SLUTSKY'S PRAXEOLOGY AND HIS CRITIQUE OF BÖHM-BAWERK*

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The two articles by Slutsky (Evgeniĭ Evgenievich Slutskiĭ) being reproduced here in English translation are apparently his last writings on economic theory. From the dates Slutsky supplied at the end of each article we learn that the later publication (1927b) was in fact written first (26 December 1925) and the earlier one (1926b) completed a month later (25 January 1926). They were clearly both parts of an ongoing project. In the earlier-written article Slutsky starts out by saying, "The critical considerations before you of several aspects of Böhm-Bawerk's theory of value are meant to lay the groundwork for the final clarification of the received marginal utility theory." And at the end of section II he states in a footnote that "on another occasion" he will return to the question of measurability of utility. That article being a vigorous criticism of Böhm-Bawerk's theory of value (1909–1912), it is clearly alluded to in the introduction to the subsequently-written article when he states: "The fact that in our undertaking we are able to proceed without at all employing the concept of value or any categories of consciousness serves to demonstrate that certain empiricist trends of contemporary thought cannot be denied a relative validity." It is clear, however, that this article cannot be considered to be "the final clarification of the received marginal utility theory" promised in the earlier-written article. This final clarification never appeared.

As Kolmogorov (1948, p. 143; 2002, p. 67) stated, Slutsky "came to mathematics from economics." But even Kolmogorov did not list Slutsky's unpublished 1910 thesis on marginal utility theory (Slutskiĭ 1910) in his bibliography.[1] As pointed out by Kolmogorov's translator Sheynin (Kolmogorov 2002, p. 68), "Slutsky abandoned economics by the end of the 1920s because of the general political situation in the USSR."

Both articles, as well as Slutsky's earlier fundamental article on probability and statistics (Slutsky 1925, pp. 21n, 24) and his famous article on random causes of business cycles (Slutskiĭ 1927a: Slutzky 1937a, p. 119n), show the rather surprising influence of the philosopher Husserl. Slutsky begins his critique of Böhm-Bawerk (1912) by criticizing the latter's

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†This Introduction could not have been written without the help of Guido Rauscher (who discovered and brought my attention to many of the references cited here, and tracked down the whereabouts of Claus Wittich whose translation of Slutsky (1926b) had been referred to by Smolinski (1984), and persuaded him to produce the final version of his translation reproduced here; and made many very valuable criticisms and suggestions); and of Claus Wittich himself (who provided detailed critical readings of the various drafts and made many valuable and stimulating comments). Neither is responsible for deficiencies that remain. I wish also to thank Cherie Weston of the University of Minnesota Library's Interlibrary Loan department for finding many of the references cited here.

[1]This bibliography, which was also reproduced in Allen (1950), was prepared by Slutsky's friend N. S. Chetverikov. Claus Wittich informs me that the degree in question was probably that of an undergraduate "diploma". Even though the thesis earned him a gold medal, he was at the time refused access to graduate study ("aspirantura") for political reasons.

"psychologism"—a term introduced by Husserl (1900; 1913a, I; 1970, I; Chs. III–VIII) in his polemic against Mill.[2] §5, p. 183) that the to be an *a priori* generalisations from false there is a Gödel (1931) later pp. 204ff, and §64, that under Mill's uncertainly and instead of *directly* comparing two alternatives, such as an apple with seven plums (second-to-last paragraph of section I), Böhm-Bawerk insists on comparing the sensation of pleasure derived from the apple with that derived from the seven plums;[3] in fact—still worse—with the *sum* of the seven sensations of pleasure derived from each plum, a procedure which plainly contradicts the principle of diminishing marginal utility.[4] In paragraph 5 of section IV Slutsky asks: "Why a detour through intensities of sensations?"[5]

Slutsky (1927b) also made some very interesting observations about hedonism and the utilitarian calculus. He showed that Böhm-Bawerk's claim that the terminology of hedonism could be dispensed with was inconsistent with his concept of measurable "sensations" (paragraph 3 of section III): "What ...if one had to choose between pleasure and duty, or honor and life? ...The alchemy of hedonism supposes itself capable of transforming all these things: honor, duty, passion and who knows what else into sheer sensations of pleasure and pain; but to our author, after his turn away from hedonism, this route remains as good as closed."

A key statement in paragraph 6 of section IV sums up Slutsky's position: "*What a choice signifies ... is to be drawn directly from the meaning of the act of choosing*". Here we have an interesting anticipation of the idea of "revealed preference" later introduced by Samuelson (1938).

