"A terrible piece of bad metaphysics"? Towards a history of abstraction in nineteenth- and early twentieth-century probability theory, mathematics and logic
Verburgt, L.M.

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CHAPTER 10

The place of probability in Hilbert’s axiomatization of physics, ca. 1900-1926

0. Introduction

It has become a commonplace to refer to the ‘sixth problem’ of David Hilbert’s (1862-1943) famous Paris lecture of 1900 as the central starting point for modern axiomatized probability theory (Hochkirchen, 1999; Schafer & Vovk, 2003; Schafer & Vovk, 2006; Von Plato, 1994, chapter 7). Here, Hilbert proposed to treat ‘by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank [is] the theory of probabilities’ (Hilbert, 1900 [2000], 418). The inclusion of, on the one hand, the axiomatization of physics among the other twenty-two unsolved mathematical problems and, on the other hand, the ‘theory of probabilities’ among the physical theories to be axiomatized, has long puzzled commentators. Although it has been abundantly shown, in recent years, that ‘Hilbert’s [lifelong] interest in physics was an integral part of his mathematical world’ (Corry, 2004, 3) (e.g. Corry, 1997; Corry, 1999) the place of probability within his own project of the axiomatization of physics has received comparatively little explicit attention.

It is the aim of this paper to provide an account of the development of Hilbert’s approach to probability between 1900 – the year in which called for the axiomatization of physics in his Paris address – and 1926 – the year in which he attempted to axiomatize quantum mechanics. On the basis of the extensive primary and secondary literature available, this period can be separated into

1 See, for example, Browder (1976), Corry (1997), Gray (2000a), Gray (2000b), Reid (1996, chapter 10), Wightman (1976) and Yandell (2002) for accounts of the background and influence of Hilbert’s lecture entitled ‘Mathematical Problems’ (Hilbert 1900 [2000]).
four partially overlapping sub-periods: 1900-1905 (section 1), 1910-1914 (section 2), 1915-1923 (section 3) and 1922-1926 (section 4). From the fact that each of these sub-periods corresponds to a specific position vis-à-vis, on the one hand, the foundations of physics and, on the other hand, probability theory follows the paper’s main observation; namely, that there was a fundamental change in Hilbert’s approach to probability in the period 1900-1926 – one which suggests that Hilbert himself eventually came to question the very possibility of achieving the goal of the mathematization of probability in the way described in the famous ‘sixth problem’ (‘Mathematical treatment of the axioms of physics’).

In brief, Hilbert understood probability, firstly, as a mathematizable and axiomatizable branch of physics (1900-1905), secondly, as a vague statistical mathematical tool for the atomistic-inspired reduction of all physical disciplines to mechanics (1910-1914), thirdly, as an unaxiomatizable theory attached to the subjective and anthropomorphic part of the fundamental laws for the electrodynamical reduction of physics (1915-1923) and, fourthly, as a physical concept associated to mechanical quantities that is to be implicitly defined through the axioms for quantum mechanics (1922-1926). Because Hilbert tended not to stress the ‘state of flux, criticism, and improvement’ (Corry, 2004, 332) that his deepest thoughts about physical and mathematical issues were often in and there is, thus, a certain amount of speculation involved in connecting these four sub-periods, what follows is to be considered as one possible way of accounting for his remarkable change of mind in the period 1900-1926.

0.1 Overview of the argument

1 Between the years 1900-1905, Hilbert proposed not only to extend his axiomatic treatment of geometry to the physical theory of probability, but also to let this treatment be accompanied by the further development of its (inverse) applications in mathematical-physical disciplines (Corry, 2006c). On the one

2 The first part begins with Hilbert’s 1900 Paris lecture and ends with the promises for the axiomatization of several physical disciplines in his 1905 lecture course. Following the fifth chapter of Corry (2004), entitled ‘From mechanical to electromagnetic reductionism: 1910-1914’, the second part begins with Hilbert’s lecture courses on mechanics and the kinetic theory and ends with his discussion of theories of matter and electromagnetism. The third part begins with the completion of the ‘Foundations of Physics’ and ends with his lectures of 1921-1923. Obviously, these parts of the period between 1898-1923 are not separated by clean-cut boundaries – as, for instance, Corry (1999b) makes clear.
hand, given that an axiomatization is to be carried out retrospectively, the suggestion that geometry was to serve as a model for the axiomatization of probability implied that Hilbert thought of the theory as a more or less well-established scientific discipline. On the other hand, the fact that the statistical ‘method of mean values’ for the kinetic theory of gases was to be rigorized by means of probability theory’s axiomatization pointed, firstly, to the unsettled of probabilistic methods in physics and, secondly, to the possibility of having the axiomatic method restore it. [2] The years 1910-1914 could be separated into three phases. Firstly, from 1910-1912/1913, Hilbert explicitly elaborated the atomistic hypothesis as a possible ground for a reductionistic mechanical foundation for the whole of physics in the context of several physical topics based on it (Corry, 1997a; Corry, 1998; Corry, 1999d; Corry, 2000; Corry, 2004; Corry, 2006a). It was under the influence of his increasing acknowledgment of the disturbing role of probabilistic methods (e.g. averaging) in the mathematical difficulties involved in the axiomatization of ‘physics in general and [the] kinetic theory in particular (Corry, 2004, 239) from the atomistic hypothesis that Hilbert, secondly, became more and more interested (from late-1911/early-1912 on) in coming to terms with these difficulties via an investigation into the structure of matter (Corry, 1999a; 1999d; Corry, 2010). Thirdly, as a result of his consideration of the mathematical foundations of physics, in the sense of radiation and molecular theory as going beyond the kinetic model ‘as far as its degree of mathematical sophisticated and exactitude is concerned ‘ (Corry, 2004, 237), Hilbert eventually came to uphold Mie’s electrodynamical theory of matter by late-1913/early-1914 (Corry, 1999b; Mehra, 1973; section 3.4, see also Battimelli, 2005; McCormmach, 1970). [3] The third period (1915-1923) pivots around the appearance, in 1915, of the ‘Foundations of Physics’ in which Hilbert presented a unified field theory, based on an electrodynamical reductionism, that combined Mie’s theory and Einstein’s (non-covariant) theory of gravitation and general relativity (e.g. Corry, 2004; Majer & Sauer, 2005; Renn & Stachel, 1999; Sauer, 1999; Sauer, 2005; Stachel, 1999, see also Earman & Glymour, 1978; Vizgin, 2001). Hilbert’s philosophical reflections on his theory dealt of the early 1920s with the epistemological implications of general covariance such as time-reversal invariance and new conditions for the objectivity and completeness of physical theories based on general relativity and quantum mechanics. Where probability was here accepted as a subjective ‘accessorial’ principle implied in the application of the laws of the new modern physics to nature, [4] in his later contributions to the axiomatization of quantum
mechanics empirical probabilities were implicitly defined through the axioms of a yet uninterpreted formalism after physical requirements had been put upon them (e.g. Lacki, 2000).

1. **First period. The axiomatization of probability as a physical discipline: 1900-1905**

Hilbert’s *Grundlagen der Geometrie* of 1899 resulted from his attempt to lay down a simple and complete system of independent axioms for the undefined objects ‘points’, ‘lines’ and ‘planes’ that establish the mutual relations that these objects are to satisfy (Hilbert, 1899 [1902], see also Hilbert, 1891 [2004]; Hilbert, 1894 [2004], Toepell, 1986b). In the lecture notes to a course on the ‘Foundations of Geometry’ of 1894, Hilbert defined the task of the application of the axiomatic method to geometry as one of determining

‘the necessary, sufficient, and mutually independent conditions that must be postulated for a *system of things*, in order that any of their properties correspond to a *geometrical fact* and, conversely, in order that a complete description and arrangement of all the geometrical facts be possible by means of this system of things’ (Hilbert 1894\(^3\) quoted in Toepell, 1986a, 58-59, my emphasis).

Hilbert was of the opinion that his axiomatization of elementary geometry was part of a more general program of axiomatization for all of natural science (e.g. Majer, 1995) and that geometry, as the science of the properties of space, must be considered as ‘the most perfect of the natural sciences’ (Hilbert 1898/1899\(^4\) quoted in Toepell 1986a, vii, see also Corry, 2006b; Majer, 2001). The fact that Hilbert’s axioms for geometry were chosen so as to reflect spatial intuition not only indicates that the axiomatic method itself is a tool for the *retrospective*, or post-hoc, investigation of the logical structure of *“concrete, well-established and elaborated [… ] entities”* (Corry, 2004, 99). But it also suggests that the difference between geometry and, for example, mechanics pertained solely to the historical stage of the development of both sciences. Where the basic facts

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\(^3\) In 1894 Hilbert gave a lecture course on ‘The foundations of geometry’.

\(^4\) In 1898/1899 Hilbert gave a lecture on ‘The foundations of Euclidean geometry’.
of geometry ‘are so irrefutably and so generally acknowledged [that] no further proof of them is deemed necessary [and] all that is needed is to derive [the] foundations from a minimal set of […] axioms’ (Hilbert 1898-1899 quoted in Corry, 2004, 90), in mechanics

‘all physicists recognize its most basic facts [but] the arrangement [is] still subject to a change in perception [and] therefore mechanics cannot yet be [turned into] a pure mathematical discipline, at least to the same extent that geometry is’ (Hilbert 1898-1899 quoted in Corry, 2004, 90).

Hilbert’s motivation for the application of the axiomatic method to physical theories was to create a secure foundation from which all its known theorems could be deduced so as to avoid the recurrent situation in which new hypotheses intended to explain newly discovered phenomena are added to existing theories without showing that the former do not contradict the latter. It is against this background that Hilbert’s inclusion of the axiomatization of physics in his 1900 Paris lecture must be understood.

