model of elegance and precision. No leading fact ever escapes him. […] *Whatever he omits does not deserve to be cited.*

Did Fourier note Laplace's mistake? Or, was he also still ignorant of the real cause of eccentricities?

Newton had indeed explained why are the planetary paths eccentric, but did he eliminate chance? No, not at all! Indeed, a similar question remains about the planetary velocities: why are *they* different? I do not know whether this question was formulated earlier.

I have only touched on the general problem of the role of randomness in natural sciences and only allow myself one pertinent reference (out of several possible) to Maxwell (1873b, p. 274) which also shows that randomness is not at all banished from the system of the world:

The form and dimension of the orbits of the planets […] *are not determined by any law of nature, but depend upon a particular collocation of matter. The same is the case with respect to the size of the earth.*

I prefer to say: the particular arrangement of matter and velocities in the Solar system.

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V
Elementary exposition of Gauss' final justification of least squares

Legendre (1805) was the first to publish the principle of least squares (known to Gauss since 1795), but it was Gauss who introduced the method of least squares; he reasonably rejected his own first attempt (1809) and offered its final justification (1823b – 1828) based on the principle of maximum weight (minimal variance). I begin with a few words about Legendre and Laplace and continue with describing Gauss' final justification of least squares. It is extremely complicated, but modern authors removed this difficulty. My own exposition (§ 3) is quite elementary and, I think, methodically necessary.

1. Legendre and Laplace

1.1. Legendre. Here is his crucial statement (1805, pp. 72 – 73): *It is necessary that the extreme errors without regarding their signs be restricted between the shortest possible boundaries. ...*

His equations can be written as

$$a_i x + b_i y + ... + l_i = v_i,\ i = 1, 2, ..., n. \qquad (1)$$

The free terms l_i are the results of physically independent observations whose number, n, is larger than the number of the unknowns, k. The coefficients a_i, b_i, … are given by the appropriate theory, and the linearity is not restrictive since the approximate values of the unknowns can be calculated (for example, from any k equations). For equations appearing in practice no solution is possible and any set of $\hat{x}, \hat{y}, ...$ leading to reasonable residual free terms v_i is assumed as the solution.

The optimal approach which he applied was to make the sum of the squares of the errors the least possible. This approach, as stated, was wrong: actually, Legendre minimized the sum of the squares of the residual free terms of his equations. His first statement implies that the principle of least squares is at the same time the minimax principle

$$|v_{\max}| = \min$$

where the maximum allows for the appropriate magnitudes of all the equations, and the minimum is thought to cover any set of $\hat{x}, \hat{y}, ...$ Actually, as it is easy to prove, the minimax principle is tantamount to making minimal the sum of v_i^{2n} with $n \to \infty$.

1.2. Laplace. He is known to have non-rigorously proved several versions of the central limit theorem and, accordingly, presumed that the observations were numerous and that their errors were normally distributed (a later term). Then, he based the adjustment of observations on minimal absolute expectation of error, which meant that calculations were only practically possible for the normal distribution. Each of the two assumptions made his method of adjustment barely useful and Gauss (1821) criticized it. Laplace did sometimes apply the mean square error (the root of the sample variance) as his criterion, but on the whole he led French mathematicians including Poisson away from Gauss; this was made easier by the priority strife between Legendre and Gauss.

2. Gauss

2.1. Prior to 1805. There is no direct proof that he applied the principle of least squares before 1805. Gerardy (1977, p. 19, Note 16) came close to achieving this, but regrettably he concentrated on elementary geodetic calculations. On the other hand, it is impossible to refute Gauss' claim of having applied it. First, Gauss made many mistakes in his computations (Maenchen 1918/1930, p. 65ff); one example is in § 2.2.2-1 below. Second, he could have assigned differing weights to his observations; third, he (1809, § 185) allowed himself some deviation from strict procedure; fourth and last, he could have well mostly applied least squares for trial computations unknown to us.

Add to this that his contemporaries including Laplace (1812/1886, p. 353) believed Gauss and that he informed his friends and colleagues about his innovation. Among those were Bessel (1832, p. 27), Wolfgang Bolyai (Sartorius von Waltershausen 1856/1965, p. 43), the father of János Bolyai, one of the discoverers of the non-Euclidean geometry, and the astronomer Olbers.

There still exists a misunderstanding about the last-mentioned. The main point is this: in 1812 Olbers agreed to confirm Gauss in that he had indeed come to know the principle of least squares from Gauss before 1805, but he only publicly stated that in 1816. However, the *Catalogue of Scientific Literature* published by the Royal Society lists, in its proper volume, Olbers' contributions, and it is seen that during 1812 – 1815 he did not publish anything suitable for inserting such a statement.