In section V, Slutsky brings out the inconsistency of Böhm-Bawerk's espousal of hedonism and his acceptance of Ehrenfels's (1893–1894, 1982) concept of "intrinsic value" (*Eigenwert*), arguing that the latter would at least have permitted him to incorporate non-hedonistic motivating forces such duty and honor, etc. He concludes his brilliant critique by tracing Böhm-Bawerk's errors to the "too many traces of the rationalism and atomism of the 17th" century, and his "too little ... positive regard for the facts in their own nature."

I now turn to the "praxeology" article. Unlike the earlier-written article, this one will mystify most economic readers, because of its high level of abstraction combined with its vague, nonmathematical method of exposition. The article was aptly described by Smolinski (1984) as one on "metaeconomics". The very first enigma concerns the word "praxeology"

[2]The statement by Mill criticized by Husserl reads (Mill 1865, Ch. XX, pp. 388–9; 1889, p. 461): "Logic ...is not a Science distinct from, and co-ordinate with, Psychology. So far as it is a science at all, it is a part, or branch, of Psychology; differing from it on the one hand, as a part differs from the whole, and on the other, as an Art differs from a Science. Its theoretical grounds are wholly borrowed from Psychology, and include as much of that science as is required to justify the rules of the art." Mill also made the seemingly contradictory statement (1865, p. 388; 1889, p. 461): "It is not with Thought as Thought, but only as Valid thought, that Logic is concerned"—a statement that Husserl would presumably have approved.

[3]Cf. Böhm-Bawerk (1912, III. Buch, I. Abschnitt, X.: pp. 334–337; Exkurs X.: pp. *280–283*), (1921, II.1, pp. 249–251; II.2, pp. 205–207), (1959, II, pp. 198–199; III, pp. 124–125). Also Kirzner (1994, pp. 339–355).

[4]This objection had already been made by Čuhel (1907, §§269–270, pp. 196–198; 1994, pp. 320–322) in response to Böhm-Bawerk's earlier exposition (1886, pp. 48–50); but Böhm-Bawerk's response cannot be considered satisfactory.

[5]In the last paragraph of the introduction to his praxeology article, Slutsky mentions Russell along with Husserl as the two authors who most influenced him. Russell (1903, Ch. XXVII, p. 233n) provided the following nice illustration: "Thus in mathematical Economics, pleasure and pain may be taken as positive and negative without logical error, by the theory (whose psychological correctness we need not examine) that a man must be paid to endure pain, and must pay to obtain pleasure. The opposition of pleasure and pain is thus correlated with that of money paid and money received, which is an opposition of positive and negative in the sense of elementary Arithmetic."

itself. Slutsky's article was cited by Mises (1933, p. 15n; 1960–1981, pp. 15 and 22 (note 14); 2003, p. 16n), where the term "praxeology" was taken to mean the science of human action; Mises subsequently (1949, p. 3) traced the term back to Espinas (1890).[6] Lange (1959; 1963, p. 189n) cited the Polish philosopher Kotarbiński (1955) as providing the "first systematic treatment", mentioning that this writer had "[begun] his work on praxiology much earlier". Indeed, in a 1923 conference paper not published until 1927,[7] Kotarbiński introduced the term independently of Espinas, and in Kotarbiński (1947)—where he discussed the Greek etymology of the word ($\pi\rho\hat{\alpha}\xi\iota\varsigma$, genitive $\pi\rho\acute{\alpha}\xi\varepsilon\omega\varsigma$)—he referred to Slutsky (1926b) apparently for the first time.[8] He cited Slutsky again in his Treatise (Kotarbiński 1955, p. 19; 1965a, p. 9); and in an important historical survey (Kotarbiński 1961, 1965b) he discussed the work of Espinas, Slutsky, and others in detail.

We do not know whether Slutsky had any awareness of Espinas (1890, 1897) or of the 1923 conference paper of Kotarbiński (1927) given in nearby Lwów. A possible explanation for his terminology, suggested to me by Guido Rauscher, is that it was based on Husserl's concept of "practical logic" (1900, 1913a, §13, p. 31n; 1970, I, p. 75n).[9] Slutsky, who was an extremely well-educated man, may well have invented the word himself to mean the study of (human) action.[10] It is with this meaning that the word has been used by Mises (2003, 1949). Kotarbiński (1955, 1965a) used it to denote the study of "efficient action"; Lange (1963, pp. 189–193) adopted it to mean the study of "rational action"—a definition later accepted by Kotarbiński (1965c)—and gave as illustrations the disciplines of operations research and programming, which are similar in spirit to some of Kotarbiński's (1972) applications.[11]

[6]It has since been traced back further to Bourdeau (1882); cf. Gasparski (1992), Alexandre (2000).

