Science in Context **17**(3), 391–422 (2004). Copyright © Cambridge University Press
DOI: 10.1017/S0269889704000195 Printed in the United Kingdom

Classical Text in Translation

Translated by Oscar Sheynin

With footnotes by Reinhard Siegmund-Schultze

On A. Ya. Khinchin's paper "R. Mises' Frequentist Theory and the Modern Concepts of the Theory of Probability"[1]

B. V. Gnedenko (1961)

The paper presented here was written as early as sometime between 1939 and 1944[2] by the eminent mathematician Aleksandr Yakovlevich Khinchin, who is well known for his contributions to the theory of probability, statistical physics, theory of numbers, and theory of functions. For reasons unknown to me, it remained unpublished, although I remember that Khinchin had submitted it to the periodical *Uspekhi matematicheskikh nauk*.[3] After he died, while I was putting in order the scientific and literary heritage of Khinchin, I remembered this work and began looking for it. Regrettably, I was unable to find any copies of a final version and the editorial office of *Uspekhi* did not have any record of the article. So I decided to make use of a copy that had been retyped in 1946 by my students, E. L. Rvacheva and D. G. Meyzler,[4] even though it had some lacunae. I am convinced that even in this state, Khinchin's work is of considerable interest.

Indeed, year after year, methods relating to the theory of probability are gaining importance in various fields of knowledge. Therefore, ascertaining the nature of random events, and ascertaining an approach for defining the fundamental concept – the probability of a random event – is an important matter. The concepts of Mises, outwardly attractive and convincing at first sight, continue to find many supporters, especially among members of schools of thought outside mathematical research. Therefore, a logical and philosophical analysis of these ideas should still be considered topical today. This is especially true since Mises' original methodological viewpoints are all retained without modification in the comparatively recent (already posthumous) English edition of his well-known book, *Probability, Statistics, and Truth* (London 1957).

[1] *Voprosy filosofii. Nauchno-tekhnicheskiy zhurnal* 15, no. 1: 91/92. See also Gnedenko, B. V. and A. N. Kolmogorov 1960. "Aleksandr Yakovlevich Khinchin (1894–1959). Obituary." *Russian Mathematical Surveys* 15:93–106.
[2] This assumption is somewhat doubtful as argued below. Probably the paper was written already around 1936.
[3] Khinchin's paper, as will be seen, was more philosophical than mathematical and Gnedenko himself decided to publish it in *Voprosy filosofii*.
[4] We were unable to trace any biographical information about the two mathematicians.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

The publication of Khinchin's paper shows once again the need for Soviet mathematicians and philosophers to work out and develop their opinions on the nature of probability, and on the interrelation between the theory of probability and the world of real phenomena. The time for this is ripe, and Khinchin's paper can serve as an appropriate starting point for such a debate. In fact, when it was read aloud and discussed at two joint meetings of the philosophical seminar and the seminar on the history of mathematics at Moscow State University, it was decided unanimously that the paper should be published.

I have just added a few notes in order to facilitate its reading; in addition, I adduce a short description of Khinchin's work in the field of the modern theory of probability.

Aleksandr Yakovlevich Khinchin (1894–1959) is deservedly considered one of the founders of modern probability theory. His name is connected with the important period when the set-theoretic approach to constructing the foundations of probability theory was developed. He is also remembered for establishing the basis for the general theory of stationary random processes.[5] These subjects were part of a wider program that he conceived for ascertaining the role of statistical regularities in various fields of mathematics and the natural sciences. This methodological viewpoint stimulated his research in the metric theory of numbers, in statistical physics, in the summing of independent random variables and queueing theory (in these [two] last-mentioned fields he was directly connected with practical telephony) and, during the last period of his life, in problems concerning the transmission of information.

It was of course impossible to carry out this program without discussing problems of a philosophical nature; the logic of the development of science inevitably compelled this great scientist to investigate them. And, indeed, Khinchin invariably turned to the philosophical interpretation of the central problems of probability theory and its applications to the natural sciences. He discussed this subject in special philosophical articles and in monographs devoted to solving concrete mathematical problems, as well as in his lectures to students and his talks in seminars on methodology.

A.Ya. Khinchin was among those Soviet scientists who take a systematic interest in the methodology of their science and stand firmly on the ground of dialectical materialism while solving burning philosophical problems of modern natural science. He repeatedly expounded his viewpoint in print and also in talks and in seminars on methodology. L. E. Maystrov [Maistrov][6] reminded me of one of these talks, which Khinchin gave in 1951 at the methodological seminar of the V. A. Steklov Mathematical Institute, where he sharply criticized idealistic conceptions in the modern theory of probability. In Khinchin's papers a notebook is preserved with the handwritten

[5] A special class of stochastic processes the distribution of which is independent ("homogeneous") with respect to translation of time. This theory was closely connected with Khinchin's work in ergodic theory. Cf. Khinchin, A. Ya 1934. "Korrelationstheorie der stationären stochastischen Prozesse." *Mathematische Annalen* 109:604–615.

[6] Maystrov (1920–1986) is known especially for his book *Probability Theory: A Historical Sketch*. New York: Academic Press, 1974.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

title, "On some idealistic tendencies in probability theory," where he registered his fragmentary ideas on that topic.

For more than forty years, Khinchin was connected with Moscow University as a student, post-graduate, and professor. In 1939 he was elected Corresponding Member of the Academy of Sciences of the USSR. Since 1944, when the Academy of Pedagogic Sciences of the Russian Federation was established, he was a full member and acted as a member of its presidium.

R. Mises' Frequentist Theory and the Modern Concepts of the Theory of Probability

A. Ya. Khinchin[7]

Introduction

About twenty years ago, the German scholar Richard von Mises began criticizing the generally accepted foundations of the theory of probability.[8] His sharp criticism appeared consistently in the pages of mathematical and philosophical periodicals. At the same time he offered his own, new basis for this science, which he still tirelessly continues to advocate in mathematical and philosophical articles, in popular monographs,[9] and in his well-known course on the theory of probability.[10] His so-called "frequentist" theory of probability found a large number of followers among mathematicians and especially among representatives of natural and applied sciences, in particular, physicists.

The literature devoted to the problems of probability theory is immense and discussions are heated; in a word, the frequentist theory became so essential for the life of the modern doctrine of probability that every representative of our science feels obliged to adopt some definite stand with respect to it. It is already becoming impossible to ignore or disregard the frequentist theory, or brush it aside. Nevertheless, we still do not know of any attempt to analyze critically and exhaustively the role of

[7] *Voprosy filosofii. Nauchno-tekhnicheskiy zhurnal* 15, no.1:92–102, no.2:77–89 (Russian).

[8] Mises (1919) is the decisive publication, which contains Mises' axioms for the "collectives." But Mises (1912) has already used the word *Kollektivmasslehre* in the title, which was coined by physicist and psychologist Gustav Theodor Fechner (1801–1887), and hints at the frequentist interpretation of probability. One can therefore assume that Khinchin's paper was written in 1939 at the latest. See Mises, R. von 1912. "Über die Grundbegriffe der Kollektivmasslehre." *Jahresbericht der Deutschen Mathematiker-Vereinigung* 21:9–20, and Mises, R. von 1919. "Grundlagen der Wahrscheinlichkeitsrechnung." *Mathematische Zeitschrift* 5:52–99. See also Heidelberger, M. 1987. "Fechner's Indeterminism: From Freedom to Laws of Chance." In *The Probabilistic Revolution: Volume 1: Ideas in History*, ed. by L. Krüger, L. J. Daston and M. Heidelberger, 117–156. Cambridge: MIT Press.

[9] Obviously referring to Mises (1928) in its various editions, which was translated into Russian in 1930 under Khinchin's supervision. See Mises, R. von 1928. *Wahrscheinlichkeit, Statistik und Wahrheit*. Wien: Springer. Mises (1936) is the second German edition with the same publisher.

[10] Mises, R. von 1931. *Wahrscheinlichkeitsrechnung und ihre Anwendungen in der Statistik und der theoretischen Physik*. Leipzig und Wien: Franz Deuticke.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

this theory in the historical development of our science and its place within an array of different attempts at founding the theory of probability.[11]

It is typical that the most authoritative proponents of the modern probabilistic ideas (Bernshteyn [Bernstein] and Kolmogorov in the USSR, Borel, Lévy and Fréchet in France, Cantelli and Finetti in Italy, Cramér in Sweden, and others) have hardly ever expressed their views on this problem.[12] The overwhelming majority of the relevant statements are made either by second-rate specialists[13] or by philosophers and physicists. In addition, these statements are almost always of a rather particular kind: they discuss this or that isolated feature, criticize a specific proposition, and suggest this or that change or improvement. A scornful, and almost ironic attitude prevails among mathematicians with respect to the frequentist theory. In private talks you will almost always hear that, undoubtedly, "not everything is in order" with it, that it suffers from incurable logical flaws, and that, from the mathematical point of view, it therefore cannot even be seriously approached. A physicist will usually object by stating that, even if this were true, it is the business and the duty of mathematicians to remove these formal defects; and that it is inadmissible to reject on principle, because of its inherent temporary and purely formal imperfections, this theory which so brilliantly conforms to the essence and requirements of scientific practice.

However, the broader questions – such as, whether the frequentist theory truly meets the requirements of the applied sciences better than all the other theories? and if so, why? or what are the theory's formal imperfections? – these wider questions have never been considered in sufficient generality and completeness. Almost always the discussion has been restricted in a polemical way to some isolated features. It is interesting to note that in this polemic, the so-called classical theory of probability based on considering equally possible cases – against which the main accusations made by the adepts of the frequentist theory and Mises himself were and are directed – was

[11] [As far as I know, no analysis of this kind exists at present. The article by the well-known logician Church and the remarks in the book by R. Péter (p. 218) have a narrower aim. Cf. Church, A., On the Concept of a Random Sequence*; Bulletin of the American Mathematical Society* 46 (1940), 130–135, and Péter, R., *Recursive Functions* (Russian translation of German original which appeared in Budapest 1951); Moscow 1954. (B.G.)].

Meanwhile (in 2004) several analyses of von Mises' notion of probability have been published. Cf. Martin-Löf, P. 1969. "The Literature on von Mises' Kollektivs Revisited." *Theoria* 35:12–37, and Hochkirchen, Th. 1999. *Die Axiomatisierung der Wahrscheinlichkeitsrechnung und ihre Kontexte. Von Hilberts sechstem Problem zu Kolmogoroffs Grundbegriffen*. Göttingen: Vandenhoeck & Ruprecht. See also the unpublished thesis Bernhardt, H. 1984. *Richard von Mises und sein Beitrag zur Grundlegung der Wahrscheinlichkeitsrechnung im 20. Jahrhundert*. Berlin, 226 pp.