2.2. The year 1823

2.2.1. General remarks. In § 2 (with an explanation of a term in § 1) Gauss restricted his investigation by excluding systematic errors from consideration. He repeated this point in § 17 and promised to present a new investigation of the case in which systematic errors are not totally excluded, but he never fulfilled his intention.

Then, in § 18 Gauss offered his definition, although not quite formal, of independent functions of observations: they should not have contained common observations. In § 19 he specified that those functions were linear; otherwise his statement would have contradicted the Student – Fisher theorem on the independence of the sample variance and the arithmetic mean.

Gauss (§ 6) introduced his measure of precision (the variance, as it is now called). In his letter to Bessel of 1839, he (W-8, pp. 146 – 147) stressed that an integral measure of precision was preferable to a local measure. In the same § 6 he indicated that the quadratic function was the simplest [from integral measures] and in his preliminary report he (1821/1887, p. 192) noted that his choice was connected with other advantages but did not elaborate. I leave it at that. At the end of § 17 Gauss somewhat elliptically explained that minimal variance was his criterion for adjusting observations.

The main body of Gauss (1823b) is extremely difficult to read which had undoubtedly been one of the reasons for numerous textbook authors to discuss Gauss' first substantiation of least squares (1809) rather than the second one. Those wishing to acquaint themselves with that main body without leaving aside his deliberations can consult Helmert (1972). Modern exposition is provided, for example, by Kolmogorov (1946) and Hald (1998, pp. 471 – 475).

2.2.2. The sample variance. Then, in § 38, Gauss derived his celebrated formula for the sample variance, as it is now called:

$$\sigma = \sqrt{\frac{[vv]}{n-k}} \tag{2}$$

where, in Gauss' notation, $[vv]$ is the sum of the squares of v_i. More precisely, Gauss calculated the expectation of σ and had to assume that σ itself was equal to it. The reader can find the derivation in many sources, for example Helmert (1872/1924, pp. 102 – 104) and Kolmogorov (1946).

2.2.2-1. The precision of the sample variance. Gauss (§§ 39 – 40) derived the variance of σ^2. His direct approach was somewhat laborious but easy to follow and his final formula provided the boundaries for σ^2. Additionally, he remarked that for the normal distribution

$$\text{var}\,\sigma^2 = \frac{2\sigma^4}{n-k}. \tag{3}$$

One of the boundaries was wrong; Helmert (1904) corrected that mistake and Kolmogorov et al (1947) independently derived the same formula as Helmert did:

$$\frac{\nu_4 - s^4}{n-k} < \text{var}\,\sigma^2 < \frac{\nu_4 - s^4}{n-k} + \frac{k}{n}\cdot\frac{3s^4 - \nu^4}{n-k}$$

for $v^4 - 3s^4 < 0$ with a similar formula for the alternative. Here, $s^2 = E\sigma^2$. In a companion paper, Maltzev (1947) proved that both inequalities can be understood as being conditional.

2.2.2-2. Unbiasedness. At least in geodesy, the estimator of precision is σ rather than σ^2 and, unlike σ^2, it is biased. Anyway, how important is unbiasedness? It seems that bias is now somewhat tolerated (Sprott 1978, p. 194) and in any case unbiased estimates sometimes just do not exist.

An additional consideration is interesting. Czuber (1891, p. 460) discussed the problem of bias with Helmert, and they concluded that the main point was not bias itself, but the relative value of $\text{var}\sigma^2/\sigma^2$. Eddington (1933, p. 280) independently stated the same.

For a biased estimate of the sample variance, i. e., for $k = 0$ instead of $k = 1$, Cramér (1946, § 27.4) derived the formula

$$\text{var}\,\sigma^2 = \frac{\mu_4 - \mu_2^2}{n} - \frac{2(\mu_4 - 2\mu_2^2)}{n^2} + \frac{\mu_4 - 3\mu_2^2}{n^3}$$

in terms of the central moments μ_2 and μ_4. In case of normality he (Ibidem) additionally offered the formula

$$\operatorname{var}\sigma^2 = \frac{2(n-1)}{n^2}\sigma^4.$$

2.2.2-3. Application of the formula. As noted in § 2.2.1, Gauss did not consider systematic errors. In particular, this meant that formula (2) was practically inadequate, and Gauss understood it perfectly well. When performing geodetic work, he measured each angle as many times as he felt necessary, see W-9, pp. 278 – 281 or Schreiber (1879, p. 141). In at least three letters Gauss recommended, when the number of observations was not large, to derive a single value of σ^2 for several stations. These letters were: in 1821, to Bessel, see Gauss (1880/1975, p. 382); and in 1844 and 1847, to Gerling (1927/1975, pp. 687 and 744). At least once Laplace acted the same way even earlier, see Supplement No. 3 of ca. 1819 to his treatise (1812/1886) and a modern author (Ku 1967/1969, p. 309) expressed the same opinion.