1.2 The place of probability in the ‘sixth problem’ of Hilbert’s 1900 Paris lecture

The central goal of Hilbert’s invited lecture at the Second International Congress of Mathematicians, held in Paris, was to ‘lift the veil behind which the future [of mathematics] lies hidden’ (Hilbert, 1900 [2000], 407) by formulating, for its ‘gifted masters and many zealous and enthusiastic disciples’ (Hilbert, 1900 [2000], 436), twenty-three problems ‘which the science of today sets and whose solution we expect from the future’ (Hilbert, 1900 [2000], 407). Following the advice of his friends Adolf Hurwitz (1859-1919) and Minkowski – who both saw clearly that the publication of the Foundations of Geometry had ‘opened up an immeasurable field of mathematical investigation […] which goes far beyond the domain of geometry’ (Hurwitz quoted in Gray, 2000, 10) – Hilbert proposed that his ‘mathematics of axioms’ (Hurwitz quoted
in Gray, 2000, 10) should be applied to both the ‘empirical’ and ‘pure’ part of mathematics.\(^5\)

‘I [Hilbert] […] oppose the opinion that only the concepts of analysis […] are susceptible of a fully rigorous treatment [for this] would soon lead to the ignoring of all concepts arising [for instance] from [mathematical] physics, to a stoppage of the flow of new material from the outside world. [W]hat an important nerve, vital to mathematical science, would be cut by [its] extirpation! [I] think that wherever, from the side of […] geometry, or from the theories of […] physical science, mathematical ideas come up, the problem arises […] to investigate the principles underlying these ideas and so to establish them upon a simple and complete system of axioms, that the exactness of the new ideas and their applicability to deduction shall [not] be […] inferior to those of the old arithmetical concepts’ (Hilbert, 1900 [2000], 410).

1.2.1 The ‘sixth problem’

If Hilbert introduced the ‘sixth problem’ (‘The mathematical treatment of the axioms of physics’) by writing that it is suggested by the axiomatic ‘investigations on the foundations of geometry’ (Hilbert, 1900 [2000], 418), the structure of this investigation that was to ‘serve as a model’ (Hilbert, 1900 [2000], 419) for its solution was summarized as part of the second problem (‘The compatibility of the arithmetical axioms’) on the list:

\(^{5}\) Before presenting the problems, Hilbert made clear that it is the interaction, or ‘dialectical interplay’ (Corry, 1997, 65), between the ‘empirical’ part (i.e. the natural sciences) and ‘pure’ part (e.g. the theory of numbers and functions and algebra) of mathematics that accounts for its organic development: ‘[T]he first and oldest problems in every branch of mathematics spring from experience and are suggested by the world of external phenomena [I]n the further development of a branch of mathematics, the human mind, encouraged by the success of its solutions, becomes conscious of its independence. It evolves from itself alone […] by means of logical combination, generalization, specialization, by separating and collecting ideas in fortunate ways, new and fruitful problems, and appears then itself as the real questioner […] In the meantime, while the creative power of pure reason is at work, the outer world again comes into play, forces upon us new questions from actual experience [and] opens up new branches of mathematics’ (Hilbert, 1900 [2000], 409).
'When we are engaged in investigation the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. The axioms so set up are at the same time the definitions of those elementary ideas; and no statement within the realm of the science whose foundations we are testing is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps' (Hilbert, 1900 [2000], 414).

The ‘sixth problem’ itself was described in terms of the call for the treatment ‘in the same manner, by means of axioms, [of] those physical sciences in which mathematics plays an important part, in the first rank […] the theory of probabilities and mechanics’ (Hilbert, 1900 [2000], 418). After having mentioned the work of, on the one hand, Ernst Mach (1838-1915), Ludwig Boltzmann (1844-1906), Paul Volkmann (1856-1938) and Heinrich Hertz (1857-1894) on mechanics and, on the other hand, Georg Bohlmann (1869-1928) on the ‘calculus of probabilities’ as important contributions to these branches of physical science, Hilbert wrote that

‘[i]f geometry [serves] as a model for the treatment of physical axioms, we shall try first by a small number of axioms to include as large a class as possible of physical phenomena, and then by adjoining new axioms to arrive gradually at the more special theories’ (Hilbert, 1900 [2000], 419)."
Although the treatment of physics along the lines of an axiomatic method that was first applied to (Euclidean) geometry fitted naturally into Hilbert’s early views on the connection between (pure) mathematics, geometry and physics – and, in fact, had been part of ‘the evolution of Hilbert’s early axiomatic conception’ (Corry, 1997, 70) –, Hilbert’s inclusion of the ‘sixth problem’ on the list was remarkable. Firstly, in so far as it neither satisfies his own criteria as to what a meaningful problem in mathematics is nor sits well with his statement that a solution to a mathematical problem should be obtained in a finite number of steps,7 it is, secondly, better understood as a general task or research program. And thirdly, if ‘it is difficult to understand the place of this project as part of [Hilbert’s] work [on mathematical physics] up to that time […]’, unlike most of the other [problems] in the list, [it] is [fourthly] not the kind of issue that mainstream mathematical research had been pointing to in past years’ (Corry, 1997, 69). The fact that, especially, the mentioning of probability theory as one of the main physical sciences that are amenable to axiomatization ‘has often puzzled readers’ (Corry, 1997, 68), suggests that, apart from these four general features of the ‘sixth problem’, there are other more specific features peculiar to Hilbert’s treatment of this theory.

1.2.2 Hilbert, Bohlmann and probability theory

In his Paris lecture, Hilbert referred to Bohlmann’s ‘Über Versicherungsmathematik’ (1900) for a presentation of ‘the axioms of the theory of probabilities’ (Hilbert, 1900 [2000], 418). This paper reproduced several talks which Bohlmann delivered during the Easter vacations of 1900 in which he announced that his much more rigorous axiomatization of probability would appear in

7 In his ‘Mathematical Problems’, Hilbert wrote the following: ‘It remains to discuss […] what general requirements may be justly laid down for the solution of a mathematical problem. I should say [that] it shall be possible to establish the correctness of the solution by means of a finite number of steps based upon a finite number of hypotheses which are implied in the statement of the problem and which must always be exactly formulated. This requirement of logical deduction by means of a finite number of processes is simply the requirement of rigor in reasoning. [Also] [t]he conviction of the solvability of every mathematical problem is a powerful incentive to the worker […] There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no ignorabimus’ (Hilbert 1900 [2000], 409 & 412). This last statement must be read as a response to Emil du Bois-Reymond’s (1818–1896) ‘ignoramus et ignorabimus’ (‘we do not know and will not know’) (e.g. McCarty, 2005).
an article on life-insurance mathematics (‘Lebensversicherungsmathematik’) (Bohlmann, 1901) for Felix Klein’s (1849-1925) *Encyklopädie der mathematischen Wissenschaften.* Given that Hilbert, who by the end of March 1900 had not yet ‘determined the subject of his Paris talk [and] had not produced a lecture […] by June’ (Krengel, 2011, 5, see also Reid, 1996, 70), had not shown interest in probability theory prior to those months it seems certain that the problem of its axiomatization was proposed to him by his colleague at Göttingen, Bohlmann.

Hilbert ‘made no secret about the fact that he had solicited various problems by talking to other mathematicians’ (Krengel, 2011, 5), but in light of the words that Bohlmann himself used to characterize his 1900 article (‘very general’, ‘loosely formulated’), it is surprising that Hilbert decided to insist on its axiomatization. For it may be recalled that Hilbert defined the axiomatic method as a post-hoc reflection on the results of well-established physical disciplines. Two other remarks can be made about Hilbert’s description of probability as a ‘physical science in which mathematics plays an important part’. Firstly, in so far as Bohlmann’s article did not point out with which ‘physical phenomena’ the theory is concerned, it remained unclear how the first step of the treatment of its axioms, namely the inclusion of ‘as large a class as possible of physical phenomena [by] a small number of axioms’ (Hilbert, 1900 [2000], 419), was to be carried out. Secondly, if Hilbert was convinced that Bohlmann had already put forward the axioms of the physical discipline of probability, he emphasized that their ‘logical investigation’ (Hilbert, 1900 [2000], 418) was also to be instrumental to securing the role of the so-called ‘method of mean values in mathematical physics, and in particular in the kinetic theory of gases’ (Hilbert, 1900 [2000], 418). These three points – that of the issue of its doubtful status as a well-established and physical scientific discipline and that of the two-sided importance of its axiomatization – reappeared in Hilbert’s lecture course of 1905 entitled ‘Logical Principles of Mathematical Thinking’ (‘Logische Prinzipien des mathematischen Denken’).

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8 In a footnote to his 1900 article, Bohlmann remarked that the 1901 article would contain a much more precise, or rigorous, mathematical formulation of the axioms of his treatment of life insurance.

9 Bohlmann was the first lecturer of the new German institute of insurance science (‘Sterbenswahrscheinlichkeit’) at Göttingen – where he gave courses on actuarial mathematics until 1901, the year in which he became extraordinary professor of actuarial mathematics.
1.3 Probability as an application in (mathematical) physics

In the second part of this course Hilbert applied his axiomatic method to several physical disciplines (Hilbert, 1905; Corry, 1997, section 8; Corry, 2004, chapter 3);\textsuperscript{10} mechanics, thermodynamics, probability calculus, the kinetic theory of gases, insurance mathematics, electrodynamics and psychophysics. This can, indeed, be considered ‘the first clear evidence of what [he] envisaged as the solution […] of the sixth of his 1900 list of problems’ (Corry, 1997, 73). But Hilbert himself cautioned not only that the completion of the axiomatization of physics is to be understood as a research program (‘Arbeitsprogramm’), but also that in the individual physical disciplines mentioned it is merely possible to find ‘initiatives which only in some cases have been carried through’ (Hilbert 1905 quoted in Corry, 1997, 84).\textsuperscript{11} After discussing Bohlmann’s 1901 article (section 1.3.1), Hilbert also introduced three applications of probability: the theory of compensation of errors, the kinetic theory of gases and insurance mathematics (section 1.3.2).

1.3.1 Early ‘axiomatic’ systems of probability: Hilbert and Bohlmann

The axioms for probability theory that Hilbert here presented were drawn from Bohlmann’s 1901 lengthy article ‘Lebensversicherungsmathematik’ – which ‘contained a much more precise mathematical formulation of the axioms underlying the mathematical treatment of life insurance, which in [his] earlier article [of 1900] appear[ed] as very general, somewhat loosely formulated assumptions’ (Corry, 1997, 129). After formulating his main claim, namely that the mathematical foundations life insurance ground probability theory,\textsuperscript{12} Bohlmann – referring to Henri Poincaré’s (1854-1912) Calculus des Probabilités (with the telling subtitle ‘Cours de physique mathematique’) as the main source of his axiomatization – described probability in an axiomatic way, firstly, by

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\textsuperscript{10} The manuscript of this part of the lecture course has, unfortunately, not been published.

\textsuperscript{11} ‘Ansätze dazu, die nur in ganz wenigen Fällen durchgeführt sind’ (Hilbert 1905 quoted in Corry, 1997, 84).