[12] In October 1937 a *Colloque sur le calcul des probabilités* was held in Geneva. Fréchet's contribution (1938) in that colloquium deals expressly with Mises' theory. Fréchet, M. 1938. "Exposé et discussion de quelques recherches récentes sur les fondements du calcul des probabilités." *Actualités Scientifiques et Industrielles* 735:23–55. S. N. Bernshteyn, A. N. Kolmogorov, and M. E. Slutskiy published in the proceedings. The Russians apparently did not attend that meeting, but were at least aware of it. This gives support to the conjecture that Khinchin's paper was written before 1937.

[13] Wald (1938) and Ville (1939) were hardly second rate specialists, this again hints at the chronological conjecture above. Cf. Wald, A. 1938. "Die Widerspruchsfreiheit des Kollektivbegriffs." *Actualités Scientifiques et Industrielles* 735:79–99, and Ville, J. 1939. *Étude critique de la notion de collectif*. Paris: Gauthiers-Villars.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

hardly defended by anybody. Were it not that the classical conception still occupies a prominent place in textbook literature lagging considerably behind modern science, it could have been said that Mises, who even in 1936 continued his vigorous attacks against it,[14] had in essence been forcing an open door.

We must concede that in the current fight between the partisans of the frequentist theory and its opponents, the former have one obvious advantage: they at least are proposing something positive,[15] whereas their opponents, while pouncing on these proposals and noting the single shortcomings thereof, are as a rule unable to put forth suggestions of their own. How did this situation arise? We believe that until now the cause was that the development of the theory of probability has been considerably lagging behind other mathematical sciences. As a rule, a mathematical discipline begins an analysis of its foundations only after it has reached a certain scientific maturity. For probability theory, this moment occurred only in the most recent years; only lately has the theory firmly realized its place among the other disciplines of mathematics, its specific features, and its own scientific procedure. And only from the summit that it has presently attained can the broad and distinct picture of the development and the modern state of the theory of probability be grasped. Only now within this picture can the status of the frequentist theory be perceived with full clarity. Thus it becomes understandable why ten, or even five years ago, the most thoughtful representatives of our science had abstained from expounding in detail their views on the problems posed around the frequentist theory, and why the opponents of this doctrine were unable to counterbalance it with some sufficiently substantiated positive suggestions. In particular, the author of these lines has to admit that in the light of our present-day level of the doctrine of probability, his critical paper written several years ago[16] and devoted to the frequentist theory, should be judged as unsatisfactory[17] even though some of the propositions defended there remain true.

The considerations put forth above compel us to believe that the time has come for a comprehensive critical elucidation both of the role of the frequentist theory and of its place in science. Nowadays the theory of probability is already shaped to

[14] Most likely Khinchin is alluding to the second German edition (1936) of Mises (1928).

[15] The word "positive" is not unambiguous here, given the fact, that Kolmogorov's *Grundbegriffe* of 1933 existed at that point in time and Khinchin calls the latter the "predominant direction of the modern theory of probability" (below). Khinchin seems to allude rather to the problem of application and to the origin of the notion of probability in nature. As a matter of fact, already in his *Grundbegriffe* of 1933 Kolmogorov would very clearly point to the positive value of von Mises' theory for the connection to application. See Kolmogoroff, A. 1933. *Grundbegriffe der Wahrscheinlichkeitsrechung*. Berlin: Springer, p. 3. The English translation of this book is *Foundations of the Theory of Probability*. New York: Chelsea, 1950.

[16] [Discussion here is about the article "Mises' Doctrine on Probability and the Principles of Physical Statistics (Russian)," *Uspekhi fizicheskikh nauk* 9 (1929), no. 2, 141–166. (B.G.)].

[17] By "unsatisfactory" Khinchin obviously means "too positive," because recent results in ergodic theory, especially in Khinchin's interpretation, had made outdated von Mises' work on Brownian motion from 1920 to which (Khinchin 1929), as quoted by Gnedenko in the footnote, is referring. See the introduction in the present edition by R. Siegmund-Schultze.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

such an extent, and has assimilated its logical grounds to such a degree, that this critical interpretation may be undertaken not from the subjective point of view of some scholar, as it was necessarily done several years ago, but from the objective and principled position firmly secured by our science during these last years. Our paper is mostly devoted to this purpose; our critical analysis is all the more topical since exactly this year the founder of the frequentist theory, Richard von Mises, has published a detailed survey[18] of the main objections and additions to it along with an exposition of his own thorough answers to them.

1. The Merits of the Frequentist Theory

We begin by ascertaining some of the most important merits of the frequentist theory not in order to follow the traditional maxim, "First the achievements, then the shortcomings," but solely because it is by way of those merits easiest to acquaint the reader with the historical situation in which this theory was set up and developed.

In the first of his works, Mises some twenty years ago raised the alarm in connection with the scandalously unhappy situation concerning the foundations of the theory of probability. This single fact already constitutes an historical merit of such importance that one can forgive much. In those days the old system of principles, adopted without changes from Laplace, completely dominated all handbooks and treatises on probability; there is no doubt that its unfitness, its incompatibility with the level to which the mathematical science had arisen since Laplace's lifetime, were very visible to every thinking researcher. It was even possible to encounter, now and then, isolated and incidental pertinent statements; and yet, each author, while beginning his treatise, invariably spoke about equally possible and favorable cases, thereafter attempted to leave this annoying subject as soon as possible and to pass on to the subsequent calm course of the theory concealing no more reefs. In those days, the theory of probability instinctively turned its back on and avoided an unbiased and relentlessly critical revision of its foundations. Such a situation was conditioned, as it always is, by the theory's insufficient [logical perfection].[19] On the other hand, however, it became ever less tolerable in the face of all the other mathematical sciences that persistently worked on revising and rebuilding their foundations.

Of course, one could nevertheless not content oneself with merely disclosing the trouble indicated above, even by raising a vigorous and permanent alarm. And Mises, even in his first articles, did not restrict himself in this way. In order to establish a firm

[18] Khinchin mentions neither M. Fréchet, A. Wald, nor J. L. Doob, all of whom were involved in discussions with Mises in 1937 and 1941. Thus, one may assume that Khinchin wrote the paper earlier, partly in 1936, and that he is accordingly referring to Mises (1936), the second German edition of von Mises' book (1928).

[19] [In the manuscript version to which I resort, the end of the sentence is missing. I have added the words in brackets. (B.G.)].

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

basis for the theory of probability, i.e. to lay a foundation that would make it a worthy member of the family of modern mathematical disciplines, it was first necessary to describe with absolute clarity all the shortcomings of the existing system for founding probability, and to show, convincingly excluding any doubt, that no satisfactory foundations could be construed along the old lines. In a series of investigations, Mises accomplished this task with an exhaustive completeness, and here lies the second substantial merit of his doctrine. Many authors had indicated, even before Mises did, that the definition of probability by means of equally possible (i.e. in essence, equally probable) cases, amounted to a certain extent to an empty tautology. But, as Mises absolutely correctly states, this is the most harmless among the sins of the classical idea; moreover, in a certain sense, it can even be justified.[20] We may consider this definition as a reduction of the problem of finding a quantitative measure of probability in the general case, to a preceding notion of equiprobability of events; the vicious circle thus disappears and the definition itself acquires some scientific meaning.

Mises was the first to reveal, systematically and convincingly, the more essential flaws in the classical notion of probability, which are much more difficult to get rid of. The first, and the main one of these defects is the extremely restricted sphere of application. Having originated and been developed due to games of chance and simplest insurance operations, the old theory of probability had built for itself a basis fit to a certain degree for treating these simplest problems; however, once the sphere of its problems had extended in connection with the requirements of the physical and the social statistics, and later on, of biology and technology, the initially adopted foundations became too narrow. In problems reaching beyond the realm of games of chance, those equally possible cases, without which the classical concept cannot even speak about probabilities, just do not exist. Mises' celebrated example of an irregular die is unsurpassed in validity and simplicity of argumentation.

Moreover, Mises was the first to show, systematically and convincingly, the inaptitude of the classical basis for forecasting the real course of phenomena. He absolutely correctly indicates that, without new special assumptions, the conclusions of the theory of probability, built on the classical definition of its main notions, offer in essence no grounds for any, even the most unpretentious opinion about how the pertinent real processes should go on. Imagine for example twenty throws of a regular coin. For conclusions concerning the real course of this process, an additional definition of the concept of equipossibility, connecting it with experiment, is required, but the classical idea of probability is unable to provide it. Consequently, new principles, not following with logical rigor from the definitions of the main concepts, are needed for applying the classical theory practically.

We see thus that the author of the frequentist theory has the important historical merit of consistently exposing a number of fundamental defects of, and unsubstantiated

[20] See below the "Supplement."

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

claims made by the classical doctrine of probability. Along with this negative, so to say, merit, it is necessary to credit him now for his most important constructive achievement. No matter how we assess the frequentist theory and its future possibilities, we must admit that exactly its main principles reflected, for the first time ever, the opinion playing a basic role in the modern probabilistic outlook: the opinion, namely, that the theory of probability is a doctrine of mass phenomena. Of course, it was well known even before Mises that mass processes constitute the sphere of application of probability; yet, the demand that this feature of probabilistic teachings be represented already in their first principles, that all their formalism should be set forth in this spirit, – this demand was first put forward and carried out only within the frequentist theory.[21]

Being abstract, any mathematical theory must of necessity draw itself away from some properties of the studied objects. However, the classical conception of the theory of probability attempted to segregate its main principles not from statistical populations or recurring processes, which represent the true subject matter of its study, but from the specific properties of isolated objects participating in these processes. On the contrary, modern ideas recognize the very notion of probability as meaningful only in connection with mass phenomena, and therefore consider it desirable that this feature play the main part also and already in laying down the foundations of probability theory. As we have already remarked, this demand was first distinctly formulated and carried out precisely within the frequentist theory.

When speaking about probability, the physicist, biologist, technician or social statistician invariably has in mind some relative frequency. Furthermore, even a mathematician, during those special moments of his work when, interrupting the chain of formal deductions, he is compelled to turn his intuition to the material content of his concepts, – even he is in most cases apt to imagine each probability exactly as relative frequency. This does not at all mean that probability as a concept of a mathematical theory should include in itself all the totality of the properties and features peculiar to real frequencies; not even the frequentist theory does so. It only means that the theory of probability ought to be a sufficiently precise, formal image (hence an image gained by way of abstraction) of that structure, of those possibilities that take place in the world of real frequencies.