In spite of the above, geodesists have been applying formula (2), although only after completing work on a chain of triangulation. It is then possible to allow for the closures of the triangles, for the discrepancies between the baselines situated at the ends of the chain, and between the astronomically fixed end lines of the chain. In other words, applying that formula only after having revealed the influence of systematic errors as much as it was possible. Supplemented with baselines and astronomical observations, a chain is to the most possible extent independent (in Gauss' sense, see § 2.2.1) of the neighbouring chains.

2.2.2-4. Criticism. Bertrand translated Gauss' contributions on the theory of errors and least squares into French (Gauss 1855). Note that Gauss, at least by the end of his life, agreed to have some of his work appearing in French; previously, owing to political reasons, he refused to publish anything in that language. Gauss died the same year, 1855, and Bertrand (1855) made known that he, Gauss, had no time for really studying the prepared translation.

Many years later Bertrand (1888) criticized the Gauss formula (2). Tacitly assuming the normal distribution, he provided an example in which his own estimate of σ^2 was less than that provided by Gauss. He forgot, however, that formula (2) provided an unbiased estimate whereas his own estimate was biased. Then, he calculated σ^2 forgetting formula (3). It was this episode that led Czuber to the discussion described in § 2.2.2-2.

Later events seem to indicate that the Gaussian theory of errors remained for a long time almost forgotten. Chebyshev (1880/1936, p. 249) stated that *recently, some authors had begun to apply* formula (2). More generally, at least up to the middle of the 20th century statisticians of the ordinary rank did not know Gauss' second justification of least squares (Campbell 1928; Eisenhart 1964, p. 24).

In other countries the situation had been likely about the same.

Indeed, Fisher (1925/1990, p. 260) thought that the method of least squares was a *special application of the method of maximal likelihood* which was only correct for the first justification of the method. And Poincaré (1896/1912, p. 188) stated that Gauss' rejection of his own first justification of the method was *assez étrange*.

In Russia, however, the situation was somewhat different since Markov, citing Gauss, resolutely upheld the second substantiation. At the same time he stated that the method did not possess any optimal properties and thus contradicted himself: such methods do not require any substantiation. See Sheynin (2006, pp. 80 – 81).

3. Conclusion: an alternative justification of the method of least squares

After proving formula (2), Kolmogorov (1946) remarked in passing that it was only a definition of σ. Yes, if the number of the degrees of freedom is correctly allowed for. As I understand it, the formula seems plausible, but the proof is still required; after that, it can be interpreted as that definition.

Many authors beginning with Gauss had provided the proof which is not difficult. The necessary restrictions are: linearity of the equations (1), independence of their free terms (the results of observation), and the unbiasedness of the estimators $\hat{x}, \hat{y},...$ The main point, however, is that the proof does not depend on the condition of least squares. On the contrary, this condition can now be introduced at once since it means minimum variance.

The formulas derived by Gauss for constructing and solving the normal equations and calculation of the weights of $\hat{x}, \hat{y},...$ and of their linear functions will still be useful. However, Gauss had actually provided two justifications (of which I only left the second one), but why did not he even hint at this fact? I can only quote Kronecker (1901, p. 42) and Stewart (1995, p. 235):

The method of exposition in the "Disquisitiones [Arithmeticae", 1801] *as in his works in general is Euclidean. He formulates and proves theorems and diligently gets rid of all the traces of his train of thoughts which led him to his results. This dogmatic form was certainly the reason for his works remaining for so long incomprehensible.*

Gauss can be as enigmatic to us as he was to his contemporaries.

Gauss himself actually said so. His eminent biographer, Sartorius von Waltershausen (1856/1965, p. 82) testified: He had *used to say* that, after constructing a good building, the *scaffolding* should not be seen. And he had often remarked that his method of description *strongly hindered* readers *less experienced* in mathematics.

Finally, I note Gauss' words (letter to W. Olbers 30.7.1806): *Meine Wahlspruch* [motto] *ist aut Caesar, aut nihil.*

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