\textsuperscript{12} Bohlmann mentioned Karl Wagner (1855-1910), who, in his monograph entitled Das Problem von Risiko in der Lebensversicherung of 1898, had argued that probability theory and insurance are, in essence, unconnected, as an opponent of this view (Koch, 1998, section 2.2; Purkert, 2002).
distinguishing between the general axioms of probability and the specific axioms of life insurance and, secondly, by introducing three definitions and axioms for ‘the general calculus of probability’ (‘der allgemeinen Wahrscheinlichkeitsrechnung’). In brief, Bohlmann defined the probability of an event \( E \) as a nonnegative number \( p(E) \), \( 0 \leq p(E) \leq 1 \), for which it holds that

1. if \( E \) is the certain event, then \( p(E) = 1 \),
2. if \( E \) is impossible, then \( p(E) = 0 \), and
3. if \( E_1 \) and \( E_2 \) happen simultaneously with probability zero, then the probability of either \( E_1 \) or \( E_2 \) equals \( p(E_1) + p(E_2) \).

Bohlmann was the first to treat probability as a function of an event, but his system contained several flaws; the notion of an ‘event’ \( E \) remained undefined, the requirement that \( p(E) \) is rational was somewhat outdated and ‘definitions and axioms […] appear intermingled in a way that Hilbert himself would have avoided if he [would have] systematically followed the model of the [Foundations of Geometry]’ (Corry, 1997, 130).

However, Hilbert merely restated Bohlmann’s (1) and (2) using a slightly different notation. He ‘did not comment on the independence, consistency or completeness of these axioms [such that his] system was a rather crude one by [his] own criteria’ (Corry, 1997, 132). There are four remarks to be made about this situation. Firstly, Hilbert’s flexible treatment of the axiomatization of probability was at odds with the position occupied in his correspondence

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13 Bohlmann credited the first group of axioms to Emanuel Czuber (1851-1925) (Czuber, 1900) and the second group to Ladislaus von Bortkiewicz (1868-1931) (Von Bortkiewicz, 1900).

14 There is some controversy as to the historical and theoretical importance of Bohlmann’s contribution to probability theory. Where Von Plato writes that Bohlmann ‘does not do much more than call some of the basic properties of probability calculus by the name of axioms’ (Von Plato, 1994, 32), Krengel criticizes Von Plato for having ‘little feeling for the magnitude of [the] step [of defining probability as a function of an event, LV], which is even more evident if one thinks about the long time it took for this idea to be accepted’ (Krengel, 2011, 4). See also Hochkirchen (1999, 28-31) and Schneider (1988, 509) for short discussions of Bohlmann.

15 ‘The simultaneous occurrence of two events \( E_1, E_2 \) is […] denoted by \( E_1 \land E_2 \), whereas \( E_1 \times E_2 \) denotes their disjunction. Two events are mutually exclusive if \( p(E_1 \land E_2) = 0 \), while \( p(E_1 | E_2) \) denotes condition probability’ (Hilbert quoted in Corry, 2004, 165). The following two axioms were put forward as the definition of probability theory: 1. \( p(E_1 \times E_2) = v(E_1) \cdot p(E_2) \), if \( p(E_1 \lor E_2) = 0 \). II. \( p(E_1 \land E_2) = p(E_1, p(E_1 | E_2)) \)’ (Corry, 2004, 165). As to Hilbert’s presentation of Bohlmann’s axiomatization of probability theory, Corry also remarks that ‘Hilbert did not mention an additional definition appearing in Bohlmann’s article, namely, that two events \( E_1, E_2 \) independent if the probability of their simultaneous occurrence equals \( p(E_1) \cdot p(E_2) \)’ (Corry, 2004, 165).
of 1897-1902 with Gottlob Frege (1848-1925) on the axiomatic method (Blanchette, 1996; Demopoulos, 1994; Hallett, 2010; Resnik, 1974). In a letter of 22 September, 1900 Hilbert wrote that ‘a concept can only be logically fixed through its relations to other concepts. These relations, formulated in definite statements, I call axioms, and thus I arrive at the view that these axioms [...] are the definitions of these concepts’ (Hilbert quoted in Hallett, 2010, 427). If it seems likely, secondly, that Hilbert, ‘[i]n treating the axioms of probability and speaking of the need to separate – rather than to combine – axioms and definitions, [was] stressing the early state in which the theory was then found’ (Corry, 1997, 131), it may also be emphasized, thirdly, that more sophisticated attempts at the axiomatization of probability had already been made since Bohlmann’s 1901 article. It was Rudolf Laemmel (1879-1962) who, in his dissertation of 1904 entitled ‘Untersuchungen über die Ermittlung von Wahrscheinlichkeiten’, formulated, in set-theoretical terms, two axioms (of total and compound probability) and three definitions. After having established their independence and sufficiency, Laemmel wrote that the axioms allowed for the ‘hypothetical’ construction of a system for probability.¹⁸ Laemmel did not refer to Bohlmann’s articles and because it is unclear whether he was acquainted with Hilbert’s Foundations of Geometry it cannot be said that he intended, through his axiomatization, to solve the ‘sixth problem’ (Von Plato, 1994, 33). If Hilbert, for his part, was not familiar with Laemmel’s work, he did not seem to be very interested in the doctoral dissertation of his student Ugo Broggi (1880-1965) of 1907 entitled ‘Die Axiome der Wahrscheinlichkeitsrechnung’ in which he – following the criteria of Hilbert’s axiomatic method – set out to perfect the


¹⁷ Laemmel was of the opinion that in order to ‘ascertain to probabilities a greater epistemological value, one has to try to replace the intuitive or empirical procedure by a determination of probability through a process of hypotheses’ (Laemmel 1904 quoted in Von Plato, 1994, 32).

¹⁸ It may be noted that Laemmel left the problem of consistency untouched and did not explicate the concept of independence.
earlier proposals of Bohlmann and Laemmel.\textsuperscript{19} Notwithstanding his own acknowledgment of the early state of probability theory, Hilbert, thus, did neither improve upon Bohlmann’s account nor involve himself with the novel attempts of others to axiomatize the theory along the lines of his own framework. This indicates, fourthly, that ‘Hilbert, in 1905 […] was much less interested in the calculus of probabilities as such’ (Corry, 1997, 133) – let alone as a physical discipline – ‘than in its [mathematical] applications’ (Corry, 1997, 133).

1.3.2 The applications of probability theory in Hilbert’s 1905 lecture course

The topic of the application of probability had already appeared as an aspect of the ‘sixth problem’ of the Paris lecture. Here, Hilbert wrote that ‘it seems […] desirable’ that the ‘logical investigation [of] the axioms of the theory of probabilities [is] accompanied by a […] satisfactory [treatment] of the method of mean values in mathematical physics, and in particular the kinetic theory of gases’ (Hilbert, 1900 [2000], 418, my emphasis). Perhaps unsurprisingly, next to ‘Sterbenswahrscheinlichkeit’ (‘insurance mathematics’),\textsuperscript{21} the 1905 lecture course presented ‘Ausgleichungsrechnung’ (‘method of mean values’, or ‘theory of compensation of errors’ (TCE)) (Corry, 1997, 133-136, see also Aldrich, 2011; Sheynin, 1972; Sheynin, 1979) and the ‘kinetische Gastheorie’ (‘kinetic theory of gases’ (KTG)) (Corry, 1997, 136-152, see also Brush, 1976; Brush & Hall, 2003; Loeb, 1961) as the main applications of probability theory. Hilbert wanted to find a mathematical basis for the determination of average values which, for example in the contributions of James Clerk Maxwell (1831-1879) (Brush, 1998).

\textsuperscript{19} Hilbert did not notice the falsehood of Broggi’s proof of the claim that denumerable or countable additivity can be derived from finite additivity. This mistake was later exposed by another student of Hilbert, Hugo Steinhaus (1887-1972) in his ‘Les probabilités dénombrables et leur rapport à la théorie de la mesure’ of 1923 (Steinhaus, 1923, see also Hochkirchen, 1999, 236; Schafer & Vovk, 2006, 83, 85-86).

\textsuperscript{20} After proposing a system of axioms for probability, Broggi showed the completeness, mutual independence and consistency of his axioms. His first axiom stated that the certain event has value 1 and the second axiom consisted of the rule of total probability. Broggi defined probability as a ratio (in a discrete or geometric) setting and then verified his axioms. If Laemmel and Broggi’s proposals can be distinguished from the system of Bohlmann with reference to the fact that they start from set theory, Broggi moved beyond Laemmel by making use of measure theory as well.

\textsuperscript{21} It may come as no surprise that Hilbert, also here, followed Bohlmann in taking the abovementioned axioms of probability and adding more specific definitions and axioms (see Corry, 1997, 152-154).
It was under the influence of the views put forward in the mathematical physics seminar of Franz Neumann (1798-1895) and Jacob Jacobi (1804-1851) at the University of Königsberg (Olesko, 1991) that Hilbert had become convinced of the value of the exactness of measurement in physical research. He first claimed that the whole TCE could be derived from the axiom that ‘if various values have been obtained from measuring a certain magnitude, the most probable actual value of the magnitude is given by the arithmetical average of the various measurements’ (Hilbert 1905 quoted in Corry, 1997, 134). Referring to an article of the astronomer Julius Bauschinger (1860-1934), Hilbert, then, gave Gauss’s error theory and the method or principle of least squares as its two theorems. Because the axiom and the two theorems were entirely equivalent (‘vollkommen aequivalent’) – i.e. any one of them could be deduced as a mathematical consequence from the other two – it was arbitrary which one was taken as the foundation of the theory. The new work that was to be expected in this domain was to aim for the (axiomatic) reduction of these three statements to other axioms ‘with a more limited content and greater intuitive plausibility’ (Corry, 1997, 135).

It is generally agreed that probability first entered (statistical) physics in the work of Rudolf Clausius (1822-1888) and Karl Krönig (1822-1879) (Garber,

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22 In section 2 it will be made clear that this endeavor became of central importance for his search for, and eventual commitment to, a specific theory of the structure of matter that could serve as the basis for the foundation of physics.

23 Hilbert remained at the University of Königsberg between 1880-1895. It is likely that he participated in the seminar and, perhaps, even attended the lectures of Franz Neumann – who, despite his retirement in 1876, ‘was still to be seen at university gatherings and sometimes still lectured’ (Reid, 1996, 9) after that particular year.