It is precisely this thesis – whose necessity is recognized by all modern probabilistic schools regardless of their specific orientation — that also constitutes the foundation of the frequentist theory. It was first formulated by this theory and we ought to recognize this fact as one of the theory's most important merits. We shall see below that Mises' doctrine considerably diverged from the main direction of development of the modern theory of probability in its opinion on an expedient realization of the demand contained

[21] However one has to be aware of von Mises' predecessors in the nineteenth century both in Germany (Fechner, Bruns) and England (Mills, Venn). See Heidelberger (1987). Elsewhere, in 1928, Khinchin even addressed the frequentists as the "English school." See the introduction by R. Siegmund-Schultze.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

in that thesis, i.e., on how and to what extent the abstraction and formalization should be carried out. This, however, should not obscure the cardinal fact that the credit both of formulating this demand and of being the first in attempting to carry it out, doubtless completely belongs to the frequentist theory.

2. A Natural-scientific or a Mathematical Discipline?

The fundamental divergencies that exist between the frequentist theory and the predominant direction of the modern theory of probability are very deeply rooted. In the first place, they are caused by the unremovable distinctions in the opinions concerning the theory of probability as a scientific discipline. We must attentively dwell on these differences because, without completely ascertaining them, the role or the situation of the frequentist theory in the modern doctrines of probability cannot at all be determined with sufficient clarity.

For Mises' school, the theory of probability is a natural-scientific discipline[22] which widely uses mathematical methods. Mises persistently defends this thesis against two other fundamental viewpoints that differ absolutely from one another. On the one hand, being in a philosophical sense a consistent positivist of a Machist[23] persuasion, he struggles against the aprioristic, metaphysical approach of the classical theory. This campaign, no matter how interesting and instructive it was in itself, cannot be elucidated in our study since it was directed against the past, whereas we are concerned, first and foremost, with the distinction between the frequentist theory and modern progressive ideas. The main antagonism can be formulated quite simply. Contrary to Mises' main thesis, according to which the theory of probability is a discipline of the natural sciences, the modern theory has defined itself as a branch of mathematics. This basic discord determines in a decisive and, moreover, in an almost exhaustive manner all the subsequent concrete discrepancies, which we discuss below. Let us pay attention to this point.

As we see, the main criterion by which to distinguish a natural-scientific from a mathematical discipline consists in the typical manner of defining the respective field of research. Each natural-scientific discipline is determined by the material specificity of its subject matter, by the real features of the studied domain of the existing world. It is in

[22] In fact, Mises' position was more nuanced and ambiguous. M. speaks of the theory of probability as a "natural science" in (Mises 1919, 53), and in the English edition of his book of 1928, which is Mises, R. von 1957. *Probability, Statistics, and Truth*. London: George Allan and Unwin, p. 219. However, he means it rather in the restricted sense of the "mathematical" ("tautological") part of a natural science.

[23] Note that Khinchin is approving of that critical part of "Machism." But below he does not do justice to von Mises' reception of Mach. Von Mises was, indeed, impressed by Ernst Mach as a philosopher but critical of the latter's neglect of mathematics. See Mises, R. von 1938. "Ernst Mach und die empiristische Wissenschaftsauffassung," p. 517. In R.v. Mises 1963/64. *Selected Papers of Richard von Mises*, vol. II: 495–523. Providence, Rhode Island: American Mathematical Society (2 volumes = Selecta I und II).

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

this way only that physics, biology, and psychology define their subject matter. One and the same subject matter can be studied by very different methods, mathematical ones included; however, when passing from one method to another one, we always remain within the boundaries of the given (natural-scientific) discipline since its main specific feature is its subject matter as it exists in reality rather than the method of its study. (Examples: phenomenological and statistical thermodynamics – [remaining within] the mechanical theory of heat; the corpuscular and the electromagnetic theories of light, the quantum theory of light [are all] theories of light.)

In contradistinction, the determining indicator of any mathematical discipline is always some formal method potentially admitting most various material realizations and, consequently, also practical applications. Whether or not some subject matter, some phenomenon of the world in reality can be studied by means of a given mathematical method, is determined not by the concrete material nature of this subject matter or this phenomenon, but exclusively by their formal structural properties and, above all, by those quantitative relations and spatial forms (Engels)[24] in which they live or proceed. (One example: the method of differential equations in physics, chemistry, and biology; because for its applicability it is sufficient that there exist two continuously changing magnitudes, whose changes have a definite relative velocity.)

To which class of scientific disciplines should we then count the theory of probability according to the criterion just described? What constitutes the basis of the unity of its method – the material, or the formal structural properties of the subject matter it studies? It suffices merely to pose the question in this manner in order to perceive with full clarity the one possible answer. The theory of probability is a doctrine of mass phenomena. Its methods are applied if, and only if, a large number of more or less equivalent ingredients participate in an actual phenomenon; its main concept is the relative number of those components which possess one or another given attribute. What is the material content of the studied phenomenon? or the real nature of these ingredients? or the nature of the attribute applied for classifying them? – none of these questions bear any relation to the judgment about whether a given process is within the capacity of the theory of probability.

Being defined by the formal features of the aspect of reality being studied, the theory of probability can be, on the basis of our main criterion expounded above, only a mathematical theory, but by no means a natural-scientific one. For all the simplicity and cogency of this conclusion, we were led to it merely by the considerable development of the theory of probability during the latest years, allowing it nowadays to appear as an actually shaped mathematical discipline. How then does the author of

[24] Khinchin alludes to Friedrich Engels' (1820–1895) understanding of mathematics. Engels' *Dialectics of Nature* was first published posthumously in 1927, and therefore its reception was rather new in Khinchin's time. These unfinished notes, written 1873–1883, played in the past a considerable role in much Marxist philosophical discussion on mathematics.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

the frequentist theory substantiate his persistent assertion that the theory is, and must be, a branch of the natural sciences — an assertion that in this context links his doctrine to the old metaphysical idea? He often formulates that view without substantiating it by arguments and apparently does not even suppose that there exist other points of view. On those occasions when he has to defend his outlook against the opinions described by us, Mises' arguments become quite primitive, not to say naive. He argues that each probabilistic problem relates certainly to some real repetitive process or some real mass phenomena; that the theory of probability is consequently a science addressing reality and hence a natural-scientific discipline. He does not want to notice that, on the basis of such an indicator, any mathematical discipline, e.g. the theory of differential equations, may be ascribed to the natural sciences; however, it would not have crossed anybody's mind to call into question the proposition that this theory is a typical branch of mathematics.

The cause of Mises' delusion is his Machist, and therefore, idealistic philosophical standpoint that engendered and still nourishes the foundations of the frequentist theory. An idealist, keeping, as Mises and his school do, to a positivistic stand, is always in a state of fear of mathematics,[25] no matter how he extols its merits in words. For him, giving away some doctrine to mathematics always means its alienation from actual contact with reality. He does not want to, and cannot, admit that mathematics and the natural sciences alike study the real world, and only it, although mathematics studies its other aspects, and by means of other methods. This is why he labels as nihilists[26] those who want to perceive a mathematical doctrine in the theory of probability; this is why, filled with disgust and horror-stricken, he struggles against the proposition, accepted without hesitation by all advanced scientists of our day, that the theory of probability is a part of the general doctrine of functions and sets.

"Thus," he says, "the theory of probability is by no means a part of set theory; it is the theory of certain observable phenomena . . . and merely makes use of theorems put forth by set theory."[27] And in another passage: "To a logical mind this identification

[25] In *Materialism and Empirio-Criticism* and elsewhere, Lenin asserts time and again that the idealist is always in fear of dialectics. In paraphrasing Lenin here, the mathematician Khinchin is obviously interested in labelling mathematics as something "dialectical." Given von Mises' reproach against Mach's lack of mathematics, mentioned above, this commentary is particularly inappropriate. Von Mises considered himself first and foremost a mathematician. See also the introduction to this edition by R. Siegmund-Schultze.

[26] This remark by Khinchin is particularly careless and at least strongly exaggerated. In (Mises 1936, 123–124) von Mises calls "nihilists" expressly those who maintain "that there is no need at all for a definition or explanation of the notion of probability." He adds that there are certain "intermediate stages" (*Zwischenstufen*) between the nihilists and his own (uncompromising) view, and names immediately afterwards the "standpoint mathematicians like to withdraw to" which was according to von Mises represented by A. N. Kolmogorov's "beautiful and very readable booklet," namely Kolmogorov (1933).

[27] This is an English version of the original passage in von Mises' German book (Mises 1936, 127), which Khinchin translates correctly into Russian.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

of two things belonging to different categories, this confusion of task and tool is something quite unbearable."[28]

From our point of view, the theory of probability as a science of mass phenomena can be linked only to set theory as doctrine about totalities of the most general kind. All concrete mathematical facts constituting the theory of probability retain their significance irrespective of which of the two viewpoints described above is chosen; however, with respect to the problem that especially interests us here, the problem of establishing the foundations of the theory of probability, the indicated disagreement, as we shall see now, is of crucial importance.

3. Idealization and Formalization

In order to construct the basis of a natural-scientific or of a mathematical discipline, a certain idealization of the subject matter to be studied is necessary. Within the boundaries of one and the same natural-scientific domain, the degrees of idealization can be quite different, depending on the adopted method of investigation. Thus, in the theory of turbulent motion of liquids we may idealize the investigated subject matter in different ways depending on whether we intend to study it by means of classical or statistical methods.

In exact conformity with this tradition, Mises, who recognizes the doctrine of probability as a natural-scientific discipline, distinctly indicates his proposed idealization of the subject which the theory of probability is called upon to study; he believes the problem of establishing the foundation of probability theory thus to be solved. The idealization he proposes is well known and extremely simple. Keeping to it, we ought to imagine the entire totality of the ingredients of a mass phenomenon or of a repetitive process as some infinite sequence of trials or observations called collective.[29] Then, two main properties are ascribed to each of these:

1) *The existence of the limits of relative frequencies* of those elements of the collective that possess one or another of some definite group of attributes.
2) The so-called *irregularity*, i.e., the invariance of the just mentioned limits with respect to the selection, according to any definite law, of some subsequence from a given collective; the law of selection, furthermore, should not rest upon the distinction between the elements of the collective concerning their relation to the

[28] Von Mises (1936, p. 127) in his quotation juxtaposes *Werk* (task) and *Werkzeug* (tool). He is not directly referring to Kolmogorov but to one of his interpreters. We follow the English translation of the third German edition (1951) of Mises' book in (Mises 1957, 100), which is otherwise much less detailed than von Mises' first reaction to criticism of his theory in the second edition (Mises 1936).