[7]This conference paper was cited by Gasparski (1992), and the 1927 published version by Ostrowski (1973, p. 287, and p. 370, notes 56 and 63). The concept of praxeology was further developed by Kotarbiński (1938), where he referred to Espinas (1897) apparently for the first time.

[8]But he stated (Kotarbiński 1947, p. 332; 1957, p. 675) that "we are citing from memory, owing to the destruction of [our] notes"—no doubt from the war, indicating that he must have discovered Slutsky's article substantially earlier. (This passage was brought to my attention by Claus Wittich.)

[9]In a letter to Felix Kaufmann, Husserl (1936, p. 227), citing Engliš's "philosophically indeed very valuable book" (1930, 1933), said that he had found there to his surprise "a formal axiology and practice" which he himself had attempted to develop in his early days. (I owe this reference also to Guido Rauscher.) This idea of a "formal axiology [study of value] and practice" was already alluded to in Husserl (1913b, §§117, 121, pp. 242, 250n; 1982, pp. 280, 288n).

[10]Webster's Ninth New Collegiate Dictionary (1987) defines "praxeology" as "the study of human action and conduct", derived evidently from the Greek $\pi\rho\hat{\alpha}\chi\iota\varsigma$, $\lambda o\gamma\acute{\iota}\alpha$ (action + study), and dates the word at 1904 but without providing the source.

[11]Tintner (1968, p. 12) observed: "It is interesting that both the extreme proponent of laissez-faire L. von Mises and the Marxist Lange defined praxeology in similar terms." Hayek (1942, p. 277n), in discussing what in his view was an unobjectionable yet misleading use of the word "teleological" in the work of Engliš (1930), stated: "If a name is needed the term 'praxeological' sciences, deriving from A. Espinas, adopted by T. Kotarbinsky [*sic*] and E. Slutsky, and now clearly defined and extensively used by L. v. Mises ... would appear to be the most appropriate." Engliš (1930, pp. 19–20) contrasted the natural sciences—based on the relation of cause to effect—with the "teleological sciences" (p. 30)—based on the relation of means to ends (purposes); he further (1930, p. 86; 1933, p. 61) defined teleology as "the explanation of human action by its motives". In conformity more with Husserl and Böhm-Bawerk than with Slutsky he held (1930, p. 93; 1933, p. 69) that "the judgments of the practical sciences are judgments of value." Engliš used the term "teleological" to denote both purposive action (and the accompanying reasoning and valuation on the part of the acting subject) and the theory that explains such action. Thus (1930, p. 84; 1933, p. 60)): "*Teleological theory does not itself valuate—it recognizes valuation*" (author's emphasis). On the other hand (1930, p. 84): "Teleological reasoning consists in valuation". Perhaps to clarify the distinction, in Vol. 2 (1933, p. 59) he changed "teleological" (*teleologische*) in this sentence to "purposive" (*zweckmäßige*).

Slutsky's article (1926b) deals with the study of human action. It would be consistent with his emphasis on "the purposive aspect of all praxeological systems" (section 6) to describe praxeology as the study of *purposive action*;[12] whether this means "rational" action is not entirely certain, as we shall see.

Before analyzing Slutsky's paper, it is of interest to see how he himself conceived of it, as expressed in a very interesting letter to Frisch (Slutsky 1926a).[13] After thanking Frisch for sending him an offprint of his "Sur un problème d'économie pure", he promised to send him when it appeared in a few months an offprint of his praxeology article:

> Anticipating later publications I have here set out in very tight space several of the main results of my studies of many years for publication for the first time.
>
> As concerns my previous work ("Sulla teoria ...") which I am having sent to you as printed material in the same post, I have to say that I have gone significantly further in several regards. So, for example, it is now not very pleasant for me to see in this work of mine such a perceptible streak of empiricism. Even though I always highly value the role of empirical experience and especially experiments in theoretical economics, I could not now subscribe to certain utterances in my earlier work, since, as I may believe, I have now arrived at a clearer insight into the relationship between the empirical and a priori elements of our knowledge.