24 Hilbert wrote that ‘what one [...] really [...] wants to consider as the foundation when there are several possibilities, is also here arbitrary and dependent upon personal inclinations and the general state of science’ (‘Was man [...] wirklich [...] als Grundlage ansprechen will, wenn sich so verschiedene Möglichkeiten ergeben haben, ist wie stets willkürlich und hängt von persönlichen Momenten und dem allgemeinen Stande der Wissenschaft ab’) (Hilbert quoted in Corry, 1997, 135).
the basis of which was the idea that even though ‘[t]he path of each gas atom [is] very irregular, so that it eludes calculation [...] according to the laws of the probability calculus one may assume, instead of this perfect irregularity, a perfect regularity’ (Krönig quoted in Schneider, 1988, 300) on average.

In a paper of 1860, Maxwell combined a revision of Clausius’ concept of the mean free path of a molecule with John Herschel’s (1792-1871) treatment of the normal distribution of errors to derive his distribution law for molecular velocities. Maxwell wrote that ‘in order to calculate most of the observable properties of a gas it is not necessary to know the positions and velocities of all particles at a given time: it suffices to know the average number of molecules having various positions and velocities’ (Corry, 1997, 137). Similar to Boltzmann, Maxwell set out to explain the behavior of macroscopic matter in terms of statistical laws describing the motion of the atoms which themselves obey Newton’s mechanical laws of motion.

Hilbert noted that this development had made it clear not only that probabilistic assumptions had to be introduced into the description of physical systems, but also that probability theory was required for the mechanical treatment of (heat) phenomena. If he praised the theory for ‘the remarkable way in which [it] combined the postulation of far-reaching assumptions about the structure of matter [...] with the use of [both] probability [and infinitesimal] calculus’ (Corry, 2004, 169) to produce new physical results, Hilbert also cautioned that since probability theory ‘is not an exact mathematical discipline’ (‘ist keine exakte mathematische Theorie’) it may only be used ‘as a first orientation’ (‘zu einer ersten Orientierung’) – and this when its results ‘are correct and in accordance with the facts of experience or with the accepted mathematical theories’ (Hilbert 1905 quoted in Corry, 2004, 170-171, f. 158).

The principal reason for this was that probability calculations could produce results that were in conflict with the accepted laws of mechanics – or, for that matter, with the observable phenomena described by (partial) differential equations. More in

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specific, Hilbert observed that the abovementioned provisos were not always satisfied in the case of the then current KTG. For example, the application of probability could lead to (fallacious) conclusions that contradicted other well-known results of the KTG, some of the results of the probabilistic version of the KTG conflicted with the reversibility of the laws of mechanics, and some of the proofs appearing in the laws of the KTG, such as those pertaining to average velocities instead of to velocity distributions as they ‘actually occur’, are in conflict with the idea of the reduction of phenomena to mechanical interactions between rigid, atomic, particles. Hilbert

‘wished to undertake an axiomatic treatment of the [KTG] not only because it combined physical hypotheses with probabilistic reasoning in a scientifically fruitful way [but] also because [it] was a good example of a physical theory where [...] additional assumptions had been gradually added to existing knowledge without properly checking the possible [...] difficulties that would arise from this addition. [For example] the question of probability in physics was not settled in this context’ (Corry, 2004, 168).

It is important to emphasize that where in 1900 the project of the axiomatization of probability theory was said to have to be accompanied by a further development of its applications, this particular topic came to determine the core of Hilbert’s (‘instrumental’) definition of probability theory after 1905. Rather than aiming for the axiomatization of the theory per se, Hilbert thus devoted himself to the axiomatization of the KTG and, therefore, the TCE as its main theoretical applications.

Hilbert admired the KTG ‘for the way in which this theory combined the postulation of far-reaching assumptions about the [atomistic] structure of matter with the use of probability calculus’ (Corry, 2004, 168), but he increasingly came to recognize not only that the use of probabilities in the KTG and in
physics at large was still in need of justification. But also that the foundational questions involved in the attempt to justify the introduction of ‘not yet fully developed’ (Hilbert 1911-1912 quoted in Schirrmacher, 2003, 10) probabilistic methods in physical theories ‘required further investigation into the theory of matter as such’ (Corry, 2004, 284). Where Hilbert initially searched for another theory of matter that could function as the basis for a reductionistic mechanical foundation of physics in so far as it, among other things, employed new (non-probabilistic) mathematics, he eventually abandoned mechanical reductionism as a foundational assumption as such. The electromagnetic reductionism that Hilbert adopted around late-1913/early-1914 left open the question of applying probability principles in the KTG and left unanswered the problems of introducing probability concepts in statistical mechanics. However, Hilbert’s approach to probability underwent an importance change under influence of his gradual adoption of this new form of reductionism. On the one hand, there was the so-called ‘physical viewpoint’, articulated as part of the search for a theory of the structure of matter that would deepen the atomism of the kinetic theory (e.g. Wilholt, 2002), that Hilbert put forward to overcome the mathematical difficulties implied by the atomistic hypothesis and that would ‘manifest itself in [his] reconsideration of his view of mechanics as the ultimate explanation of physical phenomena’ (Corry, 1999a, 14) (section 2). On the other hand, the viewpoint was an early suggestion of both the fact that and the way in which his attempt to come to terms with Einstein’s new relativistic mechanics via electromagnetism also meant that the status of probability, rather than that of the methods of probability theory, became a central concern for Hilbert (section 3).

Corry writes that ‘[a]lready in his 1905 lectures on the axiomatization of physics Hilbert had stressed the problems implied by the combined application of analysis and the calculus of probabilities as the basis for the kinetic theory, an application which is not fully justified on mathematical grounds. In his physical courses after 1910 [he] expressed again similar concerns. Yet, the more Hilbert became involved with the study of the kinetic theory itself [...] these concerns did not diminish. Rather, they increased’ (Corry, 1999a, 13).
2. **Second period. Beyond mechanical reductionism**
   – beyond probability theory: 1910-1914

After the 1905 lecture course, Hilbert lectured on mechanics (WS 1905/1906), continuum mechanics (SS 1906 and winter semester of 1906/1907), differential equations of mechanics (WS 1907/1908) and electrodynamics (SS 1907) (see Sauer & Majer, 2009, 709-726; Corry, 2004, 450-452). Although he gave no courses on physics until 1910, Hilbert followed his friend and colleague at Göttingen Hermann Minkowski’s (1864-1909) implementation of the project of the axiomatization of physics in the context of the investigation of the role of the new principle of relativity in various physical theories (see Corry, 2004, chapter 4; Corry, 2010; Galison, 1979; Walter, 1999; Walter, 2008). Hilbert resumed his physical lectures in 1910 with a course on mechanics (WS 1910/1911) and continued to expand the scope of his interest to topics such as continuum mechanics (‘kinetic theory of gases’) (SS 1911), statistical mechanics (winter semester 1911/1912), radiation theory (SS 1912) and electromagnetic oscillations (WS 1913/1914) in years that followed. The period between 1910-1914 was characterized not only by a continual commitment to mechanical reductionism, but also by a gradual, albeit implicit, endorsement, from the end of 1913 on, of electromagnetic reductionism that would form the basis for Hilbert’s proposal for a unified foundation of physics in 1915. Hilbert remained committed to the general project of the axiomatization of physics of 1900, but under the influence of the ‘increasing mathematical difficulty that affected the treatment of disciplines based on the atomistic hypothesis, and above all the [KTG]’ (Corry, 1999a, 3), Hilbert became convinced these difficulty could only be resolved by changing the most foundational assumption behind it. This section describes the changes in Hilbert’s basic attitude vis-à-vis the axiomatization of physics from the viewpoint of the mathematical method that partly caused these difficulties, namely probability theory.

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27 In brief, the main goal of the contributions of Minkowski was to explore whether adding the new principle to the already well-established theories and principles of mechanics would lead to internal contradictions.

28 Another main factor responsible for the changes in Hilbert’s views in this particular period was the axiomatic method itself; Hilbert seemed to have always been prepared to alter his foundational views, for example on the structure of matter, when the axiomatic method impelled him to do so.
2.1 The kinetic theory of gases (KTG), 1911-1912

After having expressed his support for the atomistic hypothesis as the possible basis for the reduction of the whole of physics to mechanics in his course on mechanics (WS 1910/1911) and continuum mechanics (SS 1911), Hilbert devoted himself to the KTG – a theory which itself seemed to give ‘additional support to the foundational role of mechanics as a unifying, explanatory scheme’ (Corry, 2004, 46). Hilbert taught a course on the topic in the winter semester of 1911/1912 and published a paper entitled ‘Begründung der kinetischen Gastheorie’ in 1912 (Hilbert, 1912b). Both the course and the paper made explicit the meaning of Hilbert’s purely analytical work on the theory of integral equations (Hilbert, 1912a) for the so-called Maxwell-Boltzmann equation, or probability distribution function (e.g. Rowlinson, 2005), and emphasized once more the difficult consequences of the combined use of differential and probability calculus in the KTG (see Corry, 1999a, section 3; Corry, 2004, 228-229 & 237-241). In brief, Hilbert searched for solutions to the Maxwell-Boltzmann equation and pursued the question whether the equation can be logically derived from the time-reversible equations of classical mechanics. Hilbert’s own negative answer gave rise to a problem that occupied his attention in the period 1915-1923, namely that of determining the objective meaning of irreversibility (see section 3.2.1.).