[29] Khinchin formulates now the two famous postulates (axioms) on *Kollektivs* (the equivalent of probability distributions or random variables), which von Mises had introduced in (Mises 1919, 55–57), and defended ever thereafter. Below in § 4, Khinchin gives another definition for a special case of a "collective."

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

studied attribute.[30] The limits of the relative frequencies are called probabilities of the respective attributes.

Thus, in this case, the idealization consists in that we have, first, to replace a real statistical population, of necessity finite, by some infinite series. Second, we have to attribute to this series two properties that a real population cannot possess, because, anyway, they have a definite meaning only for infinite series. The construction of the foundations for the frequentist theory is now concluded, and it is now possible to begin posing and solving concrete problems and establishing general regularities.

If, however, one considers the theory of probability to be a mathematical discipline, as we have done, then the construction of its foundations cannot come to a stop here. A modern mathematical discipline requires for its basis more than a mere idealization of a real subject matter. It demands complete formalization, or, what amounts to the same thing, axiomatization of its domain. This means establishing some group of principal initial propositions, the so-called axioms, that are destined to describe precisely all the relations holding between the basic concepts of the given discipline, while both concepts and relations are considered to be defined by exactly the list of axioms. All the subsequent concepts required by the given theory must be consecutively determined in a formal way in terms of these basic concepts; and all the subsequent propositions (theorems) have to be formally proved issuing from the axioms. The arguments that insist on this sort of establishment of the foundations as the only possible one in modern mathematics are well known. Only when underpinned in such a manner can it be guaranteed that a theory's formal backbone has really been filtered out without any rest left unaccounted for. Only thus can we perceive with complete distinctness the formal logical connections and interrelations between the various mathematical disciplines.

It is hardly necessary to indicate that such a formalization does not substitute for the real subject-matter a new (speculative) one having nothing in common with the former, as idealistic philosophy would like to imagine.[31] Nor is there any doubt that quantitative relations and spatial forms of the external world[32] continue to be the subject matter of investigation and that the only change concerns the method of studying them.[33]

Strictly speaking, Mises nowhere offers such a formalization of his theory; quite intentionally, his collective always remains infinite and, consequently, becomes an

[30] Von Mises stipulated excluding a "gambling system," the existence of which would allow for the selection of a subsequence with a changed limit and thus reveal the original sequence as not "random" (irregular). See (Mises 1919, 58).

[31] Here the fear of the "idealism-reproach" is palpable which typically is fended off actively by attacking "idealist philosophy." See also the introduction to this edition by R. Siegmund-Schultze.

[32] As stipulated in Engels' *Dialectics of Nature*.

[33] [Note that the theory of probability built on such a formal axiomatic foundation admits of a frequentist interpretation. (B.G.)].

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

idealized sequence of real trials or observations preserving all the concrete properties and peculiarities of natural objects of the given reality.[34] In his debate with the "nihilists," the author of the frequentist theory frankly stresses the impossibility and the undesirability of abstracting the theory from these concrete properties. And it should be admitted that he acts here quite consistently: if the theory of probability is considered a natural-scientific rather than a mathematical discipline, then there are no grounds for putting higher demands on it than it is necessary to make with respect to the majority of the natural-scientific disciplines.

In contradistinction, the modern theory of probability, which considers itself a mathematical discipline, creates its axiomatics (i.e., a formal scheme of the studied real domain which represents it as precisely as possible); a scheme where the two opposing tendencies – the striving for maximal abstraction so as to achieve utmost simplicity and the aspiration for retaining a maximal number of the essential concrete properties of the studied subject – should be harmoniously synthesized.

We assume the existence of some observation or trial that at least theoretically admits of an unrestricted recurrence. Depending on chance, an isolated trial can have one or another outcome. The totality of all these possible outcomes constitutes the basic set E, the first among the basic notions of our axiomatics. Any of its subsets, i.e., any totality of possible outcomes, is, as we call it, an event. Mathematical research shows that it is inexpedient to demand here the introduction of all possible events. We restrict our description to considering some field[35] of events.[36] This we shall henceforth denote by Φ. It determines the formal structure of the totality of those events to which we, when considering a given problem, intend to ascribe definite probabilities (example with the die).

In order to accomplish our next step, we turn to a real phenomenon. The theory of probability is interested in those repetitive trials where the frequency of the occurrence of some studied event remains stable. This gives us an occasion to perceive the most important feature of the considered phenomenon, that is, that each event included in our examination is assigned some positive number called its probability. It is expedient here to abstract from everything that accompanies the real phenomenon in the shaping of that concept, in particular, its emergence as a frequency.

[34] [Mises' approach, outwardly tempting, is tantamount to positivism, pure and simple. It restricts the tasks of the researcher to the description and moderate idealization of direct observations and denies the necessity of the next stage, i.e. that of working through to the essence of phenomena. (B.G.)].

[35] This is the field of probability (*Wahrscheinlichkeitsfeld*), see Kolmogorov 1950, 2. In the Russian original one finds *telo*, which is literally "body."

[36] [A set of events is called a field, if besides the events "A" and "B" it also includes events "A or B" and "both A and B" and furthermore the impossible and the certain event. (B.G.)].

The sometimes rather careless redaction of Khinchin's paper by Gnedenko is demonstrated by this footnote, which in the original does not only have errors in writing (which may have originated with the printer), but also fails to postulate the complementary event A^c in the field (algebra) of events as well.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

Thus: 1) The probability of the event E is unity; and 2) The probability of the sum of two non-intersecting (incompatible) events is equal to the sum of their probabilities.

It is self-evident that our segregation from the frequentist picture is by no means its replacement by some other and, especially, by an aprioristic metaphysical view on the nature of probability; a correct abstraction never indeed eliminates the subject so that the possibility of returning, at any moment, from the formal scheme to the real phenomenon is assured.

Thus, in performing the described series of abstracting acts on the real phenomena that are studied by the theory of probability, we arrive at the following system of axioms:

1) Φ is some field of sets.
2) Φ includes the set E.
3) Each set A of field Φ is assigned some non-negative number $P(A)$ called its probability.
4) $P(E)$ is equal to unity.
5) If sets A and B of field Φ do not intersect, then the probability of their sum is equal to the sum of their probabilities.[37]

The subsequent development of the theory proves that the elementary theory of probability can indeed be completely constructed on the basis of these five axioms. It goes without saying that, by means of a reverse transition from the abstract scheme to concrete reality, all the conclusions of this theory can be interpreted without any hindrance in terms of that frequentist picture, which served as our point of departure for constructing our axiomatics. It is also obvious that the problem of formalization posed by us can be solved in more than one way, that other systems of axioms satisfying the stated demands are also possible.

Let us now return to the position held by Mises' theory. One might suppose that, since this theory does not recognize the theory of probability to be a mathematical science, its method of founding the theory cannot at all rival one or another axiomatic formalization. Indeed, Mises aims at quite another goal. It seems that he never established any system of axioms; the basic theoretical propositions formulated by him cannot by any means be called axioms in the mathematical acceptance of this term.

The presence of such notions as trial, observation, phenomenon of nature in the initial propositions of his theory is not a result of carelessness or of an insufficiently distinct scientific language. On the contrary, Mises is undoubtedly apt to insist on preserving those terms. For him, they are a pledge of that indissoluble connection

[37] It is remarkable that the notion of "sum" is not specified here, especially with respect to the need to include denumerably infinite sums of events, as usual in Borelian probability fields. Maybe this was another compromise with respect to the readership of a philosophical journal. Another interpretation (which was suggested to me by B. Bru – Paris) is that Khinchin is only interested in showing that the axioms of set theory can be related to properties of frequencies where infinite sums do not originally occur.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

with the actual reality that constitutes the main pathos of his doctrine. But if this is so, may we not say that the frequentist theory and the axiomatic theory can by no means be in a relationship of competition, that their opposition must be the result of a misunderstanding, and that it is necessary to axiomatize the theory of probability, thus leaving to the frequentist theory the task of describing only the connections taking place between the mathematical theory of probability and those real phenomena to which it is applicable?[38]

The frequentist theory itself is not at all inclined to restrict its aims to that modest task that we have just suggested. Following Mises, it is called the new theory of probability. It wants to compete on equal footing with any axiomatic foundation of the theory. Of course, it can secure this right to compete only by completely formalizing itself, by banishing every mention of observations and trials from its propositions and by appearing in the role of a pure axiomatics. And it actually is embarking on this path at the hands of a number of Mises' followers (Dörge, Tornier, and others),[39] whose outlook, by the way, he himself does not always share. The problem consists in that, to render possible such a full formalization, the frequentist theory will apparently be obliged to abandon the initial simplicity of its basic propositions and replace them by other, much more complicated principles.[40] Although Mises' followers willingly agree with this, he himself is not yet inclined to such concessions.

What does he say about all this? It should be noted that it is sometimes difficult to understand this thinker who can be clear to the utmost in all concrete details, but fails to be so clear when it comes to general principles. Mises never formulated his opinion on the problem that interests us now, never offered a complete formalization of his theory. In addition, it is evident that this simply was not his intention. At the same time, however, he fights for his theory as though it were formalized, and when he believes it necessary, he fearlessly opposes it to other formal theories. By the way, the path along which a complete formalization of Mises' theory could have been attempted is fairly obvious. Thus we hardly run the risk of being accused by its author of misunderstanding his ideas, when we, in fact, not only attempt to describe that path, but also discuss it critically.

We must try to solve the two main problems that appear here: Is it possible to formalize the frequentist theory? And, if the answer is positive, will the thus constructed

[38] In a similar, but more positive, non-discriminatory sense argues Kolmogorov (1933, 3), leaving von Mises' theory the "relation to experience."

[39] See Dörge, K. 1930. "Zu der von R. von Mises gegebenen Begründung der Wahrscheinlichkeitsrechnung. Erste Mitteilung: Theorie des Glücksspiels." *Mathematische Zeitschrift* 32:232–258, and Tornier, E. 1936. *Wahrscheinlichkeitsrechnung und allgemeine Integrationstheorie*. Leipzig and Berlin: Teubner.

[40] A review by Khinchin of Tornier (1936) ends with the following words: "It is not understandable why a method was chosen, which uses much more mathematical formalism, and still leads to an identification with measure theory, although a rather restricted and less promising one." See Khinchin, A. Ya 1936. "Review of E. Tornier: Wahrscheinlichkeitsrechnung und allgemeine Integrationstheorie." *Zentralblatt für Mathematik und ihre Grenzgebiete* 13:359.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

formal theory offer any advantages as compared with the one that was described in this paragraph?

