Studies

in the History of Statistics and Probability

Vol. 11. Ladislaus von Bortkiewicz

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Berlin

2018

Contents (with page numbers)

Introduction

This book consists of two contributions by Bortkiewicz in which, in Part 1, he studies the work of Lexis and Keynes whereas Part 2 is a collection of the obituaries of Bortkiewicz. Note that Russian authors called him by his initial Russian name, Vladislav Iosifovich Bortkevich.

I refer to the items of Part 1 by notation [Pt. 1, n] with n = i or ii, and to those of Part 2, simply by [n] with n = i, ii, …, xix.

Many authors in Pt. 2 praised Bortkiewicz as a statistician and Woytinsky (1961, pp. 451–452) remarked that

In Germany, he was called the Pope of statistics. […] *The publishers have stopped asking* [him] *to review their books* [because of his deep and impartial response]. […] [He was] *probably the best statistician in Europe*.

Bortkiewicz' critical review of Pareto (1898/15) was lamely arranged. Chuprov (Bortkevich & Chuprov 2005, Letter 35 of 1898) indicated this circumstance, but Bortkiewicz' answer in the next letter was of no consequence. Quite a few authors maintained that his works were difficult to understand. No wonder that Winkler [xix] received his letter stating that he expects to have five readers of one of his contributions, nor is it surprising that his works became all but forgotten. So much for his statistics!

Bortkiewicz had no mathematical education although many non-mathematical authors in Pt. 2 praised him as such and an applied mathematician he certainly became. Keynes (quoted, for example, by Gumbel [x, Supplement]) called his *mathematical argument often brilliant*. See however [xv].

For many decades his law of small numbers (1898/14) had remained the talk of the town but for more than a century now it is only recognized as an important and timely rediscovery of an essential result of Poisson. More: I (2008) noted that Bortkiewicz had tacitly introduced there a coefficient of dispersion differing from the Lexian coefficient. It had the form of the expectation of the ratio of two dependent random variables such as E /E , which he had not noted and wrongly claimed that it equalled $E(\ /\ )$. Delicate Chuprov privately noted this whereas Bortkiewicz unfoundedly stated that in any case that equality held approximately. The first to deny that law was Whittaker (1914) and Kolmogorov (1954).

Bortkiewicz (1894 – 1996/8, p. 661) thought that the difference between objective and subjective probability was insignificant *as is generally recognized*. The general acknowledgement is doubtful and the main statement patently mistaken. I (2017, § 8.1) provided an appropriate example, and many more are possible. And Chuprov noted that

The difference nevertheless exists, and is not of small importance.

His remark was on the margin of his copy of Bortkiewicz's work. Chetverikov (1968, pp. 55 – 137) translated that work from the mentioned copy and inserted Chuprov's remark on p. 74. I mention this circumstance since the definition of probability is considered in [Pt1, i]. This contribution is defective since quotations are provided without any references, some statements are incomprehensible, Lexis did not change in time at all, and (what is true about the work of Bortkiewicz in general) it is difficult to separate the described scientist, Lexis, from his reviewer, Bortkiewicz.

Contrary to Chuprov Bortkiewicz barely saw anything positive in the work of the Biometric school, and he, an outstanding economist (see Zagoroff [vii]), should have noticed the forgotten eminent economist Walter Rathenau.

Two more points. First, mathematical statistics had been repeatedly mentioned by authors in Pt. 2. However, we now attribute its birth to Fisher and Gosset (Student). Second, Bortkiewicz repeatedly mentioned moral statistics but he actually meant only suicides; when touching on the work of Quetelet he had not discussed that subject.

See a very short description of the work of Bortkiewicz in Sheynin (2017, § 15.1.2).

I inserted a bibliography of the works of Bortkiewicz at the very end of this book. It is almost complete and includes many of his reviews. I refer throughout to this bibliography by additionally mentioning the appropriate number there; thus, (1908/n) is a contribution published in 1908 and numbered n in that bibliography. A few items added at the last moment had not been included in that bibliography and have *a, b, …* instead of the missing n attached to them; thus, (1931a). Both items in Pt. 1 are supplemented by their own bibliographies but all items in Pt. 2 have a joint bibliography (at the end of that part).

Notation **S, G**, n means that the source in question is available in English in a downloadable file n on my website www.sheynin.de I am proud to add that Google is diligently copying my website, see Oscar Sheynin, Home

The mentioned sources are listed in the bibliography to Pt. 2.

Part 1

Ladislaus von Bortkiewicz

The Theory of Population and Moral Statistics according to Lexis

[1] I omit the not really interesting long introductory passage.

[2] Mass phenomena consist of single cases with which statistics cannot deal.

Therefore, the highest scientific form in which it is able to study its material is the pattern of the theory of probability. (Lexis 1903 [p. 241]).

Indeed, the viewpoint of that theory is peculiar in that it only considers definite initial and final states and in principle avoids studies of the causes which lead to the latter from the former[1]. In the theory of probability, *cause* has a special meaning absolutely different from its usual understanding. According to Lexis, it is

The condition which involves some phenomenon not certainly, but only with some probability.

Or, as I would add, a cause better determines a plurality of conditions which heighten or lower the appropriate probability.

Probability theory thus serves for estimating the final aims of statistical studies. This, however, does not wholly determine the aim of empirical social sciences. They have some advantage over natural sciences: they can directly enter the inner connection between external phenomena and in addition can reduce human acts to their motives[2].

And so, empirical social sciences can be perceived in a second possible form which assumes those individual motives as the highest and really significant notion. They manifest themselves in social interactions and this second form is especially noticeable in economics since the general essence and character of the motives of action become understandable there by psychological observations if not in each separate case. […]

Since the real motives remain constant, the external phenomena will be repeated, and this conclusion, as Lexis assumes, very much differs from induction in natural science.

[3] Until now, we have not discussed the moral assessment of interrelations which constitute social mass phenomena. However, when studying the relations between what exists in social reality and what ought to exist there, a third possible form of outlook on the social science which Lexis calls *empirical social ethics* becomes evident. It is not a science in the normative sense, it

borrows those views about what ought to be from general notions according to which we should dissemble, group and compare facts[3].

[4] We return to statistics. The indication made by Lexis about the application of the pattern of probability theory is not at all new. Even the first representatives of the scientific population statistics had been guided by the idea that there exists a real analogy between mass statistical phenomena and games of chance[4]. This analogy is especially clearly revealed in that certain statistical numerical relations only insignificantly differing in time (for example, from one year to the next one) occur when the field of observations is sufficiently extended. This behaviour of statistical numbers is similar to the [changes of the] results of games of chance.

Under the same condition of sufficiently long series of games of chance those results do not reveal any noticeable changes from one series of trials to another. In those games we may beforehand establish the exact numerical values near which their separate series will fluctuate. And this value is called mathematical probability of the appropriate event.

The stability of the results of the games in different series is caused by the possibility of considering the result of each expressed by the ratio of the numbers of the appropriate cases as an approximate value of the suitable mathematical probability. The more trials there are in each series the less is the quantitative deviations of those ratios from the main mathematical probability.

We may thus popularly explain the law of large numbers. We say that experience corroborates this theorem which belongs to the theory of probability if the empirically derived ratios precisely enough coincide with the a priori established probability. In statistics, however, something else takes place: it is in principle forbidden to establish probabilities beforehand. From the derived numerical ratios we may only conclude about the value of the mathematical probability which underlies them.

And so, we may only discuss the corroboration of the law of large numbers by experience in the sense of the coincidence of these ratios[5].

[5] But how close to reality does this coincidence happen? Exactly to this problem, which the previous authors and especially the classics of probability theory had been ignoring[6], Lexis turned his attention. He showed how it should be methodically solved. Here, indeed, is the new and independent contained in his viewpoint on the application of the theory of probability to statistics.

First of all, for checking the stability of statistical series a theoretically justified measure is established. It is similar to the measure offered by the law of large numbers if only its popular definition provided above is replaced by an exact mathematical formulation. Indeed, there exists a precisely established probability-theoretic relation between the length of the interval within which fluctuate the empirical values of the mathematical probability and the number of trials or observations which underlie those values.

It is therefore possible to establish beforehand the mean value of the deviations of the separate terms of a statistical series from the mean value for the whole series and to some extent determine how these deviations are distributed according to their values.

We only have to know the mean values of the ratios and of the number of observations. The essence or character of the mass phenomenon is of no consequence. The thus determined theoretical mean deviation (the distribution of the deviations of their values is temporarily put aside) for each given statistical series can be compared with the actually observed mean deviations.

Formally speaking, there are three possible cases which we have to take into account: the actual mean deviation is either approximately equal, or smaller or larger than its corresponding theoretical value. According to Lexis they characterize normal, super- or subnormal stability.

[6] Since a theoretical measure for the investigation of the stability of statistical ratios is established, the investigation itself can be carried out. At first, Lexis studied the sex ratio at birth. He issued from the monthly data covering two years from separate Prussian primary administrative districts and thus obtained 24 terms for each district. There were only a few exceptional cases from an acceptable agreement of the empirical mean deviations and the theoretical values. For the plurality of all the territory the criterion of normal stability was all the more satisfied since here the adjustment of the results of separate districts took place.

Lexis additionally considered the distribution of the separate deviations according to their values and here also found out a very good agreement between theory and experience. He obtained similar results for England and France, this time for yearly births, separately for the counties/départements.

Lexis concluded that the sex ratio at birth belonged to those statistical magnitudes which (if at least restricted to a certain time period and geographical region) should be considered as *random* modifications of a typical normal value. This peculiarity ought to be understood in its precise mathematical strictness rather than in

the usual vague sense: the typical normal value is the genuine mean value in the sense of the theory of probability. The probability of a certain deviation from the mean value is expressed by some analytical function[7]. In other words, changes in the sex ratio at birth should be expressed by the pattern of probability theory:

Those 816 numbers from 34 [Prussian] *districts* [34·24] *will be distributed approximately as black and white balls placed in an urn in the ratio of 1063:1000, when they are extracted 24 times with replacement*[8].

If we desire to picture this phenomenon from the physiological angle, Lexis had mentioned the simplest answer:

The very numerous <u>non-impregnated</u> *embryos in the ovaries of all females are predestined for one or the other sex. So as to name a precise sketchy assumption, the ratio of the male to the female embryos is the same for all females*[9]. *The analogy with the urn is now clear: each impregnation should be compared with an extraction of a black or white ball from the same urn*.

However, the assumption of a *constant* ratio for all females is not really needed:

Large individual differences between the districts can exist if only the <u>mean ratios</u> *of the districts* (*at least for some period*) *remain approximately constant. Fluctuations of these district ratios from month to month or from year to year might take place if only they are of the random essence*.

[7] Quite similar is the ratio of male and female deaths for children up to ca. five years of age: there also appears an approximately normal stability. And so, according to Lexis, there exists a constant totality of conditions leading to the prevalence of the deaths of boys but no external hindrances apparently exist for somewhat changing, from time to time, the mortality of either sex. We should rather assume that *because of organic* [physiological] *causes the boys' mean resistance to death is incessantly weaker in a fixed ratio than the girls'*.

Quite otherwise is the situation with the stability of this statistical magnitude in other age groups. Indeed, the mean deviation (?) is often many times larger than its expected theoretical value so that we ought to conclude that in these cases essentially variable causes are vigorously acting and specifically influence either one or the other sex. And actually, as Lexis believes, the conditions of life and the accompanying dangers are so different for the sexes that those changes can occur independently.

For those other groups a distinctly expressed subnormal stability takes place and for the ratios concerning population and moral statistics this is the rule.

The fluctuations of the observed relative numbers from year to year, even if not seemingly essential, barely agree with the norm established by the theory of probability[10]. And it is indicative to the highest degree that such deviations are larger when the number of observations is large and, on the contrary, are expressed much weaker when that number is smaller[11].

Yes, when decreasing that number by specifying the contents of the statistical materials or of their space or time extent, we can achieve a very pronounced subnormal and sometimes almost normal stability. This empirically discovered fact and its all-embracing effective theoretical explanation is not the least merit of Lexis.

[8] The usual pattern of the theory of probability which is being applied to statistical series of relative numbers is the pattern of an invariable probability. It assumes that all the terms of a series are based on one and the same probability [of the studied event] so that all of them because of the law of large numbers are its approximate values.

Lexis had modified that pattern. He assumed that the abstract or theoretical probability can change so that each term of a series becomes characterized by its own special probability as distinct from the mean abstract or theoretical probability for the whole series.

This new pattern should obviously allow more essential fluctuations. Indeed, the deviations of the separate terms from their mean value which can be supposed to be an approximate value of the appropriate mean probability are caused not only by the play of random causes (which first of all lead to the deviations between the separate terms of the series and the appropriate special probabilities) but by the inequalities of these special probabilities as well.

The action of random causes is expressed, in the words of Lexis, by the *normal random component of fluctuation*. It can be precisely enough determined theoretically [see above the statement about its play]. The second cause (the changes of the special probabilities), again in the words of Lexis, is expressed by the *physical component of fluctuation*.

It is assumed here that these changes are not caused by the combination of chances, but can be a reflection of the arbitrary changes of the main complex of conditions in time. According to a certain mathematical formula[12] these components taken together lead to the *entire fluctuation* which is determined by

direct observations of the deviations of the separate terms from their mean. That same formula also approximately establishes *the physical component of the deviations*.

The first component depends on the number of observations and decreases with their increase whereas in general the second component obviously does not depend on their number. It follows that, given a comparatively large number of observations of some mass phenomenon, the second component prevails over the first and vice versa when the number of observations is small.

If during the time of observation the special probabilities only change within close boundaries and the number of observations is moderate, the physical component will be barely noticeable whereas the first component will be almost equal to the entire fluctuation. To put it otherwise, the course of the observed relative numbers will precisely enough correspond to the hypothesis of a constant probability and the stability will be almost normal, although, strictly speaking, this will only be outwardly apparent.

A coincidence of the theory of probability and statistical experience under the usual pattern of a constant probability may thus be expected much more rapidly because of, so to say, inner necessity when the number of observations is small rather than large. However, it does not at all follow as an axiom of statistical investigations that we should keep to small numbers of observation. On the contrary, it is mostly more important to establish the physical component of fluctuation which is concealed when the number of observations is moderate. Indeed, its numerical expression is a measure of the temporal changes of the underlying probability independent from the action of *random causes*.

And we ought to turn our attention to the possible temporal heightening or lowering of that probability. The value and the direction of such changes, since we discuss the elimination of chances, are determined the more precisely the more numerous are the observations which underlie the appropriate relative numbers.

The decrease of the number of observations is thus not needed at all although when bearing in mind the general theoretical interest the study of statistical series composed of a small number of observations which lead to an approximately normal stability possibly makes sense. Such studies will empirically prove that

The theoretical law of fluctuation based on the combination of chances rather than on necessity plays the main role in the [changes] *of the numerical ratios*.

[9] The explanation of the occurring stability does not require any *inner adjusting connections* between the elements of the mass phenomenon. This will only be necessary if the measure of fluctuation derived from observations is *smaller* than that measure established according to the theoretical pattern of a constant probability. A similar fact would have been a result in a game of chance occurring with an absolutely unlikely constancy and regularity.

Then we will have to admit that the seemingly separate and isolated results are not independent either mutually or from the end numerical result which takes place after separate trials with urns [with replacement of each extracted ball] *or in the roulette game. In other words, such an upper bound of a superior (überschreitende) stability of relative numbers will indicate that the studied mass phenomenon is either united internally or obeys a certain regulating interference or a certain norm. Such phenomena more or less belong to the area of a regular arrangement or of a guiding law.*

And a supernormal stability was indeed never revealed for such phenomena which belong to population or moral statistics and are not based on any apparent direction. Normal stability was the maximal. It was proved that for those phenomena the fluctuations are restricted to wider or in any case to the same boundaries as the results of many series of extractions of black and white balls from an urn. Therefore, as Lexis believes, the usual former excessive wonder about the comparative permanency of series of relative numbers in population and moral statistics can be diminished.

There remains however the intention to acquire some understanding of the real physical meaning of that [constant] ratio, the *underlying probability*. As Lexis says,

In itself, the purely mathematical probability has no connection with reality and only gives rise to the combinatorial problems with an <u>assumed</u> equal possibility of the <u>favourable</u> and <u>unfavourable</u> cases.

The most essential in the investigations which Lexis applied to show why the notion of mathematical probability assumes a sense and meaning for statistical reality, can be approximately rendered in the following way.

First of all, we bear in mind the similar area of games of chance and imagine that an infinite set of *possibilities* which led to a certain result is connected with the sum of all the possibilities by a definite numerical ratio and thus we understand that ratio as the probability of the result. And it is likely that in a sufficiently long game such arithmetic ratio is revealed. The result of a game

is thus reduced to its *general condition* so that the specific causes leading to separate cases which compose the general result are as though neutralized.

[10] Mass phenomena in the field of population and moral statistics ought to be considered quite similarly. Here also we ought to abstract ourselves from the individual peculiarities of separate cases and perceive a statistical result as caused by general and to some extent super-individual factors. Among these latter the essence of man is decisively important and, in moral statistics, his state of mind as well. Not however the essence and the state of mind of a certain individual but of people in general, of the *abstract man*, as Lexis expressed himself.

And we certainly ought to keep to the same social notion of man rather than to his natural-scientific essence since the latter originates from the former because of the peculiarity of the milieu.

Suppose that such a perception is corroborated by the coincidence of the statistical experience and the [results of the] theory of probability. Then, according to Lexis, the main point here is that separate people who at different times can find themselves in a certain condition are in this respect to some extent interchangeable. People belonging to different generations can in some respect be combined up to a certain extent as being interchangeable.

According to Lexis, actions of men in themselves

Are mainly peculiar since they are determined in an uncountable plurality of ways by the character and energy of the excited will and competence of single individuals.

The indicated acts are therefore completely beyond the boundaries of natural regularities[13]. This, however, does not at all exclude that

People considered in multitude act and repeat their actions regularly since it is indeed possible that many coincident causes for selecting the aims are decisive in a certain way and exist for a long time.

The interchangeability of people follows and to a still greater extent does away with individuality. I consider this concept quite accurate and fruitful and another notion gets along well with it: the notion that groups of people can be distributed according to physical, spiritual, economic and social indicators. This serves as the foundation of demographic and moral-statistical studies. For such groups there exist numerically different probabilities of the occurrence of some events. For some of them, however, these probabilities can be close to unity whereas for the others they constitute decreasing sequences whose terms finally become

vanishingly low. And it is often possible to add new groups for which some event is undoubtedly impossible, for example if the yearly number of births is compared with the total number of people[14].

We may suppose that the separation into groups is so complete that they are homogeneous, i. e., that any further separation into groups which have different probabilities of the appropriate event becomes impossible. We may also say that such *elementary* groups, as I would like to call them[15], are inaccessible for statistical experience. Indeed, even when materials of population and moral statistics are specialized to the highest possible extent, we always have to consider far from elementary groups.

In the first place since the elementary group is therefore only theoretically important, we only ought to adopt the *interchangeability of separate individuals* to the same extent as in similar elementary groups.

For a better understanding of the really achieved observation of the comparative stability of relative statistical numbers we have to additionally consider whether there exists approximately the same composition of the studied group from elementary or homogeneous parts. Such a composition cannot be directly established and we may only assume that their changes in time are generally the same as they are for the statistically established similar groups. […]

Lexis indicates that in the first place the distribution of the population according to sex and age groups mainly occurs owing to the natural regularities and can therefore only change gradually. This stability of the biological constitution of the population is the main requirement for the relative firmness of the social and economic conditions and is mainly expressed by the distribution of properties and incomes and in the breakdown of the population according to professions and occupations. Here indeed is Lexis:

Sufficiently large social groups differing in those indications, in spite of the incessant changes in their composition, are only subject to slow changes which are mostly somewhat parallel to the increase in the population. This occurs simply because of the natural duration of the economic realty and connections whereas exceptions are only allowed by serious destructive catastrophes.

And so, the appropriate constancy of the correlation between the groups is once more explained by the regular changes of the states, and Lexis himself admits that that regularity is a primary phenomenon.

We may still imagine some changes of states in homogeneous groups so that all the theoretical construction which better

represents the stability of statistical ratios in heterogeneous groups is not reduced to a vicious circle.

After all, Lexis allows the derivation of statistical regularities by a certain interchangeability of people and a certain constancy of social groups. However, this viewpoint does not at all explain the details about the appearance of the stability of statistical ratios as dominating laws but at least it hampers the tentative attribution of stability to those *laws* whereas actually it is only the result of the intricate diversity of phenomena.

[11] Until now it was in general assumed that the statistical ratios whose stability is studied from the viewpoint of the theory of probability can be purely formally considered as expressions of mathematical probabilities[16]. Here, we may add that the denominators of the appropriate formulas are the numbers of the observed cases of some kind, and the numerators, those of the numbers, in which some event had occurred or a definite indication was established. The numerators thus ought to come from the denominators. And such ratios testify either about some real process or about a purely logical isolation of a partial group from a general according to some viewpoint. In these cases Lexis [1877, p. 4] mentions *genetic* and *analytical* relative numbers respectively.

A theoretical problem appears all by itself: show how to apply the given statistical material for calculating the numbers which can be considered genetic and moreover how to establish principles for the grouping of data to prove the possibility of calculating one or another genetic relative number. Especially in more remote times mistakes are known to have been often made about such calculations. Statistical materials which had not been genetic were thus labelled.

In the 1860s, K. Becker [somewhat later], Knapp, Zeuner and others predicted that that careless practice which in the first place concerned statistics of mortality will be specified[17]. The two last-mentioned authors justified the considerations about the methods of calculating mortality by a strict systematic and quite general study of the mathematical connections which exist between different in time and age groups of the living and the died. This foundation of the theory of calculating mortality or, as it can be called, of *the formal theory of population* Lexis is now describing by an original graphical construction. It ensures greater clarity and indicates which groups of the died and the living ought to be compared with each other to establish the most precise possible value of the probability of death, i. e., the most important for the statistics of mortality genetic relative number.

[12] A special difficulty which appears when calculating any genetic ratios is that during the time of observation their denominators change, and not because of such phenomena whose combination composes the numerators; for example, because of mortality due to the outflow and inflow of the population. Lexis thoroughly studied how to subject this circumstance to calculation. Just as Becker did, he derived the appropriate approximate formula without applying the calculus of infinitesimals which would have been proper and although exactly here it more promptly led to the desired aim. In the preliminary note to his book he explained that he thus intended to retain completely the elementary character of the exposition.

But still, the treatment of the materials of the statistics of mortality is not restricted to the establishment of the probabilities of death. It is also required to issue from them and derive the order of extinction. Lexis included here the most important points and touched on the possibility of achieving this aim without calculating the probabilities of death.

He also generalized the exposition of the order of extinction on other mass phenomena. He considered the life of a group of people from birth to its complete extinction which was observed not only with respect to the cases of death, but also when taking account of the instances of marriage, death of spouses, births given by women, etc. It is required, as Lexis formulated it, to establish the *demographic path of life* of the group.

A complete observation of a real generation would have required about a hundred years,

So that that path can only be established by <u>calculating</u> it for an ideal generation and presuming that the various changes of the states in each age group are occurring just as they are now.

All this construction obviously leads to a satisfactory and scientifically significant result if these probabilities manifest some stability. Only this condition secures a description of a *typical* phenomenon not with respect to the changes of states which occur under the same circumstances for *all* people, but for *abstractly studied people* having certain probabilities.

According to Lexis an abstract man is not characterized by any *certain* properties; in each respect he manifests with definite probabilities contrary properties. This is how the abstract man differs from the average man of Quetelet and becomes so to say his revised and improved edition[18].

Lexis considers the demographic path of life of abstract people as the *natural guiding star* for a satisfactory characteristic of the studied ratios. This however does not exclude the possibility of their description in the usual way by various relative numbers

from which the demographic path of life is not derivable. Here he mentions in particular the so-called coefficients of death and those adjoined to them other *coefficients of change*.

They appear when

The number of yearly changes of states of a certain kind in some age group is divided by the mean number of those who had experienced them.

The thus obtained relative numbers

Are not at all the probabilities of the change of states during a year or a finite interval. They appear as a series of an infinite set of infinitely low probabilities which during the period of observation indicate that the observed people will experience the appropriate change during the next infinitely short interval of time[19].

In accord with the method of their calculation the coefficients of change do not yield to a further probability-theoretic treatment similar to the study of the stability of statistical series.

[13] It is otherwise with the relative numbers which by themselves are not either genetic or analytic and can therefore be considered not as approximate final probabilities but rather as approximate values of their functions.

A theoretical measure of the fluctuations of such relative numbers, for example of the ratios of boys and girls among the newborn, can be determined by the known rules of the theory of probability. This is especially true for the statistical mean values when they are thought to be composed of series of separate values having differing probabilities. In such a way, i. e., as fluctuations of relative numbers which appear as probabilities or their functions, we can determine the most important in demography anthropometric magnitudes and the yearly fluctuations experienced by their mean values.

It is necessary to compare the actual stability of these means with their expected values. Lexis has no such studies although possibly he compared those means in another connection with the theory of probability. Exactly he, like Quetelet before him, imagined the functional structure of mean values and attempted to subject it to the general mathematical formula, to the so-called Gaussian law of error. To this theme belongs his theory of the *normal age at death* which, as it can certainly be said, became a general possession of [the statistical] science[20]. It is therefore permissible to dwell on this theory.

[14] When placing Lexis in the history of the development of the general ideas of population and moral statistics by allowing for all the stated above, the most important is to indicate in the first place his attitude towards the classics of the theory of

probability, then towards Quetelet, and finally with respect to the dominating views held by modern statistics.

Just as Laplace and Poisson[21], Lexis imagined that relative numbers in statistics are the approximate values of the underlying mathematical probabilities or of their functions so that attention should be directed to the deviations of the former from the latter. But towards what aim? For Laplace and Poisson it was for establishing the degree of precision of statistical magnitudes, i. e., of the final conclusion, of the conjectural (tentative) reckoning. The aim thus formulated for the theory of probability was to protect statistics from the mistake of judging by issuing from an inadequate number of observations. It was necessary to make it possible for statistics to distinguish by definite formulas of the theory of probability more reliable judgements from the less reliable.

