LADISLAUS VON BORTKIEWICZ. THE THEORY OF POPULATION AND MORAL STATISTICS ACCORDING TO W. LEXIS

ŚLĄSKI
PRZEGLĄD
STATYSTYCZNY
Nr 17(23)

Oscar Sheynin
International Statistical Institute
e-mail: oscar.sheynin@googlemail.com
ORCID: 0000-0002-5039-4314

ISSN 1644-6739
e-ISSN 2449-9765

DOI: 10.15611/sps.2019.17.05

Die Theorie der Bevölkerungs- und Moralstatistik nach Lexis. Jahrbücher f. Nationalökonomie u. Statistik, Bd. 27/82, 1904, pp. 230-254.

Summary: W. Lexis founded the continental direction of statistics which the Biometric school largely ignored. Bortkiewicz described his work but mostly without providing exact references. He criticized Lexis for paying too much attention to philosophical problems but mentioned Lexis' merits: a test for the stability of statistical series (only much later rejected by Chuprov), the study of mortality and sex ratio at birth; the application of the law of large numbers. Lexis, as Bortkiewicz concluded, essentially contributed to the general theory of statistics.

Keywords: law of large numbers, mortality, stability of statistical series.

This paper consists of two contributions by Bortkiewicz in which, in Part 1, he studies the work of Lexis. 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's critical review of Pareto [1898/15] was badly arranged. Chuprov [Bortkevich, Chuprov 2005, Letter 35 of 1898] indicated this circumstance, but Bortkiewicz's 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 [1931] 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!

ŚLĄSKI PRZEGLĄD STATYSTYCZNY
Nr 17(23)

Bortkiewicz had no mathematical education although many non-mathematical authors in Pt. 2 praised him as such, and an accomplished mathematician he certainly became. Keynes (quoted, e.g. by Gumbel [1931, Supplement]) called his *mathematical argument often brilliant*. See however Lorey [1932].

For many decades his law of small numbers [1898/14] 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: the author [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 $E\xi/E\eta$, which he had not noted and wrongly claimed that it equalled $E(\xi/\eta)$. Delicately, 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 then 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 was patently mistaken. The author [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.

This contribution is defective since quotations are provided without any references, some statements are incomprehensible, Lexis did not change in time at all, and (which 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 [1929], should have noticed the forgotten eminent economist Walter Rathenau.

Two more points. First, we now attribute the birth of mathematical statistics 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].

ŚLĄSKI PRZEGLĄD STATYSTYCZNY

Nr 17(23)

The author inserted a bibliography of the works of Bortkiewicz at the very end of this paper. It is almost complete and includes many of his reviews. The author refers 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 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 on my website www.sheynin.de. I am proud to add that Google is diligently copying my website, see Oscar Sheynin, Home.

1. The author omits 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, we could 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]

Therefore, 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

[1] This seems to be a wrong and superfluous restriction.

[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.

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 establish beforehand the exact numerical values near which their separate series will fluctuate; this value is called the 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 fewer the quantitative deviations of those ratios from the main mathematical probability.

---

[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.

[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 with the application of statistical probability instead of theoretical [Sheynin 2017 §§ 3.2.3 and 5.2].

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 underlying them.

Hence 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 his new and independent approach 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. This 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.

---

[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.

[6] A little below Bortkiewicz nevertheless recalls the precise formulation of the LLN. And in general his statement is definitely wrong.

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 commenced 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 wish to picture this phenomenon from the physiological angle, Lexis 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:

---

[7] Bortkiewicz understands this term in a wide sense.

[8] 1063:1000 ≈ 17:16, but where did Bortkiewicz find this ratio?

[9] Lexis introduced this model even before [1876, p. 242; 1877, pp. 73-74] but it is hardly satisfactory: males were completely left out.

Large individual differences between the districts can exist if only the mean ratios *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 around five years of age, there also appears an approximately normal stability. Therefore, 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*'.

The situation is quite different 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]. 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]

Yet, 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.

---

[10] Lexis [1877] provided many pertinent examples but, regrettably, did not repeat them later.

[11] If these deviations are random, his statement is evident.

ŚLĄSKI PRZEGLĄD STATYSTYCZNY

Nr 17(23)

Lexis 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.

---

[12] A few lines above Bortkiewicz remarked that Lexis had introduced variable probabilities. That, however, was due to Poisson.

New paragraph! Lexis derived this formula in a Note on pp. 196-197 of the supplement of 1902. The equation $[\Delta^2] = [\tau^2] + D^2]$ is strict. However, it is based on a small inaccuracy which Lexis had possibly noted and even overrated: he replaced $[\tau^2]$ by $nV(1 - V)/g$. Actually the expectation of $[\tau^2]$ is $V(1 - V)/g$ and if g = Const, $V(1 - V)/g = nV(1 - V)/g - (1/g)[D^2]$. Accordingly, instead of

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

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

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.

It was only indirectly possible to establish that Bortkiewicz discussed Lexis [1879].

ŚLĄSKI PRZEGLĄD STATYSTYCZNY

Nr 17(23)

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 another way, 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 usually 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*.