4. Is the Formalization of the Frequentist Theory Possible?

In order to focus all our attention on the main difficulties of the problem now facing us, we limit our investigation to the simplest probabilistic scheme, i.e., to the so-called simple alternative where a trial admits of only two outcomes. We denote these outcomes by 0 and 1 respectively.

Formalizing this pattern in terms of the axiomatics explicated in the previous paragraph is extremely simple. The field Φ consists here of only four events:[41] E (either 0 or 1); 0; 1; and M (both 0 and 1). Their probabilities are $P(E) = 1$; $P(0)$; $P(1)$; and $P(M) = 0$. All the axioms are evidently fulfilled.

The formalization of this situation in terms of the frequentist theory is much more difficult. This is not surprising because the theory deliberately does not agree with such a high degree of abstraction as provided by axiomatics. It wants to preserve more features of the actual reality, so that its schemes ought naturally to be more complicated.

In the frequentist theory, a formalized pattern is not abstracted from the image of a long series of trials, but preserves that image within itself in an idealized form. The idealization consists here, first, in the fact that this series, having been finite, becomes infinite. Accordingly, the formal scheme of the frequentist theory in our simple case looks like this. Given an infinite sequence, called collective, whose elements are symbols 0 and 1: it has to possess two properties:

1) *The existence of limits*. Denote by $\varphi(n)$ the number of zeros among the first n elements of the collective. Then, as n goes to infinity (∞), the fraction $\varphi(n)/n$ tends to a definite limit $P(0)$ of event 0, which is to be called probability of the event 0. This requirement of course involves the existence of a similarly defined probability $P(1)$ of event 1.
2) *Irregularity*. Let us select, in accordance with an arbitrary law, any subsequence out of the given collective and denote by $\varphi(n)$ the number of zeros among the first n terms of this subsequence. Then $\lim \varphi(n)/n = P(0)$ as n goes to ∞. The mentioned principle for selecting the subsequence can be absolutely arbitrary, if only the choice of each of its elements is independent of whether it be 0 or 1.

In providing examples of such selections, Mises usually indicates purely arithmetical principles, i.e., those for which the choice for an element to belong to the subsequence is completely determined by its number [position] in this subsequence (select all the

[41] [In fact, the field of events Φ has to include events "0" and "1" but also E = "either 0 or 1" and M = "both 0 and 1." (B.G.)]

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

even numbers or all the absolutely[42] prime numbers, etc.). It is obvious that any other method of choice is here banned because the elements of the collective differ one from another only in their number within the series, and in their values (0 or 1), the latter not being allowed to be made use of. In order to avoid complication we shall not dwell here on such rules of choice as, e.g., "each element preceded by a zero . . . ", which Mises nevertheless had to admit during the latest stage of the development of his theory.

Thus, we have here a formal scheme free from any concrete fittings such as "trials" or "observations." However, before we can compare it with the scheme offered by the axiomatic theory, we must consider the problem of its intrinsic consistency. This will bring us, at once, into the region of the deepest and sharpest points of the modern debates on the foundations of mathematics; therefore, it demands a thorough analysis.

It is clear above all that the first property of the collective, the existence of limits, cannot in itself be doubted; sequences consisting of symbols 0 and 1, where the relative frequency of the zeros (and, consequently, of the unities) tends to some definite limit, are the most usual objects of mathematical research, and any number of them can be constructed.[43] The difficulty, if it exists, should therefore be contained either in the second property of the collective or in jointly postulating both properties.

And, indeed, here we at once encounter considerable difficulties. Every mathematician knows well enough the impediments connected with the idea of "arbitrary" law of selection that plays a fundamental part in formulating the property of irregularity. No matter what kind of a collective is given, its zeros form one of its subsequences which we shall denote by φ. The property of irregularity evidently includes in itself, in particular, the demand that no admissible rule of selection will lead to φ being the selected subsequence. But, since any law is acceptable, the sequence φ must be "lawless"; the zeros in our collective should be arranged in a manner that will not admit of any, even a very involved arithmetical description; only in this case can our sequence be recognized as a collective.

Hence it follows that it is impossible on principle (as Mises admits) to construct an individual collective. Indeed, since we are evidently unable to indicate for each element of the collective whether it is zero or unity (the infinity of the set of elements prevents this to be done), we can only define an individual collective by means of a rule establishing the mutual arrangement of the zeros and the unities. This, however,

[42] The example of "absolutely prime" as opposed to just "prime" numbers which looks a bit strange and superfluous here may have been motivated by the need to emphasize that the selection is absolutely independent of the outcome of the trial and shall not be weakened by the condition that two outcomes have only "relatively prime" positions in the sequence.

[43] Note that cautious reference to constructibility which points to Khinchin's interest in Brouwer's intuitionism. See below.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

would have led to determining the sequence φ by means of a law; which is, as shown above, ruled out by the demand of "irregularity."

There are mathematicians who, merely because of this fact, consider the notion of collective as untenable.[44] Yet, since their point of view is not generally accepted, we must nevertheless go further on, attentively keeping in mind the fact just established.

So, if we are unable, and shall on principle never be able, to indicate any individual collective, let us ask ourselves: what sense can the notion of collective have? and can it become a subject of mathematical investigation? While discussing this problem, Mises indicates a concept introduced during the last years into mathematics by the so-called intuitionist school, the concept of "the sequence of free choice."[45] Indeed, there is a close connection with the sequences studied by the theory of probability. Concerning our case, the intuitionist school regards the sequence of empty places, each arbitrarily occupied by a zero or a unity, as the sequence of free choice. The act of filling up the empty space is considered an engendering (a making) of continuum. That the sequence of free choice can, and must, be a subject matter of mathematical research[46] is proved to a sufficient degree already by the existence of the theory of binary fractions and of the theory of probability.

Let us see now where we shall find ourselves when we understand a collective as a sequence of free choice. Above all we have to be aware that a sequence of free choice really satisfies the demand of irregularity.

To return, however, to the first demand, i.e., to the existence of limits: For anyone who had to deal with the concept "sequences of free choice," this demand now rings extremely strange. It is clear, to begin with, that there exists only one sequence of free choice. What sense, then, can there be in separating such sequences into those having, and those not having, limits?

Moreover, the very notion of limit in its usual understanding is applicable only to an individual sequence determined by some regularity. If there are no such regularities,

[44] This example is mentioned in (Mises 1936, 113), without referring to specific names of mathematicians. Martin-Löf (1969, 27), discussing a slightly different counterexample concerning the irregularity axiom alone, finds that example "very insensitive to von Mises's intentions."

[45] In the English version of von Mises' book on positivism, on page 129, appears the notion as "sequences of free formation." Mises, R. von 1951. *Positivism, a Study in Human Understanding*. Cambridge, Mass., essentially a translation of the German original from 1939 (The Hague). The concept is mentioned in (Mises 1936, 112), as *Folgen freien Werdens*. Neither in his original probability book (1928) nor in its third edition of 1957 does Mises refer to Brouwer's "sequences of free formation," which were apparently invoked by him as a temporary defense strategy against criticism of his collectives in the 1930s.

[46] Note that Khinchin in the 1920s praised Brouwer's intuitionism as revolutionary and as "mathematical bolshevism" and that Kolmogorov developed an intuitionistic "calculus of assignments." Possible connections between the intuitionistic interests of Khinchin's and Kolmogorov's and their adherence to the Moscow school of "descriptive function theory" (N. N. Luzin), which had close relations to E. Borel's semi-intuitionism, would be interesting to investigate. See Khinchin 1926. "The ideas of intuitionism in the struggle for content in modern mathematics." (Russian) *Vestnik kommunisticheskoy akademii* 16:184–192, and Kolmogorov (Kolmogoroff) 1932. "Zur Deutung der intuitionistischen Logik." *Mathematische Zeitschrift* 35:58–65.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

and if they do not exist on principle, the question about the existence or non-existence of limit may not even be posed.

What does Mises say in this connection? He admits that if a given sequence is irregular, the question about the existence or non-existence of limit cannot actually be answered in any concrete case. He adds, however, that this does not yet mean that the existence of limits is a demand contradicting the postulate of irregularity. We may try to prove the mutual consistency of these two demands and if our attempt succeeds, the definition of the collective will become mathematically appropriate, since, in mathematics, any object whose definition yields no formal contradiction is recognized as existing.[47]

We do not know whether anyone will be able to prove the mutual consistency of those demands that constitute the definition of the collective. This, however, is not the point at all; the point, rather, is that in their substance they are not applicable to the same concept. If we understand a collective as an individually (and therefore regularly) defined sequence, then, as we see, it cannot satisfy the demand of irregularity; if, however, a collective is an irregular (and therefore "lawless") sequence, then the notion of the existence of limit is not applicable to it. Note that the concepts of non-applicability and inconsistency are completely different. If some demand contradicts a given notion, then exactly the opposite statement is true with regard to this notion. Thus, the demand that a sine of a given angle be greater than unity contradicts the definition of the concept of sine. It follows that the sine is either less than, or equal to unity. If, however, we shall demand that the sine of a given angle should weigh more than one gram, then it will be impossible to say that this condition contradicts the notion of sine; the demand is simply inapplicable to it. It is thus natural that, assuming that "the sine of an angle weighs more than a gram," we cannot encounter any contradiction.

We are absolutely sure that the disjunction between the existence and non-existence of a limit is inapplicable to the concept of irregular sequence in the same sense as the disjunction between heavy and light cannot be applied to the notion of sine. We think that it is evident that the application of that disjunction is based here on an illusion: in actual fact, mathematicians always have to do with regularly defined sequences and apply that disjunction rightfully. Never encountering other cases, they unwittingly accustom themselves to the rule, according to which that disjunction is applicable to any sequence; and thus, having met with an irregular sequence, they suppose that the appropriate limit either exists or does not exist without noticing that they behave like someone who intends to weigh sines.

[47] Von Mises' position with respect to Hilbert's definition of existence was much more nuanced than here presented by Khinchin, and it changed over time. See his original position (Mises 1919, 59–60), which is still close to Hilbert's notion, but in (Mises 1936, 111), under the influence of criticism, von Mises uses arguments from Brouwer's neo-intuitionism.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

Thus, proving the mutual consistency of the two demands made on the collective is not the point at all; the point is, to show what it can mean that the limit exists with respect to irregular sequences. The entire previous history of our science does not provide us with a slightest pertinent indication, and, naturally enough, the revealing of the meaning of the appropriate demand is the duty of the person who first advances it.