For Lexis, this aim of the theory of probability is placed far in the background. He says:

The only aim of applying the theory of probability to demography and moral statistics is, to offer, on the one hand, an understandable pattern for breaking down the cases, and, on the other hand, to provide a measure for the stability of statistical relative numbers.

Unlike Bienaymé and Cournot, and first of all of Lexis, the second aim did not interest the classics at all[22]. However, exactly that aim convinced him that the pattern of a constant probability, on which the determination of the precision of statistical results in the Laplacean sense had been necessarily based, was only in rarest cases suitable for mass phenomena in human societies. It followed that such a determination of precision should not in general be applied for predicting the width of the interval within which the statistical numbers will be restricted. That determination therefore to an essential extent loses practical value and Lexis had not attached any special weight to it.

Then, a clearly expressed distinction between Lexis and especially Laplace manifests itself in that the latter had not completely allowed for the formal conditions (for those which are the foundation of the method of calculating statistical magnitudes). Among such conditions we may mention the possibility of representing a relative number as an approximate value of some mathematical probability[23]. Lexis however thoroughly took them into consideration. When deriving appropriate formulas Laplace had not at all allowed for the possibility that the values of mathematical probability for partial groups can differ, whereas Poisson took this circumstance into consideration not as thoroughly as Lexis did[24].

It is clear however that the points of contact and the differences between the representatives of the theory of probability on the one hand and Lexis on the other had concerned not all of his theory but mainly its specifically mathematical part. Since here especially Laplace went beyond mathematical boundaries[25], a deeper distinction between his notions and the viewpoint of Lexis consisted in that Laplace attached an all-embracing significance to the pattern of probability theory for human cognition whereas Lexis, as we see, considered it only suitable for definite problems.

[15] Lexis had not avoided Quetelet's influence and this is most clearly seen in the mentioned theory of the normal age at death and the connected general considerations of anthropometrical mean values. I suppose that both these theoreticians possessed two common fundamental notions which had been directing statistical thought. The first is the constancy of the relative numbers of population and moral statistics. For Lexis it was also general and should have been assumed as the initial point of any further statistical studies. And he repeatedly indicated that this constancy hardly justified the expectations excited by Quetelet. Yes, he differed from the Belgian author even in what he considered most interesting in the numbers of moral statistics and in the numerical relations of population statistics, viz., mutability rather than stability[26]. Essential changes in the values of statistical magnitudes, as Lexis assumed,

Directly point out changes in the system of the causes of the appropriate phenomena. For social sciences it is undoubtedly more important to establish these causal connections than to prove that the fluctuations of certain statistical relations correspond to the law of purely random deviations from mean values[27].

It would have been absolutely wrong, however, to attribute such statements to the generally assumed anti-Quetelet atitude. By stressing the interest to the change in numbers Lexis had in mind definite aims of statistical studies which should be based on the opinion that the unchanged general conditions of social events lead to an approximate constancy of numerical relations. How would it be possible to judge otherwise the change of the general conditions or of the guiding complex of causes?

Lexis justified the need of mass observations which are the essence of each statistical study by the understanding that comparative constancy only manifests itself in the combination of separate events into groups or masses rather than in those events taken by thermselves. For him, as for Quetelet, the approximate constancy of numerical results in population and moral statistics

which is certainly conditionally assumed rather than occurring without fail, was inseparably linked with the principle of the statistical method.

The second main point of contact of Lexis and Quetelet consisted in that for the final aim of statistical investigations the groups or masses of people which experience some event only occur as the means for cognition. They are not the real object of study or of statements constituting the highest level of cognition in the science of population or moral statistics.

The real object of such statements is rather the man considered as a typical phenomenon, the *average* man according to Quetelet and the *abstract* man as Lexis called him. The humankind is not dealt with at all since statistical results suitable for such abstract people should only be expected in extreme cases and only in historical phenomena not subject to social influences[28]. In other cases the studies concern as a rule people subdivided according to space, time and other indications of the given problem. So this is the similarity of the viewpoints of Lexis and Quetelet.

Concerning the contradictions between them, we will indicate first of all that for them the *significance* of the relative constancy of statistical results was different. In a few words, one of them [Lexis] searched for the explanation of the stability of numbers in the pattern of the theory of probability whereas the other for whom such a perception although not quite alien was still more or less in the background, pushed it back by *natural laws* or *mechanical action* understood as the interpretation of statistical regularities. This is connected with Quetelet's tendency and expectations to find mathematical formulas comparable in essence and importance with physical prescriptions for explaining such regularities. Lexis however decisively rejects such formulas and thus recognizes that he is convinced in the essential distinction of his opinion abut the final aims of statistical studies from Quetelet's statements.

But the fundamental distinction between these scientists concerns their entire scientific outlook. The common trait is the all-embracing essence of the scientific interests and education combined, for Quetelet, with a brave flight of thought and a rare gift of popularizing but at the same time with a certain incapability of clearly restricting scientific problems, strictly keeping to theoretical constructions and following them until their final conclusion, and treating materials of scientific experience somewhat [not somewhat but extremely] thoughtlessly although without pedantry.

Lexis however clearly understood the boundaries and aims of the various branches of science and the main peculiar features of

different scientific methods. His thought was logical and his studies were thorough and strict. The mathematical part of his statistics is thus on a much higher level than that of Quetelet.

Concerning the attitude of Lexis towards Quetelet there is one and *only one* fundamental point: his statement about the perception of statistical regularities as natural laws [where is it?]. Here Lexis is brought to those representatives of the previous generation of German social and philosophical sciences who had waged literary battles against Quetelet and his followers.

Their polemic contributions can to a certain extent be considered as the main works which kept to that viewpoint in the theory of statistics which is now dominating, especially in Germany and everywhere within the sphere of the influence of German science.

[16] That viewpoint with a special reference to the opinion contradicting Lexis can be described by the following remarks. First of all, the opponents of Quetelet only regard the constancy of numbers as a very minor fact: It is not at all a universal phenomenon and, if seen at all, is based on an insufficient understanding, is actually something which requires a special explanation or is even mysterious.

As far as this discussion deals with relative numbers in moral statistics, their stability is a corollary of generally unchanging motives of human actions. Incidentally, I just do not understand how the return from a certain stability of mass actions to their motives can to some extent clear up the issue. […]

The drawing in of the motives (or of the causes when actions do not depend on human will) does not solve the problem of statistical constancy but only pushes it back. Not the elements of the manifested diversity should be considered here but the peculiar in their mutual behaviour.

This will bring us to the notions of the doctrine of chances which is the foundation of the theory of probability[29]. However, the new authors resolutely question the right to apply this mathematical discipline to statistical materials.

To assume that statistical relative numbers express some magnitude with more or less essential *errors* would mean arbitrary superfluous theorizing without anything corresponding to that understanding in reality. Relative numbers are only *reduced*. When, instead of reckoning for hundreds or thousands, we sometimes consider them *per head*, we only see an outward appearance of their saying something new about a single case.

Expressions made by population and moral statistics whether formulated in absolute or relative numbers invariably concern groups of people rather than individuals. It is therefore necessary

to reject the understanding which is fundamental for the application of the theory of probability that statistical results refer to the number of observations equal to the strength of the group[30].

The object of statistical study is the *social* life as shown by various groups of people, of actions and events but not a separate life at all. Events taken by themselves are not in the least interesting for social sciences, but their mass occurrence rather than regularity makes them significant. Otherwise statistics would be not a *predicting* but a *descriptive* science which can occasionally establish similarities of different periods of time but not some difference on principle between them[31].

[17] We see that there exists a deep contradiction between the most important points of the new dominant super-realistic view and the Lexian theory. But what practical importance does it have? Perhaps it has nothing in common with the everyday work of a statistician? First of all, we ought to take into consideration that the theoretical views described above are not consistently put into practice.

Indeed, to think that it is possible to manage completely without the set of the ideas of probability theory is tantamount to somewhat deceiving yourself. Actually even the fiercest opponent of similarities with games of chance applies ideas which belong to that same area. Indeed, a scientifically minded statistician daily asks himself whether in some cases the available numerical material ensures a cancelling or an adjustment of chances.

Without any such intention or even any suspicion of doing it, he turns to probability theory although non-methodically and therefore in a rough manner of a pure empiricist. Equally mistaken are those who, as I almost wish to say, somewhat proudly state that statistics never predicts. They are entirely wrong when they suppose here that the requirements of practice correspond to administrative management.

Without essentially exaggerating we may say that for management the raison d'être of any statistical material consists in its practical application in the future. Management, just as any other practical activity, is mostly interested in establishing relations which will occur under certain assumptions. The actions of the administration, when it desires to ascertain something by statistical means, are indeed oriented correspondingly. The knowledge of the past is only important for it if the previous results can be carried over in some form to the future.

After all, we are discussing predictions based on the assumed constancy of a mass influence of certain administrative measures. It follows that in general the opinion [about that constancy?] pretty well disseminated in the modern theory of statistics does

not practically lead to any loss. This apparently occurs partly since actually that view is not seriously kept to. Hence the first point.

Second, it is quite generally wrong to suppose that the difference of opinion about the *high* problems of science inevitably tells on its entirety. Are we not used to the existence of complete agreements about more definite problems in exact sciences in spite of disputes still going on about principles? Nevertheless, in statistics as also in other sciences there are many instances when most general theoretical ideas influence the opinion about separate problems in the wrong way.

Lexis for example indicates that Adolf Wagner, Georg v. Mayr, A. v. Öttingen and others have applied methods of a quantitative establishment of various statistical sequences which were unable to satisfy sufficiently and invariably the requirements of their theory[32]. It is also possible to add that the so-called *representative method*, as the method of sampling is being recently called, can only be studied deeper and found to be admissible on principle from the viewpoint of probability theory.

It is not accidental that, for example, v. Mayr, who rejects probability as the basis of theoretical statistics, is somewhat hostile to that method as well. I refer readers to the method of *adjustment* of numerical statistical values. Knapp, for example, another and possibly the most resolute and consistent enemy of probability-theoretic ideas, considers it inadmissible. From his point of view it is proper.

[18] However, the influence of those discords is not restricted to methodological problems of collecting and treating numerical materials. It is similar to the situation with the conclusions from the numbers. For example, the opponents of the theory of probability still do not wish to admit that a stronger or weaker stability of numerical results certainly does not admit any final conclusions about the kind of causes which play some or the dominant role in the appropriate area of phenomena.

Long before Lexis Poisson (1837, p. 12) taught that the laws of chance do not depend on the essence of the causes (which is considered in separate cases). Who believes that these laws are not connected with the subject of statistics at all thinks that he can decide by issuing from the degree of stability whether physical or moral factors are prevailing in given cases. Here, the main point is possibly the opinion that in general physical factors lead to greater stability. Correspondingly, for the actions depending on human will a greater stability of the results is ensured by the causes which are rather stirred up by the sensual nature of man

whereas the spiritual and moral factors influence in the opposite direction.

But this hypothesis is not worse justified, although not better either, than other assumptions which, on the contrary, would have approved

The victory of the moral ascertainment of the will over the variable sensual excitation, the victory of the spirit over the matter.

Similar to that statement quoted from Schmoller (1888, p. 203), von Mayr (1895, p. 692) allows himself to conclude from the surprising, as he thinks, regularity of the frequency of suicides that

In the considered social phenomena [phenomenon?] *the matter concerns events which are the corollaries of a mighty and earnest corporeal and spiritual process little influenced by the pressure of the fugitive changes in outward circumstances.*

Actually the relative great stability of the number of suicides which is incidentally far from the greatest (normal random) stability, testifies that, for example the economic situation is not here generally decisive at all. Much more serious could have been such factors which do not essentially change from year to year. Whether suicides occur rather from stubbornness and thoughtless arrogance or after a mature reflection and a prolonged spiritual struggle, as von Mayr supposes, is impossible to decide by the stronger or weaker stability of numbers.

Sometimes quite insignificant incidents happen to be very stable. Schmoller (1888, p. 195), for example, additionally believes that the degree of stability depends on the *number* of causes which act in a given social mass phenomenon, so that the fluctuations become greater when that number increases. This assumption however also contradicts the theory of probability (?).

But these examples suffice. I suppose that we may consider it proved that the general theory of statistics based on *the doctrine of chances* is not as insignificant for the practice of statistical studies as it was repeatedly thought. And the man who promoted that theory as essentially as Lexis did is indirectly meritorious with respect to practical statistics as well even if we entirely forget that a part of his works (1903) dealing, for example, with calculations of mortality, is directly connected with practice.

Nevertheless, the main focus of his achievements is situated in the field of pure theory. He studied and elucidated the most general problems of population and moral statistics, their premises, methods and problems from a single viewpoint and thus showed that that science, which Quetelet had attempted to elevate to the rank of *social physics* but later abandoned his

attempt[33], nevertheless includes something essentially more than a simple social bookkeeping registration as some too sober-minded modern specialists would have understood it.

Notes

1. This seems to be a wrong and superfluous restriction. O. S.

2. Max Weber (1903, p. 1215) had recently stressed that point. Referring to another author, […] he also indicated how the *organic* social viewpoint hampers here the proper understanding of the methodological circumstances. L. B.

3. The statements described above are included in Lexis' inaugural lecture of August 1874 which he read in Dorpat [Tartu] and which are now published for the first time. L. B.

4. Bortkiewicz many times mentions games of chance in which (not always) the numbers of favourable and unfavourable chances are known. This was the received practice of statisticians for many decades (Sheynin 2017, § 10.7-7), but he should have taken the general view. Then he repeatedly discussed mathematical probability which is properly called theoretical. Finally, he never mentioned Jakob Bernoulli or De Moivre, to say nothing of Bayes, in connection of the application of statistical probability instead of theoretical (Ibidem, §§ 3.2.3 and 5.2). O. S.

5. Poisson himself (1837, § 54) understands the term *law of large numbers* (LLN, which he introduced) as the coincidence of an empirical relative number not with the appropriate mathematical probability but with another similar number based on the same probability. L. B.

6. A bit below Bortkiewicz nevertheless recalls the precise formulation of the LLN. And in general his statement is definitely wrong. O. S.

7. Bortkiewicz understands this term in a wide sense. O. S.

8. 1063:1000   17:16, but where had Bortkiewicz found this ratio? O. S.

9. Lexis introduced this model even before (1876, p. 242; 1877, pp. 73 – 74) but it is hardly satisfactory: males were completely left out. O. S.

10. Lexis (1877) provided many pertinent examples but, regrettably, did not repeat them later. L. B.

11. If these deviations are random, his statement is evident. O. S.

12. A few lines above Bortkiewicz remarked that Lexis had introduced variable probabilities. That, however, was due to Poisson. O. S.
Lexis derived this formula in a Note on pp. 196 – 197 of the supplement of 1902. The equation $[\ ^2] = [\ ^2] + D^2]$ is strict. However, it is based on a small inaccuracy which Lexis had possibly noted and even overrated: he replaced $[\ ^2]$ by $nV(1 – V)/g$. Actually the expectation of $[\ ^2]$ is $V(1 – V)/g$ and if $g =$ Const, $V(1 – V)/g = nV(1 – V)/g – (1/g)[D^2]$. Accordingly, instead of

$$R = \sqrt{r^2 + p^2} \text{ on p. 177 the more precise formula is}$$

$$R = \sqrt{r^2 + [(g-1)/g]p^2} \, .$$

If, as Lexis requires on p. 188, g is of the order of hundreds, the correction is of no consequence even for an arbitrary large p. L. B.

It was only indirectly possible to establish that Bortkiewicz discussed Lexis (1879). O. S.

13. This viewpoint is inadmissible if we agree with the understanding of the relation between natural sciences and humanities in about the same way as

Windelband and Rickert. However, since it concerns theoretical statistics, it is generally speaking of no consequence whether Lexis was mistaken or not. L. B.

A strange statement. Those so-called philosophers thought that history is a collection of facts (and therefore not really a science). O. S.

14. Lexis (1877, p. 29) indicated the cause: not all women (to say nothing of men) can give birth. On p. 24 he added other examples of statistical relations which cannot be considered as probabilities, including the sex ratio (of boys to girls, *m:f*) at birth. Indeed, [*m:f* > 1, but *f:m* < 1 and *m:f* is at least a function of a probability. O. S.]

Lexis studied such cases in more detail. Suppose that an event can occur in *G* people, the number of the observed events is G, > 1 and the number of the occurred events, *e*. Then the probable deviation of $e/G = p$ is theoretically provided by the formula

$$\sqrt{2p(1-p)/G} \ .$$

If however we calculate the probable deviation for $e/\ G$, two cases ought to be considered. If is constant, then

$$\sqrt{2p(1-p)/r^2 G} \ .$$

If however is the reciprocal of the mathematical probability of random fluctuations then (Lexis 1877, p. 230)

$$\sqrt{\frac{2(p/r)[1-(p/r)]}{rG}} \ =$$

$$\sqrt{(1/r)^2 \frac{2p(1-p)}{G} + p^2 \frac{(2/r)[1-(1/r)]}{rG}} \ .$$

This can also be derived by the theorem about the probable error of the product of two probabilities. I believe that that formula most clearly indicates by its structure why the probable deviation is larger in the second case. When considering $e/\ G$ without noticing that the denominator includes *some worthless stuff* (Lexis 1877, p. 130) we can only come to the proper measure of stability if 1/ is the empirical expression of the mathematical probability. But if is constant or subject to change in a lesser degree than required by the pattern of probability theory, the usual formulas of the probable deviation will be invalid and their application would have led to a wrong indication of supernormal stability. L. B.

In those formulas = 0.477… It means that they presume normal distributions. Lexis (1903, No. 9, p. 230) does not mention all of those formulas or the specification of the magnitude . O. S.

15. Lexis had himself previously applied in a similar sense the expression *elementary masses*. L. B.

16. In addition, it is not confirmed by the sex ratio at birth and death, see above. For more details see below. L. B.

17. I can mention the following sources: Becker (1874); Knapp (1868; 1869); Zeuner (1869). O. S.

18. This statement seems to be far-fetched. O. S.

19. No explanation provided. O. S.

20. Otherwise Lexis' doctrine had not essentially influenced statistical science. On the contrary, the main representatives of the mathematical theory of probability and its philosophical side (Kries 1886; Czuber 1899; 1903) regarded it respectfully. L. B.

21. Jakob Bernoulli, De Moivre and Bayes, are forgotten. Cf. Note 4. O. S.

22. This is wrong (Sheynin 2017, § 8.6). O. S.

23. Laplace never thought about such circumstances; he was guilty of much more serious omissions. O. S.

24. Thus, when estimating the population of France Laplace (1786) had no doubts about regarding it similar to the extraction of white balls from an urn, and the number of births, as the extraction of black balls. Then, he (1812/1886, p. 399) equated population with extractions of balls of both colours. L. B.

Laplace applied sampling. Pearson, see Sheynin (2017, § 7.1-5), noted imperfections in his work. The statement about Poisson is not substantiated and doubtful, and, for that matter, he had not considered statistics in the practical sense. O. S.

25. No explanation provided. O. S.

26. This statement is not justified. Note also that Bortkiewicz had discussed similarities but mentioned a distinction. O. S.

27. Lexis (1876, pp. 220 – 221 and 238) understood the term *purely random* as obeying the normal law. However, he (1877, § 23) also admitted less restrictive conditions as well (evenness of the density) and actually noted that it was senseless to presume some statement. Finally, Lexis (1879, § 23) mentioned fluctuations in the form of *irregular waves*. Nevertheless, he invariably assumed the ratio between the mean square and the probable error only proper for the normal distribution. O. S.

28. Do such phenomena really exist? O. S.

29. Such a doctrine (also mentioned in § 18) hardly exists even today. One of the main notions of probability theory is rather *random variable*. For some acquaintance with randomness see Chaitin (1975). O. S.

30. How else can we decide whether a certain conclusion is reliable or not? O. S.

31. Knapp (1871; 1872) finally offered a peculiar probability-theoretic viewpoint which is a specimen of pure culture [of a straightforward statement]. On the contrary, von Mayr (1895, pp. 117 and 186) parades a certain peaceful disposition with regard to both Quetelet and to the application of the theory of probability to statistics: along with the *historical* element of scientific statistics he nevertheless acknowledges its *abstract* element although not of equal worth. L. B.

At least in words Quetelet admitted the application of probability. O. S.

32. See Lexis (1879). O. S.

33. No explanation provided. O. S.

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Ladislaus von Bortkiewicz

Probability and statistical studies according to Keynes [1921]

Wahrscheinlichkeit und statistische Forschung nach Keynes.
Nordisk Statistisk Tidskrift, Bd. 2, 1923, pp. 1 – 23.

Keynes (1921) was reprinted at least twice, in 1952 and, in 1973, as vol. 8 of his *Collected Writings*. London a. o. There also exists an eBook of 2014, No. 32625. The paging in these sources naturally differs. Bortkiewicz certainly referred to the first edition but I have only seen the other two editions and was able to check those references in a few cases only.

[1] Each thinking statistician, whether he works near to, or far from mathematics, has every reason to wish to ascertain for himself the relation of the statistical method with the notions of probability and probability theory in general. It is also necessary for him to study the very different opinions about these relations. Some tend to consider all scientific statistics as the applied theory of probability whereas others think that that theory should only be applied in special problems of statistical studies. This is a problem which will still be discussed for a long time. Keynes skilfully considered it although his book was not wholly devoted to that problem. Only its last, fifth part on the theory of statistical conclusions is directly concerned with statistics. The first four parts discuss, respectively, opinions based on probability; logical basis of the theory of probability; induction and inferences by analogy; and *some philosophical applications of probabilities*.

There exists however a tightest connection between the author's general considerations and the special thoughts about the statistical method so that his book is an integral contribution quite original both in its intention and execution.

Keynes does not conceal at all the origin of the necessarily borrowed building materials; he indicates it scrupulously. He mostly feels himself akin in spirit to his English predecessors. Like they, he attempts to avoid the areas of creative phantasy and to keep himself always connected with the factual material. He therefore rejects the Laplacean enthusiasm[1] to which his contemporaries had gladly listened and for a long time remained in his captivity.

Ellis (1842) was the first who objected to that *alchemy of logic*. Then Venn (1866) developed a theory wholly based on empiricism. Actually, probability appeared there only as the statistical frequency of some event and, moreover, only when it is precisely determined by observations continued infinitely.

Keynes however resolutely objected to that *frequency theory*. Exactly the *empirical school* went too far in its response to the viewpoint of Laplace. Were our experience and science perfect, the theory of probability will not be needed at all[2]. However, if gaps are discovered in our knowledge, then, as Keynes supposes, probability-theoretic judgements become inadmissible if not supported *either* by intuition *or* by something else, i. e., by a specially intended prior principle. I will show that in his own doctrine Keynes admitted both as this principle for basing probabilistic opinions.

In spite of his strong and stressed opposition to Laplace, Keynes believes it necessary to adjoin him here. According to Laplace, each numerically measureable probability (and he supposed that probability is such indeed) rests on an incomplete knowledge of circumstances which makes it possible to distinguish two or more mutually exclusive cases whose occurrence is equally indefinite. This is achieved by calculating the probability of an event as the ratio of the number of cases favourable for it and the total number of all the possible cases. This principle of defining probability is called in different ways. Boole (1862/1952, p. 390), for example, mentioned an *equal distribution of our knowledge or rather of our ignorance.*

Von Kries [1886] chose principle of *deficient grounds*, but Keynes considered this expression unsatisfactory and suggested *principle of indifference* (p. 44 of the eBook). Actually, however, Keynes clearly agreed with the long-standing criticisms of this principle by von Kries although only insofar as the Laplace formula still held. Indeed, he believes that the principle of indifference should only be introduced more rigorously after which it will become a suitable and single useful foundation of probability theory.

[2] The Keynes viewpoint can be satisfied if we say that he contrasts the *simple* and the *special* forms of the principle of indifference. However, he offers some reservations. The most essential of them discusses irrelevance. Thus, for proposition x which is based on result h_1 the circumstance h_2, if the probability of x (more precisely, the probability that x is corroborated) does not change when h_2 is added to h_1. Or, more generally, if something follows from h_2h_1 (i. e., from h_1 when h_2 is added to h_1), but not from h_1 alone. On the contrary, relevant is such a circumstance h_2 if it alone or some corollary of h_2h_1 somehow changes the studied probability.

And we ought to note that Keynes considers x and y as well as h_1 and h_2 as propositions and therefore sometimes applies symbol $f(x)$ for h_2 which denotes a proposition connected with x. For justifying an equal probability of two different propositions x and y by a given result, this result should not include any such circumstances which have to do with x or y if they do not correspond to a suitable circumstance *of the same form* bearing on x or y.

In a symbolic form this means: if $f(x)$ is included in h_1, $f(y)$ should also be included there and vice versa[3]. Keynes explains this rule, which for the sake of brevity can be called the *rule of symmetry*, by an urn with a certain number (say, 4) of black and white balls. It is asked (and Keynes offers both solutions) should we consider five numbers, 0, 1/4, 1/2, 3/4, and 1, which are the possible ratios of the black balls to their whole number, equally probable, or believe that each ball can equally be black or white so that those ratios will be 1/16, 1/4, 3/8, 1/4, and 1/16 […]. Keynes decides in favour of the second solution but his statement is based on a vicious circle. […]

Keynes indicated that, in particular, von Kries and Stumpf had studied that example[4]. The former decided correctly in my opinion that that example resolutely does not at all admit any numerical determination of probabilities. […] Stumpf's criticism induced von Kries (1916) to return to this example and he attempted to strengthen his former viewpoint by new considerations.

Such thoughts only confirm what was clear from the very beginning: it is vain to attempt here a derivation of a general exact numerical solution. And since Keynes busies himself with such a problem, we may ask ourselves: does this attempt correspond to the need to be supported by facts (by *matter of fact*), to the principle which he adopted?

It is also surprising that in this special case he sided with the *subjectivist* Stumpf rather than with the *objectivist* von Kries[5]. True, in general he is much closer to the latter, but incidentally, he expressed some dissatisfaction by von Kries' discussion of the highly estimated by himself logical foundation of probability theory.