He proceeded to say that his negative view of his 1915 (Slutsky 1915) paper "is a secondary matter", and then summarized what he conceived to be its important contribution, the "Slutsky equation" as it is now known, saying that: "Only once this is shown is it proven that the Paretian construction [of the function $U(x_1, x_2, \ldots, x_n)$ of 'ophelimity indices'] is not an empty one, since one can actually arrive at definite theorems concerning measurable economic magnitudes [e.g., the law of demand] without having to know something about any psychological data" (the latter referring to Pareto's prior assumption of additively separable utility). What he meant by the "perceptible streak of empiricism" in his 1915 paper, we can only guess.[14] But he did state that he regarded the Paretian construct of ophelimity indices "as methodologically essential in order to clearly demarcate different layers of the problem." In short, he still regarded the hypothesis of rational choice as essential.

Now let us go through the praxeology paper. Unfortunately Slutsky does not provide us with any examples of his basic concepts.[15] If we try, however, to relate the formalism

[12] I owe to Claus Wittich the remark that the 1855 Liddell/Scott Greek-English lexicon cites from the Odyssey (3,72; 9,253), among many ancient occurrences of $\pi\rho\hat{\alpha}\xi\iota\varsigma$, the phrase $\kappa\alpha\tau\grave{\alpha}\ \pi\rho\hat{\eta}\xi\iota\nu$ ("advisedly, on purpose") with antonym $\mu\alpha\psi\iota\delta\acute{\iota}\omega\varsigma$ ("thoughtlessly, at random"). This supports Hayek's view that Engliš's purpose-oriented approach could well be described as "praxeology" rather than "teleology". Indeed Engliš (1930, pp. 49–57; 1933, pp. 31–40) provided illustrations which are fully in the spirit of Kotarbiński's.

[13] I am very grateful to Olav Bjerkholt of the University of Oslo for supplying me with his transcriptions of the Slutsky-Frisch correspondence from the holdings in the National Library of Norway in Oslo.

[14] Likely examples are his statements (1915, p. 4; 1952, pp. 30–31): "As for Gossen's law (of the satiability of wants) we must regard it simply as an empirical generalization, although lacking rigorous demonstration" and (1915, p. 17; 1952, p. 45) "Gossen's law itself remains what it always was, that is, an empirical proposition, not a rigorously demonstrated truth." Also his supposition (1915, p. 24; 1952, p. 53) that "if after an increment in the quantity of good α has occurred, an individual does not observe any modification in his subjective relations with the good β, the marginal utility of the latter has not noticeably changed and there exists the approximate equality $u_{\alpha\beta} = 0$."

[15] He provides two examples of the receiving and giving of gifts (sections 2 and 4) and one of his concept of power (section 4). As Kotarbiński (1961, pp. 5–6; 1965b, p. 6) pointed out, Slutsky's concepts are in the spirit of the undefined concepts in Hilbert (1899).

to the problem analyzed in his 1915 paper—that of the demand of a single consumer—we might interpret the "state space" of section 1 as the positive orthant of an n-dimensional "commodity space", and a "possibility set" (the first "determining circumstance") as a budget hyperplane defined by given prices and income. The second "determining circumstance" is the "directive force"; perhaps we can interpret this as the system of level sets (indifference varieties) defined by a utility function. Then with this interpretation the "optimal point" of the possibility set is the commodity bundle $(x_1, x_2, \ldots, x_n)$ for which the utility function is maximized. Note that Slutsky takes this "optimum" to be unique (in this section of the paper, at least), which would require some convexity assumptions both for the possibility set and the utility function.

There are two reasons for adopting the interpretation that the "directive force" is some kind of utility function, or an agent who maximizes such a function: (1) his use of the term "optimum"; (2) his view expressed to Frisch that a utility function is "methodologically essential". One could of course argue that, but for those two reasons, Slutsky in 1926 might have moved closer toward the positions later taken by Allen (1932) and Samuelson (1938). Allen (1932) analyzed consumer behavior in terms of infinitesimal "preference directions" and limiting "indifference directions", but denied (pp. 222–4) that these could in general be "integrated" into a utility function. Samuelson (1938) initiated a new approach, "dropping off the last vestiges of the utility analysis" (p. 62), and introducing (p. 65) what later came to be known as the "weak axiom of revealed preference." But after Houthakker's (1950) "strong axiom" was introduced, the integrability problem was solved and the existence of a utility function followed as a consequence. And Samuelson (1950) himself showed that this was equivalent to the symmetry and negative semi-definiteness of the Slutsky matrix. Finally, Hurwicz and Richter (1979) showed that just the symmetry of the Slutsky matrix was equivalent to Ville's (1946) axioms ruling out preference cycles $x^1 \succ x^2 \succ \ldots \succ x^t \succ x^1$. Of course, Slutsky could not have anticipated these results; but he was justly proud of his discovery that utility maximization implied the symmetry of the Slutsky matrix, and might have suspected that his symmetry conditions would also be necessary to rule out the possibility of cycles.