The Maxwell-Boltzmann distribution, first described by Maxwell in a paper of 1860 and modified and generalized by Boltzmann in 1877 (Maxwell, 1860; Boltzmann, 1877 [1909]), describes the probability distribution for the number of molecules having any given velocity. Maxwell and Boltzmann assumed that ‘in order to calculate most of the observable properties of a gas it is not necessary to know the positions and velocities of all particles at a given time. [I]t suffices to know the average number of molecules having various positions and velocities’ (Corry, 2004, 46). Or, in words drawn from Hilbert’s 1905 lecture course, since it is the case that even if the exact position and velocities of the particles of a gas are known it is not possible to integrate all the differential equations that describe the motions and interactions of these particles only the

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29 The content of this paper had been presented to the Göttingen Mathematical Society in December 1911.
30 In fact, the paper first appeared as chapter 22 of the book on the theory of integral equations.
averages magnitudes, as dealt with by the probabilistic kinetic theory of gases, are considered. Hilbert recognized the combined use of probability calculations and differential equations as ‘a very original mathematical contribution, which may lead to deep and interesting consequences (Corry, 2004, 169), but warned that it had not been justified as a mathematical basis for the KTG and could lead to results that conflicted with the laws of classical mechanics. For example, Hilbert, making use of Maxwell’s velocity distribution, Boltzmann’s logarithmic definition of entropy and probability theory, pointed out that the resulting law of constant increase in entropy was at odds with the idea of the reversibility of natural phenomena. Next to his recurrent concern about the reliance on averages, another more fundamental probability-related problem of the then current KTG was that

‘if Boltzmann proves [that] the Maxwell distribution [is] the most probable one from among all distributions […], this theorem possesses in itself a certain degree of interest, but it does not allow even a minimal inference concerning the velocity distribution that actually occurs in any given gas. [T]he probability of occurrence of the Maxwell velocity distribution is greater than that of any other distribution, but equally close to zero, and it is therefore almost absolutely certain that the Maxwell

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31 Corry explains that ‘by 1912, some progress had been made on the solution of the Maxwell-Boltzmann equation. The laws obtained from the partial knowledge concerning those solutions, and which described the macroscopic movement and thermal processes in gases, seemed to be qualitative correct. However, the mathematical methods used in the derivations seemed inconclusive and sometimes arbitrary. It was quite usual to depend on average magnitudes and thus the calculated values of the coefficients of heat conduction and friction appeared as unreliable’ (Corry, 1999a, 8). Here, Hilbert mentioned the following comparative example: ‘In order to lay bare the core of this question, I want to recount the following example: in a raffle with one winner out of 1000 tickets, we distribute 998 tickets among 998 persons and the remaining two were give to a single person. This person thus has the greatest chance to win, compared to all other participants. His probability of winning is the greatest, and yet it is highly improbable that he will win. The probability of this is close to zero’ (‘Um den Kernpunkt der Frage klar zu legen, will ich an folgendes Beispiel erinnern: In einer Lotterie mit einem Gewinn und von 1000 Loses seien 998 Lose auf 998 Personen verteilt, die zwei übrigen Lose möge eine andere Person erhalten. Dann hat diese Person im Vergleich zu jeder einzelnen andern die grössten Gewinnchancen. Die Wahrscheinlichkeit des Gewinnen ist für sie am grössten, aber es ist immer noch hochst unwahrscheinlich, dass sie gewinnt. Denn die Wahrscheinlichkeit ist so gut wie Null’) (Hilbert 1911/1912 quoted in Corry, 2004, 240).
distribution will not occur’ (Hilbert 1911/1912 quoted in Corry, 2004, 240, my emphasis).

‘What is needed for the theory of gases’, Hilbert continued, is a proof of the fact that ‘for a specified distribution, there is a probability very close to 1 that that distribution is asymptotically approached as the number of molecules becomes infinitely large’ (Hilbert 1911/1912 quoted in Corry, 2004, 240). Hilbert, thereby loosely referring to the probabilistic idea of convergence to the limit (e.g. Fischer, 2010; Hald, 2007), argued that in order to obtain this proof or, more generally, to provide a justification of probability theory in the KTG, it should become possible to ‘formulate the question in terms such as these: What is the probability for the occurrence of a velocity distribution that deviates from Maxwell’s by no more than a given amount? And moreover; what allowed deviation must we choose in order to obtain the probability 1 in the limit?’ (Hilbert 1911/1912 quoted in Corry, 2004, 240).

Hilbert also discussed additional difficulties arising from the application of probabilistic reasoning in the KTG. Although he suggested how these were to be mathematically resolved, Hilbert acknowledged that he could not yet give the final answers. It seems, indeed, to be the case that ‘the more Hilbert

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33 ‘Wenn z.B. Boltzmann beweist [...] dass die Maxwellsche Verteilung [...] unter allen Verteilungen von gegebener Gesamtenergie die wahrscheinlichste ist, so besitzt dieser Satz ja an und für sich ein gewisses Interesse, aber er gestattet auch nicht der geringsten Schluss auf die Geschwindigkeitsverteilung, welche in einem bestimmten Gaste wirklich eintritt [...] [D]ie Wahrscheinlichkeit für den Eintritt der Maxwellschen Geschwindigkeitsverteilung [ist] zwar grösser als die für das Eintreten einer jeden bestimmten andern, aber doch noch so gut wie Null, und es ist daher fast mit absoluter Gewissheit sicher, dass die Maxwellsche Verteilung nicht eintritt’ (Hilbert 1911/1912 quoted in Corry, 2004, 240).

34 The passage in which these phrases occur is the following: ‘Was wir fur die Gastheorie brauchen, ist sehr viel mehr. Wir wünschen zu beweisen, dass für eine gewisse ausgezeichnete Verteilung eine Wahrscheinlichkeit sehr nahe an 1 besteht, derart, dass sie sich mit Unendliche wachsender Molkulzahl der 1 asymptotisch annähert’ (Hilbert 1911/1912 quoted in Corry, 2004, 240). See also Hilbert’s ‘Vortrag über meine Gasvorlesung’ (Hilbert Cod. Ms. 588) for this point.

became involved with the study of kinetic theory, and at the same time with the deep mathematical intricacies of the theory of linear integral equations’ (Corry, 2004, 267), the more his concerns about the problems implied by the combined use of probability theory and analysis increased.

2.1.1 The 1911/1912 course on KTG: three approaches to physical disciplines

In the introduction to his lecture course on the KTG of the winter semester of 1911/1912, Hilbert distinguished three alternative approaches to physics. The first was purely phenomenological, the second assumed the atomistic hypothesis, and the third aimed at a fundamental (molecular) theory of matter.

The first approach was entertained by, for instance, Hilbert’s colleague the theoretical physicist Woldemar Voigt (1850-1919). Reflecting Neumann’s Königsberg tradition (Olesko, 1991, 387-388), Voigt upheld the view that experimentally grounded physical theories should describe phenomena completely and in either simple, direct terms or straightforward equations that directly correspond to the empirical features of the phenomena (see Corry, 2004, 79, 79-80, Katzir, 2006, 145-147). Given his ‘(mathematical) phenomenology’, Voigt, for instance in his ‘Phänomenologische und atomistische Betrachtungsweise’ of 1915, dismissed atomistic-reductionist explanations and remained working on theories (e.g. piezo-electricity) related to nineteenth-century fields of research such as optics and crystallography from a somewhat eclectic non-mathematical, physical-visual perspective (Katzir, 2003; Jungnickel & McCormmach, 1986, chapter 19). Together with the experimental Eduard Riecke (1845-1915), Voigt represented the mathematical physics institute at Göttingen. But because of their phenomenological approach to physics both scientists were isolated not only from Hilbert’s circle in specific, but also from ‘the kinds of interest pursued by [the] colleagues in Germany and elsewhere in Europe’ (Corry, 2004, 234) at large. Hilbert, in his 1911/1912 lecture course on the KTG, not only criticized Voigt for fragmenting physics into various chapters – each of which is treated ‘using different assumptions, peculiar to each of them, and deriving from these assumptions different mathematical consequences’ (Corry, 1999a, 492). But he also reproached him for rejecting the general idea that physics could be given

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36 This course was announced as a course on ‘continuum mechanics’, but its Ausarbeitung, by Hilbert’s assistant Erich Hecke (1887-1947), was entitled ‘Kinetic theory of gases’.
‘a substantially more thorough [treatment] on the basis of the atomic theory’ (Hilbert 1911/1912 quoted in Schirrmacher, 1999, 5, f. 53). Hilbert insisted that even though a phenomenological approach à la Voigt is indispensable as a makeshift stage on the way to knowledge, it must ‘urgently be left behind in order to penetrate into the real sanctuary of theoretical physics’ (Hilbert 1911/1912 quoted in Schirrmacher, 2003, 10).

The second, ‘atomistic’, approach to physical theories aimed for this very goal by means of the creation of an axiomatic system that holds for the whole of physics and which ‘enables all physical phenomena to be explained from a unified point of view’ (Hilbert 1911/1912 quoted in Corry, 2004, 236).

Another difference is that where the phenomenological approach solely made use of partial differential equations, the atomistic approach employed mathematical methods that can subsumed under the ‘entirely different’ and ‘not yet fully developed’ probability calculus (Schirrmacher, 2003, 10) – as found in the KTG and radiation theory (see Hilbert, 1912a, chapter 12; Hilbert, 1912b; Hilbert, 1912c [2009]; see also Corry, 1998). Because mathematical analysis is ‘not yet developed sufficiently to provide for all [its] demands’ (Corry, 2004, 237) the atomistic approach does not have at its disposal rigorous logical deductions and ‘must be satisfied with rather vague mathematical formulae’ (Hilbert 1911/1912 quoted in Corry, 2004, 237). At the same time, the use of probabilistic methods did lead to interesting new results that seemed to correspond to the experimental facts.

The third approach was to aim for the development of a ‘molecular theory of the structure of matter’ that is ‘based on atomic theory and would permit all

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38 The full passage goes as follows: ‘Wenn man auf diesem Standpunkt steht, so wird man den früheren nur als einer Notbehlf bezeichnen, der nötig ist als eine erste Stufe der Erkenntnis, über die man aber eilig hinwegschreiten muss, um in die eigentlichen Heiligtümer der theoretischen Physik einzudringen’ (Hilbert 1911/1912 quoted in Schirrmacher, 2003, 10). Schirrmacher’s translation has here been slightly amended.
39 The full passage goes as follows: ‘Hier ist das Betreben, ein Axiomensystem zu schaffen, welches für die ganze Physik gilt, und aus diesem einheitlichen Gesichtspunkt alle Erscheinungen zu erklären’ (Hilbert 1911/1912 quoted in Corry, 2004, 236, f. 30).
40 ‘[…] sich mit etwas verschwommenen mathematischen Formulierungen zufrieden geben muss’ (Hilbert 1911/1912 quoted in Corry, 2004, 237, f. 32).
physical properties to be deduced from something even deeper than the system of axioms called for in [the ‘atomistic’] approach’ (Schirrmacher, 2003, 11). Hilbert introduced this ‘molecular’ approach as having to be accompanied by an advanced or novel mathematics (see Schirrmacher, 2003, 10-11) – one that would go beyond the atomists’ probability theory ‘as far as its degree of mathematical sophistication and exactitude is concerned’ (Corry, 2004, 237) and that would somehow reveal the identity of the (mathematical) model and (physical) reality. What this third approach should look like in terms of its unifying power, knowledge status and mathematical method (see Schirrmacher, 2003, 10) was not made clear in the 1911/1912 lecture course, but Hilbert promised to consider in detail the molecular theory in the following year, which he did in several courses taught, in 1912-1913, under the header of ‘mathematical foundations of physics’ (see section 2.2). Both in these courses and in subsequent publications and public lectures, Hilbert put forward his theory of linear integral equations as the mathematical framework ‘suited to formulate and resolve conceptual gaps’ (Sauer & Majer, 2009, p. 439) in such fields as kinetic and radiation theory.