This possibly happened at least partly since he only considers the book of von Kries (1886) but not a series of his papers (1888) or his *Logik* (1916). May we wonder that Keynes was barely successful with the main, as he thinks, condition of the principle of indifference when studying the example in which the fruitfulness of that condition was necessary to reveal? I do not think so. Does not the symbolism which Keynes applies with a special liking imitate the non-existing precision?

The notation $f(x)$ and $f(y)$ ought to show that the forms of the pertinent propositions connected with x and y respectively are the same. But how should we understand the *form*? In the example now considered Keynes thought that those forms were different because one of them stated something about the number of combinations out of four taken two at a time whereas the second, out of one at a time. Does not it testify to the surprisingly petty interpretation of the expression f*orm*?

And the rule of symmetry which Keynes had established becomes no more definite, it only allows us to see whether the circumstance h_2

is relevant or not (p. 111). Intuition ought to support the application of that rule (pp. 53 – 54 and 64). And in addition the result h_1 invariably includes an inexhaustible set of various circumstances so that here only intuition can help us once more.

A similar difficulty certainly appears in each inductive study of the causes of phenomena but the practical inexhaustibility of h_1 remains as a circumstance which is still not much facilitated by the rule of symmetry. According to Keynes this rule is the most important but nevertheless not the only means for attaching more rigour to the principle of indifference.

Other measures of precaution also exist, but they do not change the fact that under some circumstances that principle can lead to differing but still to some extent equally justified probabilistic decisions. And Keynes himself does not maintain that it is possible to confirm that principle by a unique solution [elimination] in *all* cases of such a contradiction, a circumstance which critics had expressed against it. It is already somewhat surprising that Keynes (p. 52) so earnestly attempts to *rehabilitate* that principle.

[3] At the same time we ought to take into consideration that in the Keynes theory it is far from occupying a governing place as it did in the Laplacean theory. The latter justified the very notion of probability on the concept of *equal indecision*. In Keynes' terminology, on the principle of indifference, which is most closely connected with Laplace's understanding on principle that each probability is a proper fraction. The structure of equally possible cases which serve as parts of such fractions is indeed based on the principle of indifference**6**.

On the contrary, Keynes issues from a much more general notion of probability and his subject is thus not the theory of probability as such (numerous formulas in his book mostly concern not its field but logical calculations) and not only its logical foundation but the general methodology of probabilistic judgement. The doctrine of probabilities in the Keynesian sense (p. 97) is concerned with logical causes which prompt us to trust something rather than anything else. He (p. 98) bears in mind all kinds of arguments which issue from some assumptions and lead to reasonable although not certain conclusions.

Keynes distinguishes measurable and immeasurable probabilities**7** and considers both. He believes that we are often incapable of comparing the degrees (Grad) of the latter and wittingly justifies this statement (pp. 34 – 40). If we agree that not each probability can be numerically expressed and if, on the strength of the principle of indifference, the probabilities of some conclusions obtain unequal numerical values depending on the approach to the appropriate problem, – then we miss that strange which in such cases mostly inherent in them. And contrasting solutions are sometimes based on different premises so that the values of their probabilities cannot be

united by a single, so to say, higher value of the probability. Indeed, it is required here that the probabilities of the corresponding premises be numerically represented which is not occurring here.

Keynes is stressfully convinced in that the immeasurable probabilities exist along with the measurable and that exactly they have more weight in each kind of inductive studies, and this viewpoint is revealed in his entire composition both when he develops his own thoughts or criticizes alien viewpoints. This belief indeed dominates in the last part of his book. Each time when a statistical *characteristic*, be it a relative number, a mean value or a correlation coefficient, is transferred on unobserved cases (which is really the *inductive*, as opposed to the *descriptive* role of statistics) this method leads to a result which lacks any certainty and only possesses a better or worse justified probability.

[4] It is this probability that cannot be expressed in any quantitative form and Keynes (p. 367) who specially refers to Laplace mentioned the attempts to allegedly specify the probability of some inductive corollary by formulas of probability theory:

We will […] *endeavour to discredit the mathematical charlatanry by which, for a hundred years past, the basis of theoretical statistics has been greatly undermined*.

Keynes (p. 369) quoted Leibniz[8]:

Estimation of probabilities is extremely useful, although in examples taken from the law or political sciences delicate calculations are not as necessary as an exact listing of all the circumstances.

Keynes also noted that the essential in his views were thus expressed by Leibniz. He had thus largely agreed that in statistics a quantitative application of certain formulas of the theory of probability often led to serious abuse, but we still ought to ask, had not Leibniz gone too far in belittling calculations in general and did not Keynes too eagerly agree with him here. The quotation from Leibniz is taken from the supplement of 3 Dec. to his letter of 26 Nov. 1703 to Jakob Bernoulli, which was the answer to the latter's letter. There, J. B., in particular, explained by example the method of empirically determining probability […][9].

Were not these arguments of Bernoulli decisive? He was certainly tending to develop the theory of probability as seen in his thesis (1685/1969, pp. 269 – 270):

Quanto caeteris scientiis praestet [he discusses mathematics] *vel ex eo constat, quod cum reliquae de rebus, in se certissimis ac constantantissimis, non nisi probabiliter, illa de rebus maxime fortuitis et casualibus, v. gr. sortitionibus, apodictice et certissimo ratiocinio discurrit*.

Even if we disregard such extreme statements and remain in the boundaries of the dispute between Bernoulli and Leibniz, it is still

impossible, as I think, not to agree with the former whereas the latter looks not so well[10]. Once more he revealed his poor understanding of the theory of probability which surprisingly contrasts with his predilection for combinatorics (noted previously by Couturat[11] et al). Indeed, he thought that 11 and 12 points in a throw of two dice were equally probable! And he certainly was unable to solve more difficult problems, as for example the calculation of the present value of an annuity (Bortkiewicz 1907/44, pp. 71 – 72n).

In his correspondence with Jakob Bernoulli he unsuccessfully contrasted *calculations* and *enumeration of circumstances*. Apart from games of chance the sought probabilities cannot be derived from circumstances[12]. Their most accurate regard can therefore be only understood when it is intended to go over from probabilities derived from observations to unobserved cases while taking care of ensuring the most possible coincidence of the general conditions. This is troublesome.

But the observance of these directions does not change anything in the essence of the necessary calculations and will mostly only lead to their justification by observations restricted to a more tight area and correspondingly to dealing with smaller numbers.

It is perhaps somewhat interesting that Keynes (p. 268n) strongly and possibly too strongly criticized Mill (1843/1886, p. 353), censured him for the method (surprisingly similar to Leibniz' way of thought) of taking circumstances into account:

Even when the probabilities are derived from observation and experiment, a very slight improvement of the data by better observations or by taking into fuller consideration the special circumstances of the case, is of more use than the most elaborate application of the calculus of probabilities founded on the data in their previous state of inferiority. The neglect of this obvious reflection has given rise to misapplications of the calculus of probabilities which have made it the real opprobrium of mathematics.

Yes, certainly, since mathematicians repeatedly applied statistical numbers without more thoroughly checking their reliability and in addition often unjustifiably and too sketchily dealt with the peculiarity of the objects of study.

In this sense Mill's statement is indeed worthy of attention but it concerns the basis, i. e., the legitimacy, the structure and formulas of probability theory in their application to statistical materials as little as the remarks addressed long ago by Leibniz to Jakob Bernoulli.

[5] Keynes himself says however that *Leibniz's reply goes to the root of the difficulty*. Full of anti-calculation tendency, which he thus possesses along not only with Leibniz, but with Mill, Keynes particularly reproaches the estimation of precision in statistics, i. e.,

those calculations which ought to establish tentative or limiting deviations of the obtained results from reality[13].

Czuber (1910, Bd. 2, pp. 15 – 16), for example, issued from the number of boys and girls born during a certain period and calculated the most probable number of newborn girls for a later period in Austria after assuming that the number of boys was known. Applying some formulas of probability theory he established a very high probability barely distinguishable from certainty that the real number of newborn girls will be comparatively very near to the number provided by him.

Keynes first of all criticized Czuber for transferring the sex ratio onto a larger number of births and for believing that in spite of this circumstance the result was practically true. This, as he stated, contradicts *good sense* and some theoretically derived requirement.

Second, without sufficient grounds Czuber thought that that sex ratio was stable. To what extent this is inadmissible is already seen from a still later period of 1895 – 1905 (Keynes pp. 351 – 353): deviations were situated outside limits to which the same method had attributed practical certainty.

We only ought to discuss this second objection since it is much more important. Indeed, the reference to *good sense* in the first one was unconvincing whereas the discussion of the *requirement* would have led me too far.

In essence, Czuber's calculation assumes that the sex ratio has a normal dispersion, that is, possesses the highest possible degree of stability. For this reason his results are to a certain extent usual and he himself (p. 13) makes it known when he preliminarily refers to the

Previously generally assumed notion about the constancy of statistical relative numbers.

And on p. 16 he mentions the subsequent modifying statement. Keynes had not here taken the context into consideration. Czuber himself was not touched upon by Keynes but in its essential part the criticism remains completely valid with respect to a countless number of calculations if a direct practical meaning is attached to them. Nevertheless Keynes had not said anything new.

Already Venn reasoned about the transfer of statistical frequencies from observed to unobserved cases and especially about transferring them to the future, when the assumption of the constancy or of only insignificant change of the general conditions is decisive[14]. This is the cause of the uncertainty which, as he formulates it, belongs to the area of induction rather than probability. This should mean that a mathematical approach by the rules of probability theory to uncertainty caused by the assumption of stability is useless, see Mill (1843, chapters 1 and 6).

It is somewhat surprising that Keynes, who thoroughly discusses Venn and considers him to some extent as a precursor of Lexis (see below), does not say a single word about his important reasoning. Incidentally, it appears to me that in general Keynes describes Venn's views somewhat crudely. In essence he pays no attention to the very important for Venn's viewpoint mental replacement of actual by *imaginary series*. The *frequency theory* thus loses its excessively empirical side although naturally without an establishment of its certain basis. And not to be reckoned a follower of that theory, I indeed intended to note that circumstance.

It, that theory, suffers in my opinion exactly because of attempts to reduce the notion of probability to two ideas, to an *irregular order* of the elements of a series and to the *approach* of the appropriate empirical frequency to a fixed boundary value which can only be more precisely indicated by probability theory. Neither Keynes nor Bosanquet, a long-standing opponent of the frequency theory, had noticed its indicated weaknesses[15]. We may consider Venn its main representative and I am thus in complete agreement with Keynes about its conclusion, i. e., I agree to deny it, if not quite in regard to causes.

Nevertheless, Keynes' criticism of Venn seems to me as though a Cambridge man is not quite impartial to another one. As Keynes shows him, Venn is hardly characterised by a *remarkable acumen* (Edgeworth 1911, p. 403/1996, vol. 1, p. 152).

[6] Some uncertainty unyielding to calculation is inherent in each transfer of the statistical frequency from observed to unobserved cases. Lexis later confirmed this idea by studying the real behaviour of series of population and moral statistics. This idea was precisely formulated in the theory which he had derived for explaining his results. In accord with that theory, apart from rare exceptions, the indicated kind of uncertainty is caused by the physical or essential component of fluctuations which acts along with its normal random or nonessential component.

Only the first, but not the second component, as Lexis reasoned, admits a probability-theoretic interpretation. It immediately follows that in statistics estimations of precision, insofar as they inevitably only consider the nonessential and miss the essential component, are therefore illusory.

Already Venn declared that *the causes of uncertainty in the area of induction* are the more significant the longer is the series of unknown cases for which a certain frequency is postulated. This statement should have meant that in statistics the estimation of precision becomes the less reliable the larger is the number of the appropriate observations[16]. The same conclusion follows from the Lexian theory

according to which the essential component ever more violently perturbs the fluctuations as that number increases.

However, Venn thinks that that influence is caused by the lengthening of the period of observations with the increase in that number, but Lexis explains this phenomenon more generally: the field of observation can be extended by a longer period of observation, by widening it, by a larger number of large groups [?] and each such possibility leads to a stronger action of the essential component of fluctuation.

We may therefore say that in general an estimation of precision is the less suitable the more observations serve as its basis. I would also refer to the reasoning of von Kries (1886, pp. 178 – 181) and Karl Wagner (1898). Keynes had not included the latter contribution in his Bibliography but mistakenly attributed a paper of the same author to Adolph Wagner, a specialist in life insurance.

Keynes does not at all indicate that in statistics the significance of the estimation of precision exactly in the sense which follows from the Lexian theory of dispersion depends on the number of observations. Had he drawn that theory in for judging the suitability of the estimation of precision, he in any case would have been more conciliatory inclined and possibly acknowledged that in the struggle against such estimates Leibniz does not justify hopes as an ally.

Indeed, as stated above, the Leibniz postulate leads to the most precise registration of the *circumstances* so as to work with a relatively smaller number of observations[17]. However, the smaller is that number the more admissible and even the more indicated is the estimation of precision and the more are we induced to apply the formulas of the theory of probability! It is not accidental that such a specialist in similar investigations as Westergaard is among those who come out for estimating precision and often apply it.

[7] If however Keynes, as I suppose, unjustly refuses to adjoin Lexis it certainly happens not because of his objection to the Lexian direction. He rather is earnestly interested in Lexis, stresses that Lexis influences his own ideas and believes that, in spite of some essential reservations about the notion of probability, Lexis is more fruitful with respect to the notion of probability and better suited to the principles of proper induction than the Pearsonian direction.

Keynes quite favourably judges my papers on the theory of dispersion which had appeared a quarter of a century ago and even earlier. As to my law of small numbers, he naturally thinks that I had hardly proved anything except that the Lexian criterion of stability is not applicable to the case of rare events.

Blaschke (1898) had also pronounced that opinion. It would have been correct had the small number of the occurrences of an event led to the impossibility of the coefficient of dispersion to exceed unity

considerably because of a purely arithmetical reason having nothing in common with probability theory grounds. But this is not so. Definite conditions which can only be formulated in the language of the theory of probability should be rather added for that coefficient to remain in the vicinity of unity. One and only one example can explain it.

It has to do with the blowing up of steam boilers. Prussian statistics (*Jahrbuch* 1910, p. 136) shows under two different headings the yearly number of such accidents and the number of the killed workers. For 1890 – 1909 the mean yearly numbers were 3.3 boilers and 1.8 killed. The second number is smaller but the coefficient of dispersion for the blowing up of the boilers is 0.86, whereas for the killed, on the contrary, 1.67. In the second case the mean error of the coefficient is 0.16, so that it is out of the question to say that the difference equal to 0.67 [1.67 – 0.86 – 0.26 = 0.65] was random. It is rather explained by *acute solidarity of separate cases*[18]. Sapienti sat [Sufficient for the clever].

In spite of his unsuccessful, in my opinion, criticism of the law of small numbers, Keynes does not deny completely his interest in it. He (pp. 403, 404) estimates my later work in mathematical statistics essentially otherwise and argues that *Bortkiewicz does not get any less obscure as he goes on*. Instead of clearing up a very simple matter, I have befogged it with a profusion of mathematical formulas and new technical terms[19]. *Like many other students of Probability he is eccentric, preferring algebra to earth*.

[8] Keynes justifies this rebuke in a few marginal remarks about my paper (1918). I have chosen its theme just as my methods, to say the truth, not out of the blue. There are many indications in the special literature about whether homogeneity influences the stability of frequencies and how does the greater or lesser homogeneity of a statistical group be here manifested.

I decided to ascertain this problem by statistical data. As criteria of the degree of stability I had at my disposal the coefficient of dispersion and, since it is independent from the width of the field of observation, the essential component of the fluctuations. However, with respect to the second criterion it was necessary to show how to calculate it since Lexis sometimes applied an inconvenient method of determining it.

Then I established a criterion for the degree of the homogeneity of a statistical group and explained that, if a magnitude cannot be quantitatively estimated, we may still be sure that, in accord with that criterion, a total group can never show a higher degree of homogeneity than the mean of its partial groups. Indeed, the population of Germany, for example, is in any possible sense less homogeneous than the populations of its separate regions on average.

I have thus prepared the ground for a study of the relation between homogeneity and stability. I successfully showed by a series of examples that the coefficient of dispersion for a total group, although higher than for the mean of separate groups, was not however as high as was possible to expect because of the larger number of its separate cases as compared with the partial groups.

In other words, it occurred that the essential component of the fluctuation for a total group was lower than for the mean of the partial groups. According to the stated above, this means no more and no less than a combination of lesser homogeneity with a higher stability and vice versa. This was required to be explained and I managed to establish a mathematical connection between the component of the fluctuation of a total group and the components of the partial groups.

[9] The formula which expresses that connection includes a factor composed after the coefficient of correlation and called [by me] the coefficient of syndromy[20]. It shows the measure of the mutual correspondence of the appropriate statistical series composed of separate partial groups. When the correspondence is absolute (isodromy) this coefficient is 1; when it is more or less considerable (homodromy), it is contained between 0 and 1; if there is no correspondence at all (paradromy), the coefficient is 0; and, finally, when the processes, which are described by the series, are proceeding antagonistically (antidromy), the coefficient of syndromy is negative.

The smaller is the coefficient of syndromy, the more decreases the essential component of the total group relative to the mean value of that component for the partial groups which it equals in case of isodromy.

And thus the cause of the established mutual relations between homogeneity and stability is that isodromy never really occurs and invariably the other forms of the syndromy take place. If this explanation is correct, then, as I said to myself, the stability of the total group ought to be essentially higher than for the partial groups if it is composed of absolutely incompatible parts.

Indeed, in this case the most likely will be paradromy or even antidromy. And so it really happened for the statistics of marriages in 1899 – 1908 in six cities, Barcelona, Birmingham, Boston, Leipzig, Melbourne and Rome taken together. The essential component of the fluctuation was so small as compared with the data about those same cities taken separately that the coefficient of dispersion for the artificially created total group was lower than the mean of the indicated parts. At the same time according to the Lexian pattern of probabilities changing serially, it should have been considerably higher than for that mean.

Indeed, Lexis derived the relation between the value of the coefficient of dispersion and the number of the separate cases (trials)

for (the hardly occurring) isodromy[21]. I suppose therefore that my study is a contribution to the theory of stability of statistical series and that, in spite of Keynes' remarks, it is exactly in the direction of a more accurate understanding of reality.

[10] At the end of my paper I indicated that the statement about the antagonistic relation between homogeneity and stability had been for a long time actually applied in the practice of insurance. It became known that to ensure a calm existence of an insurance establishment a stability of the numbers in which its activity is represented is naturally needed which was assisted by a most possible breakdown of insurance according to territorial or other conditions rather than its concentration in a small district and a small variety of the ensured risks.

Keynes however thinks that this argument is only an example provided earlier by me of the difference between the general probability p and its components, separate probabilities p_1, p_2, … This is what he said (p. 403):

If we are basing our calculations on p and do not know p_1, p_2, …, then these calculations are more likely to be borne out by the result if the instances are selected by a method which spreads them over all the groups 1, 2, … than if they are selected by a method which concentrates them on group 1. In other words the actuary does not like an undue proportion of his cases to be drawn from a group which may be subject to a common relevant influence <u>*for which he has not allowed*</u>*.*

If the à priori calculations are based on the average over a field which is not homogeneous in all its parts, greater stability of result will be obtained if the instances are drawn from all parts of the non-homogeneous total field, than if they are drawn now from one homogeneous subfield and now from another. This is not at all paradoxical. Yet I believe, though with hesitation, that this is all that von Bortkiewicz elaborately supported mathematical conclusion amounts to.

Let us suppose that, for example, in case of fire insurance we deal with two types of buildings, dwellings and factories with differing risks of fire which was not however taken into account. The premiums would have rather depended on the existence of a definite ratio of those risks to each other. Then, according to Keynes, insurance of both types of buildings covering the entire considered period would have assisted a greater stability of the results of insurance rather than an insurance of only one of the types of the buildings and only for a year, then only of the other type for the next year etc.

This is really so (and not at all unusual)[22] but it is irrelevant to my statement about the antagonistic behaviour of homogeneity and stability. For a connection to my thesis we should rather contrast the following two cases. In the first one, both types of the buildings are

insured for some years with a definite relation [of risks] to each other (which only undergoes normal random oscillations). And if the risks for both types of the buildings are not too considerably different, a scale of premiums [introduction of differing premiums] for them will but little change anything. (Such action increases the stability of the insurance activity.)

In the other case only one type of the buildings is insured for a number of years and the premiums correspond to the risks of fire. My statement would have meant that the first case with a lower homogeneity ensures a higher stability as compared with the possibility that for both types of buildings risks change from year to year but in such a way (normal random oscillations are naturally not considered here) that these changes at least partially compensate each other.

Indeed, nothing except such a compensation takes place under homodromy, paradromy or antidromy. In addition, the higher stability in the first case was not connected with the condition that the mean risks for the two types of buildings over all the period are different. Even if not different, but those types do not quite correspond to each other in respect to the yearly oscillations of the risks (apart from those normal random), a higher stability will occur in the first case.

The same conclusion followed from my example of 1918: the difference of the mean probabilities in separate partial groups was the factor which should be taken into account although the essence of the problem did not change[23].

[11] How could it happen that Keynes did not understand not some part of my paper, but all of it, and to such an extent? As far as I understand the only explanation which has a higher (although unmeasurable) probability is that the materials from which he issued were too extensive. Already the number of the sources which he had looked through excludes the possibility of a balanced scrupulous study of each of them. Moreover, the matter concerns a contribution which studies somewhat complicated connections and is not as easily coped with as with most materials although not absolutely inscrutable.

Such particulars are hardly significant as compared with essential virtues which isolate his contribution exactly as critical information about the merits of other authors. Sometimes Keynes refers to German authors just for fun, as for example to Bobek (1891). In his barely known textbook he (Keynes, p. 383) calculated the probability of invariable sunrises during the next 4000 years and concluded that it was only 2/3.[24]

However Keynes provides plenty references to German authors and justly recognises the success of the German spirit and zeal in the philosophical, mathematical and applied theory of probability. He

found forgotten long ago books, for example the contribution of Kahle (1735), a teacher of public and church rights, and praised them.

Keynes' erudition in the widely branched field of probability is surprising. In this respect he rivals Chuprov. Incidentally, he characterizes Chuprov as an intermediate link between the *German* and the *English* school and praised him once more although without being acquainted with his main work (1909)[25] since it was published in Russian.

And, when recalling that Keynes according to his main position is a professor of national economy, and a publicist and politician known to the whole world, we cannot refuse him our exquisite recognition of such a comprehensiveness of the highest grade[26].

Notes

1. Laplace had indeed enthusiastically described his humane political views partly based on general stochastic ideas and on the compensating, as I would say, action of randomness in mass random phenomena. We do not know what exactly had Bortkiewicz in mind, but in any case he (like many other commentators) had here and below somehow belittled Laplace whose initial views were allegedly reduced to the introduction (after De Moivre and actually even after Jakob Bernoulli) of the *classical* definition of probability. However, first, until the advent of axiomatics that definition was remaining in use, although justly rejected, and became only rivalled by Mises.

Second, Laplace had not at all restricted his activities to introducing that definition, he repeated many times over that hypotheses (perhaps including the number of cases as well) ought to be incessantly corrected by new observations, see for example his *Essai* (1814/1995, p. 116). This does not correspond with his statement that probability rests on incomplete knowledge.

Finally, Laplace (1812, Chapter 5) considered geometrical probability and (Ibidem, Chapter 6) solved some problems by the Bayesian approach.

2. However, mass random phenomena can only be studied by the theory of probability. Then, instability of motion, and especially the newly studied chaotic movement prove that that statement was wrong.

3. Bortkiewicz unsystematically connected $f(x)$ with both h_1 and h_2.

4. I have found the appropriate place: von Kries (1886, p. 33). He noted that each ball could be either black or white but that other considerations lead to quite different results. Bortkiewicz, as we see, elaborated this statement but had not noted that his efforts were in line with Bertrand's celebrated problem of 1888 about the probability of the length of a random chord. Bortkiewicz considered his problem in detail and indicated that its quantitative solution was impossible.

Incidentally, Bertrand's problem was studied for more than a century and commentators finally agreed that the sought probability was 1/2. This corresponded, as I (2017, Chapter 12) remarked, to the conclusion of the theory of information: absolute ignorance. I also noticed that in 1881 Darwin considered several versions of uniform randomness and rejected all of them in favour of a determinate act.

5. Keynes refers once to von Kries and three times to Stumpf, see my Bibliography. I added two other contributions of Kries since they are also needed.

Bortkiewicz (1899/17) publicly criticized Stumpf as well.

6. Laplace (1812/1886, p. 181) adds a reservation: probability is that ratio when there is nothing for believing that one case arrives oftener than the others. Then he

(1814/1995, p. 6), however, he states that the definition of probability is a principle [and does not therefore need any justification].

7. Bortkiewicz should have referred to Cournot (1843, §§ 233, 236) who had introduced unmeasurable (*philosophical*) probabilities. Hardly known is that Fries (1842, p. 188) forestalled Cournot.

8. I have translated this Latin piece from its German translation (Gini 1946, p. 405).

9. Bortkiewicz describes Jakob Bernoulli's reasoning in great detail although a German translation of the *Ars Conjectandi* had already appeared (in 1899).

10. Calculations should not be opposed to the study of circumstances. I note that Gauss (*Werke*, Bd. 12, pp. 201 – 204) stated that the nature of the studied object ought to be taken into account. W. E. Weber described his opinion in a letter of 1841 (Gauss, *Werke*, Bd. 12, pp. 201 – 204).

11. Possibly Couturat (1901). A bit below Bortkiewicz mentions a surprising mistake made by Leibniz, see Todhunter (1865, p. 48).

12. The sought probabilities do not exist always.

13. Bortkiewicz (1894 – 1896) had earlier denied the need to estimate precision (Sheynin 2017, p. 176). This theme is treated in detail below.

14. About 1965 – 1968 I had been in touch with Login Nikolaevich Bol'shev who was Kolmogorov's student and who about 1967 became head of the section on mathematical statistics of the Academic Mathematical Institute. He told me once that because of financial considerations it was even necessary to make conclusions after only one observation. I had an impression that he spoke about some military matters. Regrettably, Bol'shev died prematurely.