Already in section 1 Slutsky discusses "*passive* changes" in the state of the system followed by a "transition to a new optimum", which could be taken to be a discussion of the stability of equilibrium, the disturbances to equilibrium being brought about by random processes.

Starting in section 2 Slutsky takes up a dynamic and stochastic approach, in which each "state of a system [including the optimal state?] refers to a temporary and indeed fleeting determinateness". In section 3 he formulates the concepts of "existential operations" consisting of creation, maintenance, or destruction of an object; exchange between two individuals is then considered as a simultaneous "destruction" of the commodity given up and "creation" of the commodity received by each individual, and the capacity of performing existential operations is called "power". This may leave room for the idea of bargaining power, and thus of "terms of exchange" between two people, and in particular for parametric market prices and the corresponding individual budget sets consisting of subjectively conceived "possibility sets". In this section Slutsky introduces the concept of "economic endowment" as "the entirety of all those economic objects presently in the ownership of an agent", suggesting that the "possibility set" of section 1 should instead be interpreted as a budget hyperplane defined by prices and endowments—although nowhere does Slutsky mention prices in his exposition.

Section 4, which further analyzes endowments, introduces concepts of intertemporal re-

lationships, in particular the distinction among (1) power over (i.e., ownership of) present objects, (2) "disposition potentials" (claims over future objects), and (3) expectations of disposition potentials.

Section 5 introduces a distinction between random selection and random outcomes. Random selection necessarily occurs if there is not a unique optimal point. If there is a continuous set of optima and a continuous unimodal distribution over this set, then Slutsky defines the optimum as the modal (maximum-likelihood) point, which in general will differ from the selected one. In the discrete case he distinguishes between the selected and the "actual" state; it is not clear whether by the latter he means the same thing as the "endowment" defined in section 3. By "random outcomes" Slutsky apparently means outcomes dependent on a probability distribution over the state space as opposed to one over choices *given* the state space. In this section Slutsky introduces the concept of a "practical degree of certainty"; this idea may be what he alluded to in one of his later articles on probability theory (Slutsky 1937b)—apparently his only subsequent reference to his 1926 article.[16] In the last paragraph of this section Slutsky mentions the possibility of what in the contemporary literature is called "stochastic choice"; but he does not pursue it.

The sixth and last section is the most difficult; it deals with an individual's perception of the future, presumably including future possibility sets. Slutsky calls the set of all past, present, and future states the *observation space.* Since the future cannot affect the present, he introduces the device of a *complementary* system or space whose components are the images of those of the observation space. If the correspondence is one-to-one, it is called *normal.* The *image* of the observation space (that is, the complementary system itself), is called the *observation apparatus*; its inverse image is called the *field of vision.* Elements ("components") of the field of vision are called "visible". They seem to play the role of *certainty equivalents* in contemporary terminology. In the non-normal case, visible components, i.e., components of the observation space that are inverse images of the observation apparatus, are called *real*, while the remainder are called *imaginary.* In his Case III, all (visible) present components are real, while all future components are imaginary; then he states that the latter are "derivable" on the basis of "some more or less complex rule from the components of the observation space" (perhaps a process of adaptive expectations?).

Clearly, Slutsky's praxeology article can be considered only as a sketch of an uncompleted project. How should we conceive of it? In the last paragraph, he tells us that it is meant to offer some "glimpses into the boundless richness of formal praxeology, a field that represents

[16] In this 1937 article, in discussing propositions of the form $P\{x \in A\} = p$ (the probability that the outcome x belongs to the event A equals p) as being either categorical (*assigning* a probability to an event) or problematic (concerned with the *handling* of an event with a given probability), Slutsky explained the latter in the following way (1937b, p. 183):

> The second aspect becomes practically important when one of the two complementary probabilities
>
> $$P\{x \in A\} = p, \quad P\{x \in \bar{A}\} = q, \quad (p + q = 1)$$
>
> is very small, since we may consider as a praxeological law, that is, one conforming to human mentality, the act of *absolutely* ignoring events with a sufficiently small probability of occurrence.

A footnote following the word "praxeological" referred to his 1926 article. This footnote was expunged from the Russian translation (Slutskiĭ 1960, p. 269), which instead emphasized the word "practically" in the above-quoted passage.