Now 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 are discussing the elimination of chances, are determined more precisely the more numerous 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 the result in a game of chance occurring with an absolutely unlikely constancy and regularity.

ŚLĄSKI PRZEGLĄD STATYSTYCZNY

Nr 17(23)

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.*

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*.

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

First of all, we should 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. 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 the latter the essence of Man is decisively important and, in moral statistics, his state of mind as well, however not the essence and the state of mind of a certain individual but of people in general, of the *abstract man*, as Lexis expressed it himself.

We certainly ought to keep to the same social notion of mankind rather than to its 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 people 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 their 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. The author considers 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. It is often possible to add new groups for which

[13] This viewpoint is inadmissible if we agree with the understanding of the relation between natural sciences and humanities in just 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.

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

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 one 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 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.

---

[14] Lexis [1877, p. 29] indicated the cause: not all women (to say nothing of men) can give birth. On page 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.]

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

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

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

$$\rho\sqrt{2p(1-p)/\alpha^2 G}\,.$$

If however α is the reciprocal of the mathematical probability of random fluctuations then [Lexis 1877, p. 230]

$$\rho\sqrt{\frac{2(p/\alpha)[1-(p/\alpha)]}{\alpha G}} = \rho\sqrt{(1/\alpha)^2\frac{2p(1-p)}{G} + p^2\frac{(2/\alpha)[1-(1/\alpha)]}{\alpha G}}\,.$$

This can also be derived by the theorem about the probable error of the product of two probabilities. This author believes that that formula most clearly indicates by its structure why the probable deviation is larger in the second case. When considering $e/\alpha G$ without noticing that the denominator includes *some worthless stuff* [Lexis 1877, p. 130] we can only come to the proper measure of stability if $1/\alpha$ is the empirical expression of the mathematical probability. But if α 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.

In those formulas $\rho = 0.477\ldots$ 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 α.

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

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 firstly 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 reality and connections whereas exceptions are only allowed by serious destructive catastrophes.

Therefore, 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 constructions which better represent the stability of statistical ratios in heterogeneous groups are 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 generally 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

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

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. 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: to 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 distant 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 primarily concerned statistics of mortality will be specified.[17] The latter two 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 dead. 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. This ensures greater clarity and indicates which groups of the dead 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 genetic relative number for the statistics of mortality.

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.

[17] The author can mention the following sources: Becker [1874]; Knapp [1868; 1869]; Zeuner [1869].

ŚLĄSKI
PRZEGLĄD
STATYSTYCZNY

Nr 17(23)

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 set out 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, etc. This 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 calculating 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 manifesting with definite probability 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 other *coefficients of change* adjoined to them.

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.

---

[18] This statement seems to be far-fetched.

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 accordance 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. This 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 new-born, 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 anthropometric magnitudes in demography 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. He, just 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 that was stated above, it most important to indicate first of all 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.

---

[19] No explanation provided.

[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.

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 judgement by starting 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 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 the 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 allowed at all for the possibility that the values of mathematical probability for partial groups can differ, whereas Poisson

[21] Jakob Bernoulli, De Moivre and Bayes, are forgotten. Cf. Note 4.

[22] This is wrong [Sheynin 2017, § 8.6].

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

did not take this circumstance into consideration 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, 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 did not avoid 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. One 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. He repeatedly indicated that this constancy hardly justified the expectations excited by Quetelet. He certainly 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]

---

[24] Thus when estimating the population of France, Laplace [1786] had no doubt 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.

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.

[25] No explanation provided.

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

[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.

It would have been absolutely wrong, however, to attribute such statements to the generally assumed anti-Quetelet attitude. By stressing the interest to the change in numbers, Lexis had in mind the 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 for 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 themselves. 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 Man considered as a typical phenomenon, the *average* man according to Quetelet and the *abstract* man as Lexis called him. 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. Thus 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 brief, 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 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

[28] Do such phenomena really exist?

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 about the final aims of statistical studies from Quetelet's statements.

Yet 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 now dominates, 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, one just does 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. The elements of the manifested diversity should be considered here, but the peculiarity in their mutual behaviour.

ŚLĄSKI
PRZEGLĄD
STATYSTYCZNY

Nr 17(23)

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 can only see the 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.

---

[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].

[30] How else can we decide whether a certain conclusion is reliable or not?

[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. At least in words Quetelet admitted the application of probability.

Indeed, to think that it is possible to manage completely without the set of the ideas of probability theory is tantamount to somewhat deceiving oneself. Actually even the fiercest opponent of similarities with games of chance applies ideas which belong to that same area. Indeed, scientifically minded statisticians daily ask themselves 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, they turns to probability theory although non-methodically and therefore in the rough manner of a pure empiricist. Equally mistaken are those who, as the author almost wishes to say, somewhat proudly state that statistics never predict. 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 the 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 von Mayr, von Öttingen and others, 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.

[32] See [Lexis 1879].

It is not accidental that, for example, von Mayr, who rejects probability as the basis of theoretical statistics, is somewhat hostile to that method as well. The author refers 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 the methodological problems of collecting and treating numerical materials, which 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). Someone who believes that these laws are not connected with the subject of statistics at all thinks that it can be decided 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 stirred up by the sensual nature of man, whereas the spiritual and moral factors influence in the opposite direction.