15. Bortkiewicz mentioned the English philosopher Bernard Bosanquet (1848 – 1923). The true originator of the frequentist theory was Mises. Here are his words (1928/1972, pp. 26 – 27): his theory is not *quite new*, Venn had *thoroughly described* the idea of determining probability by frequency. His other forerunners were, as he stated, Fechner and Bruns who had proposed a doctrine of the collective. However, the *clearest* in this direction was Helm (1902), but still neither these authors nor many others were able to create a perfect [?] theory of probability since they had not introduced disorder, the *decisive* indication of the collective. Note however that in the Introduction to the 1931 edition of his book Mises named Ellis and Cournot as his predecessors. Mises regrettably omitted Bayes (Sheynin 2017, pp. 68 – 69).

A bit above Bortkiewicz mentioned disorder as a weakness of the frequency theory which only makes sense only insofar as a mathematical definition of disorder was not offered even now. Mises very easily surmounted the second weakness, as Bortkiewicz called it, by defining probability as the limit of frequency.

16. The statement in the last sentence is repeated below. A comparison with the treatment of observations is possible: random and systematic errors are as though two components of fluctuations, but the estimation of precision in the case of a small number of observations is difficult.

Now, unobserved cases became observed. Then, the Lexian theory allegedly leads to the increase of the physical component of fluctuations with the number of observations (below), but Lexis made no such statement. On the contrary, he (1879, § 15) qualitatively asserted that the more observations there is in each series the nearer is Q to unity and the smaller is that component, see also his formula in § 11. Elsewhere Bortkiewicz (1931, p. 5) stated that that the physical component tends to increase with a decreasing number of observations.

17. This assumption is too restrictive. Concerning the next sentence cf. beginning of Note 16.

18. Why only one mean error is sufficient? And what about the error of the first number? *Mean* error apparently meant *mean square* error. Lexis (1876, p. 214)

defined the latter term but called i*t mean error.* Elsewhere Bortkiewicz (1931, p. 9) applied the three sigma test. Bortkiewicz introduced acute solidarity earlier (1898) and Chuprov (1905) applied that term later.

19. I copied the next sentence from Bortkiewicz (1931/108, pp. 19 – 20) where it was naturally written in English. Bortkiewicz (1923/93) provided both neighbouring sentences in their original English.

20. Five new terms. Bortkiewicz introduced them previously (1918/68, pp. 42 – 43) and once more afterwards (1931/108, p. 10) but I doubt that anyone else picked them up.

21. I have not found that derivation.

22. This statement is not clear enough.

23. Later (1931/108) Bortkiewicz better described the example concerning insurance.

24. Keynes had provided no details but anyway Bortkiewicz was wrong: the sunrise problem became classical and there was nothing funny about referring to Bobek.

25. That contribution (1909) was written by the yet non-mathematically minded Chuprov (a mathematician by education) and my opinion about it is quite negative, see at least Sheynin (2011, pp. 124, 142 and Note 14.10 on p. 172). Later in life Chuprov hardly referred to it and Chetverikov, Chuprov's closest student, testified that sometime after 1910 Chuprov refused to reprint his contribution; its edition of 1959 appeared long after his death.

Chuprov as an intermediate link: Keynes apparently thought about Chuprov's efforts to unite these schools.

26. Here is one more criticism pronounced by that exquisitely recognized person (Keynes 1921/1973, p. 440, Note 2):

The mathematical argument is right enough and often brilliant. But what it is all really about, and what it really amounts to and what the premises are, it becomes increasingly perplexing to decide.

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Abbreviation: JNÖS = *Jahrbücher f. Nationalökonomie u. Statistik*

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Part 2

Contents

Bibliography

I

From the Documents (NNo. 1 – 12)
of the Berlin Humboldt University Archive and
Other Establishments Concerning Ladislaus von Bortkiewicz

1. Minister for Religious, Educational and Medical Affairs, 15 Jan. 1901. To the Philosophical Faculty, Friedrich-Wilhelm University

U I 5091 No. 135. Archiv, Phil. Fak. UK PA B 347

The Director of the railway establishment for life insurance[1] at the Ministry of Public Works, Russia, as of today[2] is appointed Extraordinary Professor of the Philosophical Faculty at the Friedrich-Wilhelm University in Berlin.

He is imposed with the duty to represent statistics and kindred disciplines (insurance science, doctrine of population etc.) and if necessary to assist in complementing the educational programme in the field of economics.

Notes

1. Actually, an employee of a pension fund, see [iii].
2. Actually, as of 1 March.

2. Attachment: Bortkiewicz' Questionnaire Completed 12 March 1901.

Archiv, UK PA B 347

Apart from well-known information: Bortkiewicz called himself a Roman catholic.

In another partly completed questionnaire with no date or signature Bortkiewicz indicated that he had graduated from a humanities-oriented gymnasium and for eight terms studied the law at Petersburg University.

3. Minister for Religious and Educational Affairs, 23 June 1913. To the Philosophical Faculty, Friedrch-Wilhelm University

U I No. 6355, 417. Archiv, Phil. Fak. 1466, Bl. 186

Bortkiewich is appointed one of the heads of the University Seminar on Economics.

4. Minister for Religious and Educational Affairs, 30 October 1916. To Exrtraordinary Professor Dr. von Bortkiewicz

U I No. 6451.1. Archiv, Phil. Fak. 1467, Bl. 123

According to the request of the State Ministry of Internal Affairs I am asking you to fill the position of an employee on scientific statistics in the Civil Department of the Governor-Generalship in Warsaw for the present term with a release from reading lectures[1].

Note

1. That Generalship was established in August 1915. On 5 November 1916 Germany and Austria established instead a Kingdom of Poland on a restricted territory. Biographers of Bortkiewicz hardly ever mentioned this episode in his life. Nothing is known about his work in Poland.

5. Minister for Religious and Educational Affairs, 21 February 1917. To the Philosophical Faculty, Friedrich-Wilhelm University.

U I No. 5138. Archiv, Phil. Fak. 1467, Bl. 195

Following [Letter 4] I inform the Faculty that, according to his request, the extraordinary professor Dr. von Bortkiewicz is released from his service at the Civil Department […] as of 31 January of this year.

6. The Prussian Minister of Science, Arts and People's Education, 6 July 1920. To extraordinary professor Dr. Ladislaus von Bortkiewicz

U I No. 6665.1. Archiv, Phil. Fak. 1469, Bl. 67

Von Bortkiewicz is appointed ordinary professor of the Philosophical Faculty.

An undated and unsigned manuscript (Ibidem, Bl. 63) apparently written somewhat earlier acknowledges the *adjusting importance* of Bortkiewicz as a researcher and adds: *we had been concerned about his nationality* but that concern *has noticeably weakened after von Bortkiewicz had admitted in writing his Germanism*.

7. The Prussian Minister of Science, Arts and People's Education, 17 June 1929.

U I No. 1027. Archiv, UK PA B 347

von Bortkiewicz is appointed member of the Berlin examinational commission on economics from 1.10.1929 to 30.9.1931.

8. From the Minutes of the Berlin Commercial School, 30 May 1906

Archiv, UK PA B 347

Von Bortkiewicz is appointed as pluralist-docent of the Commercial School of Berlin Merchants. He will read a two-hour lecture [weekly] on insurance science.

9. Just after Bortkiewicz's death, on 17 July 1931.
177. Archiv, UK PA B 347

Deißmann, the Rector of the Friedrich-Wilhelm University informed the Russian Scientific Institute and the Russian Academic Society about the death of Bortkiewicz[1].

Note

1. That same day these institutions had sent a common reply containing a thankful remembrance of Bortkiewicz' collaboration. See photo of the text of their telegram in Sheynin (2011, p. 57).

10. The Board of Guardians of the Commercial School, 30 July 1931. To Helene von Bortkiewicz. Signed: Dr Demuth
K3000/31. Archiv, UK PA B 347

We express our sincere condolences with regard to the heavy loss which you experienced on the death of your brother. The deceased had been closely connected with us and exemplary taught his students from the establishment of our school until the winter term of 1922/1923. He was able to combine in an exceptionally worthy way scientific thoroughness with an understandable method of teaching[1]. We will honourably and thankfully remember the deceased.

Allow us also to express our thanks to the deceased by paying 200 Reichsmark for a gravestone.

Note

1. Here is Letter 79 of 1905 from Bortkiewicz to Chuprov (Bortkevich & Chuprov 2005): *I do not value myself especially high as a lecturer or head* [of a seminar?].

11. The Business-Manager of the Friedrich-Wilhelm University, 15 August 1931. To the Minister of Science, Arts and People's Education
U I 7733. Archiv, UK PA B 347

The sister of the deceased, Helene von Bortkiewicz[1], kept house for her bachelor brother until his death and lived with him in a common economy. He was her only breadwinner. She is 61, has no close relatives who would have been obliged to support her.

Following is information about the lump sum allowance granted her and the allowance which she will obtain.

Note

1. Helene moved to Berlin in 1918 after her brother had become able to achieve this feat, see his Letter 143 of 1918 to Chuprov (Bortkevich & Chuprov 2005). In

1916, in its NNo. 6 and 7 the Russian journal *Matematicheskoe Obrazovanie* published her translations from Italian.

12. The Secretariat of the School of Economics [the former Commercial School]. **Document of 11 February 1938.** Signature undecipherable.

   WHB 603/1

The portrait of the late teacher von Bortkiewicz has disappeared from the school hall. The Secretariat suspects that it was removed by an outsider who had mistakenly thought that von Bortkiewicz was not of German blood[1].

Note

1. This document was probably intended for the School's archive.

   Quite in the Nazi spirit this letter would have obviously justified a disappearance of a foreigner's portrait. Bortkiewicz was a Russian Pole and *At heart, a great Polonophil* (Chuprov's letter to his Father of 29 May 1900, see Sheynin (2011, pp. 56)).

II

Anonymous

Bortkevich (Vladislav Iosifovich)

Enziklopedicheskiy Slovar Brockhaus & Efron,
Additional halfvolume 3, 1905, p. 301

Economist and statistician. Born 1868, graduated from Petersburg University. In 1895 – 1897 read lectures on the insurance of workers and the theory of statistics at Strasbourg University. In 1899 – 1901 taught statistics at the Imp. Aleksandrovsky Lyceum in Petersburg. From 1901 professorial chair in Berlin University.

Many works in Russian and German, especially on theoretical problems of statistics ad political economy, collaborated with the *Handwörterbuch d. Staatswissenschaften* and foreign sources on economic problems, also in this Encyclopaedic dictionary[1].

Note

1. Most entries in that *Slovar* (or dictionary) are anonymous and it is extremely difficult if not impossible to find the appropriate places. Then, Bortkiewicz possibly assisted in editorial work although only until 1897. In Letter 22 of that year Bortkiewicz (Bortkevich & Chuprov 2005) informed his correspondent that owing to a somewhat fishy behaviour of the Editor, he breaks off his relations with the *Slovar*. Finally, I note that after 1898 Bortkiewicz became generally known by his notorious law of large numbers, and the Editor himself could have written this piece (and obscured the issue). Only one entry written by Bortkiewicz is known (1897/12).

III

Ch. [A. A. Chuprov]

Bortkevich

Novi Enziklopedicheskiy Slovar Brockhaus & Efron, vol. 7, 1912, p. 647

Bortkevich Vladislav Iosifovich, statistician. Born 1868, graduated from the law faculty, Petersburg University. From 1895 to 1897 as a privat-docent read lectures on the insurance of workers and on statistics at Strasbourg University. Later was in Petersburg a clerk at the administrative department of the pension fund of the employees of the state-owned railways and taught statistics at the Imp. Aleksandrovsky Lyceum. From 1901 professor at Berlin University and teaches mathematical statistics and related disciplines. Even as a student submitted an investigation of the mortality and longevity of the Orthodox population of European Russia (1890/3; 1891/4). These contributions included mortality tables and a general essay on the elements of the theory of measuring mortality.

Bortkevich submitted an extended form of this essay to Göttingen University as a doctor dissertation (1893/6). Bortkevich' contributions on the theory of the stability of statistical series are very important. They obviously adjoined the works of Lexis but in many respects essentially furthered the development of that problem. His paper (1894 – 1896/8) and especially his investigation (1898/14) belong to the main contributions in this area[1]. In many periodicals and collections Bortkevich also published numerous reviews and papers on theoretical statistics, calculus of probability, demography, insurance and economic theory.

Note

1. The statement about the law of small numbers is puzzling, see Chuprov's real feeling (Sheynin 2011, p. 59).

IV

Anonymous

Bortkevich, Vladislav Iosifovich

Great Sov. Enc., first edition, vol. 7, 1927, p. 198

Bortkevich, Vladislav Iosifovich. Born in Russia in 1868; statistician and economist. Graduated from Petersburg University, law faculty, was left at the university [*to prepare himself for professorial duties*]. In 1898 – 1901 worked in the Ministry for railway

communications[1] and read statistics at the [prestigious] Aleksandrovsky Lyceum. From 1901, professor of Berlin University.

B. published numerous, although mostly short contributions and is one of the most prominent modern theoreticians of statistics. Apart from special areas (formal theory of population, mathematical theory of insurance) the central place in his work is occupied by the study of *variance*. In this field, B. is a fervent successor of the Lexian theory, an argent follower of Lexis' main idea about the stability of statistical series as the norm. He most completely developed this idea in his law of small numbers, LSN (1898/14).

For readers, who keep to dialectical materialism and understand a norm as something movable rather than invariably being at rest, for such readers it becomes obvious that the LSN, not devoid of mystery, conceals an extremely simple idea: the variability of barely perceptible magnitudes is barely noticeable[2].

As an economist, B. holds a conciliatory viewpoint between the classical and Austrian schools and widely applies the mathematical method.

Notes

1. See his Letter 25 to Chuprov of 1897 (Bortkevich & Chuprov 2005) and the contribution of Pokotilov (1909). I have not seen this source, but it was very shortly described by Idelson (1910). The subtitle of Pokotilov (1909) is sufficient for my purpose.

2. The worst is still to come, see Starovsky (1933, p. 279) in the same edition of the *Encyclopaedia*:

The theoreticians of the bourgeois statistics (Süssmilch, Quetelet, Lexis, Bortkevich, Pearson [...] Bowley [...] Chuprov et al) had attempted to prove the invariability *and* eternity *of the capitalist order and the* stability *of its laws.*

In 1958, he was rewarded for his stupid Bolshevik viewpoint, – elected corresponding member of the Academy of Sciences and had been holding highest administrative positions. Bowley was mistakenly included in his list of the infidels: in the Introduction to his book of 1924 the lustful Maria Smit stated that its usefulness outbalanced its ideological harm.

Another anonymous note in the second edition of the same source (vol. 5, 1950, p. 605) once more called Bortkevich a defender of the reactionary theory of stability of statistical series, but neither capitalism nor Starovsky were mentioned. In the third edition of the same *Encyclopaedia* (translated into English), vol. 3, 1970, p. 583, signed by F. D. Lifshitz the only negative statement maintained that Bortkevich eclectically combined the subjective and objective theories of value and both theories of money. His stated date of publication of the LSN was wrong.

On the non-existing LSN see Whitaker (1914) and Sheynin (2008).

V

D. Michaikoff

Ladislaus von Bortkiewicz

Rev. trimestrielle de la Direction générale de la statistique,
No. 1, year 1, 1929, pp. 7 – 9 (Bulgarian and French)

On 7 September 1928, Bortkiewicz, professor of Berlin University, became 60 years old. Elsewhere in the same source the reader will find more detailed information about his life and scientific activities, but, during my stay in Berlin I was able to work for two years under his guidance. This imposes on me a special duty (the French text: *this cannot hinder me, s'empêcher, from paying my special duty* etc.) which I owe to that venerable scientist.

More than twenty years have passed since. During that time all of us, the students and admirers of that great scholar, had seen how Bortkiewicz as a teacher by far surpassed the boundaries of both the Berlin University and German science. He became the overseer of scientific thought in the statistical and economic sciences and he has to check inevitably each new and essential theoretical idea for it to acquire citizenship in science.

But what justifies that special and honourable position? I attempt to evade standard expressions which are usually applied in such cases the more so since his case is unusual. Indeed, it is a rare instance that such deep and clear thought is harmoniously combined with an extensiveness of the viewpoints and lack of presumptions, pose or any servility.

The latest mentioned circumstance is the reason why certain circles are giving him a cold shoulder. This, however, did not and will not ever preclude the influence which Bortkiewicz exerts on science. His authority has not at all depended on the affection of a group of friends, but on the respect of the whole scientific world. And he does feel that respect, ever more categorically from day to day.

Although somewhat belatedly, the editorial committee of this periodical decided that it was a compulsory duty to acquaint our society with the personality of that great teacher. It also asks Bortkiewicz, on the occasion of his jubilee, to accept its congratulations and respect along with wishes to continue for many years to contribute to the solution of difficult problems of the statistical and economic theory and to prevent by his wisdom one-sided (French: exclusive) and extreme passions.

VI

O. Anderson

Professor V. Bortkevich

Rev. trimestrielle de la Direction générale de la statistique,
No. 1, year 1, 1929, pp. 7 – 9 (Bulgarian with a French summary)

On 7 August of the previous year the accepted leader of the *Continental School* of mathematical statistics, ordinary professor of

the Berlin University, Vladislav Iosifovich Bortkevich reached 60 years of age and still fully blossoms with respect to scientific power and teaching activity. However, for a scientist the age of 60 is a tentative boundary, after which it is customary to cast a summary glance at his works. That is why, in response to the invitation of the editorial staff, I decided that it is my duty to submit this short description of Bortkevich' scientific activity, mostly in the field of the theory of statistics.

   The life of Bortkevich is not rich in external events. He was born in Russia; after graduating from Petersburg University he went to Germany for further scientific development and obtained there his doctorate [Doctor of Philosophy].

   In 1895 – 1897 he read lectures on the insurance of workers and the theory of statistics in Strasbourg University (Prof. G. Knapp). Then, in 1899 – 1901 he taught statistics in the privileged Imperial Aleksandrovsky Lyceum (Petersburg), which was previously in Tsarskoe Selo and from which graduated Pushkin, Delvig, Prince Gorchakov[1] and many other celebrated Russian statesmen.

   In 1901 Bortkevich became chair in the Berlin University and is holding that position for almost 28 years. Having no family, he wholly devotes himself to science. His main feature is his surprisingly sharp, cool and, I would say, merciless mind. So charming in personal relations, he nevertheless becomes a fearful opponent in any scientific discussion.

   His studies are free from logical or mathematical errors. If, after studying some work of another author he finds it correct (which does not happen often), that author becomes sure that his scientific inferences were irreproachable. The extensiveness of Bortkevich' knowledge and the scope of his scientific interests are really immense. A generally known economist who penned more than 20 remarkable works on various problems of the theory of political economy; an excellent mathematician; a generally recognized authority on the theory of probability and insurance; the head of an entire statistical school with more than 40 monographs, he still finds time for presenting physicists a remarkable contribution (1913/59), for studying various systems of proportional representation (1919/74; 1920/76). He writes about the justification of the Leibniz formula of the discount (1907/44), explains whether there even exists Deportgeschäft (1920/82) etc., etc.

   The scientific work of Bortkevich is peculiar and somewhat approaches the manner of Edgeworth[2]. Until now, he had not published either a single *capital* work or one of those bulky *Handbücher* so typical of German scientists. According to P. B. Struve they are so called because it is difficult to hold them.

Bortkevich writes comparatively short monographs devoted to those separate problems which at the moment interest his creative thoughts. He mostly describes the results of his own studies. And when he expounds the ideas of other scientists (Poisson, Lexis, Dmitriev[3], Helmert et al), he inserts so much of his own, explains and supplements them in such a manner that the outcome is something quite new and original.

The main tone of his works and an especially wide application of the mathematical arsenal which he wholly masters put him on a special place among the representatives of German statistical science. Indeed, even up to now they are paying their main attention to the methods of collecting and preliminarily summarizing statistical observations. This is why, although Bortkevich had spent more than a half of his life as a teacher in two German universities, for those representatives he is still to a certain degree *an alien body*. He ought to be recognized as an international or even Russian, rather than German professor. From the Englishmen of the Pearsonian school he differs by higher requirements, again in the spirit of Russian mathematicians, for precision and conclusiveness of mathematical proofs.

Partly because of the heterogeneity of his scientific activities, and perhaps partly because of the ineradicable antipathy felt by the great majority of German economists against mathematics which is connected with the known peculiarity of the economic education in German universities, – because of all this, Bortkevich' works are scattered in various periodicals not only in Germany but in other countries as well and often barely available for non-specialists.

Thus, out of the 42 known to me monographs on statistics and connected problems of the theory of probability only three have appeared as books (1893/6; 1898/14; 1917/66) which are now for a long time out of print. The rest 39 are published in 26 periodicals in Germany, Russia, Austria, Sweden, Switzerland, Italy, in the publications of the International Statistical Institute etc. And 22 of his works in economics are in twelve periodicals. Numerous reviews of various scientific books should be added as well as his articles in the Russian *Brockhaus & Efron Encyclopaedic Dictionary*[4], in the *Handwörterbuch der Staatswissenschaften* etc.

One more circumstance. The place of publication, the title, the manner of exposition of a monograph, often do not conform to what a specialist expects by those indications. Where is a mathematician or physicist who will search for deep studies of the theory of probability in the paper (1894 – 1896/8)? And who among statisticians will carefully consider the publications of the Berliner math. Gesellschaft or search for the study of the Pearson criterion of goodness of fit or

important inferences about the Lexian coefficient of dispersion in Bortkevich' paper (1922/92)?

This circumstance along with the sufficiently high general scientific and mathematical training of the reader which are usually required for the Bortkevich' work[5] is apparently the reason why, to the great shame of our science, some of his contributions had not yet met an unconditionally deserved response. Even today their most general application is a problem for the future, hopefully not very remote.

And so it is indeed required to collect all the scattered monographs of Bortkevich perhaps a bit shortening some of them, suppressing repetitions and subordinating all of them to a single plan. As far as I know, preparatory steps have already been carried out, but his students and followers are duty bound to insist that this useful undertaking were accomplished as soon as possible.

In short, Bortkevich' *life oeuvre* in theoretical statistics is briefly reduced to the following. First of all, he has the honour of heading that powerful movement of statistical thought which originated in the 1870s with a few comparatively short notes by Lexis but which would not have developed as it did without the resolute support by Bortkevich and, later, Chuprov.

Our generation of statisticians can barely imagine the mire in which the statistical science found itself after the collapse of the Quetelet system and from which it was only rescued by Lexis and Bortkevich.

Bortkevich had explained everything that was contained in the comparatively short formulation of the Lexian ideas. He generalized them, transferred the study of the stability of series onto series of extensive magnitudes[6], developed and concluded and in some cases specified or even corrected his predecessor. In this connection especially remarkable are his works (1894 – 1896/8; 1899/16) which have not lost their freshness or convincingness, as well as his paper (1901/22).

Bortkevich put into logical completeness the theory of the coefficient of dispersion. He also published a number of monographs on the philosophical justification of the statistical method and the explanation of its inseparable link with the theory of probability. It was he who cleared up all the importance of Poisson's ideas for the theory of statistics and threw light on statistical regularities.

Finally, his great merit was the transfer of the fruitful notion of *expectation* into the theory of mathematical statistics[7], the notion that now to an ever increasing extent becomes the fundament of all of its methods, see especially (1912/66).

From Bortkevich' separate doctrines (?) the most widely known became his LSN (1898/14) although I think that it is practically much less important than his main works cited above[8].

Unusually interesting and important is his deep study of the theory of index numbers (1923 – 1924/96) which had therefore overstepped the boundaries of a purely statistical contribution. After that Bortkevich worked much on population statistics, moral statistics (not less than sixteen original publications) and the theory of insurance.

Our paper will not be comprehensive without mentioning Bortkevich' intimate friendship of many years that connected him with another ascetic and leading figure of the statistical science, six years younger, with the already late (alas!) Aleksandr Chuprov. They had been in incessant scientific intercourse and in some respects much influenced each other. We may consider Chuprov, especially in the beginning of his scientific life, as Bortkevich' student[8].

Notes

1. Tsarskoe Selo is near Petersburg. I. I. Delvig: poet, a close friend of Pushkin. Prince Gorchakov: several people can be mentioned here, all of them eminent statesmen.

2. Edgeworth's contributions are known to make difficult reading, and the same is true about Bortkevich, see also [xix]. Anderson, however, hardly thought about such a similarity.

3. V. K. Dmitriev (1868 – 1913), economist, statistician. He attempted to reconcile the theories of value and marginal utility.

4. It is almost certain that Bortkevich had published only one contribution (1897/12) in that source.

5. See Note 2.

6. Bortkevich had described his own achievements in a letter to Chuprov of 1908 (Sheynin 2011, pp. 138 – 139). See also pp. 59 – 62 where I severely criticize the LSN. In his booklet (1898/14) Bortkevich had introduced his own test, Q, not coinciding with the Lexian Q; thus, unlike the latter, Q could not be less than 1 (p. 31). Later Bortkiewicz (1901/22, p. 833) noted that $EQ = Q$ but mistakenly justified this equality by believing that, for dependent random variables   and , E / = E /E . Then, he (1918/70, p. 125n) only admitted that the equality was insignificantly approximate. Chuprov (1922) devoted a paper to that subject.

7. Mathematical statistics had really originated with the works of Fisher and Gosset (Student). Concerning expectations see Note 6! And the method of expectations is due to Pearson (Cramér 1946, § 3.3.1; Sheynin 2011, p. 147).

The LSN is *an obsolete name for the Poisson limit theorem* (Kolmogorov 1954), see Sheynin (2011, p. 59), and Anderson himself (above) noted *the high importance of Poisson's ideas*. Whittaker (1914) was the first to deny that law.

8. This is not exactly so, see Sheynin (2011, p. 55 and especially p. 59).

VII

Slavcho Zagoroff

Bortkiewicz as an economist

Rev. trimestrielle de la Direction générale de la statistique,
No. 1, year 1, 1929, pp. 10 – 12 (Bulgarian with a French summary)

[1] *Bortkiewicz belongs to the most interesting and peculiar personalities in the German scientific world. One of the most eminent specialists in national economy, who had resolutely explained his viewpoint on the fundamental problems of theoretical national economy, he is at the same time a mathematician enjoying an established status and reputation* (Altschul 1928 [viii]).