We may understand an irregular sequence only as an uncompleted object, as an object in the making. We shall not be able to connect it with any other concept and the demand concerning the existence of limits may be applicable only with respect to completed, to utterly fixed sequences. It follows inevitably that the frequentist theory, at least in that shape in which Mises himself insists, cannot at all be satisfactorily formalized. In any case, we do not now have a satisfactory formalization.

5. Is the Formalization of the Frequentist Theory Expedient?

While Mises himself did not give up hope of fully including both main postulates of his theory within a common formal system, his followers have apparently already realized the despair of such attempts at present. At the very least, some partisans are prepared to formalize the frequentist theory at the cost of abandoning one or another of its requirements, and many endeavors have been already made in this direction, viz., replacing infinite collectives by finite ones, completely abandoning the demand of irregularity (Kamke),[48] partially giving up this condition, i.e., requiring the invariance of the limits with respect not to any choice of the subsequence, but only to some previously restricted set of such selections (Dörge, Tornier, Copeland and others).[49] Concerning most of these attempts, it is necessary to note that Mises himself adheres to an uncompromisingly negative position. In particular, he believes that, once the postulate of irregularity is even partially abandoned, the formal scheme by necessity acquires features sharply contradicting reality and that this approach cannot therefore be admitted.[50]

From a formal point of view, hardly any of these endeavors of founding the frequentist theory is objectionable. More: after modifying Mises' main postulates in

[48] Cf. Kamke, E. 1932. *Einführung in die Wahrscheinlichkeitstheorie*. Leipzig: Hirzel. This is partly commented upon in (Hochkirchen 1999, 209–10). Von Mises called Kamke's standpoint to renounce irregularity a "curious standpoint" (*sonderbarer Standpunkt*) (Mises 1936, 116).

[49] Cf. Dörge (1930), Tornier (1936), and Copeland, A. H. 1936. "Point set theory applied to the random selection of the digits of an admissible number." *American Journal of Mathematics* 58:181–192.

[50] It is interesting to note that von Mises welcomed measure-theoretic arguments if they supported his theory. Mises stressed that in Copeland's model (1936) of a special collective the limit of relative frequency remained fixed for "almost all" subsequences after place selection. Cf. (Mises 1936, 118) and (Mises 1957, 92). Von Mises' was not fully satisfied with Copeland's "admissible numbers" (Martin-Löf 1969, 24). His willingness to compromise, however, contradicts somewhat Khinchin's remark.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

one or another manner, this theory can undoubtedly be irreproachably formalized. As such, it may be admitted as a rival of the axiomatic theory, and we ought to perceive the outcome of this competition.

From the formal point of view, the mutual relations between the axiomatic and the frequentist theories are characterized above all by the former's higher degree of abstraction. Indeed, the foundation of the frequentist theory (in the simplest case) consists of number sequences, i.e., collectives. To each event corresponds a definite sequence of this collective, having a definite limit of the relative frequency, and this limit is called the probability of the event. Event E is defined as such to whom the entire collective corresponds as the chosen subsequence; the equality $P(E) = 1$ is proved as a theorem. The relation $P(A + B) = P(A) + P(B)$ for incompatible events A and B is another theorem. The axiomatic theory abstracts itself from the number sequences which, in the frequentist theory, define the events and their probabilities. It retains only a small number of properties of these concepts necessary for the further development of the theory. For this theory, events are merely elements of some field; their probabilities are simply some numbers made to correspond to these events and to satisfy a small number of simple demands stated in the axioms. The axiomatic theory results by way of abstraction from all the rest that is included in the frequentist theory.

Closely linked with this distinguishing feature is the fact that the theory of probability can be developed on the basis of the axiomatic theory in an incomparably simpler and easier way than when it is founded on the frequentist theory. The latter requires, at least initially, that the unwieldy concept of collective be permanently borne in mind. Especially when the restricting postulate of irregularity is introduced, this is indeed a construction of great formal complexity. And it is natural that in the formal respect the more abstract theory is always simpler.

When two formal theories are in competition, one of them being an abstraction of a higher degree than the other, then we ought to consider the following points in order to determine which one is to be preferred: 1) The more abstract theory is always simpler in the formal respect. 2) Contrarywise, the more concrete (i.e., the less abstract) theory is always richer in features which draw it nearer to reality, and it is therefore able to nourish creative intuition to a larger extent. In each given case the problem is: which of the two advantages might play a greater part?

We may consider it established by the entire modern development of the theory of probability that the high degree of abstraction peculiar to the founding axiomatic system never actually led to emasculating the richness or the diversity of creative intuition. Historically speaking, the comparatively poor equipment of this system with concrete features, if considered a shortcoming, was in any case negligible as weighed against those benefits that are provided by its greater formal simplicity. This is strikingly testified already by the fact that the entire many-sided building of the modern theory of probability was created by people thinking "axiomatically" rather than in the frequentist way; suffice it to mention M. Fréchet and P. Lévy; F. Cantelli and B. Finetti; H. Cramér,

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

S. N. Bernshteyn, A. N. Kolmogorov, and E. E. Slutskiy.[51] But what indeed did Mises' followers, who were brought up on the frequentist theory, achieve? Can they boast of at least one considerable finding, or at least one essential discovery enriching the theory of probability?

While discussing the scientific value of the act of abstraction, Mises adduces a vivid and apt example in one passage of his popularizing book *Probability and Statistics*.[52] He notes that geometry, while studying the forms of real space by means of an abstract method, abstracts in particular from the width and the thickness of those objects that we are inclined to call straight lines. Then he recounts that attempts were made to construct a geometry that would have treated stripes of small but differing-from-zero width, instead of straight lines.[53] This lowered the degree of abstraction and, obviously, made the closeness to reality greater. However, these attempts came to nothing and were abandoned. The formal complexity necessary for their realization was not compensated by approximating to reality. And, strictly speaking, there was no need for such an approximation. Experience shows that our mind has excellently learned how to work with the concept of a straight line having no width, and to originate for itself, on this basis, appropriate visual ideas necessary for the functioning of creative intuition.

We consider it obvious that the situation in our case is absolutely similar, and that all attempts at a formalization of the "frequentist" type will sooner or later be abandoned, just as the geometrical theories working with "wide" straight lines were, and for the same reason: They introduce an incomparably greater formal complexity without offering in exchange any practical benefits except for a simple, and so to speak, unselfish satisfaction from sensing that the formal scheme has come closer to actual reality.

When moving to a higher degree of abstraction, we narrow down, in a formal logical way, the content of the concepts in our theory, depriving them of some of their properties. According to the laws of formal logic, this should be accompanied by a certain widening of the extension of these concepts by making possible the inclusion of

[51] [Khinchin himself undoubtedly should also be named here. (B.G.)].

[52] Khinchin refers here to (Mises 1928) in its Russian translation of 1930, which he himself organized. This translation indeed omits the word "Truth" in the title. Obviously the publishers were wary of unwanted propaganda for the philosophical content of von Mises' book. Cf. Mises, R. von 1930. *Probability and Statistics.* (Russian translation of Mises 1928, "under the redaction of Prof. A. Ya. Khinchin"). Moscow, Leningrad: State Publishers (Gosizdat). See the cover illustration and the introduction by R. Siegmund-Schultze.

[53] Von Mises mentions that example several times and discusses it in more detail in "Diskussion über Wahrscheinlichkeit," *Erkenntnis* 1 (1930/31), 260–285. p. 279. He thereby alludes to Felix Klein (1849–1925), who had considered such a kind of geometry to be a part of *Approximationsmathematik*, as opposed to *Präzisionsmathematik*. In that *Diskussion* which took place in Prague in 1929, Mises stated that the geometry mentioned was not (!) very successful. He went on to say that his theory of probability introduced infinite sequence as an idealization (i.e. as a kind of *Präzisionsmathematik*) because "for practical purposes" it was useful to have such abstractions. It appears rather strange and careless that Khinchin/Gnedenko would turn of all examples exactly this one against Mises' probability theory.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

additional objects into our new and more abstract formal scheme. Practically speaking, in our case it means that, when passing from the frequentist to the more abstract axiomatic formalization, we may envisage such new situations whose interpretation in terms of the "collectives" was altogether impossible. That this is really true cannot be doubted at all.

One of the very simple patterns of the axiomatic theory is this. Let the events be all the Lebesgue-measurable sets of the interval [0, 1]. The probability of each event is then the measure of the corresponding set. This is a well-known interpretation of the problem concerning a mass point thrown "at random" on some unit interval. It is easy to see that the frequentist theory has no place for such a scheme. Indeed, here, the collective should have been the sequence of the abscissas of the fallen point when the series of the throws is unbounded. For any of these abscissas, their totality M is a countable set, its measure is therefore zero and, in agreement with the conditions of the scheme, the probability of the point falling on set M should also be zero. At the same time, however, all the elements of our collective belong to set M so that the probability sought, calculated according to the rules of the frequentist theory, should be equal to unity. The contradiction encountered shows that this pattern of the axiomatic theory cannot indeed be realized under the frequentist system as a basis of probability theory. The representatives of the frequentist theory infer, however, that the distribution described above cannot on principle be experimentally revealed since a statistical experiment amounts only to an empirical determination of frequencies in repeated trials. In accord with their Machist philosophical outlook, they regard their statement as equivalent to proving that such a distribution cannot exist in reality.

Thus the partisans of the frequentist theory claim that the axiomatic system of basing the theory of probability necessarily leads to considering formal schemes that have no prototypes in the real world studied by our science. They wish to regard the described fact as an inherent defect of the axiomatic theory. Several centuries ago the forefathers of those knights of "realism" have been battling against the introduction of negative, and later on, of imaginary numbers. They believed that these had no real prototypes in the actual world. Nowadays these battles seem excusable since we take into account the insufficiently clear understanding, in that early period, of the nature and the methods of mathematics. What should be said, however, about a mathematician of today who would insist on the need of constructing the algebra of polynomials, or the theory of analytic functions in the real, rather than in the complex field since no real magnitudes can correspond to complex numbers? Meanwhile, however, what is the attitude of Tornier, that champion of the frequentist theory? Striving to "avoid the degeneration of animated essential concepts into lifeless mathematical formalism,"[54] he forbids the

[54] This is quoted from (Tornier 1936, iii), a book which received a critical review by Khinchin (1936), as seen above. It has to be mentioned that Tornier was a proponent of the intrusion of Nazi-ideology in mathematical research in the manner of the racist pseudo-doctrine *Deutsche Mathematik* and that Khinchin was certainly aware of this. Cf. also Hochkirchen, Th. 1998. "Wahrscheinlichkeitsrechnung im Spannungsfeld von Mass- und

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

usage of schemes that are not confined to the frequentist interpretation in the theory of probability. For the sake of attaining his goal he constructs a formal and incomparably bulkier structure and is obliged to give up not only the solution, but also the posing of a number of problems, quite elementary from the viewpoint of the wider axiomatic theory. The Lebesgue definition of measure is of course ousted, being replaced by Jordan's unwieldy and stiff definition.[55]

6. The Frequentist Theory and Scientific Forecasts

As shown in detail in §1, Mises believes that one of the main flaws of the classical concept of probability theory is its incapacity to forecast something about the real course of events without introducing new special assumptions. We have recognized this critical remark as absolutely true. In contradistinction, he maintains that the conclusions reached by the frequentist theory contain explicit forecasts about how the real world will be proceeding under certain conditions. We ought to examine now to what extent he is correct.