The passage is of interest not only in its forerunning of the concept of "almost sure" events in contemporary probability theory, but also in its foreshadowing of statistical decision theory.

a 'definite multiplicity' in Husserl's sense." This term (*definite Mannigfaltigkeit*) was used by Husserl (1913b, §72, p. 135; 1982, p. 163; 1929, §31, pp. 82–85; 1969, pp. 94–97) to mean a formal system derived from a complete and finite system of axioms, of which Hilbert's geometry (1899, 1902) is the model. Slutsky had earlier (1925, p. 22) confined the term "multiplicity theory" to the study of "spatial objects", and after citing Husserl (1913a, 1913b) had commented as follows (1925, p. 21):

> What has happened to geometry is particularly instructive. The "geometry" of Hilbert's *Grundlagen* is in fact no longer geometry, not a theory of space; its points, straight lines, and planes are "points", "straight lines", and "planes" in quotation marks—pure symbols for indefinitely conceived objects in the sense of a logical "something". It would of course be more correct to no longer call such a discipline "geometry" but rather "multiplicity theory" [*Mannigfaltigkeitslehre*], all the more so as the correct name already exists.

The term *Mannigfaltigkeiten* was borrowed by Husserl from Riemann, the creator of topology, who used it in quite a different sense, namely to designate spaces that are continuous and differentiable deformations of Euclidean space (now known as "manifolds" and "Riemann surfaces").[17] Slutsky's above characterization of Hilbert's contribution appears to overlook Hilbert's reasons for omitting definitions of the primitive terms,[18] and likewise Slutsky's praxeology concentrates on basic concepts and (despite the promise in the third paragraph) omits the corresponding axioms (such as transitivity and continuity of choice, compactness of constraint sets).[19] But unlike Hilbert's geometry, which can apply to strange collections of objects in addition to Euclidean space, with "sphere-bundles" instead of points, etc. (cf. Stegmüller 1975, p. 31), Slutsky's praxeology is intended to apply only to human action.

Unfortunately, circumstances forced Slutsky to leave us only these two fragments of his work on economic theory following his brilliant 1915 article. We must savor what we can.

[17]Riemann's contributions were contained in his fundamental papers of 1851 and 1854 (cf. Riemann 1892, Memoir I, pp. 3–48 (esp. p. 36); Memoir XIII, pp. 272–287). The French translator L. Laugel of the 1851 paper rendered the unqualified term *Mannigfaltigkeit* as *ensemble* ("set"), and the term *stetige Mannigfaltigkeit* as *variété continue* ("continuous variety") (cf. Riemann 1968, p. 44). The French translator J. Hoüel of the 1854 paper rendered *Mannigfaltigkeit* throughout as *variété* (p. 282), tracing the terms *varietas* and *Mannigfaltigkeit* back to Gauss. This agrees with Georgescu-Roegen's (1936) generalization of Pareto's (1909) "indifference curves" to "indifference varieties"; Georgescu-Roegen attributed the term "variety" (*Mannigfaltigkeit?*) to Gustav Lejeune Dirichlet (Riemann's teacher in Berlin and predecessor in Göttingen). On the other hand, Bourbaki (1960, pp. 147–8) consistently translates *Mannigfaltigkeit* in Riemann's 1854 paper as *multiplicité*, in agreement with Husserl's student Cairns's translation of the term (Husserl 1929, pp. 81–85; 1969, pp. 93–97). However, Slutsky does not seem to use the term *Mannigfaltigkeit* in Georgescu-Roegen's sense. Russell (1903) made frequent reference to Cantor (1883), but as Jourdain (1915, p. 54) makes clear, Cantor's *Mannichfaltigkeitslehre* was his early name for set theory (*Mengenlehre*).

[18]Namely that concepts such as "points", "straight lines", etc. cannot be rigorously defined—any more than can words in a dictionary (e.g., "violin" = small viola, and "viola" = large violin)—except by means of axioms specifying their mutual relationship.

[19]As Kolmogorov remarked (1948, p. 144; 2002, p. 69): "In the area of foundations of probability theory, the technical aspect of choosing a set of axioms, checking their independence, etc., remained remote from Slutsky's interests." But he went on to say: "However, he was the first to draw a correct picture of the purely mathematical essence of probability theory [(Slutskiĭ 1922, Slutsky 1925)]." Coming from the author of the definitive treatise on the subject (Kolmogorov 1933, 1950), this is indeed high praise.

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