Everyone, who is acquainted with the scientific activity of Bortkiewicz will agree with this conclusion. Bortkiewicz mostly worked in the field of prices and money. Along with his statistical monograph on the indices of the general movement of prices (1923 – 1924/96) in which he also studied the connection between these two sides of the phenomenon [prices and money?] his greatest economic contribution is (1907/40). There, he criticized the Marxist theory of pricing in the capitalist national economy.

Marx defined the ratio for the exchange of commodities by two methods. The first was based on their *values*, i. e., it had conformed to *the law of value*. In this case the total cost W of a commodity manufactured at a definite time in a given branch of industry is equal to the sum of the spent constant capital (of the spent means of production) ac, the cost of the variable capital (the spent daily wages) v, and the surplus value (the value created by the worker for the capitalist during the working hours less the value equivalent to the obtained wages), m:

$$W = ac + v + m.$$

Assuming also that the percentage of the surplus value ($100m/v$) is the same in all the branches of industry, Marx calculated W for those branches, then determined the ratio of the exchange of the commodities according to the prices (according to *the law of the equal percentages of profit*)

$$P = ac + v + m_1.$$

Here, P is the total cost price of the manufactured commodity (its *cost prise ac +v*) plus the profit of the capitalist. Assuming that the percentage of the profit is the same in all branches and equal to the ratio of the surplus value to the total value of the constant and variable capitals in the national economy, Marx calculated P for the branches of the industry.

The results of the two determinations (Wert- und Preisrechnung) did not coincide, which he himself emphasized. However, he thought that the prices which he determined more or less approached reality (*Das Kapital*, chapter 1, pp. 1 – 2; chapter 9, pp. 132 – 151).

Bortkiewicz deeply investigated these constructions, derived a formula for the difference between *W* and *P* and proved (1907/40, pp. 15 – 16) that the Marxist method of determining the prices by issuing from the *cost* was mistaken (he arbitrarily transferred the elements of one pattern to the other) and that therefore also mistaken was the entire theory of pricing. Neither had the generally known Marxist *law of the decline of the percentage of profit* resisted that criticism and did not stand fast.

Bortkiewicz' constructive opinion about that theme is expounded elsewhere (1921/86). There, he attempted to reconcile the two known and contrary theories of pricing, the theory of the cost of production and of the utility theory. He thought that in all cases a prolonged change of the cost of production influences the prices whereas the changes of utility solely influence the price of such commodities whose production can only be increased by increasing the cost of production.

Bortkiewicz thus recognized that both objective and subjective causes were determining the prices. He therefore approached the viewpoint of Marshal, Walras, Cassel[1] and other mathematically oriented economists. They had been teaching that the prices and their causes were interacting. Bortkiewicz stressed that interaction and kept to the opinion that a researcher may nonetheless ask about the cause which in a given case compels the prices to change (Ibidem, pp. 20 – 22 of the reprint).

[2] In [the theory of] money Bortkiewicz is a defender of a moderate metallism and an opponent of the extreme nominalism. He criticized the Knapp theory and concluded (1906/38) that the contradiction between the moderate metallism and the nominalism is actually not large. As a proof, he considered two main propositions of Knapp: the nominal essence of the monetary unit and the state essence of money.

According to Knapp, the first proposition expresses first of all that 1) it is possible to have circulating money not completely independent from any metallic substance and 2) there can be a close connection between money and metal without their identity.

Bortkiewicz argued however that this is what the moderates of metallism were saying: paper money not exchanged for metal in a compulsory rate is accepted as real money although normally that money is of a metallic substance (Ibidem, p. 1322). Second, Bortkiewicz (p. 1337ff) also found it possible to reconcile both sides, the viewpoint of nominalists that the value of paper or metallic money is provided by state authority whereas the opinion of the followers of metallism is that when metallic currency is coupled with a free coining of money the coins possess value since they are coined from precious metals. He argued that these viewpoints were not completely incompatible. For

an agreement between them it is only necessary to [solve] two questions: *has* a banknote any value; and, if it has, *what value* does it have?

It will then become clear that the state may coin or immobilize coins but is unable to establish some definite price for the coins without proper objective conditions being met, for example, without considering their metallic content or the value of the issue.

Knapp was almost indifferent to the purchasing power of money; on the contrary, Bortkiewicz is keenly interested in that problem. He rejected the idea that each person is at the same time a creditor and a debtor and is therefore unable to estimate clearly the inflation or deflation as some evil. He indicated that the changes of a monetary unit and in the supply of the national economy with means of payment do not tell uniformly on the incomes and expenses of separate economies [families?].

When studying these problems Bortkiewicz keeps to the quantitative theory of money which he, however, widely interprets in the psychological sense. He recognizes the tentative and complicated essence of the magnitudes included in the proportions of exchange. Contrary to the strict followers of that theory (for example, Irving Fisher) he believes that prices are not a quite passive element of those proportions, see his report in September 1924 (125/97) in which he studied the phenomenon noticed in many countries after WWI: during inflation, beginning at some moment the level of prices heightens much more than the increase in the quantity of [paper] money.

Bortkiewicz agrees with Marshal, Keynes, L. Mises, I. Fisher, Hahn[2] and other economists that during inflation the circulation of money becomes more rapid and that the mistrust in the currency, the fear of the increasing depreciation of money is the reason why the increase in the prices overcomes the increase in the quantity of the means of payment. However, Bortkiewicz opposes the view that the fear of inflation acts mostly only on buyers and compels them to get rid of the depreciating money as fast as possible. He thinks that the fear mostly influences the sellers and compels them to increase prices bearing in mind the incessant general rise in prices. Therefore, the increase in the rapidity of the circulation of money becomes the corollary rather than the cause of an incessant heightening of prices (1923/97, pp. 266 – 267).

[3] In his critical paper (1890/2) Bortkiewicz expressed his viewpoint on the problem of percentage [on loaning money] directed against the theory of Böhm-Bawerk. The latter indicated three causes which justified the value of the percentage: the change with time of the ratio between the requirements and their coverage; systematic underestimation of the future requirements and the means for covering

them; and the technical superiority (?) of the existing commodities over the future commodities of the same type.

Bortkiewicz typically doubts that the imposition of percentages can be explained by technical conditions of production. He questions the independence of the third cause (partly reduces it to the first cause) and considers the second cause entirely unfounded. He only leaves the first one although with some reservations and supposes that the Böhm-Bawerk problem was hampered by his attempts to justify the value of the percentage by the action of those same forces which substantiate its very existence. He (1906/37) supposes that these two problems could and should be considered separately[3].

[4] Especially important from the methodological and philosophical viewpoint is his critical paper (1898/15) on the Pareto political economy[4]. In spite of associating himself with the author in that in many cases a dependence between economic phenomena is best represented by a system of equations, Borkiewicz (p. 1191) spoke out against reckless applications of mathematics in economic studies:

I cannot share Pareto's optimism. He declares that a precise and detailed statistics will ensure in future the possibility of attaining such a degree of scientific knowledge that both the direction of the changes of [measurable – S. Z.] *phenomena B, C, D, … after A had changed, and the precise values of that changes* […] *will be known. Pareto himself had stressed the complicated essence of economic and social phenomena which hampers such attainments. The possibility of an assistance rendered by statistics in numerical applications of the formulas of political economy is a phantasy since these formulas contain magnitudes which cannot be determined by observation.*

Although criticizing some of the most eminent representatives of the doctrine of marginal utility, Bortkiewicz did not reject it. In many places he himself stated that at first he accepted that doctrine, but [at present] he thinks that the idea of that doctrine cannot be the starting point for deriving an urgently necessary single and complete economic theory.

If I ought to name the ideological current to which Bortkiewicz belongs, and to describe his social and economic Weltanschauung, I think that it is proper just to call him an individualist.

Notes

1. Gustav Cassel, 1866 – 1945.

2. I have not found either Hahn or Hann, or Han.

3. This is difficult to understand. Anyway, the method rejected by Bortkiewicz was and is the only one applied in natural science.

4. In Letters of 27.8 and 18.9.1898 to Bortkiewicz Chuprov (Bortkevich & Chuprov 2005) severely but politely criticised the former's manner of writing which did not take care of his readers, but Bortkiewicz did not mend his ways.

VIII

Eugen Altschul

L. v. Bortkiewicz

Editorial explanation. On 7 August Professor Ladislaus von Bortkiewicz, a renown representative of economics and statistics at the Berlin University will be *60 years old*. Consequently, our worker, Doctor Eugen Altschul, by answering our request described for us the significance of that scientist.

Bortkiewicz belongs to the most interesting and peculiar personalities in the German scientific world. One of the most eminent specialists in national economy, who had resolutely explained his viewpoint on the fundamental problems of theoretical national economy, he is at the same time a mathematician enjoying an established status and reputation. Even in an early contribution (1898/14) he creatively interpreted in his favourite field, the theory of probability by the Poisson extension of the Bernoulli theorem and by indicating new paths for study. Somewhat earlier, when considering the philosophical basis of the theory of probability, Bortkiewicz (1894 – 1896/8) created a logical basis of modern statistics justified by mathematics.

He combines a mathematically delicate and shrewd theoretical analysis with a rare knowledge of the application of the theory of probability to the most extensive areas of natural and social sciences. Bortkiewicz, the supreme ruler, exercises dominion over various branches of investigations situated far apart. Exercises, but to what extent? This is evident since he offered physicists a new methodologically directing monograph (1913/59).

Bortkiewicz is also astonishingly versatile. He is an outstanding specialist in the mathematical school of economics (Walras – Pareto) as well as a delicate interpreter of the Marxist doctrine. However, he does not belong to those national economists who had developed their own systems; he is insufficiently dogmatic and excessively tends to criticise. Along with his sometimes too sharp criticism Bortkiewicz offered irreplaceable views of problems and their solution and had thus laid the foundation for constructing economics [anew].

In a short time he published his renown work (1906 – 1907/40) about Marx which also today is distinguished from the ocean of Marxist literature, a paper about Aristotle as a theoretician of the science of population (1906/39) and one more study on the Böhm-

Bawerk theory of interest on loans (1907/93). After that there appeared his contribution on the state theory of money according to Knapp (1920/81). Nowadays, more than twenty years later[1], all these works are remaining not only valuable, they indicate numerous connections and excite a desire to continue the study of those themes.

Bortkiewicz is only interested in throwing light on problems as a whole by studying them from a still unknown or unacceptable viewpoint and the most suitable form of accomplishing it is through articles. The essence of both of his bulkier works published until now (1913/59; 1917/66) is also typical of papers.

Bortkiewicz' study of indexes best reveals the manner of his writing. He thought of discussing the Irving Fisher capital work but the result was a monograph 130 pages long (1923 – 1924/96), one of the most distinguished works in that sphere.

That manner of writing resulted in the scatter of Bortkiewicz' papers over numerous periodicals so that even for specialists it is often difficult to get hold of them. Only some physicists (?) knew that a fundamental investigation on the application of the theory of probability had appeared in the *Jahrbücher* (1894 – 1896/8) and the statisticians most often did not know that an important paper on theoretical statistics can be found in a Scandinavian journal barely known in Germany, or in the *Berichte* of the Berliner math. *Vereinigung* [see correct title: (1918/70)].

A similar situation had occurred with Bortkiewicz' papers on economics. His continuing for decades fruitful and almost encyclopaedically oriented calm scientific studies already for this very reason are partly unknown at all. German literature will become immeasurably richer if some researcher in his tireless battles with new problems discloses the results of Bortkiewicz' fruitful life work for a wider circle of readers.

Note

1. This remark does not apply to the last-mentioned source.

IX

Eugen Altschul

Ladislaus von Bortkiewicz

Magazin der Wirtschaft, No. 30, 7. Jg., 1931, pp. 1183 – 1184

On 15 July death had carried away from science Ladislaus von Bortkiewicz, one of the most outstanding German economists. This happened shortly before he reached age 64.

His merit consisted in the special attention to, and care about economics during those few decades when this science had remained in a fateful neglect. In the interpretation of the theory of probability Bortkiewicz enjoyed international reputation. He had carried out unusually versatile and fruitful studies and, until his last days, developed urgent problems in the midst of new areas.

His creative activity in such difficult sciences as economics and mathematics, his encyclopaedic knowledge, surprising erudition in neighbouring fields as well, clear and confident opinions, ability to combine scrupulous preliminary work and comprehensive analysis, all this ensured his special authority in international science. The peculiarity of his thoughts and style explains why, if even entirely apart from his preference to mathematical descriptions he has remained unknown to a wider circle of readers and too difficult.

Bortkiewicz was unusually inclined towards criticism. During all his [scientific] life he had liked protracted and sharp controversies. Being only 21, he (1890/2) began to contradict even Léon Walras. We can explain how acute had been his opinion in those early years: Walras, three decades older, the recognized head of the Lausanne [economic] school, entered into a correspondence with that beginner [published in 1965 by William Jaffé] which lasted for a very long time and included a discussion of the most difficult problems of mathematical economics.

However, Bortkiewicz was interested not in controversies as such although each line of his texts indicated an earnest sense of responsibility peculiar to a real investigator … No! He rather entered into polemics only since they appeared for him as a most suitable form for explaining logical connections. The problems which he considered were too diverse (vielgestaltig) and I cannot even briefly discuss them.

A student of Lexis, one of the creators of the new mathematical statistics, Bortkiewicz for some time had turned to studies in the theory of probability [published by other authors] and destructively criticized them. In spite of their differing viewpoints on mathematics [on its application?], Knapp, with whom he, just as with Lexis, for a long time had remained in close friendly relations, encouraged him and in 1895 Bortkiewicz became a [privat] docent in Strasbourg.

At that time there had appeared his publication on the law of small numbers (LSN) (1898/14) which later became generally known. Somewhat earlier Bortkiewicz published an innovatory contribution (1894 – 1896/8) on theoretical statistics. And it was he who continued further studies along the way paved by Lexis. By a shrewd mathematical and cognitive analysis of the Poisson generalization of the Bernoulli theorem he was able to build a solid logical justification for a sensible application of probability to statistics. These contributions had been preliminarily completed (1901/22).

Bortkiewicz had temporarily worked as a teacher in his native Petersburg [near Petersburg] but in 1901 he accepted an invitation to the Berlin University. And there he remained for three decades, from 1920, as ordinary professor of economics and statistics. Typical for his viewpoint on political economy was his paper on Böhm-Bawerk [1906/37] so distinctively entitled *The main mistake of the Böhm-Bawerk theory of the interest on loans* and the still discussed contribution (1906 – 1907/73). But only fifteen years later Bortkiewicz (1921/86) attempted to expound systematically his study of the theory of value and thus to combine both its directions.

An important paper on the Knapp theory of money had also appeared (1919/73) as well as numerous contributions on the theory of money [in general], population statistics, theory of probability and insurance mathematics. They were published in most various German and foreign periodicals. However, all through his writings there was a study of problems in the theory or probability and various applications of mathematical statistics.

A special monograph (1913/59) was devoted to its mathematical and methodological application in radioactivity and earned serious respect among physicists. In the field of mathematical statistics Bortkiewicz' publications include a fundamental work (1923 – 1924/96) which regrettably had appeared in a barely available [in Germany] Swedish journal and a study of income presented at a session of the International Statistical Institute in 1930 (1930/104). There, as it was usual for him, mathematical analysis was brought to a preliminary conclusion[1].

Bortkiewicz had obviously been more inclined to analytical rather than synthetic contributions, which was the reason why he had not constructed any system. However, his viewpoint on each decisive problem of political economy (Wirtschaftstheorie) was critical and the elucidation of the [appearing] problems had essentially fostered further studies. Many years will have to pass until Bortkiewicz' ideas are recognized in full and systematically built into political economy.

Bortkiewicz devoted immeasurable care and attention to revise and incessantly develop his lectures. However, he distasted pedagogic activity. He saw no possibility of sufficiently informing a wider circle about his delicately complicated course of ideas, but all the more had he influenced separate and closely connected with him scientists. He was as sharp in his contributions as unusually gentle in his Berlin home which had for a few decades remained a place for gatherings of the most eminent scientists from everywhere. International science is mourning over the man of so unusual qualities.

Note

1. See [xiii, Note 2].

X

E. J. Gumbel

L. von Bortkiewicz

The death of the master had inflicted an essential loss to our science. These meagre lines do not allow me to describe clearly all the significance of this man. The field of his work covered population statistics, the formal theory of population, mathematical insurance of life and social insurance, the theory of probability, adjustment calculations (1910 – 1912/56), and political arithmetic up to mathematical economics. In short, covered the totality of all the border disciplines which ever more distinctly squeeze themselves between mathematics and economics.

And he also applied mathematical statistics to problems of physics. Each of those disciplines which, because of their contents, appear as separate entities, but methodically are very similar, and Bortkiewicz essentially succeeded in, and seriously contributed to their unification into an independent science. His name will live along with the names of the masters of the theory of probability as a classic of mathematical statistics.

His first investigations had to do with the theory of mortality tables. From a large number of functions which characterize it [mortality], he selected and worked with the most important notions of expected life, and especially the expected life of a newborn. In a stationary population the birth rate equals the death rate, and the expected life of a newborn equals their reciprocal. It was thought that for an increasing population the expected life could be obtained from the observed birth and death rates which are not equal here, but Bortkiewicz (1893/6) showed that the correct answer can only be got by constructing a table of mortality.

He returned to this problem when studying in his last years two other methods of comparing the rates of mortality of two populations (1904/29; 1911/57). In connection with those problems were the studies of the formal theory of population which represented a development of the works of Knapp.

As compared with a stationary population, an increasing population numbers more infants and less old people. Both these ages experience a relatively essential mortality but the former fact heightens, and the latter fact lowers the general mortality if only the order of extinction is the same as in a stationary population.

Bortkiewicz proved that as a rule the influence of the second fact had prevailed and the rate of mortality tended to lower. He presented the pertinent theorems and their corollaries in a small popular volume devoted to the doctrine of population.

From studying the order of extinction Bortkiewicz directly turned to insurance mathematics for which this notion is fundamental, see his work on adjustment calculations in the theory of errors and lowered mortality (1909/50; 1910 – 1912/56), on mortality of disability pensioners (1899/18) and on security in cases of social insurance (1909/49) which (welche) are connected with the so-called independent probabilities.

A booklet of 60 pages (1898/14) called *the law of small numbers* (LSN) secured him universal fame. From the time of Poisson it became known that beyond the usual borders of the Bernoulli theorem which presupposes a large number of trials and probabilities near to 1/2, there exists a second proposition, also for a large number of trials, but extremely low probabilities[1].

No one had thought of attaching practical significance to those [new] formulas, and only Bortkiewicz, by his example (the number of soldiers who died after being kicked by a horse) that became famous[2], showed that the new pattern possessed a quite real practical importance and connected it with the Lexian theory of dispersion. He thus enriched the theory of probability by an essentially new tool, was able to defend it from unjustified attacks and specifically proved that he had thus achieved a quite original success. Only the name of that law, to which he stubbornly kept, was unfortunate since it denoted a non-existing contrast to the law of large numbers. In essence, a better name would have been *law of rare events*.

The theory of probability is known to have been attacked from two directions. Some thought that seldom events occur oftener, others – rarer than the theory can suppose. Marbe [1916 – 1919] was a notable representative of the second group. He stated that iterations (repeated occurrences) happen more seldom but in a bulky book Bortkiewicz (1917/66) refuted him and showed that Marbe's opinion was only based on the application of a wrong pattern. A suitable model, the law of rare events, provided a satisfactory agreement between theory and experience.

Bortkiewicz devoted a book (1913/59) to an apparently utterly abandoned field of radioactivity. He connected that phenomenon with successively occurring random events and showed that the regularities which had been thought to be peculiar to radioactivity were actually known theorems of the theory of probability.

Out of numerous works in this field [theory of probability and statistics] we only mention studies on the law of distribution of the squared sum of random errors (1922/92), on the relations between the

expectation of an error, the mean error and the density of the arithmetic mean (1923/94) and on the range of the observations governed by the normal law (1922/90). In that last-mentioned instance he showed that even the extreme observations which were usually thought unfit for describing the frequencies were quite regularly connected with the mean expectation of errors and the number of observations. Bortkiewicz corroborated this conclusion by rich empirical knowledge.

The study of dispersion has been occupying the central place in Bortkiewicz' thoughts. In his critical papers on theoretical statistics he generalized and justified the fruitful course of considerations in the Lexian theory and established its boundaries by calculating the mean error of the square of the coefficient of dispersion. In his earliest works he followed Lexis, and later defended the originality of his teacher. One of his last works (1931/108) was also devoted to that theme[3].

Even in his earliest years Bortkiewicz investigated theoretical economics and kept to a quite original viewpoint which even now the specialists do not sufficiently recognize. Like Lexis, he refused to repeat the often pronounced popular arguments against Marx and was the first to show the Marxist dry pattern in a mathematical form and to check the method of transition from value to cost price and the determination of the norm of the mean profit. Regrettably, the difficult manner of Bortkiewicz' writing has been keeping the followers of Marx from perceiving him.

Among the other contributions to mathematical economics remarkable first of all were Bortkiewicz' investigation of indices (1923 – 1924/96). Following Irving Fisher who had unsystematically introduced great many indices, he achieved clarity and order by requiring that one index was sufficient. In one of his last large works Bortkiewicz (1930/104) partly sided with the so-called Pareto law and systematized all the methods of measuring the concentration of the amount of incomes[4].

Bortkiewicz had a special way of working. He most thoroughly formulated each problem and considered it from all angles and studied a large number of pertinent sources. This heightened the reliability of his results, but the very thoroughness sometimes precluded a direct course of thought. Numerous side currents flowed into the main channel at each study and bulky discussions were described. His contributions required serious concentration, but he offered much to those who were really studying him.

Bortkiewicz fruitfully influenced many scientists but left no school which was partly because he was a difficult man. He actually underestimated his own work and even mistakenly doubted its practical importance. This circumstance could have contributed to his

reluctance to attract gifted men: being excessively responsible, he feared to raise their hopes. His reserved disposition had forbidden him to secure honour. He never yielded to wide-trodden expressions and left elegancy and pomp to tailors and shoemakers, remained morally honest in the face of any fashionable expressions, for example, statements about *ill-timed opinions*. He was a scientist of the old school.

Bortkiewicz' life went on envyingly calmly. In 1892 he defended his dissertation in Göttingen, in 1895 became a [Privat] docent in Strasbourg and in 1901 an extraordinary professor in Berlin. In spite of his grand international reputation he was seldom honoured. He was member of the International Statistical Institute and only in 1920 became a personal ordinary professor so that now this position is threatened[4].

The author provided a bibliography of Bortkiewicz.

Notes

1. Near to 1/2: this restriction is too severe.

2. This is Bortkiewicz' *Paradebeispiel*. He also provided quite a few examples from population statistics. Poisson had followers even before Bortkiewicz (1904/22) who himself in this connection mentioned Lexis.

3. Cf. Gumbel (1958).

4. See [xviii, § 6 and Note 15].

5. Personal professor: a professorial position which does not necessarily persist after his retirement/death/resignation.

Appendix

Additional information gleaned
from an English source Gumbel (1978)

Bortkiewicz (1917/66) also contributed to the theory of runs.

The LSN says that rare events usually show normal dispersion (as Lexis called it); for explanation see Gosset (1919).

Bortkiewicz' deliberations *created an important instrument for mathematical statistics and probability theory*.

The generalization of the Lexian methods *led to the modern analysis of variance*.

Borkiewicz' paper (1910/55) is *a masterpiece on the theory of rent*.

He criticized Pearson for the use of empirical formulas having no theoretical meaning.

Bortkiewicz *criticized with equal zeal and profundity important and insignificant mistakes, printing errors and numerical miscalculations*.

Quotation from Keynes (1921/1952, p. 403, note 2):

Bortkiewicz does not get any less obscure as he goes on. The mathematical argument is right enough and often brilliant. But what it is all about, and what it really amounts to, and what the premises are, it becomes increasingly perplexing to decide.

Bortkiewicz' *dry presentation prevented the Marxists* (*except for Klimpt* [1936]) *from accepting his method*.

Sheynin (1966), see also Kendall (1971), has indicated the priority of Abbe in deriving the chi-squared distribution. I (1970) have also indicated that Bortkevich had took a postgraduate course in political economy and statistics and that he was member of the Swedish Academy of Sciences, of the Royal Statistical Society and American Statistical Association.

XI

von Mises

Ladislaus von Bortkiewicz

Chronik der Friedrich-Wilhelms Universität zu Berlin 1931 – 1932.
Berlin [, 1932], pp. 14 – 15

Ladislaus von Bortkiewicz was born on 7 August 1868 in Petersburg, but he was almost exclusively indebted to Germany for his scientific upbringing. All his life he thought that he belonged to the German scientific community[1].

He was a student of Lexis in Göttingen and of Knapp in Strasbourg and there in 1895 he became [privat] docent. After a short period of work in Petersburg he moved to Berlin in 1901 as extraordinary professor at the university there. From 1920 to his untimely death on 15 July 1931 he remained there as ordinary professor of economics at the philosophical faculty.

Bortkiewicz' scientific activity belonged to theoretical economics whose methods of study he understood already then when the historical school had entirely been predominant in Germany. He also understood mathematical statistics one of whose most important representatives he undoubtedly was.

In his works on economics mostly devoted to the theories of value, money and pricing, Bortkiewicz was mostly critically inclined. In the first place he was interested in thoroughly analysing the theoretical systems described in literature. His works (1906/37; 1906 – 1907/40) as well as (1921/86) are of a lasting significance. In all of these contributions he showed himself as a shrewd critic endowed with a sharp feeling of justice, as an author who only delivers his verdict after a thorough study of the sources.

Bortkiewicz' merits in the field of the applied theory of probability and mathematical statistics are quite appreciable. Best known is his law of small numbers (1898/14) which put in the forefront the long

since disregarded side of statistical thought. His paper (1918/68) is an essential progress in the Lexian theory of dispersion.