It is easy to convince ourselves above all that the propositions of the frequentist theory do not on principle admit of experiential checks without the introduction of some new particular assumptions.[56] This is due to the special kind of idealization to which that theory resorts. Consider, indeed, the statement that the probability of throwing a six with a given die is equal to $\frac{1}{8}$. According to the idealized interpretation, this means that the relative frequency of this outcome tends to $\frac{1}{8}$ as long as the experiment is continued infinitely. Turning now from the ideal to the real situation, we ought to understand this in the following way: no matter how small a positive number δ is, the relative frequency of the occurrence of a six when the number of throws is sufficiently large, will be contained between $\frac{1}{8} - \delta$ and $\frac{1}{8} + \delta$.

Assume now that, wishing to check this proposition by experience, we made a very large number of throws and that the relative frequency of "6" occurred to be equal to $\frac{1}{4}$. Issuing from the principles of the frequentist theory, may we now admit, with any justification, that our proposition was wrong? We believe that we have no cause at all for such an inference. The established experiential fact (the relative frequency amounts

Häufigkeitstheorie – Leben und Werk des 'Deutschen' Mathematikers Erhard Tornier (1894–1982)." *NTM-Schriftenreihe* (N.S.) 6:22–41.

[55] Camille Jordan's (1838–1922) notion of measure and integral was replaced around 1900 by the more modern ones by É. Borel (1871–1956) and H. Lebesgue (1875–1941).

[56] Von Mises was, in fact, aware of the problems connected to infinite collectives and the availability of only finite sections in practice. In (Mises 1936, 107) he discussed this problem of application for instance with respect to an article by Hempel, C. G. 1935. "Über den Gehalt von Wahrscheinlichkeitsaussagen." *Erkenntnis* 5:228–260.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

to $\frac{1}{4}$) is evidently quite compatible both with the frequency tending, or not tending to $\frac{1}{8}$ as the number of throws increases.[57]

To this argument, which is often put forward, Mises usually objects by the remark that we have to do with the same situation in any physical theory since an experiment always provides only an approximate value of the examined magnitude.[58] Let us see to what extent his opinion is correct. Suppose that our theoretical calculation shows us that the specific weight of some substance must be 1.5, whereas an experiment that uses a device yielding specific weights with a precision of 0.01, furnishes a specific weight of 1.57. It follows that the theoretical conclusion is refuted as incompatible with the experiment. But had the specific weight measured in the experiment been 1.497, we would have been quite justified in assuming that the theoretical conclusion was corroborated, in the sense, of course, that it differed from the truth not more than by some small magnitude known beforehand.

There is nothing similar in the applications of the frequentist theory. No matter how many times we throw our die (i.e., no matter how much we increase the "precision" of our experiment), the result will always remain compatible with any assumption concerning the probability of a six, and no principle of the frequentist theory will tell us which of the possible values of this probability is preferable. Mises likes to compare a repeated statistical trial with a physical experiment; we see that the former is carried out by means of an instrument whose precision is not only unknown, but cannot on principle be determined. So how can the indications of such a device have any meaning when corroborating theoretical conclusions? And may we seriously declare that, these circumstances being given, the situation is here not worse than in any physical theory?

If the frequentist theory itself does not admit of experimental checking of its inferences, is it nevertheless able, as Mises maintains, to provide some forecasts about the actual course of phenomena? The law of large numbers is known to be the cornerstone of practically formulated probabilistic propositions on the proceeding of mass phenomena. Mises quite correctly indicates that, assuming the classical notion of probability, this law is never able without additional assumptions to state anything about the actual course of a given phenomenon. He believes, however, that in the context of the frequentist theory the law of large numbers characterizes the real course of events. Let us see whether this is really so.

Suppose, for example, that the probability that an insured person, belonging to a given category, dies during a certain period of time, is 0.016. According to the law

[57] [In fact, in the process of changing, a variable may, prior to approaching sufficiently a limit (in case it exists), assume values arbitrarily distant from the limit. Therefore, from the fact that the frequency is close to 1/4 after a finite number of trials, it does not follow at all that it converges to 1/4, if the number of trials goes to infinity. That limit can be arbitrary. (B.G.)].

This last footnote by Gnedenko, which uses the fundamental but rather trivial notion of limit in mathematical analysis, is quite obviously intended for non-mathematicians.

[58] Khinchin is obviously referring to Mises (1936, 106), where he is, in fact, discussing measurements of specific weights and compares them with conclusions in statistics. Gnedenko uses exactly the same example from physics in order to criticize von Mises' theory. Cf. Gnedenko, B. V. 1962. *The Theory of Probability*. New York: Chelsea, p. 51.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

of large numbers it follows that, with probability p rather close to unity, from among 10,000 such insured persons the number of deaths will be close to 160. Following Mises' incessant appeals, let us understand probability in its scientific (in the sense of the frequentist theory) rather than in its everyday meaning and let us ask ourselves what we can infer about the fate of the given real totality of 10,000 persons when issuing from the law of large numbers and its conclusion. In the sense of the frequentist theory, the answer given above by this law literally means that "If an experiment with the fate of the 10,000 persons is repeated infinitely, the relative frequency of those cases, where the number of deaths is contained between 155 and 165, will have the given number p as its limit." But what relevance does this have to our question? Indeed, we do not intend to repeat the insurance of 10,000 persons a great number of times. We are only interested in the fate of the given totality. What can the frequentist theory tell us about it? Strictly nothing. It has made its conclusion formulated above and it is unable to say a single word more.[59] How does Mises object to this argument? He declares, again and again, that all this is of course true, but that the situation is exactly the same in any physical theory as well.

We do not at all believe it compulsory for a mathematical or a natural-scientific theory to contain in itself all the principles of its practical application. On the contrary, these are usually formulated beyond the given theory. In particular, neither the classical concept, nor the modern axiomatic foundation of probability theory taken in themselves, are capable without additional assumptions to state anything about the real course of events. Therefore the frequentist theory cannot be blamed for being incapable of such prediction; in this respect it fares in no way better or worse than the other methods of founding the theory of probability. The point is, however, that Mises asserts the contrary and believes that one of the theory's main advantages lies exactly in its capability of making such predictions. We have shown that this claim is entirely based on a misunderstanding.

In actual fact, whichever system of founding probability we adopt – the classical, the frequentist, or the axiomatic –, the situation will remain invariably the same. In order to connect the theory with practice, additional assumptions are needed. For example, if the probability of an event is very low, you can be sure that, practically speaking, it will not happen. No matter how simple and clear this principle is, it cannot at all be derived by issuing from the theory of probability itself.

7. Conclusions

1) It is necessary to admit that Mises' most important historical merit is his systematic criticism of the "classical" foundation of the theory of probability revealing its most significant defects: the vicious circle in its definition of probability, the

[59] Mises (1936, 105), says: "I must defend myself most emphatically against the recurring misapprehension that in our theory infinite sequences are substituted for all finite sequences of observations."

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

extremely restricted applicability of this definition, and the unsubstantiated claims of the classical theory to a direct practical applicability of its conclusions.

2) Mises' second merit is the systematic and persistent indication that the theory of probability is a doctrine of mass phenomena and that, consequently, it is necessary to derive its main notions and propositions by abstracting[60] it from real statistical populations or repetitive processes rather than to determine them by issuing from the properties of single objects.
3) Mises recognizes the theory of probability not as a mathematical, but as a natural-scientific discipline making use of mathematical methods. This is the reason why, above all, he never and nowhere carries out a complete formalization of his theory; he does not fashion it as a purely axiomatic structure restricting his efforts with idealizing real processes to a certain degree and leaving intact such notions as "trials," "observations," "process," etc., when formulating his main propositions.
4) In contradistinction, the approach that recognizes the theory of probability as a mathematical science believes that its complete formalization is necessary and that a system of its axioms should be constructed just as it is done in geometry, in algebra and in other mathematical sciences.
5) The foundation of the theory of probability offered by the frequentist theory is not therefore a logical basis in the sense of modern mathematics. Nevertheless, the frequentist theory claims to be a rival of the purely formal foundational schemes. In order to determine whether this claim is justified, it is necessary, in the first place, to examine the possibility of a complete formalization of the frequentist theory.
6) Within a formalized pattern, the main concept of the frequentist theory, the collective, is represented by a number sequence satisfying two main requirements: the existence of the limits of relative frequencies of the attributes, and the so-called irregularity. The possibility of completely formalizing the frequentist theory under both these demands appears at least dubious. With respect to those notions, which modern mathematics connects with the concept of an irregular sequence,[61] the existence of limits becomes meaningless.
7) These considerations forced Mises' followers to drop this or that element of the two requirements. Most often we encounter a restriction concerning irregularity. Given this relaxation, a complete formalization of the frequentist theory is possible without difficulties.
8) It is quite possible to compare the formalized frequentist theory with the axiomatic theory, which considers the doctrine of probability a part of the metric theory of sets and functions. Such a comparison shows that, with respect to

[60] [The modern theory of probability does not abandon altogether the classical approach to defining probability which issues from the principle of symmetry. On this more in the Supplement (B.G.)].

[61] This remark, if maybe justified for the time when the article was written in the 1930s, appears strange given the renaissance of von Mises' notion of randomness around 1960. At least some commentary on the part of Gnedenko would have been appropriate here.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

the latter, the former possesses a lower degree of abstraction. Consequently, an inherent feature of the frequentist theory is its incomparably greater complexity, not at all compensated by its being more concrete and therefore closer to actual reality. The axiomatic theory therefore deserves unquestionable preference, whereas the formalization of the frequentist theory cannot be recognized as expedient.