Taken together, by 1911-1912 Hilbert seemed to have envisioned his project of the axiomatization of physics from the viewpoint of mechanistic reductionism as follows:

After a ‘first step in understanding’ by phenomenological means [and] and [a] successful axiomatization ‘on the basis of the atomic theory’, [the] objective [...] was to relate the basic notions employed in the axiomatization of physics to actual physical objects’ (Schirrmacher, 2003, 11, my emphasis).

Hilbert himself had been aware that the justification for the belief in the validity of the atomistic hypothesis that, first implicitly and later explicitly, accompanied his mechanical reductionism ‘was the prospect that it would provide a more accurate and detailed explanation of natural phenomena once the tools were developed for a comprehensive mathematical treatment of theories based on it’ (Corry, 2004, 267). After the 1911/1912 lecture course, his main task became that of addressing the physical domain of the molecular theory of the structure of matter itself – not in the least because it promised to give a mathematically exact description of natural phenomena as they actually occurred.
in reality, rather than merely an approximation of their possible occurrence expressed in terms of averages.

2.2 *Theories of the structure of matter and relativity, 1912-1914*

The 1912/1913 lecture course on ‘the molecular theory of matter’ further pursued the idea that the way in which the mathematical difficulties of the atomistic hypothesis could be resolved would be to adopt a so-called ‘physical point of view’. Hilbert suggested to make clear ‘through the use of the axiomatic method, those places in which physics intervenes into mathematical deduction’ (Corry, 1999a, 498-499) such that it becomes possible to separate three levels of any physical theory:

‘[First] what is adopted as a logically arbitrary definition or taken as an assumption of experience, second, what could be concluded a priori from these assumptions but cannot be concluded with certainty given current mathematical difficulties, and third, what is a proven mathematical conclusion’ (Hilbert 1912/1913 quoted in Corry, 1999a, 499).

The course itself also consisted of three parts; where the first part discussed certain properties of matter related to the state equation for a completely homogeneous body understood as a mechanical system of molecules, the second part presented more complex physical and chemical properties of matter (see Corry, 2004, 267-270). The third part expressed these results in the form of

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41 Corry’s translation has been slightly amended. The original passage goes as follows: ‘Wir voneinander trennen, was erstens logisch willkürliche Definition oder Annahme der Erfahrung entnommen wird, zweitens das, was a priori sich aus diesen Annahmen folgern liess, aber wegen mathematischer Schwierigkeiten zur Zeit noch nicht sicher gefolgt werden kann, und dritten, das, was bewiesene mathematische Folgerung ist’ (Hilbert quoted in Corry, 1999a, 499, f. 16).
The place of probability in Hilbert's axiomatization of physics, ca. 1900-1926

new axioms. Because the a priori derivation of these axioms from mechanical principles was to be done in kinetic terms, it was, once again, necessary to have recourse to the ‘fundamental principle of statistical mechanics’, namely that the states of a physical system are equally probable.

After his brief consideration of the molecular theory, Hilbert soon devoted himself to electron theory – or, more specifically, to the application of the kinetic theory to the study of the motion of the electron. In the summer of 1913 Hilbert organized a series of lectures on the theory and gave a course in which he presented the electron theory as the ‘foundation of the whole of physics’ (Hilbert 1913 quoted in Corry, 2004, 271). The course contained an explicit treatment of the status of Lorentz covariance and Minkowski’s ‘word-postulate’ (Welt-postulate) as the fundamental principles of the new relativistic physics – a topic that Hilbert had already briefly touched upon in his 1910/1911 course on mechanics. Following Minkowski’s ‘macroscopic’ (or ‘phenomenological’) approach and Max Born’s (1882-1970) ‘microscopic’ approach to establishing the validity of the basic principle of relativity (see Corry, 1999b), in his course Hilbert described the goal of reconstructing the whole of physics in terms of as few basic concepts as possible as follows:

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42 Hilbert wrote that ‘Um in einzelnen Falle die karakteristische Funktion in ihrere Abhängigkeit von der eigentlichen Veränderlichen und den Massen der unabhängigen Bestandteile zu ermitteln, müssen verschiedenen neue Axiome hinzugezogen werden’ (Hilbert 1912/1913 quoted in Corry, 2004, 269, f. 136).

43 It is important to note, with Corry, that ‘Hilbert never performed for a physical theory exactly the same kind of axiomatic analysis he had done for geometry, though he very often declared this to be the case. Also, his derivations of the basic laws of the various disciplines from the axioms were always rather sketchy, when they appeared at all’ (Corry, 1999d, 15).

44 The full passage goes as follows: ‘Um die empirisch gegebenen und zu mathematischen Formeln verallgemeinerten Ergebnisse [...] a priori und zwar auf rein mechanischem Wegen abzuleiten, greifen wir wieder auf des Grundprinzip des statischen Mechanik zurück, von der wir bereits im ersten Teil ausgegangen waren’ (Hilbert 1912/1913 quoted in Corry, 2004, 270, f. 141).


46 It was Minkowski who first referred to the principle of covariance or relativity as the ‘word-postulate’ in his ‘Space and Time’ of 1909. Here, he wrote that ‘the postulate comes to mean that only the four-dimensional world of space and time is given by phenomena, but that the projection in space and in time may still be undertaken with a certain degree of freedom, I prefer to call it the postulate of the absolute world (or briefly the world-postulate)’ (Minkowski, 1909 [1952], 104).
‘The most important concepts are the concept of force and of rigidity.\textsuperscript{47} From this point of view the electrodynamics would appear as the foundation of all of physics. But the attempt to develop this idea systematically must be postponed for a later opportunity. In fact, it has to start from the motion of one, of two, etc. electrons, and there are serious difficulties on the way to such an understanding’ (Hilbert 1913 quoted in Corry, 2004, 272).\textsuperscript{48}

Hilbert explained these difficulties as arising from the need to introduce probabilistic mathematical considerations from kinetic theory as soon as the description concerns the motion of and interactions between more than a single electron (the so-called ‘\textit{n}-electron problem’).\textsuperscript{49} In other words, the situation such that because the explanation of the magnetic and electric interactions among electrons in mechanical terms is only an approximation it must be admitted that ‘we [either] only speak […] of averages’ (Corry, 1999a, 500) or settle for describing the motion of one electron. Although he seems to have been more aware than ever of ‘the mathematical and physical difficulties […] associated with a conception of nature based on the model underlying kinetic

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\textsuperscript{47} At this point, it is interesting to observe, with Renn and Stachel, that ‘[i]n spite of the conceptual revolution brought about by special relativity concerning the revision of the concepts of space and time but also the conceptual autonomy of the field concept from that of the ether, Hilbert nevertheless continued to count on traditional concepts such as force and rigidity as the building blocks for his axiomatization program’ (Renn & Stachel, 1999, 7-8). See also footnote 59.


\textsuperscript{49} There was, of course, also a highly complex system of integro-differential equations involved in the search for the equations of motion for a system of electrons.
theory’ (Corry, 2004, 273) and even proposed, in one specific context,\textsuperscript{50} to substitute the mechanical approach derived from the theory of gases by an electromagnetical one, Hilbert remained committed to the atomistic-mechanical outlook. The 1913 lecture course on the molecular theory of matter even contained the statement that atomism is to be considered as a necessary consequence of the principle of relativity.\textsuperscript{51}

It was in his lecture course on ‘electromagnetic oscillations’ of the winter semester of 1913/1914 that Hilbert made explicit for the first time his view that the whole of physics arises from electrodynamics, rather than from mechanics\textsuperscript{52} and that ‘relativistic mechanics, or four-dimensional electrodynamics’ was ‘on the verge’ of being ‘assimilated by mathematics’ (Hilbert 1913/1914 quoted in Corry, 2004, 280).\textsuperscript{53} However, Hilbert also acknowledged not only that ‘[w]e are really still very distant from a full realization of [...] reducing all

\textsuperscript{50} Corry refers to the fact that Hilbert, ‘in order to describe the conduction of electricity in metals, [...] developed a mechanical picture derived from the theory of gases, which he then later wanted to substitute by an electrodynamical one’ (Corry, 2004, 272). The original passage in which this idea appears is the following: ‘Unser nächstes Ziel ist, eine Erklärung der Elektrizitätsleitung in Metallen zu gewinnen. Zu diesem Zwecke machen wir uns von der Elektronen zunächst folgendes der Gastheorie entnommene mechanische Bild, das wir später durch ein elektrodynamisches ersetzen werden’ (Hilbert 1913 quoted in Corry, 2004, 272, f. 151).

\textsuperscript{51} ‘Es sind somit die zum Aufbau der Physik unentbehrlichen starren Körper nur in den kleinsten Teilen möglich; man könnte sagen: das Relativitätsprinzip ergibt also als notwendige Folge die Atomistik’ (Hilbert 1913 quoted in Corry, 2004, 274, f. 157).

\textsuperscript{52} Es scheint indessen, als ob die theoretische Physik schliesslich ganz und gar in der Elektrodynamik aufgeht, insofern jede einzelne noch so spezielle Frage in letzter Instanz an die Elektrodynamik appellieren muss’ (Hilbert 1913/1914 quoted in Corry, 2004, 280, f. 169).

\textsuperscript{53} ‘Nun glaube ich aber, dass es der höchste Ruhm einer jeden Wissenschaft ist, von der Mathematik assimiliert zu warden, und dass auch die theoretische Physik jetzt im Begriff steht, sich diesen Ruhm zu erwerben. In erster Linie gilt dies von der Relativitätsmechanik oder vierdimensionalen Elektro-dynamik,’ (Hilbert 1913/1914 quoted in Corry, 2004, 280, f. 168). It may here be remarked that Hilbert also permitted himself to claim that ‘one could [now] divide mathematics [into] one-dimensional mathematics, i.e. arithmetic, then function theory, which essentially limits itself to two dimensions; then geometry, and finally four-dimensional mechanics’ (Hilbert 1913/1914 quoted in Corry, 2004, 280) (‘[M]an könnte [...] die Mathematik einteilen in die eindimensionale Mathematik, die Arithmetik, ferner in die Funktionentheorie, die sich im wesentlichen auf zwei Dimensionen beschränkt, in die Geometrie, und schliesslich in die vierdimensionale Mechanik’ (Hilbert 1913/1914 quoted in Corry, 2004, 280, f. 169)).
physical phenomena to the \( n \)-electron problem’ (Hilbert 1913/1914 quoted in Corry, 2004, 281), but also that if ‘one can do little with the \( n \)-body problem, it is even less fruitful to proceed on the basis of the treatment of the \( n \)-electron problem’. These difficulties of the reductionist program forced Hilbert to make two concessions. Firstly, ‘instead of a mathematical foundation based on the equations of motion of the electrons, we still need to adopt partly arbitrary assumptions, partly temporary hypothesis [and] certain very fundamental assumptions that we later need to modify’ (Hilbert 1913/1914 quoted in Corry, 2004, 281).