Bortkiewicz necessarily busied himself with the problems of the theory of probability which did not directly belong to the narrower area of statistics, for example, in the books (1913/59; 1917/66). On the border between political economy and mathematical statistics is his investigation of the problems of index numbers (1923 – 1924/96) which is important for a policy on money and currency.

Note

1. Anderson (1932, p. 242/1963, p. 530) reasonably stressed Bortkiewicz' Russian initial upbringing and in any case his relations with the German scientific community did not square with the opinion of Mises.

XII

O. Anderson

Professor V. I. Bortkevich as a statistician

Newspaper *Rossiya i Slavianstvo* (Paris), 15 Aug. 1931, p. 3

[This obituary notice almost repeats the author's publication of 1929 and I am only adding the new material (fragments now put together without gaps).]

With Bortkevich went to the grave one of the most eminent and at the same time the most peculiar theoretician of statistics who ranks with Quetelet, Lexis, Chuprov and Pearson. Some scientific isolation of Bortkevich who enjoyed incomparably greater recognition abroad than in Germany (where he had almost no students) was possibly even one of the elements of his life personal drama.

His great merit is the transfer into the theory of mathematical statistics of a very powerful and fruitful *method of expectations* which originated in the works of the mathematicians Bienaymé and Chebyshev[1].

For many years and until his death Bortkevich remained the *superior inspector* of sorts of the scientific thought in the sphere of his specialty. Many of those who had published a contribution to the theory of statistics or political economy nervously and sometimes trembly awaited his opinion, not rarely severe, at times cruel but always impartial and justified.

On the other hand, a short approval of that ascetic of science meant more than the most ardent praise of others. This is why Bortkevich' significance should be measured by what he wrote himself and what others had written owing to his criticism and indications. For that

matter, his great merit is that possibly a very large number of mediocre and weak scientific work had *not* appeared since their authors feared his destructive criticism.

Note

1. See [vi, Note 7].

XIII

R. Meerwarth

Ladislaus von Bortkiewicz, 1868 – 1931

Bull. Intern. Stat. Inst., No. 1, vol. 26, 1936, pp. 254 – 258

Ladislaus von Bortkiewicz, ordinary professor of economics and statistics, died on 15 July 1931 in Berlin. He was an acute student of a peculiar stamp.

Bortkiewicz was born on 7 August 1868 in Petersburg. At first he studied in the Petersburg University and already in 1890 he published a paper (1890/3) [then (1891/4) followed]. He then continued his education in Germany and in 1893 became doctor [of philosophy] in Göttingen for his dissertation (1893/6). Even there the powerful influence of Knapp but mostly of Lexis on the young scientist had been manifested.

In 1895, after further studies, especially in the fields of theoretical and population statistics, Bortkiewicz became [privat] docent in Strasbourg University. The first result of his investigations, which had been issuing from Lexis [indirectly] (whom we repeatedly mention below) and oriented to him, was the contribution (1894 – 1896/8). In the first place it was devoted to the applicability of the pattern of notions of probability theory to statistics, and to the measure of such applications. As Bortkiewicz put it in the very first lines of that work:

My aim is the study of the conditions for applying the calculus of probability to the doctrine of social mass phenomena and for studying them deeper than it is usually done. In addition, I strive to show that in some respects the borders of that kind of treating statistical results were set too narrowly but that at the same time the practical importance of the theory of probability for statistics was often overestimated.

Already there we clearly see Bortkiewicz' critical vein and gift supported by thorough philosophical and mathematical knowledge. From the pedagogical viewpoint he also offered a remarkable description of his viewpoint on the application of the theory of probability to statistics in an encyclopaedic paper (1901/22).

But at that time, 1901, he was back in Petersburg and from 1899 to 1901 read lectures at the Aleksandrovsky Lyceum in Petersburg. In 1901, having been invited to fill the position of extraordinary professor in Berlin University, he presented as a review (1904/29) an excellent summary of the collected works of Lexis (1903). With unusual precision he described the everlasting significance of his entire *school*.

In his theoretical studies Bortkiewicz had in many respects consolidated the achievements of his teacher, and in this connection I mention his booklet (1898/14). Apart from the field of theoretical statistics he studied population statistics. In the first place I ought to remind the reader of his works (1892/5; 1894/9; 1903/27; 1911/57). He became pretty successful in that field by his *generally understandable* booklet (1919/72). There, he wholly declined the use of mathematical formulas and quite clearly presented theoretical considerations, for example, concerning the recognition of the pattern of stationary population.

It was often remarked that the works of Bortkiewicz are difficult to get hold of since they had appeared in most various sources. Thus, a study in the theory of probability (1917/66) was in essence a criticism of Marbe [1916 – 1919] although it contained an important contribution to the law of large numbers, the chi-squared method etc. His report (1918/68) in which he continued the study of the Lexian theory of dispersion belongs to the same period.

Keynes (1921/1952, p. 403, Note 2 or 1973, p. 440, Note 2) raised an essential objection to these studies permeated with higher mathematics:

The mathematical argument is right enough and often brilliant. But what it is all really about, and what it really amounts to and what the premises are, it becomes increasingly perplexing to decide.

Shortly before his death Bortkiewicz (1931/108) attempted to refute this objection.

For ten years up to that time he, just like his friend Chuprov, had preferred to publish his work in an imperceptible journal, *Nordisk Statistisk Tidskrift* edited by Thor Andersson[1]. Two of his papers (1921/89), and as I especially notice, (1923 – 1924/96) appeared there. This latter was connected with a critical description of I. Fisher (1922) but was later extended and became an essential systematic account and criticism of index numbers. I also indicate the paper (1930/104) as a continuation of this social statistical method[2].

I have singled out especially important statistical contributions. It has been counted however that Bortkiewicz penned 54 larger statistical monographs, four of them books. Apart from them (which included the theory of probability) Bortkiewicz essentially engendered mathematics (including insurance), physics (1913/59) and, in the first

place, theoretical economics, in particular the problems of the theories of money and pricing.

For many terms [the last-mentioned problems] remained as the themes of his lectures at Berlin University. In these theories he attempted to follow a middle course between objectivism and subjectivism (1921/86). In many papers Bortkiewicz argued with Walras, Pareto, Böhm-Bawerk, Marx, Knapp (concerning the theory of money), Alfred Weber (on the placement of heavy industry) etc.

In most cases these papers were based on an unprecedented knowledge of literature and are masterpieces of the spirit of constructive criticism. As a critic of the formal history of economics he protected with all his peculiar honesty the heritage of the masters and sternly punished each mistake and false attribution of successes. I cannot deny that his criticism sometimes became pedantic and dull, it thoroughly castigated both essential and unimportant defects, but no doubts about its essence or usefulness had ever been expressed.

Many recall his very detailed criticism of 1911 of the methods used at an extensive study of the selection and later adaptation of employees of large industrial enterprises, see [xiv, § 4].

Bortkiewicz had naturally been especially interested in the application of the mathematical way of thought in economics. And exactly here his death pierced a perceptible breach. I imagine the whole [economic] world flooded by *quantitative analysis* clad in armour of higher mathematics and geometry, and it is here that the absence of his warning criticism will be felt. Those who, for example, read his thorough statement (1910/51) against Alfred Weber about the barely useful geometric descriptions and methods of proof in the doctrine of the placement of industrial enterprises will often regret the absence of Bortkiewicz' guiding pen.

Those statisticians who discern danger rather than usefulness in reckless applications of the notions of probability theory to the solution of economic problems (for example, to representative sampling in the field of statistics of national economy), those statisticians will miss his warnings.

It was he who always understood the borders within which that theory is applicable; and he had not identified mathematical statistics and statistics even if oriented to probability theory. When adopting the viewpoint of the real problems of statistical studies he, just like his teacher Knapp, understood the reduction of statistical results to empirical formulas (which is *comme il faut* nowadays) as something entirely subordinated.

Here is a quote from his paper (1915/61 [, p. 244]):

The existence of descriptive formulas is only justified, if at all, when the domain of their approximate effectiveness is not too narrow. If,

however, such a formula is only applicable in a special case for which it was derived, its theoretical and practical importance is zero[3].

Anyway, there will hardly appear a guide better than Bortkiewicz in the struggle with a wrongly understood and/or applied mathematical statistics.

In *Wendegang und Schriften der Mitglieder*. Kölner Verlags-Anstalt und Druckerei. Köln, 1929 (Vereinigung der Deutschen Sozial- und wirtschaftswissenschaftlichen Hochschullehrer) and especially in its Supplement (Breslau 1931) we find an almost complete list of Bortkiewicz' numerous publications. Brief reviews are not however included.

Notes

1. This journal was new (and very interesting, see its first volume). Both Bortkiewicz and Chuprov had close ties with Scandinavian countries.

2. In 1913, Chuprov (Bortkevich & Chuprov 2005, Letter 122) informed Borftkiewicz about Gini's paper and added that he could send it. However (Letter 123) Bortkiewicz arrogantly answered: the appropriate source is unavailable in the Royal Berlin Library (the present Staatsbibliothek zu Berlin) and he is *perfectly in the right* to ignore it. Bortkiewicz (1931/108a) defended himself but certainly concealed his arrogance.

3. This statement can apply to any formula.

XIV

Wilhelm Lorey

Ladislaus von Bortkiewicz

Versicherungsarchiv, Bd. 3, 1932, pp. 199 – 206

[1] Professor Dr. von Bortkiewicz, acting as the president of the mathematical section of the *Deutsch. Verein für Versicherungswissenschaft*, invited some people to Berlin in connection with the theses proposed for the next international congress devoted to insurance business.

However, he was unable to chair the scheduled meeting: an aorta illness which had lasted five years, with which he attempted to struggle by treatment in Wiessee, became complicated by influenza and powerfully influenced his heart. A cure in Nauheim helped, but in summer a chill worsened his condition.

Nevertheless, he continued to read his lectures until 9 July. On 15 July there happened the last, horrible heart attack. He recovered for a short time and became able to busy himself with something at home, but that same day a peaceful death delivered him from his suffering[1].

Ladislaus von Bortkiewicz was born on 7 August 1868 in Petersburg. His father was a colonel who had also been teaching mathematics in military schools[2]. After graduating from a gymnasium in 1886, Bortkiewicz studied the law [in Petersburg University] but at the same time learned mathematical statistics and became able to publish two papers [1890/3; 1891/4]. He had passed the state examination as a lawyer and the government sent him to Germany for continuing his education, at first for two terms to Strasbourg where he especially attended lectures on statistics read by Knapp.

At the beginning of the summer term of 1892 Bortkiewicz moved to Göttingen where he studied economics and statistics as well as philosophy. Mostly however he attached himself to Lexis and by the end of that term became a doctor [of philosophy] and the dissertation (1893/6) 118 pages long already revealed his peculiar attitude of thorough essential criticism and indispensable bearing on sources. His criticism was then directed mainly at the generally recognized book of Roghè (1890) which is now considered groundless.

In winter, after obtaining his doctorate, Bortkiewicz apparently visited Vienna. Indeed, he (1892/93) later mentioned his work there. Mostly, however, he had been preparing himself for obtaining the status [of docent] in Göttingen [which had not occurred]. There, certainly from Lexis himself, he heard about plans of establishing a seminar on insurance science. These plans had originated under the impression of a report by L. Kiepert at the mathematical section of the Vienna conference of naturalists in 1894 (Versammlung deutscher Naturforscher)[3].

That seminar began to work in Göttingen in October 1895 when Bortkiewicz was already privat-dozent in Strasbourg. The two parts of his work (1894 – 1896/8) occurred quite acceptable as a contribution for obtaining the status of docent. As Bortkiewicz himself noted, that work had aimed at a somewhat more thorough than usual justification of the conditions for applying the theory of probability to the doctrine of mass social phenomena. It contained a special proof that in some sense rather too narrow borders had been established for such investigations of statistical observations. He also noted that the practical significance of the theory of probability for statistics had been overestimated. But at the end of the third part of that work Bortkiewicz stated[4]:

*When discussing somewhat more complicated problems, a statistician needs theoretical indications. They can now be found in that part of the so-called mathematical statistics which studies the formal (*mathematical*), not material, physical, relations between the considered relative and mean statistical figures in statistical masses or totalities. Those relations are left in our minds and our memories*

owing to an analytical or geometrical description and methods of proof.

When indicating those geometrical descriptions and methods Bortkiewicz had first of all recalled the methods which his teacher Lexis had applied in population statistics. He himself, as shown by almost all of his numerous works in which he applied supplementary mathematical means, was obviously not inclined geometrically.

[2] During his Strasbourg period Bortkiewicz published his law of small numbers (1898/14) devoted [not dedicated] to Lexis. It is now more commonly known as the law of rare events. It studies the practical applicability of the Poisson formula of density

$$w_x = \frac{m^x e^{-m}}{x!}$$

where *m* is the expected number of the occurrences of a rare event. The importance of that formula has later increased, also in mathematics of insurance, see for example the works of [Alf] Guldberg [1922]. Quite well has the hope expressed in the Introduction come true: the booklet *will animatingly influence the applied theory of probability and foster the interest in its field.*

Nevertheless, Bortkiewicz (1915/61[, p. 256]) had to argue sharply with Gini and especially with Pearson [indirectly] and his students [L. Whittaker]:

I believe that my law of small numbers which is really oriented to the Lexian theory of dispersion will retain its special place along with that theory and still be recognized long after the Pearsonian negative binomials will be quite deservedly consigned to oblivion[5].

During the mid-1890s mathematicians came to think about a publication of an encyclopaedia of mathematical sciences and their applications. By that time Bortkiewicz became well known by his works and life in Göttingen, especially to Felix Klein. He received an invitation to compile an entry to the future encyclopaedia. When his quite well-founded contribution (1901/22), later extended by Oltramare for the 1909 French edition of that source, had appeared, he was already once more in Petersburg as a functionary of the Ministry of railways and, at the same time, a lecturer at the Aleksandovsky Lyceum[6].

However, that same year, 1901, Bortkiewicz moved to Berlin to occupy the position of extraordinary professor of economics and statistics. The initiative apparently came from Lexis[7] who maintained close ties with the Ministerialdirektor Althoff [at the Ministry of religious, educational and medical affairs]. In 1920 Bortkiewicz became ordinary professor.

[3] Soon Bortkiewicz entered into relations with the recently established German Union of the Science of Insurance (Deutsch. Verein f. Versicherungswissenschaft). In 1903 he became member of its committee, and, from 1926, head of the section on mathematics of insurance.

Apart from reviews he published in their periodical five papers [1906/35; 1909/50; 1910 – 1912/56; 1915/62; 1916/65]. The paper of 1915 was devoted to the 75th anniversary of his teacher, Lexis. In his second and third contributions Bortkiewicz argued with Patzig with whom A. Tauber sided in 1931 in *Assekuranzjahrbuch*. In the last paper he came out against the state control agency (Reichsaufsichtsamt) and stressed the importance of a preliminary study of mathematics. Nevertheless, he included a reservation:

A preliminary study of mathematics certainly does not unquestionably lead to a satisfactory solution of statistical problems.

A similar opinion is contained in another article (1913/58):

As a rule, mathematicians do not possess the ideas usual in the sociological sciences. They are connected to a proper appreciation of mathematics in statistical studies. Understandably, they attempt to find absolute solutions which under some circumstances assume whimsical forms.

I had once remarked, contradicting a mathematician of international fame, that the calculation of premiums in life insurance is based on two different mortality tables depending on whether the insurance is in case of death or survival. He then disgustedly exclaimed: Then all of it is a deceit!

However, to say the truth, a preliminary and purely mathematical preparation, if not accomplished earlier, will be quite proper for a mathematical statistician. The main ideas and the aims of mathematical statistics and of the general scientific statistics are the same, but the former is working more precisely and at least more consciously.

Here and elsewhere Bortkiewicz says just what can be repeatedly found in the work of his Russian friend, Chuprov, especially in mathematical computations. He was obviously a self-educated mathematician. As compared with a real mathematician his undoubtedly well-founded mathematical considerations are sometimes rather verbose[8]. His Göttingen dissertation (1892/15) contains an apparently still unnoticed mistake which however was there unimportant: he thought that continuity leads to differentiability.

It seems that in most cases Bortkiewicz attempted to apply more elementary mathematical means and he (1917/66, Introduction) said so himself. In the second chapter of that book which appeared as the result of his dispute with Marbe [1916 – 1919] we find an elementary and quite clear explanation of the notion of expectation And I think

that that notion becomes ever more important, for insurance mathematics as well.

Bortkiewicz participated in the fourth, fifth and sixth international congresses on insurance science. In 1903, in New York, he was the German reporter [1904/30] about the university instruction in mathematical insurance. Distinctly expounding the importance of this application of mathematics for the insurance right and economics, he added that it will be easy to prove that some unsuccessful or defective law constructions as well as certain unfounded demands made by economists on insurance had been caused by insufficient knowledge of the main propositions of the insurance of life.

At the Berlin congress of 1906 Bortkiewicz participated in the discussion on social insurance and especially on insurance of children. At the Vienna congress of 1909 he [1909/49] reported about the methods of coverage (of assurance) in social insurance.

[4] His later paper (1929/103) is quite understandable and interesting, but his original report was not really captivating. I happened to hear the same about his reports at the Berlin mathematical society and Scandinavian unions of actuaries although their published form should be insistently recommended for study[9]. Just the same can undoubtedly be said about them as what Max Weber had dropped in 1911 at the conference of the Verein f. Sozialpolitik[10]:

As believes at least the majority of those who attended, the dullest among the reports delivered today was that of Prof. Bortkiewicz. At the same time, it was the most sensible and the critical remarks which were contained there have been to the highest degree suited for professionally assisting us.

Bortkiewicz was always quite sensible. However, a listener who attended his first lecture on insurance mathematics in Berlin testified that as a lecturer he was fairly dry. But that same listener had compiled quite good notes of that lecture.

From 1920 to 1930 Bortkiewicz participated in the defence of fifteen dissertations in Berlin. Two of them (Gahler 1927; Roß 1929)[11] were devoted to insurance science. He was also an opponent at the defence of doctor of medicine Freudenberg (1926) who had earned the degree of doctor of philosophy and later compiled a good obituary of his teacher[12].

[Almost] the last work of Bortkiewicz (1931/108) borders his earlier contribution (1918/68) and answers the criticism pronounced by Keynes (1921). He explained his remark by an interesting example from the insurance against fire.

In this obituary it was impossible to draw even a slightest comprehensive picture of Bortkiewicz' fruitful scientific activity, and I had to be satisfied by stressing his importance for insurance science and mathematical statistics. A superficial testimony about it is

provided by the very often citation of his works in the third edition of the *Versicherungslexikon* [edited by A. Manes. Berlin, 1930].

Notes

1. For this and other biographical information I am grateful to Helene v. Bortkiewicz, the sister of the deceased. W. L.

2. The father of Bortkiewicz had published a mathematical textbook for gymnasiums (I. I. Bortkiewicz 1872) which Chebyshev severely criticized, perhaps even too severely. O. S.

3. I (1922) have described in detail the establishment of that seminar by drawing on the documents of the Berlin Ministry of Science, Art and People's Education. Elsewhere I (1925) described the importance of Lexis in the insurance science. W. L.

4. I have not found this statement either in the indicated place or elsewhere. Bortkiewicz (1894/1896/8) more clearly explained the role of that contribution than Lorey (see a bit above). See [xiii]. Note the *so-called* mathematical statistics, apparently the only suchlike reservation voiced by Bortkiewicz. O. S.

5. Unlike the law of small numbers the negative binomial distribution is still living. Actually, however, Bortkiewicz categorically called in question the results of computation which led to nonsensical values of the parameters of empirical formulas. Cf. his statement about such formulas in [xiii]. O. S.

Polya (1928, p. 705, Example 15) described an interesting case from medical statistics: the law of rare events provided more importance to some observations than assumed by the author of the original paper. W. L.

6. See [i, Note 1]. Bortkiewicz became lecturer somewhat later. O. S.

7. From the materials of the Ministry and the philosophical faculty, as the Ministry councillor Schellenberg and Professor von Mises told me, there is nothing contradicting my assumption. See also Nybolle (1932) whose obituary I have noticed later. W. L.

8. Bortkiewicz reasonably decided that mathematicians had insufficiently recognized him. After thanking me for congratulating him on his 60th birthday he added that that was all the more gladdening since it was the only one which came from mathematicians. However, we may repeat a few lines written by Mises [xi]:

He was the most eminent researcher in the field of mathematical statistics in Germany and his works had been widely known abroad. W. L.

This phrase essentially differs from what Mises wrote. O. S.

9. These papers are: (1918/69; 1920/76 and 79; 1922/90; 1923/94; 1926/98). In the same journal that published the last of these papers Bortkiewicz published two other papers (1918/68; 1927/102). I mention the first one below; the second one is a brief note in which he very favourably described his aspiration for managing by elementary means. And here are two more of his papers published in mathematical periodicals: (1918/70; 1922/92). W. L.

10. Included in [xiii]. W. L.

11. Professor von Mises gave me a complete list of the doctor dissertations in which Bortkiewicz was the first reporter [opponent]. W. L.

12. My opinion about Freudenberg's obituary is opposite. Perhaps Lorey had no time for reading and still praised it. And why was Bortkiewicz the teacher of that physician? O. S.

XV

Jos. A. Schumpeter

Ladislaus von Bortkiewicz (Aug. 7, 1868 – July 15, 1931)

Von Bortkiewicz, by far the most eminent German statistician since Lexis whose pupil he was in important respects, was not a German by descent. He came from one of those Polish families which made their peace with Poland's Russian lords, and was brought up in St. Petersburg, his birthplace, where he also went to the University and where he later on taught for a time[1]. Connections formed during a prolonged stay in Germany, where in 1895 he had become a Privat-Dozent in the University of Strasbourg, led to his being appointed, in 1901, to an *extraordinary* (= assistant) professorship at Berlin. Characteristically enough, this eminent man was never thought of as a candidate for one of the great chairs, either in Berlin or at any other University, and it was not until 1920, when by a measure intended to *democratise* faculties, *all* extraordinary professors became full professors *ad personam*[2], that he obtained that rank, without however ceasing to be entirely isolated.

There were several reasons for this. He was a foreigner. Although not a clumsy speaker or writer, he was not a good lecturer, and his lectures, which he elaborated with a minute and conscientious attention to details all his own, were said to be delivered to rather empty classrooms. His critical acumen made people fear him, but it hardly contributed to making them love him.

Those colleagues whose duty it would have been to propose his name to the Ministries (?) of Education were hardly in a position to understand his contributions. He did not seem to mind but kept aloof in dignified reserve, enjoying the respect with which everyone looked upon him, and a quiet scientific life to be cut short in the fullness of his powers by an unexpected death. A bibliography of (as far as I can see) his whole published work has been drawn up by Professor Oskar Anderson[3] to which I refer the reader.

Nature – it is not often that the goddess makes up her mind so decidedly – had made him a critic, so much so that even his original contributions assumed the form of criticisms, and that critique became his very breath. This critical faculty, or rather passion, which did not stop short at small blunders in numerical examples, stands out particularly in his work as an economist.

Here he was not an originator, and I believe he just missed greatness by refusing to put to full use the mathematical tools at his command[4], which at the time of his prime might have made him rival the fame of Edgeworth or of Barone[5].

But he upheld the flag of economic theory – professing the Marshallian creed – at an epoch and in a country in which hardly anyone would hear of it, and he cleared the ground of many battlefields by his powerful sword. By far his most important achievement is his analysis of the theoretical framework of the Marxian system (1906 – 1907/40; 1907/42). It is much the best thing ever written on it and incidentally on its other critics. A similar masterpiece is his paper on the theories of rent (1910/55).

Where blunders are secondary and fundamentals sound, as in the cases of Walras, Pareto and Böhm-Bawerk, the stern critic shows no less advantage. As a writer on monetary theory and policy, he ranks high among German authors. The subjects of the gold standard, of banking credit, of velocity of circulation owe much to him[6]. The best he did in this field however is his work on index numbers (1923 – 1924/96), a masterly review of Irving Fisher's work amounting to an original contribution in the matter of tests.

In the field of statistical method, his *aristeia* [feat] among Germans is of course undoubted. As the discoverer of the *law of small numbers* (1898/14) and the leader of the Lexian school[7] he has won an international name which will go down to posterity. His book on probability (1917/66), his only *book* – he had so great an inhibition on giving to the public that he lost some of the claims to high originality which he would otherwise have had – is an admirable piece of work even when looked at without any predilection for the fundamental conception of probability which underlies it. It is impossible, nor would it be proper in an economic journal to unfold the long list of Bortkiewicz' contributions to the theory of statistics.

A few instances of special importance to the economist must suffice. No one has done more to clear up the important subject of the measures of inequality of incomes (1930/104). Most of us will read with profit and pleasure those excellent papers on the quadrature of empirical curves (1926/98) and on homogeneity and stability in statistics (1918/68), or on the one on variability under the Gaussian law (1922/90), or on the property common to all laws of error (1923/94)[8], or on the succession in time of chance events (1915/64), not to mention any of his papers on mortality and insurance, some of which are treasures of their kind.

But in order to give an idea f the compass of his mind it is necessary to point out one more opusculum of his, far removed though it is from economics: his pamphlet (1913/59). In turning over the pages of this paragon one seems to discern the true contour lines of the mind of the *economist* who wrote it, and one begins to wonder whether one can rely on what he published as a measure of the range of his possibilities.

Notes

1. This is a mistake: Bortkiewicz never taught there. Corrected by A. L. Dmitriev who confirmed my doubts.

2. Personal professor: see [x, Note 7].

3. [The author cited the bibliography compiled by Anderson in 1929 [vi]. Meerwarth [xiii] mentioned another bibliography. The best one is apparently appended here. The author continues:] When writing about a man who was a paragon of conscientiousness I may perhaps allow myself for once to follow the example set by him and to point out a misprint occurring on p. 279, sub No. 2, of the list of his economic papers. He did not, in his critique of Pareto's Cones, reproach the marginal utility school with fostering an *ultra-radical* economic policy, but an ultra-liberal one. J. S.

4. Here is how Bortkiewicz himself described his feeling about mathematical tools. In 1896, in a letter to Chuprov, he (Sheynin 2011, p. 60) wrote about his calculation of the variance in a concrete case: he refused to apply generating functions and consecutive differentiations as mentioned by Markov in a talk with him. That method was apparently little known by statisticians and it would have been better to explain it to them.