But this hypothesis is not less justified, although not more 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.

Similarly 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. Far 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 (?).

These examples suffice. The author supposes 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 was repeatedly thought. 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.

Bibliography

Becker K., 1874, *Zur Berechnung von Sterbetafeln an die Bevölkerungsstatistik zu stellende Anforderungen*, Berlin.

Chaitin G.J., 1975, *Randomness and mathematical proof*, Scient. American, vol. 232, pp. 47-52.

Czuber E., 1899, *Die Entwicklung der Wahrscheinlichkeitstheorie und ihrer Anwendungen,* Jahresber. Deutsche Mathematiker-Vereinigung, Bd. 7, No. 2.

Czuber E., 1903, *Wahrscheinlichkeitsrechnung und ihre Anwendung auf Fehlerausgleichung, Statistik und Lebensversicherung*, Bde 1 – 2, Leipzig, New York 1968.

Gumbel E.J., 1931, *L. von Bortkiewicz, Deutsches Zentralbl.,* Bd. 8, pp. 231-236.

Kant I., 1781, *Kritik der reinen Vernunft,* Werke, Bd. 3, Berlin 1911.

Knapp G.F., 1868, *Über die Ermittlung der Sterblichkeit aus den Aufzeichnungen der Bevölkerungsstatistik*, Leipzig.

Knapp G.F., 1869, *Die Sterblichkeit in Sachsen nach amtliche Quellen dargestellt,* Tle 1-2. Leipzig.

Knapp G.F., 1871, *Die neueren Ansichten über Moralstatistik.* Jena.

Knapp G.F., 1872, *Quetelet als Theoretiker*, Jahrbücher f. Nationalökonomie u. Statistik, Bd. 18, pp. 89-124.

[33] No explanation provided.

Kries J. von, 1886, *Die Prinzipien der Wahrscheinlichkeitsrechnung*, Freiburg i/B, Tübingen 1927.

Laplace P.-S., 1786, *Sur les naissances, les mariages et les morts etc*., Oeuvr. Compl., t. 11, Paris 1895, pp. 35-46.

Laplace P.-S., 1812, *Théorie analytique des probabilités,* Oeuvr. Compl., t. 7, Paris 1886.

Lexis W., 1859, *De generalibus motus legibus*, Bonn.

Lexis W., 1875, *Einleitung in die Theorie der Bevölkerungsstatistik*, Strassburg.

Lexis W., 1876, *Das Geschlechtsverhältnis der Geborenen und der Wahrscheinlichkeits-rechnung*, Jahrbücher f. Nationalökonomie u. Statistik, Bd. 27, pp. 209-245.

Lexis W., 1877, *Zur Theorie der Massenerscheinungen in der menschlichen Gesellschaft,* Freiburg i/B.

Lexis W., 1903, *Abhandlungen zur Theorie der Bevölkerungs und Moralstatistik*, Jena.

Lorey W., 1932, *Ladislaus von Bortkiewicz*. Versicherungsarchiv, Bd. 3, pp. 199- 206.

Mayr G. von, 1895, *Paper in Handwörterbuch der Staatswissenschaften*, 1. Suppl.-Bd.

Mayr G. von, 1895-1913, *Statistik und Gesellschaftslehre*, Bde 1-3. Freiburg i/B, first volume (1895); reprinted: Tübingen 1914.

Pearson K. (1928), *On a method of ascertaining limits to the actual number of marked members from a sample*, Biometrika, vol. 20A, pp. 149-174

Poisson S.-D. (1837), *Recherches sur la probabilité des jugements*, Paris (Paris 2003; English translation: S, G, p. 53).

Schmoller G., 1888, *Zur Literaturgeschichte der Staats- und Sozialwissenschaften*, Leipzig.

Schmoller G., 1900, *Grundriß der allgemeinen Volkswirtschaftslehre*, Tl. 1. Leipzig (edition of 1923 reprinted in 1978, Berlin).

Sheynin O., 1976, *Laplace's work on probability*, *Arch. Hist. Ex. Sci.,* vol. 16, pp. 137-187.

Sheynin O., 2017, *Theory of Probability. Historical Essay*, Berlin, S, G, 10.

Weber M., 1903, *Roscher und Kries und die logischen Probleme der historischen Nationalökonomie, Schmollers Jahrb. f. Gesetzgebung, Verwaltung u. Volkswirtschaft,* Bd. 27, pp. 1181-1221.

Winkler W.,1931, *Ladislaus von Bortkiewicz as Statistiker*, Schmollers Jahrbuch f. Gesetzgebung, Verwaltung u. Volkswirtschaft im Deutschen Reiche, 55. Jahrgang, pp. 1025-1033.

Woytinsky W.S., 1961, *Stormy Passage*, New York.

Zagoroff S., 1929, *Борткевичъ като икономистъ*, Rev. Trimesrtrielle de la Direction générale de la statistique, no. 1, година 1, pp. 10-12, Bulgarian with a French summary.

Zeuner G., 1869, *Abhandlungen aus der mathematischen Statistik*, Leipzig.