Hilbert did not describe these three kinds of assumptions, but it seems plausible that he referred to the atomistic hypothesis and the earlier mentioned condition of equiprobability. Secondly, Hilbert had it that with regard to the \( n \)-electron problem and thus, more generally, to the further development of electrodynamics, ‘the point for us is rather to silence [verstümmeln] the [problem], [to] integrate the simplified equations and to ascend from their solutions to more general solutions by means of corrections’ (Hilbert 1913/1914 quoted in Corry, 2004, 281).

Where the phenomenological approach and its partial differential equations were merely a first step in the understanding of natural phenomena, the foundational problems that accompanied the atomistic approach and its probabilistic methods required further investigation into the (‘molecular’ and ‘electron’) theory of the structure of matter. Hilbert hoped that the novel non-probabilistic mathematics of this theory-of-matter-approach ‘would reveal the identity of the axiomatic model and physical reality’ (Schirrmacher, 2003, 11). But the

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54 ‘Von der Verwirklichung unseres leitenden Gedankens, alle physikalischen Vorgänge auf das \( n \)-Elektronenproblem zurückzuführen, sind wir freilich noch sehr weit entfernt’ (Hilbert 1913/1914 quoted in Corry, 2004, 281, f. 171).

55 ‘So wenig man schon mit dem \( n \)-Körperproblem arbeiten kann, so wäre es noch fruchtloser, auf die Behandlung des \( n \)-Elektronenproblems einzugehen’ (Hilbert 1913/1914, quoted in Corry, 2004, 281, f. 170).


57 ‘Es handelt sich vielmehr für uns darum, das \( n \)-Elektronenproblem zu verstümmeln, die vereinfachte Gleichungen zu integrieren und von ihren Lösungen durch Korrekturen zu allgemeineren Lösungen aufzusteigen’ (Hilbert 1913/1914 quoted in Corry, 2004, 281, f. 170).
more Hilbert tried to come to terms with the fundamental $n$-electron problem, the more he seems to have despaired at the insurmountable role of probability theory, as a kind of mathematics rather than a physical discipline, and, more in specific, of the probabilistic method of mean values.


The pre-history of Hilbert’s two notes entitled the ‘Foundations of physics’ of 1915 and 1917 in which he himself thought to have accomplished the formulation of the physics ‘in general, rather than just of a particular kind of phenomena’ (Corry, 2004, 33) is marked by two central events. Firstly, the ‘belated’ adoption of Born’s reformulation, in 1912 and 1913 (see Corry, 2004, 309-315), of Gustav Mie’s (1868-1957) electromagnetic theory of the structure of matter in which the existence of electrons and, therefore, of atoms and matter in general, is mathematically derived from the electric field (e.g. Mie, 1912a; Mie, 1912b; see also Born, 1914; Corry, 1999a; Corry, 1999b; Corry, 2004).

58 The ‘First Communication’ was originally delivered (as a talk entitled ‘The fundamental equations of physics’) to the Göttingen Academy of Science in November 1915, submitted to its Proceedings on 19 November of that year and published in March 1916 (Hilbert, 1916 [2009]).

59 The ‘Second Communication’ underwent several major revisions before being submitted to the Göttingen Academy of Science on 23 December 1916 and appearing in 1917 (Hilbert, 1917a [2009]).

60 As Corry has noted: ‘[T]he lecture notes of the courses Hilbert taught in the winter semester of 1912/1912 (‘Molecular theory of matter’) and in the following semester (‘Electron theory’) in spite of their obvious, direct connection […] show no evidence of a sudden interest in Mie’s theory or in the point of view developed in it […] Possible, this was connected to the fact that Mie’s strong electromagnetic reductionism was contrary to Hilbert’s current views, which also favored reductionism, but still from a mechanistic perspective at the time’ (Corry, 2004, 310). See also Renn & Stachel (1999, 8).

61 Reflecting on Born’s reformulation of Mie’s theory, Corry writes that ‘[w]hereas Lorentz’s theory of the electron was based on certain hypotheses concerning the nature of matter (e.g. the rigidity of the electron) […] Mie attempted to derive mathematically the existence of electrons […] from a modified [i.e. ‘non-linear’] version of the Maxwell equations, i.e. without starting from any particular conception concerning the nature of physical phenomena’ (Corry, 2004, 312).
Secondly, the negotiation of Albert Einstein’s (1879-1955) generalization of the principle of relativity and his theory of gravitation of 1913-1914 (e.g. Einstein & Grossmann, 1913; Einstein, 1914, see also Lehner, 2005; Renn, 2005; Stachel, 1989).\textsuperscript{62} Hilbert’s attempt, in the first note (Hilbert, 1916 [2009]), at a unified field theory of electromagnetism and gravitation was a result of a complex ‘axiomatic synthesis’ of the speculative physical theories of Mie and Einstein (see Renn & Stachel, 1999; Sauer, 1999). In brief, the theory consisted of a generally covariant reformulation of Mie’s theory of matter and Einstein’s theory of gravitation such that both could be derived from a single variational principle for a Lagrangian and the (number of) field equations following from the variational principle would allow for the claim that electromagnetism is an effect of gravitation. Hilbert originally wanted his second note (Hilbert, 1917a [2009]) to be about the physical consequences of his own unified field theory of the first note, but, much in line with the content of his courses of 1916-1917\textsuperscript{63}, the published version solely concerned the general theory of relativity. Because those lectures, given between the years 1919 and 1923, in which Hilbert would return to the status of probability (section 3.2) were written as philosophical reflections on issues related to one of the central topics with which he was occupied in 1916-1917, namely the ‘causality quandary’,\textsuperscript{64} the (background of the) second note is taken up in the following (sub-)section.

\textsuperscript{62} Much has been written on the priority of the discovery of the field equations of general relativity. Where the majority of textbooks spoke of ‘Einstein’s equations’, according to the commonly accepted academic view Hilbert completed the general theory of relativity some five days before Einstein. Corry, Renn and Stachel have shown that Hilbert did not anticipate Einstein because the first set of the proofs of his 1915 paper ‘is not generally covariant and does not include the explicit form of the field equations of general relativity’ (Corry, Renn & Stachel, 1997). In this context see, for instance, also Earman & Glymour (1978), Lehner, Renn & Schemmel (2012), Mehra (1973, chapter 7) and Stachel (1999).

\textsuperscript{63} Hilbert taught a lecture course entitled ‘Foundations of physics I (general relativity)’ in the summer semester of 1916 and one entitled ‘Foundations of physics II (general relativity)’ in the winter semester of 1916/1917 (see Corry, 2004, 451).

\textsuperscript{64} Hilbert delivered a lecture with this title (‘The principle of causality’ in 1917 (Hilbert, 1917b [2009])).
3.1 The ‘causality quandary’

Hilbert’s first note on the ‘Foundations of physics’ of March 1916 contained two fundamental axioms: ‘Axiom I: Mie’s axiom of the world-function’ and ‘II. Axiom of general covariance’. The so-called *Leitmotiv* (‘Theorem I’) of the theory stated that in so far as it is the case that ‘in the system of \( n \) differential equations on \( n \) variables […] four of these equations are always a consequence of the other \( n - 4 \)’ the equations for the four electromagnetic potentials \( \Phi \) are the consequence of the equations for the ten gravitational potentials \( \phi \). Because Theorem I ‘shows that Axioms I and II can only provide ten essentially independent equations for the 14 potentials’, Hilbert argued that

‘in order to keep the deterministic character of the fundamental equations of physics, in correspondence with Cauchy’s theory of differential equations, the requirement of four further non-invariant equations to

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65 The first axiom consisted of a variational argument for a scalar Hamiltonian (\( H' \)) (or ‘Lagrangian’ (\( L' \)) (world)function \( H \) with space-time coordinates (or world-parameters) – i.e. (Einstein’s) ten gravitational potentials with their first and second derivatives and (Mie’s) four electromagnetic potentials with their first derivatives – determining the coordinates of a four-dimensional manifold. Hilbert used the Hamiltonian ‘to derive the basic equations of the theory, starting from the assumption that, under infinitesimal variations of its parameters, the variation of the integral \( \int H \sqrt{g} \, d\omega \) […] vanishes for any of the potentials’ (Corry, 1999a, 518).

66 The second axiom postulated that the ‘world-function’ \( H \) (see footnote 63) is generally covariant – i.e. retains its form with respect to arbitrary coordinate (or ‘world-parameter’) transformations.

67 In brief, Hilbert described this theorem as follows: ‘[I]n the system of \( n \) differential equations on \( n \) variables […] four of these equations are always a consequence of the other \( n - 4 \), in the sense that four linearly independent combinations of the \( n \) differential equations and their total derivatives are always identically satisfied’ (Hilbert, 1916 [2009], 30-31).

68 Hilbert formulated the theorem as follows: ‘Theorem I. Let \( \mathcal{J} \) be a scalar expression of \( n \) magnitudes and their derivatives that is invariant under arbitrary transformations of the four world-parameters, and let the Lagrange variational equations corresponding to the \( n \) magnitudes be derived from the integral \( \int \mathcal{J} \sqrt{g} \, d\omega = 0 \). Then, in the system of \( n \) differential equations on \( n \) variables obtained in this way, four of these equations are always a consequence of the other \( n - 4 \), in the sense that four linearly independent combinations of the \( n \) differential equations and their total derivatives identically satisfied’ (Hilbert 1916 [2009], 30-31, original translation).