5. Enrico Barone, 1859 – 1924, an Italian economist.

6. In 1923, Slutsky published two Russian papers on the circulation of paper money. In 2010 I have translated many papers of Slutsky including those two (**S, G,** 25). Bortkiewicz (1925/97) had likely studied the same theme but he certainly did not know about his predecessor.

7. Concerning his law of small numbers see [vi, Note 6]. Bortkiewicz himself described his furthering of the Lexian theory (Sheynin 2011, pp. 138 – 139). However, at least in 1919 Chuprov had lost faith in that theory but somehow kept to it until 1921 (Ibidem, pp. 142 – 143). See also Sheynin (2017, § 15.1.2).

8. I doubt however that all the laws discussed there by Bortkiewicz had been applied in the theory of errors.

XVI

Hermann Schumacher

Ladislaus von Bortkiewicz. A Memorial Speech
at the cremation on 18 July 1931

Allg. stat. Archiv, Bd. 21, 1931, pp. 573 – 576

The man at whose grave we are gathered was extremely modest and our calm solemn mourning complies with that characteristic. His sister had been caring for her brother up to his last day and all the admirers and friends of the deceased think about her with heartfelt thanks and deep condolence. Only a narrow circle of men and women had been near to Ladislaus von Bortkiewicz, the scientist, the man and his works.

In accord with his wish instead of clerics only the representatives of science and friends will speak today. (He was brought up as a Uniat[1].) The Berlin University, in which he had been teaching for 30 years and

especially the philosophical faculty where for the latest ten years he had been ordinary professor, and his scientific colleagues entrusted me the agonizing and honourable duty to take leave for the last time of him and once more recall what exactly have we got from, and what have we lost with him.

In economics Bortkiewicz occupied a unique position not only in Germany but in the whole world and a place special to the highest extent. I cannot name anyone either modern or not who can be placed alongside. Even in future this situation will hardly change. He devoted himself to science so selflessly as though following the Biblical command (Exodus 20:3): *You shall have no gods before me*.

Bortkiewicz considered it his sacred duty first of all to defend in his science the treasury of one and a half century of theoretical knowledge and to defend it from corruption. There apparently never before was anyone who so thoroughly acquainted himself with all parts of that heritage and so lovingly studied them. His remarkable memory rendered him unusual service for achieving this. He so confidently memorized an inscrutable number of both successful and unfortunate formulations that any check was thought useless.

This remarkably accurate knowledge of history he acquired not for effectively applying it in his own accounts and representing it in a favourable light. No! He had not thought about that at all, but when someone had wrongly reported or interpreted the opinion of a master whom he appreciated, he appeared on the stage clad in full scientific armour. We have to wonder, time and time again, how heavy was the artillery which he arranged and how precisely he aimed it on his target.

He only opposed corruption ad mistakes but remained very remote from securing certain views by his authority. He was a critic rather than a fighter. If he thought that he had fulfilled his duty for science in a knightly way, he considered that the problem was solved. What kind of conclusions will the others formulate from his explanations, was for him situated on the other side of science and hence indifferent. Undoubtedly his scientific activity had thus repeatedly revealed a streak of passivity and negation. Deep respect for the achievements of the past mercilessly disposed him against others and made him extremely modest.

So it happened that a large part of his scientific work was scattered among periodicals and in many brief notes which were reviews in essence if not formally. But even there we often find a deep and all-embracing knowledge and a more thorough work of the mind than in many arrogant papers or thick books. For a long time, and especially after WWI they have not aroused such attention as they deserved, but we may hope that, to the benefit of science, they will be published collectively.

Bortkiewicz is only partly described as a critic with a masterly knowledge of history; he might also be considered as the mathematician of the German political economy, but not in the superficial sense as though smugly introducing mathematical formulas into economic propositions. No, we bear in mind a much deeper significance. Mathematics attracted him as being the most precise formulation of knowledge and first of all in statistics. And as his interest in formal history weakened, his interest in mathematical statistics heightened.

The less visible became the numerical material in economics, the more urgent became the problem of mastering it; the closer became the boundaries of the so-called qualitative analysis, the more urgent became the problem of how much can it be supplemented by a quantitative analysis[2].

Such were the essential problems of a constructive essence and Bortkiewicz was successful and happy since they became ever more significant. In his last years he quite consciously transferred the centre of his activity to the realm of theoretical statistics. Here, he was not only a critic, he was able to be content with constructive success and by right considered himself as occupying the first place in Germany. And beyond its borders there were not many scientists who could be named alongside him. As a theoretician of statistics he became member of the International Statistical Institute and the Swedish Academy of Sciences in Stockholm.

However, when mentioning Bortkiewicz as a mathematician of German political economy I intended to say something else. At the time when scientific responsibility for a separate word is largely lost his example and critique attempted to stress anew the sense of scientific precision not only in numbers but in words as well.

He understood each careless or ambiguous expression as a sin against the spirit of science. He possibly nagged sometimes at amateurs and diffusely established the meaning of a single word. But exactly in such cases he expressed the real earnestness peculiar to his entire scientific activity.

When surveying his life only devoted to scientific work which was cut short so early, we form a strong impression of an unusual integrity and unity. Randomness is often decisive in the life of other scientists, and the more so in the field of economics; here, however, everything was being apparently developing from within. Among the long succession of eminent scholars representing the older generation of economists in Germany Bortkiewicz, even in his youth, became able to single out by a surprisingly faultless flair both radically different men who to the largest extent suited his own essence, Georg Friedrich Knapp and Wilhelm Lexis.

Both were devoted to science with all their hearts and souls even forgetting all the rest and, like Wagner and Schmoller[3], without feeling any inner constraint have not thought themselves obliged to enter scientifically economic and political struggles. They stood aside as calm observers, Knapp not without a weak smile which had often appeared in the same way upon the face of the deceased. Both were thoroughly instructed in mathematics rather than remaining amateurs and exactly in this sense they were intimate with statistics.

In the vast area of political economy they expressed an understanding and special interest in the problems of the theory of money although because of entirely different causes. And in general among that older generation there were barely two people who would have busied themselves with them [with those problems?] more thoroughly.

All this was repeated in the person of Bortkiewicz. All alien to his essence although inherent in his teachers (as Knapp's deep understanding of history or Lexis' administrative and pedagogic work) left him absolutely calm. However, he, just like they, regarded science with a profound respect, left alone national politics but displayed a strong interest in statistics. And not only to its results, but to its methods as well and really understood the importance and the difficulty of the problems of the theory of money.

Exactly in this area the post-war [after 1918] period offered that implacable critic a pretty good possibility of weeding. His work had not regrettably attracted much attention. His voice was only understood by specialists but had not reached politicians or the leaders of economics[4]. Realization of the meaningless of his science finally somewhat disappointed even him, the apostle of science remote from life. It was conducive to his ever greater detachment from political economy and approach to statistics.

The deceased combined a revering of science with respect for his teachers. It was impossible to be attached to them with greater gratitude than he characteristically was. Most often the fixed features of the face of this scientist, who was only interested in his own business, changed and his voice acquired a gentle tone whenever he mentioned Lexis or even Knapp. And then it became evident that a soft heart was beating in the breast of that merciless critic. [Just the same,] in spite of his critical sharpness he mainly treated his students with an unusually soft heart and kindness.

Bortkiewicz had not nurtured many followers from them but, even if they were unable to understand wholly his statements, a large number of students got an impression about his real scientific earnestness. Who had felt a breath of his spirit will be able to recall thankfully the peculiar image of Bortkiewicz. His labours will outlive

him and his name will not disappear from the history of German political economy.

Notes

1. In 1901 Bortkiewicz [i, No. 2] called himself a Roman catholic. I am unable to comment.

2. But where is sampling? Or preliminary study of statistical data? Anderson [vi] mentioned such studies somewhat negatively.

3. Karl Wagner (1893 – 1963) and Gustav von Schmoller (1838 – 1917) were most eminent scientists (statistician and economist respectively).

4. Bortkiewicz never mentioned Walther Rathenau (1867 – 1922) who was an economist of the highest calibre. In 1925, his *Ges. Schriften* were published in five or six volumes.

XVII

Ferdinand Tönnies

Ladislaus v. Bortkiewicz, 1868 – 1931

[1932.] *Gesamtausgabe*, Bd. 22. Berlin, 1998, pp. 315 – 319

Ordinary professor and head of the seminar on university statistics and statistics of Berlin University, Doctor Ladislaus von Bortkiewicz, born 7 August 1868 in Petersburg in a Polish military family. His father was colonel in the Russian army.

He was brought up in the same city, studied the law in the university there and passed the state examination in that science. Already at 21 he wrote an investigation in mathematical statistics which turned such attention that was accepted for publication by the Petersburg Academy of Sciences (1890/3; 1891/4). This circumstance became the reason why the Russian Ministry of Public Education sent him abroad for further education.

After being a student of G. F. Knapp in Strasbourg, then of Lexis in Göttingen, he obviously felt himself fascinated either only or mostly by German science. He continued to study university statistics in Vienna and Leipzig[1], and in 1895 became a docent in Strasbourg and returned for a few years to Russia. There, he was an official in the Russian Ministry of Railroads but in 1901 became extraordinary professor of Berlin University and only in 1920 an ordinary professor there also. He remained a bachelor.

Bortkiewicz' scientific activity mainly belonged to the statistical method and theory of statistics. For a long time his small booklet (1898/14) remained very significant, and, along with many other valuable works, became the reason why he was accepted as honourable member in [scientific bodies in] many countries and

earned other signs of distinction. Thus, he became an effective member of the International Statistical Institute, of Strasbourg Scientific Society (Frankfurt/Main) and member of the Swedish Academy of Sciences.

I would like to say a few words about the law of small numbers. Already the excellent Süssmilch knew the essence of the law of large numbers which certainly only Poisson mathematically justified and formulated[2]. It meant that a plenty number of quantitative results is regular: the deviations from the mean become ever smaller. This is usually explained by an example of playing dice. In such regularities Süssmilch discerned a *Divine order* in the variability of mankind.

Bortkiewicz established that there exist many statistical series of small in absolute value numbers which therefore hardly deserved to be noticed by statisticians since in such cases random causes indeed exert a too powerful influence. But that acute mathematician was able to study the laws of chance exactly for such series and thus to explain whether the theory of probability was applicable to them. He established that the fluctuations almost completely corresponded to the assumptions of the theory which is the essence of the law of small numbers[3].

Bortkiewicz followed Lexis and devoted the third chapter of his booklet (1898/14) to his theory. There, he justified the hypothesis and the pattern of a *changing probability* of the occurrence of the studied event. He thought of minimizing the influence of these changes by applying a small number of the occurrences of that event and thus of achieving an almost normal dispersion. Even those who are unable to understand the mathematical foundation of that doctrine (?) will not fail to comprehend the fundamental importance of the theme.

Von Mayr remained quite a non-mathematical statistician which does not at all diminish his merits in statistics as understood in his sense. He thought that the stability of small numbers which Bortkiewicz considered was essentially due to constant principal circumstances. However, I maintain that the very study of these small numbers and their relative stability are sufficient for seriously shaking Mayr's definition of statistics (1914, Bd. 1, p. 31) as a science of the status and phenomena of social life as far as it (solche) is expressed by statistically perceived social masses[4]. However, I ought to leave this subject.

Bortkiewicz repeatedly studied the theory of population which is very important for theoretical sociology and for social biology in general. As far as I know, he began that work by contribution (1908/46). He (1919/72) continued his study in a volume which regrettably was poorly published: the pages of its first part (population statistics presented in wide boundaries) are now loosened. The second part, *Historical description of the doctrine of population*, briefly

describes the main problems and especially the Malthusian theory of population and therefore is remarkable both for the adepts of this theory and its opponents. His defence of the theory dominates. Indeed, Bortkiewicz believes that the clearly manifested since the time of Malthus and not foreseen by him decrease of the fertility of marriages in the countries of European culture does not impair the foundation of that theory.

Last year, on 30 September[5], Bortkiewicz attended a meeting of our subgroup of sociography[6] and was the first to discuss [my report]. In our minutes, his speech occupied 5 1/2 pages. He left before the end of the meeting, but I had briefly answered him even before my concluding remarks and told him that for me his approval, an approval of a reliable expert whom I highly appreciate, would have meant especially much. He mentioned his intention to return in writing to the discussed problem but as far as I know had not carried it out.

After that I have regrettably not seen him anymore. In his speech he conceded much to me since he made it known that he had cleared up some doubts about sociography. I have therefore reason to think that I would have been able to convince him, if not to attract our respected colleague to us and to our science, in that sociography placed instead of the alleged *statistics as a science* can become the starting point for essential progress with the statistical *method* becoming more although not *exceptionally* important[7].

Mayr had clearly established that *his* "statistics as a science" is only based on the material of *statistical art*. He was convinced that it serves *especially for the aim of public management with a subsequent scientific aspiration to knowledge*. This means that statistics as a science is not free but restrained not only by statistics as a method, but by statistics as an art. In other words, it is restrained by the achievements of official statistics which is certainly mainly very important and irreplaceable also for sociography. But sociography conforms to the main idea of science, it is essentially free and applies any method appropriate for its goals and is based on any materials suitable for these goals.

On April 4 of this year, from Bad Nauheim, the now deceased scientist answered my circular letter about sociography. He is ready to cooperate by discussing suitable work:

I am interested in the inner migration of the population. For some years the Ministry of Religion has been interested in that problem as well.

He also wrote that he had overcome protracted influenza and wishes to begin lecturing from the beginning of May. He died 15th July 1931.

Bortkiewicz was scientifically honest to an unusual extent. The *German* thoroughness had found in him, as in many other not belonging to us by birth, one of its best representatives. By his own

will he became a good German, a conscious citizen[8] of the German Republic and joined the German Democratic Party. I do not know whether, along with it, he transferred to the State party (Staatspartei).

He had plain tastes, was restrained, quite cautious, and enjoyed much favour of many of his students, colleagues and friends. His name will honourably remain in the German Society of Sociology as well!

Notes

1. Nothing is known about Leipzig. On the other hand, Bortkiewicz (1892/93) described his work in Vienna.

2. Jakob Bernoulli and De Moivre are forgotten!

3. This remark is important since its essence is rarely mentioned.

4. Statistics explained by statistics!

5. This meeting obviously took place in 1930. Sociography originated in the beginning of the 20th century as an empirical meta-discipline. It described the structure of separate groups (professional religious etc.) of populations and became somewhat important.

6. This was the meeting of the German Sociologists (*Schriften* 1931, pp. 207 – 212). Editor.

Tönnies (1855 – 1936) was a co-creator of German sociology. O. S.

7. This is difficult to understand.

8. In 1932, Tönnies was alarmed [by the situation in Germany]. Editor.

The latter party, a union of democrats with a nationalist party, had soon disintegrated and the democrats remained alone under a new name. Nowadays, there exists a Social-Democratic Party. In the German language a state party also means the only allowed party in dictatorial states. O. S.

XVIII

T. Andersson

Ladislaus von Bortkiewicz, 1868 – 1931

[1] The founder of the present-day statistical science is William Lexis. The greatest of his pupils was Ladislaus von Bortkiewicz, who was professor at the University of Berlin in 1901 – 1931. One of von Borkiewicz's foremost pupils, Karl Freudenberg, has said that the classical period of theoretical statistical science began about 1876 with the publication of Lexis' first great work and ended on July 15th 1931, the day of von Bortkiewicz's death[1].

Von Bortkiewicz was born on August 7th 1868 in St. Petersburg and was of Polish descent. After having studied the law at the university in his native city, he was sent by the Russian government to study abroad

and received the degree of Dr Phil. of Göttingen in 1892 and in 1895 became a fellow of Strasbourg. In 1898, he resigned his fellowship[2] to enter the service of the Russian government and worked for three years in the central department of the pension fund for the people employed on the state-owned Railways. In 1901 he became an associate professor and in 1920 a professor at the University of Berlin where he continued to work until a few days before his death.

Even during his years as a student in his native city, von Bortkiewicz had become interested in the branches of science of estimation of probability, in statistics, insurance and economics in which he was to become a world-renown scientist. Even before he reached the age of twenty he had gone so far in his studies of the estimation of mortality that he could, in a letter of July 10th 1888 to the prominent statistician Knapp, suggest a reform of the methods used in these estimations[3]. The main lines in his suggestions won the approval of the master to such an extent that he asked von Bortkiewicz to tell him who he was and how it had happened that he had gone in for such rare and out of the way studies. Knapp finishes his first letter to von Bortkiewicz by saying:

It would please me still more if I should have an opportunity some time of making your personal acquaintance.

When Knapp later had the pleasure of having von Bortkiewicz come to study under him at Strasbourg he was delighted by the thoroughness of his pupil and the great acumen which characterized his work.

When von Bortkiewicz went to Strasbourg in May 1891, Knapp was the principal of the university. His duties as principal prevented further scientific teaching so that a special vacation course beginning in September was agreed on. During six weeks, three or four hours a day, often on the blackboard in the teachers' training school[4], Knapp demonstrated the results of his mathematical-statistical investigations and found himself richly rewarded by the expert participation of his pupil in this extraordinary undertaking. In 1894 Knapp wrote to von Bortkiewicz:

Should I ever receive an inquiry about your skill, I shall express my great delight.

In the previous year Knapp had written that he rejoiced at having found such an excellent continuer of the labours which had been such a burden to him before. In 1893 he wrote[5]:

Only think that with the exception of Lexis and me there are no higher statisticians and neither are there prospects of any.

At that time, von Bortkiewicz had already, in the preceding year, been made a doctor by Lexis. The closeness of the relations between the two had as a result that when there was a question of calling Lexis to the University of Berlin at the beginning of the new century, the

latter who did not wish to go to the imperial capital himself was able to propose the appointment of von Bortkiewicz in his stead[6].

[2] When von Bortkiewicz became professor in Berlin in 1901 he was a world-renown celebrity in other branches than statistical science. Eleven years earlier, i. e., at the age of 21, he had published in Western Europe his first article on the theory of economics (1890/2) which immediately brought him to the notice of the world and soon placed him in the centre of the work of theoretical economics.

There he remained until his death. He has taken up his own critical standpoint with reference to all the determining points in the economic theory and has helped considerably in the work of research. For decades he has stood in the van in the attempts to adopt strictly scientific rules in the treatment of the most difficult problems of the economic theory. His many works on economics have not yet received by any means the attention to which they are entitled. In any attempt to develop the science of economics von Bortkiewicz's works will be of very considerable importance. One of the foremost of von Bortkiewicz's economic works is (1921/86).

Wherever in the world statistical science exists, the work of von Bortkiewicz influenced and still greatly influences the development of the science. This work has been and is of great importance to all the Nordic countries and especially to Sweden. Since there was hardly any science of statistics in the modern sense in existence at the beginning of the century in the country of Wargentin[7], it was von Bortkiewicz who placed that country in contact with Lexis' work and his own. Some of the very best work ever done by von Bortkiewicz in the science of statistics on homogeneity and stability in statistics (1918/68) was first put forth in a lecture in 1917 before the Swedish Association of Actuaries.

He has also spoken before the associations of actuaries at Copenhagen and at Oslo and has also given a number of lectures at the universities there. He had also been in touch with many of those in the Nordic countries who were engaged in his own branches of science. Among the letters left behind him at his death there are letters from Frisch, Guldberg, Meidell, Steffensen, Westergaard, K. and S. Wicksell.

The most important are from Walras and Tschuprow. It was by examination of the former's *Elements* (1890) that the then 21 years old author (1890/2) first attracted the attention of the world in such a manner that those who were really interested in the progress of the science never afterwards ceased to follow the writings and other activities of von Bortkiewicz. This examination was followed by many years' correspondence between the examiner and the author – certainly one of the most important which has ever occurred in economics.

Among von Bortkiewicz's contemporaries in the science of statistics who were nearer his own age the most important is the Russian A. A. Tschuprow in connection both to the science itself and to personal intercourse. Tschuprow, who was six years younger, had first learned from von Bortkiewicz and soon became his equal and occupied with him the foremost positions in the work of theoretic statistics. Between the two there has been a correspondence which is of hitherto unsurpassed importance in the history of statistics [Bortkevich & Chuprov (2005)].

[3] The first published works of von Bortkiewicz deal specially with the question of mortality and length of life which are of fundamental importance in the insurance world. The work (1893/6) which first won for the author a widely-known name dealt with the latter subject. During the whole of his scientific activity he had returned repeatedly to questions of great importance in insurance work and illustrated them in such a manner that his contributions must be regarded among the most valuable assets of the science of insurance. This is equally true of his many works with special reference to insurance and of his more general works on the estimation of probability, on statistics and economics.

He has thus been particularly active during the whole of his career as teacher and author with a view to better the education in statistics and mathematics of those who were destined to take part in the practical work of insurance. In fact, one of the first series of lectures he gave at the University of Berlin dealt with the mathematical and statistical bases of life insurance together with practical exercises connected therewith, on which he placed great value. These lectures have later been repeated several times after thorough revision.

But however useful and indispensable mathematics can be in insurance work, the mathematical formulae alone are still not sufficient for one who is employed in practical insurance or annuity work[8]. Empirical bases are necessary to arrive at a numerical application of the expressions obtained from the algebraic calculation. This is the reason why statistics now holds the first place in education in the science of insurance. The history of insurance is rich in cases in which even prominent mathematicians have stood helpless in face of quite simple insurance problems because they lacked a sufficient knowledge of statistics.

As early as the beginning of the present century, von Bortkiewicz pointed out that the training of actuaries, even in England where it was considered to have advanced furthest at that time, had been exclusively adapted to the conditions ruling in the private insurance companies. But the actuary is usually not at all conscious of the relativity of his science in this respect and is therefore inclined to reject on principle the methods of calculation which are used in the

compulsory insurance arranged by the State. There, the same premises do not exist and certain departures from the practice of the private companies appear to be possible and in many cases necessary.

It may be said that without a sufficient statistical and insurance-mathematical foundation all investigations of the problems of insurance equity are built on air. It is easy to prove that certain abortive or inefficient juridical constructions as well as certain untenable demands which have been made on insurance by political economists have been due to a lack of understanding the principles of life insurance. Thus, the lawyer must, like the insurance economist, be acquainted with at least the main points in the mathematics of insurance so that he does not need to stand in front of the insurance technician as before a priest of a strange religion.

It is no longer permissible either in private or social insurance that there should be such a division of labour that the mathematical side is separated from the rest and handed over to the insurance mathematician while the lawyer and the economist take charge of the rest. In this way, there is great danger that the mathematics of insurance is not made right use of, or, even with best intentions it is misused[9] so as to defeat the most excellent intentions of the lawmakers. One falls in (?) with the judgement of the mathematician when he declares that this or that is *not permissible from the technical insurance point of view.*

[4] It is the aim of the science of statistics to achieve the greatest possible accuracy in the observations and conclusions obtained from statistics. For this purpose, the auxiliary science mathematics is necessary. Knapp writes:

I see with pleasure how you succeed in formulating mathematical facts. It is one of your natural gifts of which you make the most praiseworthy use.

As early as 1897 he praises his pupil's economy with formulae, something which is usually not arrived at until a mature age when one does not place so much value on a lot of formulae. These words came from a man who with Lexis was best acquainted with the erudition of von Bortkiewicz in mathematics and its application in statistics. This did not prevent Georg von Mayr from opposing von Bortkiewicz and refusing an article which had been offered to his periodical. The following extract from von Bortkiewicz's reply may be quoted[10]:

The only thing against which I wish to protest is that I have been represented as a mathematician with no understanding for <u>the State science of statistics</u>. My very education shows how unjustified is this description. […] *In addition, I am appointed by the Government as associate professor of statistics at the largest university in Germany: the description of my subject is <u>statistics and similar spheres of political science</u>. Since none of my colleagues in Berlin except myself*

has been appointed to instruct in statistics, I may surely regard myself as professor of statistics at the University of Berlin. It would be a bad case if, as such, I did not know what statistics were. […]

When you compare my formulae to instruments of torture you use the expression <u>torture of the brain</u>. It seems to me that this simile is more obscure than similes usually are, since anyone who, owing to his lack of mathematical education, does not understand the formulae cannot suffer since he will simply not concern himself with them. For a mathematician, please believe me, these formulae are much too easily understood to cause any trouble at all.

Still I would not feel the rejection of my article (I have been working as an author since 1889 and this is the first time anything of the sort has happened to me) as a personal degradation if your periodical did not sometimes accept papers of a mathematical character. But that is just what happens. I mention specially Scukarev's article in volume 9, which is by the way in the highest degree incorrect and worthless from a scientific point of view[11], and Gumbel's articles in volumes 8 and 9 and his latest not yet published article (I have the printer's proof). Far be it to place Dr Gumbel, though he is somewhat unstable and still very immature, even as a statistical mathematician, on the same level as Scukarev.

It is however a characteristic common to both of them that they assume in the reader knowledge of higher mathematics, especially infinitesimal calculus. They are thus guilty of exactly the same as has caused the rejection of my article. (In my article [1915/61] I have managed without applying the infinitesimal calculus.)

I cannot therefore fail to find, even though it does not appear from your letter, that the rejection of my paper is a grave insult to me from the editor. The editor may rest assured that I shall never again trouble him with anything from my pen. At the same time I regard my connection with the German Statistical Society whose organ is your journal as severed. Today my resignation[12] is sent to […]

[5] It must surely be considered as an invariable principle in academic tuition that the only teacher as such who is capable of achieving real success is the one who does not confine himself as a go-between between the science and its students but presents himself to his listeners as a co-creator of the science. In other words, he does not merely market scientific treasures but creates them himself.

His colleagues had always the impression that every sentence he uttered was the result of independent study and had been weighed and tested a hundred times. And it was the same in the training school[13].

Von Bortkiewicz has used these words about his teacher, Lexis, and they are equally true about himself. Especially in the practice in the training school, the importance of whose part in a scientific university education is constantly increasing, there is a living memory of the

great scientist and the stern man who sharply exposed the too often very considerable faults in the lectures held [by others?].