9) Some partisans of the frequentist theory (Tornier) advocate the denial of the more abstract foundations under the pretext that they lead to schemes that cannot on principle be realized in actual reality. This approach contradicts the very spirit of modern mathematical science and ought to be considered as an obscurantist[62] survival of the remote past.

10) The frequentist theory maintains that, among the existing systems of foundation, it alone enables the theory of probability to express statements directly depicting the real course of events. An analysis shows, however, that this claim is based on an illusion and cannot therefore be recognized as justified. In actual fact, the frequentist scheme as a basis for probability theory is no more capable than any other one of attaching to the theory of probability the role of even approximate direct forecasting of the real course of some phenomena. Each system of foundation requires additional assumptions to the same extent, and in this respect the frequentist theory offers no advantage and does not present any exception.

Supplement. The Idea of Equipossibility: Its Importance and Perspectives.[63]

The logical defects of the idea of equipossibility that prevent it from becoming the formal basis of the theory of probability should not, however, give occasion for

[62] This very wording "obscurantist" is at the same time an allusion to the Nazi-ideology involved in Tornier's arguments as mentioned above (see Hochkirchen 1998).

[63] This supplement is obviously influenced by Khinchin's turn toward statistical mechanics in the second half of the 1930s and in particular by the so-called "theory of objective probability in classical systems" and the "method of arbitrary functions" in the tradition of Henri Poincaré and M. Smoluchowski. In the 1930s, this theory, which represented one branch of ergodic theory, was particularly pursued by the German Eberhard Hopf (1902–1983). See Hopf, E. 1936. "Über die Bedeutung der willkürlichen Funktionen für die Wahrscheinlichkeitstheorie." *Jahresbericht Deutsche Mathematiker Vereinigung* 46, II. Abt.: 179–195. Von Mises reflected rarely on this kind of investigation which aimed at explaining statistical regularities, independence of events and other problems which the von Mises theory largely took as lying outside the proper theory of probability. See Engel (1992) for a modern revival, and Plato (1983) for the history. Engel, E. 1992. *A Road to Randomness in Physical Systems*; Lecture Notes in Statistics 71; Berlin etc.: Springer. Plato, J. von (1983). "The Method of Arbitrary Functions." *British Journal for the Philosophy of Science* 34: 37–47. Also Gnedenko was obviously very impressed by this theory which seemed to give support for major tenets of Marxist epistemology, particularly in connection to probability, and enlarged the philosophical explanatory potential of mathematics as a whole. Khinchin gives a similar philosophical interpretation as here in Khinchin, A. 1952. "The Method of Arbitrary Functions and the Struggle against Idealism in the Theory of Probability" (Russian). In *Filosofskiye voprosy sovremennoy fizik*i (Philosophical Problems of Modern Physics), pp. 522–538 (German Translation in *Sowjetwissenschaft. Naturwissenschaftliche Abteilung* 7 (1954), no. 2: 261–273. French translation in *Questions Scientifiques* 5 (1954):7–24).

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

underestimating its extremely great methodological importance either for our everyday practice or for the most subtle problems of modern science. In some (although not in a formal-logical) sense this idea, especially as shown by a number of recent investigations,[64] is able to offer more to the doctrine of mass phenomena than any formal foundational scheme. For the sake of comprehensiveness of the picture we have sketched in our survey, let us briefly consider this problem.

The point is not to move backwards from the frequentist interpretation of the idea of probability adopted by the entire modern science, to its old, metaphysical concept. We shall still understand probability as a number that is capable of offering some notion of the relative frequency of the occurrence of an event under certain, absolutely definite conditions. However, if science is something more than a simple, if very economical description of real processes,[65] we obviously should be interested in how to forecast at an earlier point of time (i.e., before the conclusion of the experiment) the frequencies, or what is on principle the same, the probabilities of some events.

When we want to determine the probability of a "6" for a given die, no other means is open to us, as Mises indicated, except throwing it a sufficient number of times and calculating the appropriate relative frequency. Theoretically, this is correct; practically speaking, however, no gambler will act on this method. He knows that the probability sought is $\frac{1}{6}$. How does he know? Perhaps, Mises says, the gambler had many times experienced similar dice, for whose overwhelming majority the probability occurred to be $\frac{1}{6}$. But the matter is different. The matter is that the gambler's life experience prompts him to this simple conjecture. The six faces of the die have no distinction between themselves of essential importance for the frequency of their occurrence since it is natural to assume that the material of the die is more or less homogeneous. It follows that there is every reason to expect that all the faces will turn up with roughly the same frequency.

Such conjectures — hypotheses based on the idea of symmetry — are encountered at every step, and in each case we forecast the equiprobability of certain events issuing from ideas prior to the given phenomenon. It is self-evident that this assumption may never claim to be more than a working hypothesis demanding an experimental check.

The role of working hypotheses in science is well known. In the sense of a working hypothesis, the idea of equipossibility may be, and actually is, of utmost importance for the theory of mass phenomena. True, this feature by no means expresses itself in the direction in which the classical system of founding the theory of probability wished to apply it. The idea of equiprobability appears not as a formal logical base of the

[64] See e.g. Hopf (1936, 180), which interestingly alludes to the results of the Moscow school of descriptive function theory, referring the result that "measurability is not very different from continuity" to N. N. Luzin the teacher of both Khinchin and Kolmogorov. See also Hopf, E. 1937. *Ergodentheorie*. Berlin: Springer. (*Ergebnisse der Mathematik und ihrer Grenzgebiete* V, 2).

[65] One may think of Mach's *Denkökonomie* here, because von Mises was his adherent.

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

doctrine of mass phenomena, rather it is in single concrete situations our sole method of theoretically forecasting the probabilities of events.

Seemingly, from what was said on the idea of equipossibility in §1, its domain of application is very narrow. And, indeed, on the face of it, it is difficult to imagine such a scheme as going considerably beyond the province of games of chance. Nevertheless, this is not so. Investigations dating back to Poincaré, and especially extended in recent years, show that in a somewhat generalized form this idea is able to cover a very wide class of phenomena, in mechanics and physics in the first place. It is about these investigations that we wish to say a few words now. Imagine a usual roulette game and suppose that, in the ideal case, friction is eliminated, so that the ball moves round with a constant velocity.[66] The ball's location at a definite moment after it is set in motion is therefore uniquely determined by specifying its initial velocity v. We are now asking ourselves: what theoretical considerations may be put forward in favor of the natural hypothesis that, for a sufficiently high v, all the positions of the ball on the roulette's circumference are equally probable?

The probability of the ball's being in some part of the circumference evidently depends only on the comparison between the probabilities of the various values of the initial velocity. Of course, the assumption that these values are equally probable greatly deviates from reality since the motive force will have its traditional "favorite" values. However, this assumption is not needed. Mathematical analysis shows that, for whichever distribution $\Phi(x)$ of the velocity, if only it is continuous, the probabilities of all the positions of the ball become equal as the period of time increases infinitely (ergodicity). This remarkable result, important as a splendid scientific precedent, gains a simple and clear interpretation in terms of the idea of equipossibility. Indeed, what does the demand of continuity of the function $\Phi(x)$ signify in real terms? Its obvious meaning is that the probabilities of close values of the initial velocity should also be close to one another. This has to do with some differential, or, as it is now usually formulated, local equiprobability. It is indeed remarkable that the relative values of the probabilities of the more or less considerably differing initial velocities are here absolutely irrelevant. It is only essential that no exceptional values of the velocity occur, for any reason, with a frequency much larger than the frequencies of their closest neighboring values. It is evident that we have every reason to assume that this demand is fulfilled in reality. We thus obtain a scientific theory capable of explaining how and why all the participants in a roulette game have equal chances. We regard this explanation especially satisfactory exactly because it does not require any special assumptions concerning the appropriate distributions. Local equipossibility is an extremely general demand since it takes place for most various distributions.

[66] According to Krengel (1990, 476), the following result on the roulette based on the "method of arbitrary functions" is due to Hopf. Cf. Krengel, U. 1990. "Wahrscheinlichkeitstheorie." In *Ein Jahrhundert Mathematik 1890–1990. Festschrift zum Jubiläum der DMV*. ed. by G. Fischer et. al., 457–489. Braunschweig: Vieweg. The same example is also discussed in Khinchin (1952).

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press

We have chosen the roulette game as an example for applying the idea of local equipossibility since its mathematical analysis presents no difficulties. Essentially the same method can be made use of in a very large number of problems belonging in the first place to mechanics. In most cases, however, the purely mathematical difficulties still prove insurmountable.

In particular, consider the now classical example of an irregular die. Mises incessantly put it forward as a specimen of problems where the idea of equipossiblity is inapplicable; however, from our generalized viewpoint,[67] it can indeed be on principle interpreted by issuing from this idea. Assume that we know exactly the mass distribution inside the die and imagine that, in the ideal case, it is thrown in such a manner that the face that appears is completely determined by the die's initial position. The frequencies of the various faces will then depend on the frequencies of some values of a small number of parameters determining this initial position. And there is every reason to believe that, assuming also a local equiprobability of these parameters, each face of the die will have a definite probability of turning up depending only on the above-mentioned mass distribution.[68]

So, we see that the notion of equipossible cases, after being upgraded to local equiprobability, is able on principle to bear beautiful fruit for our science. This example shows how many treasures that might enrich modern science are buried in classical ideas. It is only necessary to approach them with modern scientific methods that might provide fertile criticism rather than with servility to age-old traditions.

Translator's Note

Until 1990, when Vladimir A. Uspenskiy (Uspensky), Aleksey L. Semënov, and Aleksandr Kh. Shen published an article tacitly corroborating Khinchin's opinion, it had seemed "impossible to embody Mises' intention in a definition of randomness that was satisfactory from any point of view." The name of their article was "Can an (individual) sequence of zeros and unities be random?" and it was published in *Uspekhi matematischeskikh nauk* (1990) 45:105–162 (see § 1.3.4). The journal is being translated into English as *Russian Mathematical Surveys*.

[67] This means, based on "local equiprobability" instead of equipossibility.

[68] [The ideas incidentally mentioned here by Khinchin were more fully developed in his contribution, "The Method of Arbitrary Functions and the Struggle against Idealism in the Theory of Probability" (Russian) in *Filosofskiye voprosy sovremennoy fiziki* (Philosophical Problems of Modern Physics), 1952, pp. 522–538. (B.G.)].

https://doi.org/10.1017/S0269889704000195 Published online by Cambridge University Press