But strict criticism based on the deepest knowledge of the subject, especially when it is also constructive, is an inevitable condition for scientific progress. Von Bortkiewicz criticized in plain words what had been done but he had the splendid characteristic of being able to show how it could have been done better. It was not then the strict professor at his desk who appeared but the fine man who stepped forward with kind forbearance to guide the halting steps of the disciple.

The severity with which von Bortkiewicz treated his pupils was mild in comparison with the utmost strictness which he exercised towards himself. Few scientists have demanded more of their own work than did von Bortkiewicz. A working day of thirteen or fourteen hours was quite common for him. Right up to the end when disease had gripped him so hard, his work continued with constantly increasing regrets that he had effected so little, though his writings would fill about twenty volumes of the same size as a bound volume of this journal.

Von Bortkiewicz has said about his teacher, Knapp, that he had always anxiously avoided publishing work that was only half finished. This is also true about von Bortkiewicz. It also explains the form in which he liked to appear. It is the best for scientific and all other literary production, viz., for an article in a periodical to confine it to one main subject. This is almost impossible when the book form is chosen. Even for a prominent scientist it is hardly possible to deal with all the parts of a book with the same degree of exactness and care.

In addition, the compilation and publication of a large book take such a long time that parts of it are bound to be a bit out of date when they appear. This is not necessarily so for an article in a periodical. A master can quickly complete such an article and then adapt it by means of later articles as may be requisite. The series of articles by von Bortkiewicz (1923 – 1924/96) are, like many others from his hand, masterpieces of expression also from the point of view of actuality.

On an occasion Lexis, himself a master of sharpness, said to me[14]: *He is so sharp*. Many of his victims have complained about von Bortkiewicz's sharpness, and not always expressed in a suitable scientific form. He knew his world and especially those who were attempting to work the same fields of science as he. He knew that many of them lacked not only great mental gifts, but, which was worse, the proper disposition of the mind. Without it, a good deal can really be done in the world of formulae but not much that will bear the press of time in the science of statistics which he, like his teacher

Lexis, regarded as the foundation of a future empirical scheme of social ethics.

Indeed, Lexis had no complaints to make about the sharpness of his pupil. In 1913, when the remark was made, he was aware that acerbity was necessary in his own country and added that it was unfortunately only too true that it was likely to remain necessary for some considerable time,

For many of the so-called statisticians and other social experts have not sufficient decency either from a scientific or from other points of view.

[6] Von Bortkiewicz appeared for the first time on the scientific platform of Western Europe and the whole world in a dispute (1890/2) regarding the principal work of Walras (1883). He continued to fight on that platform almost to his last breath. Many and shrewd were the blows exchanged. Many were the fights from which he emerged as the victor in the eyes of the outer world but with internal wounds which never completely healed owing to the fact that the struggles often showed the meanness of the world for whose scientific progress, also from the ethical point of view, he had been one of the greatest champions of his time.

For this reason he often took the many attacks on his works too personally, and this was especially true of the attacks by Pearson and those who followed him concerning the law of small numbers[15]. Von Bortkiewicz's opinion regarding Pearson as a statistician did not differ from that of the greatest English statisticians now living or from that of Denmark's great scientist Wilhelm Johanssen[16].

Of Pearson as an anthropologist von Bortkiewicz (1922/125) has spoken in such a way that Bohlmann, with whom he was in intimate scientific and personal contact for more than 30 years, could, after receiving this expression of opinion, write to its author:

I have not seen the book [1922]*, but must ask myself after having seen your article, whether Pearson can still be taken seriously by his own countrymen*.

The last months of his life were rendered more painful physically and embittered spiritually by the attacks made by Gini and others on his great treatise (1930/104) and the manner in which they were dealt with by the International Statistical Institute. The Italian attacks accused him of plagiarism.

Exactly 50 years had passed before the controversy regarding Lexis and Dormoy was brought to its scientific conclusion by an article of von Bortkiewicz himself (1930/105) in this journal. So this journal which would hardly have come into existence without him and the connections obtained through him with the true science of statistics has therefore considered it a duty to reprint in this volume [pp. 27 – 70] the documents published in connection with the congress of the

Institute in Madrid together with the correspondence left behind by von Bortkiewicz at his death and to try to keep track of any further developments which may occur in this matter. In Madrid it was said that further documents might be published later in the transactions of the Institute.

The following documents give witness with regard to the truth of the accusations of plagiarism in a manner regarding which we may reserve our judgement until the further documents have appeared as promised. It is of importance for a coming and much needed history of statistics to have this controversy settled as soon as possible. A future writer of the history of statistics will find so much to occupy him in so many other claims to precedence from authors on statistical subjects that the sooner the controversy now in progress is finished, the better, especially since the war cries raised have not been those of eagles but rather of vultures and have become coarser and coarser the longer the struggle has continued.

As late as June von Bortkiewicz had not quite given up the hope of being able to go to Madrid in September to deliver the final engagement there and then resign from the Institute. He made clear his attitude to the accusations in a letter as early as March 24th to Cantelli, the general secretary of the Institute of Italian Actuaries […][17].

[7] The champion of light is no longer in the land of the living. To posterity his image will always appear as that of manliness and humanity, of the brave warrior and of the man who was always in his inmost mind conscious of his own faults. At the death of the great Tschuprow von Bortkiewicz wrote to the former's relations[18]:

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

These words may now be used by those who had the privilege of fighting together with von Bortkiewicz for the development and progress of the human race.

[The author appended a bibliography (*catalogue*) of the works of Bortkiewicz and continued.]

[8] The foregoing catalogue […] is based on the copies of writings which were found in his library at his death. It comprises the copies of his works found there in the form of books, contributions to cyclopaedias and articles published in periodicals. Although he was most careful in looking after his very valuable library, it is probable that at his death copies of some of the works which ought to be included in the abovementioned categories were missing. This is

particularly the case with articles in Slavic publications. Among other works which are not given in the above list may be noted a memorial written last year at the instance of the Polish Government covering 44 pages of print about life insurance of mortgagers. In such cases the insurance serves to pay the whole or part of the debt of a mortgager and is called total or partial respectively. In his memorial, von Bortkiewicz only deals in full detail with the former. Towards the end, however, he deals to some extent with the latter.

A very important portion of his printed writings is composed of the hundreds of his criticisms and reviews of published works. They are also excellent products of a scientist who had a great capacity for separating the essential from the non-essential, for giving its due to the valuable and subjecting the rest to a criticism which demonstrated the character of the work[19]. His reviews are therefore not merely of momentary value. They throw light not only on their author but also, before all else, on the period in which they appeared and the writer to whom they refer. As long as science exists they will be indispensable documents in the study of the struggle for truly scientific methods in the social sciences, and especially in statistics and political economy.

His reviews some of which cover only a few lines while others cover more than a page[20] include some of the finest work ever done by von Bortkiewicz. First and foremost we may note the previously mentioned review of Léon Walras (1890) […]. His great literary article of 25 pages (1904/29), to mention just one more, belongs to the literature which a statistician both of today and of the future must know.

His last great work (1932/109) dealt with the measurement of the fluctuations in the buying power of gold. It was carried out at the request of the Gold Delegation [of the League of Nations?] at Geneva. The chairman of the delegation has given it very high praise and described it by such expressions as *this most valuable memorandum for the Gold Delegation* or *this very excellent and scholarly study*. Von Bortkiewicz wrote it in German and it will also be published in that language here (this periodical, vol. 4, pp. 1ff.).

When before long, hopefully, the collected works of von Bortkiewicz are published, an important place in them will be occupied by his many articles written as results of his attendance at the meetings of congresses, associations and other societies. Wherever he appeared, he was right in the centre of the discussions in which he was often the principal contributor.

He rigorously defended his position and his opponents were regularly defeated. These discussions were usually concerned with fundamental questions regarding the branches of the science in which he was principally interested.

Among the writings must be included his many letters. As mentioned above, the most important of them are those to Tschuprow and Walras. The letters to these great men of science as well as those to other prominent scientists in the Old and New World often contain statements in a mathematical form about fundamental scientific questions and their publication can only be to the advantage of science.

By no means the least valuable of the works of von Bortkiewicz are his lectures. Their manuscripts on statistics, social politics, economics and technical insurance are preserved in perfect order in cardboard boxes of which no less than 25 deal with statistical subjects; nine boxes contain the manuscripts of lectures on the general theory of statistics; one, on the legal documents concerning statistics; four, on population statistics and studies of population; seven, on population policy and population statistics; and finally there are three manuscripts of statistical and one of social statistical exercises.

When von Bortkiewicz gave a series of lectures for the first time, he usually had a well worked out manuscript which was often practically ready for printing. When the same subjects were dealt with in later lectures, the manuscripts were subjected to a more or less thorough revision which was often so considerable that the new lectures had not much more in common with the original ones than the name. This is especially the case with lectures on subjects concerned with population policy and economics. A collection of the works of von Bortkiewicz will draw much of value from the manuscripts of his lectures.

Notes

1. Andersson had thus tacitly agreed with Freudenberg who had made an absurd statement unworthy of Bortkiewicz. That Lexis was the founder of modern statistics was apparently Freudenberg's invention as well. He apparently was Bortkiewicz' pupil (possibly the only one) since in 1932 – 1933 he published three papers on population statistics, but anyway he was barely known. He (1931) published an obituary of Bortkiewicz which I refuse to translate.

2. Actually, see Bortkiewicz' letter No. 27, 1897, to Chuprov (Bortkevich & Chuprov 2005), he kept back his work in Russia from Strasbourg University: privat-docents were forbidden to pluralize.

3. Andersson repeatedly refers to Bortkiewicz' letters. They, and the manuscript texts of his lectures are kept (perhaps not completely) in the Uppsala (Sweden) University Library. It was Guido Rauscher (Vienna) who discovered the whereabouts of the Bortkiewicz' archive.

4. Andersson several times mentions that school. Nothing apparently is known about it.

5. I (2011, Note 7.5, p. 165) quoted this letter in somewhat more detail.

6. See [xiv, end of § 2]. Voigt (1994, p. 337) added: Althoff had wished that Bortkiewicz will discuss scientific problems of Russian state life. It was also resolved and apparently implemented that he will read lectures on the economic relations of Russia and conduct classes in connection with the Russian seminar.

7. See Nordenmark (1929).

8. An annuity is an insurance as well.

9. An obvious mistake.

10. This periodical which Andersson had not named was the *Allgemeines statistisches Archiv*. Instead, he mentioned some mysterious statistical archives which I omitted in my translation. The editors of its volume 9 (see below) were Mayr and F. Zahn.

Bortkiewicz wrote his letter in 1915 or 1916. In a letter of 1898 to his father Chuprov (Sheynin 2011, p. 26) wrote:

For me, Georg Mayr in Strasbourg is as unbearable as was Adolph Wagner in Berlin.

And Bortkiewicz, in letter No. 109 of 1898 wrote to Chuprov (Bortkevich & Chuprov 2005) that Mayr had declared that statistics did not need mathematical formulas.

11. I am not surprised. In 1928 Slutsky politely wrote Shchukarev that his paper of the same year was mistaken: the Maxwell distribution cannot be proved independently from stochastic considerations. See Sheynin (1999, p. 134).

12. I have acted somewhat similar to Bortkiewicz. In 2005, two authors have published a paper about De Morgan in the *Journal of the Royal Statistical Society* obviously without reading De Morgan's important paper. As an honourable member of the Society, I wrote a letter for its journal refuting their statements. For a few months the Society kept silent, then, answering my request, it sent me a delaying letter. The President did not answer my complaints and my letter was finally rejected without due justification. I resigned from that *antistatistical* society which is afraid of criticism.

13. The first lines of § 4 and the quotation are borrowed from (1915/62, p. 119).

14. The *Nordisk Statistisk Tidskrift* existed from 1922, and the English *Nordic Statistical Journal*, from 1929. Andersson was editor of both, but the conversation with Lexis had occurred in 1913.

15. Lucy Whittaker (1914) should be named. Even the title of her paper denies Bortkiewicz' discovery.

16. Johannsen (1929) maintained that statisticians including Pearson had not yet understood the essence of the evolution theory. The main word here, *yet*, is my own.

Concerning a few lines below, I add that Pearson's writings are indeed open to criticism, but great scientists (Bernstein) have left most positive references to him as well, see Sheynin (2011). I can name only one great English statistician of that time, Fisher, who had been essentially (but not always justly) opposed to Pearson.

17. See [xiii, Note 2]. I need not translate the text of Andersson anymore.

18. Bortkiewicz had certainly written this letter (apparently kept in Uppsala) in Russian, so who translated it?

19. Quite a few authors *reasonably* noted the opposite: Bortkiewicz had not separated essential and non-essential.

20. *More than a page* is certainly too weak. For that matter, some of the valuable books of Bortkiewicz are known to have been compiled out of never published reviews.

XIX

Wilhelm Winkler

Ladislaus v. Bortkiewicz As a Statistician

Schmollers Jahrbuch f. Gesetzgebung, Verwaltung und Volkswirtschaft

On 15 July of this year L. v. Bortkiewicz had completed his life full of work. His studies were mostly devoted to the successful solution of problems in the theory of statistics[1]. This journal ought to offer a review of his statistical results.

Lexis and his students have mostly busied themselves with two groups of statistical problems, those of the *formal* (statistical) theory of population and of the application of the calculus of probability to statistical phenomena. It is therefore understandable that L. v. Bortkiewicz who was a student of Lexis, had followed his teacher also in respect of the selection of the areas, to which he devoted the work of his life. He remained true to these areas when much later he additionally mastered other territories.

L. v. Bortkiewicz' first statistical study belonged to the formal theory of population. He (1893/6) entered German science after publishing a Russian paper (1890/3) [and (1891/4)]. Adjoining the Knapp theory of same in its analytical form of the occurring totalities, v. Bortkiewicz thoroughly and critically examined the methodical means of measuring mortality and above all the number of died and the probabilities of death, the mean lifespan and the mortality tables which serve for its calculation.

In the second part of that contribution he studied the relations between the mean lifespan, the mean age at death, and numbers of died and born. Some of the increased populations emigrated (abwandelte) and he analytically described the influence of such changes on the mean lifespan and the numbers which he compared with each other. He thus arrived at conclusions important for theoretical and practical statistics. Until then they had been barely taken into consideration.

v. Bortkiewicz repeatedly returned to the formal theory of population in his thorough works, in (1894 – 1896/8) and more specifically partly in its second, but mostly third part; in (1903/27; 1911/57), as also in his papers (1892/5; 1894/9; 1911a).

Among many conclusions made I especially stress that v. Bortkiewicz clearly discerned that the measurement of mortality by the age structure of the population alone was mistaken and argued in favour of the application of mortality tables as being the measure of mortality[2].

The second main area of the work of v. Bortkievich was, as mentioned above, the applicability of the probability theory to social phenomena. He entered this area by (1894 – 1896/8). In its first part he arrived at a remarkable proof that the main essence of a probability (its composition from unequal components) does not generally influence its variance. This means that heterogeneity in the

composition of the mass (Masse) does not exclude the validity of the law of large numbers. A decrease of the variance as compared with *normal* only occurs when the components belong to certain subpopulations (mean probability composed of constant components) as opposed to the former *mean probability in its authentic sense*.

In the second part of that contribution he establishes no less remarkable facts on the strength of some considerations about the independence of the separate cases: not only the probabilities but the mean values as well may be treated by the pattern of probabilities.

Still more important than these conclusions about separate problems seems to be that not the principle of causality but of probability explains mass phenomena in human societies (see end of second part). This statement certainly is only a continuation of Lexis' basic idea which he formulated in his struggle against Quetelet (1869)[3].

However, for Lexis the application of the forms of probability-theoretic thought still was rather a means for that struggle to refute the supposed natural regularity of social events. Therefore, the appearance of such thoughts in the Bortkiewicz' formulation was in a positive sense a basic principle of statistics situated in the centre of statistical theory[4]. The definitely correct conclusion, also stated there, was that the theory of probability was benefiting statistics more theoretically than practically[5].

The second important work in that area is his booklet (1898/14), which perhaps [not perhaps but certainly] mostly made him known at home and abroad. There also v. Bortkiewicz elaborated Lexis: he opened up new areas of application for the Lexian measurement of dispersion by investigating the main problem in the study of dispersion and applying it not to the relative numbers but to materials of a special kind, to small numbers of events. He questioned how far social events can be brought under the strict pattern of games of chance and how to explain the deviations from that pattern.

In other words, to what extent is the theory of probability suitable for explaining statistical numbers. It was also obviously most important to establish the negative demarcation, i. e., to indicate the extent of the agreement between the behaviour of social phenomena and the regulations of games of chance, which formally arrive here from beyond. These games are therefore insignificant for [explaining] the inner realization (innere Zusammenkommen) of statistical numbers.

v. Bortkiewicz made such a negative statement in his booklet (1898/14): the absolute numbers of the arrival of events given a very large number of observations and therefore a very low probability of successes obey the pattern of the games of chance even if the stability of the main probability does not take place.

v. Bortkiewicz thought that the Lexian results explain this fact: for a small number of observations the change of the basic probability will be covered by random fluctuations and give the impression of a normal dispersion. His booklet was strongly criticized, see my book (1931). Critics especially required a change of the proud title, law of small numbers[4].

Indeed, it was impulsive and it seemed that v. Bortkiewicz had along with the generally known and fundamental law of large numbers discovered an equally matched law of small numbers. However, here we have not a new discovery; v. Bortkiewicz only continued in more detail with formulas, examples and tables, what Poisson whom he naturally referred to[6], had previously found. And the term *small numbers* concerned the number of the occurrences [of the studied event], whereas for the law of large numbers the numbers were those of observations. At the same time this shows that v. Bortkiewicz wrongly interpreted his law: it has nothing in common with the mentioned Lexian ascertainment since the phenomenon treated by him occurred exactly when a very large number of observations were made. Critics did not fail to stress these shortcomings in v. Bortkiewicz' concept. He himself had later (1918/68) found out that according to Lexis the explained coverage of the essential differences in the basic probability becomes relatively more readily seen owing to the random fluctuations when the variances of the whole and of its parts are considered at the same time. v. Bortkiewicz suspected that a certain process of compensation begins to act since being caused by the governing heterogeneity of the structure.

These statements possibly bring us nearer to the explanation of his law of small numbers than those which he himself had offered. And if we disregard the path from the preceding to the achieved, as it is always done, there still will be left a considerable remainder: v. Bortkiewicz' merit of extensively and intensively moving ahead the Lexian dispersion theory, of making an intermediate step for further studies in this area.

In a large number of less extensive papers v. Bortkiewicz contributed to the theories of probability and dispersion. And, over and above that, he published two works (1917/66; 1915/64) in which he thoroughly studied the application of the theory of probability to statistics.

The former work was an occasional contribution prompted by Marbes (1916 – 1919) and initially planned as a review. It grew however and became a book more than two hundred pages long. Its main point is the probability-theoretic justification of the doctrine of iterations, of sequences of the same events (for example, of newborn boys) among a population (of births in general). For the statistical theory there was no rich insight as compared with the arsenal of the

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applied formulas since we have here a remote special case of statistical considerations.

Nevertheless, v. Bortkiewicz seized the opportunity to take a stand about the required main notions such as statistical mass and group, the law of large numbers, the theory of sequences, etc. He thus essentially contributed to the theory of the appropriate area. The expressions *sylleptic* and *stochastic* which he borrowed from previous authors (stochastic, from Jakob Bernoulli) and his own *syntagmatic* had found some application in the new statistics[7].

In the latter contribution v. Bortkiewicz studied whether in 1890 – 1902 the intervals between the deaths of the then deceased members of the International Statistical Institute obeyed some probability-theoretic criterion. Or, otherwise, can we consider that the distribution of the deaths was random? He had developed here a multitude of formulas which can give us an impression of his proceeding rather because of being happy with mathematical derivations than out of the importance of the problem.

In later years, apparently because of entering further into problems of economics, we see that v. Bortkiewicz (1923 – 1924/96) captured new ground for theoretical statistics. Being stimulated by the appearance of Irving Fisher's book on the index of prices, he published a series of critical papers about the theory of such indexes and took the occasion to treat all the main pertinent problems. He rejected Fisher's *ideal formula* and preferred the equally matched formulas of indexes of Laspeyres and Paasche [see Kendall (1919)].

In one of his last statistical works v. Bortkiewicz (1930/104) critically examined the then new measure of the differences in the distribution of incomes and the connections existing between them. His results should be important for the theory and practice of the statistics of income taxes.

But dismal to the same extent were the mighty attacks of some Italian statisticians on him. They reproached v. Bortkiewicz with inadequate citation of previous contributions and more or less obviously with plagiarism. Indeed, some of the results among many others derived by him had been discovered earlier by other authors.

However, those who are accustomed with the thoroughness of v. Bortkiewicz' thinking out the problems under his discussion, will be satisfied by his explanation (1931a): he had not seen Gini [see my Bibliography].

Each German researcher who knows the neediness of German libraries which existed after WWI will completely understand this[8]. There remain priority conflicts about not all too important mathematical results which were decided by later explanations made by v. Bortkiewicz.

v. Bortkiewicz' repeated involvement in similarly sharp polemics could have apparently been occasioned by his brusque way of expressing judgement about others. It was also seemingly connected with the method of representing his scientific works in general: he made no license for the understanding of the readers but relentlessly urged to the target which the readers did not see and towards which he willingly moved through a jumble of mathematical formulas.

This hardly pedagogical way of presenting his works was the reason why only an extremely small circle of people read his statistical contributions. v. Bortkiewicz was aware of this fact. After I had sent him some comments on his work (1923 – 1924/96) he wrote me:

I am glad to have found in you one of my five expected readers.

I cannot say whether both these features of his work, the brusqueness of his critic and lack of guidelines, revealed themselves in his contacts with colleagues and students. I did not regrettably manage to become personally acquainted with the deceased. In his later years he had not anymore taken part in German or international statistical conferences. However, after I examined his trustworthy description published in a professional journal on the occasion of his 60th birthday, it seems likely to me that in his personal intercourse v. Bortkiewicz attempted to be friendly and benevolent.

When surveying as a whole his statistical effectiveness we ought to defend him first of all from G. Blaschke who [when? where?] accused him of dealing mostly with uniqueness. If at all true this statement was only directed against two contributions (1917/66) and (1915/64) and meant that v. Bortkiewicz had been tempted to exert efforts as though, considered objectively, the special objects there treated could have deserved it[9].

In no case this accusation can be extended to his contributions on statistics of population or earlier probability-theoretic works. Even if his obvious disinclination or lack of energy for dealing with larger theoretical areas had been expressed there, the results obtained in the territories in which he worked had a general significance and were important for statistics as a whole.

The activity of v. Bortkiewicz in theoretical statistics did not evade some tragedy. He only compiled his main works there in the preceding [the nineteenth] century. They concerned areas which then, owing to his teacher Lexis, were in the centre of interest in theoretical statistics and attached prestige and glory to his [whose exactly?] name in international statistics. Then, however, a great extension of statistics into the theory of sequences[10] had begun abroad. v. Bortkiewicz however continued to deal with the former areas from which international interests had essentially moved away and for a long time he had been unable to participate in that extension.

He stepped ever further in the background, politely respected abroad but not understood and ineffective at home. Only in his last years, on the threshold of extreme old age, v. Bortkiewicz once more achieved a connection with the statistical theory of the world. He began once more to participate in pronouncing important statements but death checked his attempt to discover new facts.

We ought to mourn his too early death. The belated second blooming of his researches in theoretical statistics had recently seen rich fruit and his previous works and especially those belonging to population statistics contain many still unearthed treasures. The seeds which were sowed there will sprout only later when the common (angemessen) statistical language, the language of mathematics, will be also more generally understood in the German lands.

German statistics has every ground to maintain an honourable memory of the creator and disseminator of an essential amount of [the science of] statistics.

Notes

1. Economics should have been mentioned. The *new territories* (see below) was too indefinite.

2. More detailed and partly critical is my discussion (1925, pp. 115ff). W. W.

3. Quetelet actually illustrated the dialectic connection of randomness and necessity. Regrettably, he all but forgot Poisson.

4. This is difficult to understand.

5. Note 3 shows the opposite.

6. Bortkiewicz referred to Poisson on the very first page of his booklet, but without explanation. In Letter 106 of 1911 to Chuprov (Bortkevich & Chuprov 2005) he stated that Poisson *cannot at all be considered the own father of the law of small numbers*. Rarity, as he also maintained, can be understood as a small number of the occurrences of the studied event when the number of observations is also small but he thus undermined his law.

Concerning the term *law of small numbers* (see a bit below), Lexis and Markov, among others, recommended to change it, see Bortkiewicz' Letters 3 and 27 of 1896 to Chuprov (Bortkevich & Chuprov 2005). Chuprov (1909/1959, pp. 284 – 285) had discussed several possible meanings of that term but Bortkiewicz did not comment. Moreover, in Letter 69a of 1915 or 1916 to Markov Chuprov (Sheynin 2011, pp. 91 – 92) noted that Bortkiewicz *regards criticisms* of his law *very painfully*.

See also Note 6.

7. *Stochastic* certainly became indispensable, but the other terms are forgotten.

8. Bortkiewicz was duly punished for his arrogance [xviii, Note 17].

9. Quite a few authors whom I have translated here had expressed a reasonable opposite opinion.

10. Winkler mentioned *Reihen* (series) but he hardly thought about Gram or Charlier since Bortkiewicz had not *participated* in dealing with their series although he (1922/127) published a review of a book of Charlier. My translation, *sequences*, is not better, so I do not know what Winkler had thought about.

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L. von Bortkiewicz

Abbreviation

AGSA = *Arch. f. Geschichte d. Sozialismus u. der Arbeiterbewegung*

ASWSP = *Arch. f. Sozialwiss. u. Sozialpolitik*

Bull. ISI = *Bull. Intern. Stat. Inst.*

Hdwb = *Handwörterbuch*

JGVV = *Jahrbuch f. Gesetzgebung, Verwaltung u. Volkswirtschaft im Deutschen Reich*

JNÖS = *Jahrbücher f. Nationalökonomie u. Statistik*

NST = *Nordisk Statistisk Tidskrift*

SAT = *Skandinavisk Aktuarietidskrift*

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