69 Here, ‘Hilbert, 1916’ refers to the ‘December Proofs’ of Hilbert’s first note that have been preserved in Hilbert’s Nachlass (DHN 634).
Although the published version of the first note assumed the possibility of using generally-covariant field equations not supplemented by coordinate restrictions in spite of Einstein’s ‘hole-argument’\textsuperscript{70} (Howard, 1992; Howard & Norton, 1993; Iftime & Stachel, 2006),\textsuperscript{71} in this passage from the December Proofs Hilbert emphasized the necessity of introducing four additional non-covariant equations (or differential relations) in order to obtain a unique determination of the evolution of all 14 variables. The Axiom III (‘Axiom of Space and Time’) that contained these four equations thus allowed Hilbert to extract a physically acceptable ‘Cauchy-determinate structure within an otherwise generally covariant theory’ (Brading & Ryckman, 2012, 188).

After the appearance of the published version of the first note, Hilbert repeatedly made it clear that he ‘was still in quandary about how to treat the causality issue’ (Renn & Stachel, 1999, 73). For example, in the lecture course ‘The foundations of physics, I’ of the summer semester of 1916, Hilbert noted that the status of the ‘causality principle’ (\textit{Kausalitätsprinzip}) within generally-covariant physics was ‘not yet clarified’\textsuperscript{72} (Hilbert 1916 quoted in Renn & Stachel, 1999, 7) by his own unified field theory. The newly found solution to the problem of causality that Hilbert brought to the fore in his second note on the ‘Foundations of physics’ of 1917 had already appeared in his undated ‘Causality lecture’ of 1917 (Hilbert, 1917a \citeyear{2009}; Hilbert, 1917b \citeyear{2009}). Remarkably, the solution was formulated in terms of a revision of Kantian epistemology

\textsuperscript{70} After having realized that both the field equations as well as the law of energy-momentum conservation of the \textit{Entwurf} theory of 1913 were not generally covariant, Einstein devised a highly complex argument (the ‘hole argument’) which was to prove that there cannot exist generally covariant field equations that completely determine the field.

\textsuperscript{71} It may here be remarked that Einstein’s final presentation of his theory of gravitation proved that ‘generally covariant field equations do not need to be supplemented by additional, non-covariant, equations in order to arrive at a satisfactory theory of gravitation; and the same should be the case when electromagnetism is included’ (Renn & Stachel, 1999, 73).

\textsuperscript{72} Reflecting on his field equations for gravitation and electromagnetism, Hilbert said that ‘[d]ies sind 14 Gleichungen für die 14 unbekannten Funktionen […] Das Kausalitätsprinzip kann erfüllt sein, oder nicht (Die Theorie hat diesen Punkt noch nicht aufgeklärt)’ (Hilbert 1916 quoted in Renn & Stachel, 1999, 74).
in light of the principle of objectivity implicit in Einstein’s general relativity (see Brading & Ryckman, 2012, section 8.6; Hallet, 1994; Majer, 1993; Majer & Sauer, 2005; Majer & Sauer, 2006; Ryckman, 2008).

3.1.1 The new solution of the ‘Causality lecture’ and the second note on the ‘Foundations of physics’

Both the ‘Causality lectures’ and the second note opened with the explicit statement that causality cannot be restored on the basis of the (mathematically false)\(^7\) idea of postulating four additional non-covariant equations (see Hilbert, 1917a [2009], Hilbert, 1917b [2009], see also Renn & Stachel, 1999, 74-75). The starting point of the new solution was the distinction between two parts of the causality problem; the issue of, firstly, causal ordering (I-CO) and, secondly that of univocal determination (I-UD). Where the I-CO pertained to the fact that ‘a conflict with the [experienced] causal order [arises] if two world-points [Welt-punkte] lying along the same time-like curve [Zeitlinie], and standing in [a] relation of cause and effect, can be transformed so that they become simultaneous (Brading & Ryckman, 2007, 33), the I-UD amounted to the situation that from the knowledge of physical magnitudes in the present past, it is no longer possible to univocally deduce their values in the future. Hilbert solved the I-CO by introducing ‘proper coordinate systems’ [eigentliche Koordinatensystem] the transformations among which do not reverse the temporal order of cause and effect (see Brading & Ryckman, 2007, section 6.5) and proposed to come to terms with the I-UD by means of a redefinition of physically meaningful statements as those statements for which it holds that they are generally covariant and satisfy the requirement of causality inherent in proper coordinate systems.

This fundamental notion of ‘proper’ coordinate systems must be understood with reference to Hilbert’s new distinction between ‘being a possible object of

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\(^7\) This characterization of the ‘old’ solution can only be found in the ‘Causality lecture’. It does not appear in the second note or in any later publication (see Renn & Stachel, 1999, 75).
experience’ and ‘being a possible object of physics’, that is, between ‘Kantian’ objects for which holds that they exist in space and time and satisfy the condition of causality and ‘non-Kantian’ objects for which these requirements no longer hold. These (‘non-Kantian’) possible objects of physics are governed by the new ‘superordinate criterion of physical objectivity’, namely that of general covariance as the regulative ideal for the search for the fundamental laws of physics:

‘[A] more far reaching objectification is necessary to be obtained by emancipating ourselves from the subjective moments of human intuition with respect to space and time. This emancipation, which is at the same time the high-point of scientific objectification, is achieved in Einstein’s theory; it means a radical elimination of anthropomorphic slag, and leads us to that kind of description of nature which is independent of our senses and intuition’ (Hilbert 1921 quoted in Majer, 1998b, 55).

Hilbert was now able to argue that the conflict between general covariance and the experienced causal ordering of events, that is, the I-CO is only a seeming problem. For ‘it is not the nature of our cognitive experience [...] that leads to the requirement [of] proper coordinate systems [and coordinate restrictions] – it has to do not with the possible objects of physics [...] but with the possible objects of experience [...] as these are represented standing in causal relationships within spatio-temporal empirical intuition’ (Brading & Ryckman, 2007, 35). Hilbert preserved univocal determination by means of the reformulation of physically meaningful statements ‘in terms of its unique determination by a Cauchy problem’, thereby turning the I-UD into its own solution on the basis of understanding it as satisfying causality as a constraint upon human understanding.

Hilbert’s second note of 1917 formulated the view of physics as the project of providing a non-anthropomorphomorphic account of nature – one in which general

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74 Although there is some debate as to the Kantian ‘nature’ of Hilbert’s treatment of the issue of causality, for example, Brading and Ryckman (2012) Majer & Sauer (2006, 2005) provide convincing arguments for this reading. Furthermore, Hilbert himself explicitly alluded to Kant at several places in his lectures of the years 1915–1923 (Hilbert, 2009). For somewhat more general discussions of Hilbert’s views on Kant intuition’ see, for instance, Friedman (2012), Majer (1995), Tieszen (1989, chapter 1),
covariance functions as the ‘concept of reason that transcends all experience and through which the concrete is completed so as to form a [systematic unity]’. Given that this gradual emancipation from the possible objects of experience towards the possible objects of physics is never fully attainable, Hilbert accepts that ‘proper coordinate systems’ – accounting for the restoration of causality and univocal determination – ‘is a bit of “anthropomorphic slag” […] that remains as a condition of possible physical experience [even though it] is no longer a condition governing the ideal conception in physics of a mind-independent world as the object of physical inquiry’ (Brading & Ryckman, 2007, 49).

It is this combination of the adoption and modification of Kantian epistemology that characterized Hilbert’s (semi-popular) contributions to the discussion of general relativity from a more comprehensive, philosophical, point of view during the years 1917-1923. There were two topics that Hilbert considered to be of central importance for this discussion, namely that of general covariance as the ‘principle of objectivity’ for physics, and that of the notion of probability. The link between these two topics is the problematic status of irreversibility (in statistical mechanics) vis-à-vis the time-reversal covariance of the fundamental laws of physics. Put differently, it is that of the difficulties which probability causes for the attempt of reconciling general covariance and univocal determination with reference to the reformulated notion of causality. It is crucial to observe that Hilbert set out to circumvent this situation by means of approaching probability as a (non-mathematical) feature of human thought that accompanied his field equations as one of its anthropomorphic ‘accessorial principles’.

3.2 Probability as an ‘anthropomorphic accessorial principle’ of the new physics, 1921-1923

The places in his oeuvre of the early-1920s in which Hilbert discussed the role of probability in the new physics can be divided into two parts. Firstly there were part two (‘Die Landläufige Auffassung von der Physik und ihre Berichtigung’) and three (‘Fragen philosophisches Charakters’) of his Natur
Hilbert’s Natur und Mathematisches Erkennen

Hilbert commenced his Natur und Mathematisches Erkennen by summarizing, in terms of the axiomatic method, the distinction between physical theories prior to and physical theories posterior to general relativity. Firstly, where the first provide ‘immediately discernible’ (‘unmittelbar [zu] konstatieren’) general descriptions of experience, the second aim to gradually turn these descriptions of intuitively presented facts into a logical-formal system of natural relations (‘Naturzusammenhänge’) that, in the end, is free of subjectivity. It is the requirement of general covariance that embodies the central step along the (axiomatic) path from the intuition of space, time and causality – which, as necessary conditions of the possibility of experience, merely reflect the subjective origin of cognition in sensible experience (Ryckman, 2008) – toward the systematically unified and observer-free description of nature. Secondly, where physical theories prior to general relativity satisfied Cauchy determination in so far as, on the one hand, the initial data can be freely chosen without explicit regard to special constraints and, on the other hand, the relation of causality is fully determinate, for instance, because of ‘the fixed background of space, and the unique global direction of time’ (Brading & Ryckman, 2007, 48), this no longer holds in the case of in generally covariant physics (see section 3.2.2). In brief, Hilbert acknowledged that several assumptions have to be made in order to resolve this situation; firstly, that natural phenomena obey and can be described

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75 This book contains the lectures that Hilbert delivered under this header in Gottingen between the years 1919-1920.
76 Hilbert delivered this lecture on ‘die Gesetze des Zufalls’ on February 2, 1920. It is found in Hilbert’s Nachlass, Cod. Ms. Hilbert 599.
77 Hilbert’s ‘Vorlesung’ entitled ‘Statistische Mechanik’ which was held in the summer semester of 1922 was ‘ausgearbeitet’ by Lothar Nordheim. It is found in Hilbert’s Nachlass, Cod. Ms. Hilbert 565.
by means of simple mathematical laws,\textsuperscript{78} secondly, given that ‘the simplicity of the laws of nature do not in any sense imply the simplicity of natural phenomena’ (Hilbert, 1992, 69),\textsuperscript{79} that ‘the same happens under similar circumstances’ (Hilbert, 1992, 63),\textsuperscript{80} and, thirdly, in so far as ‘the relation between cause and effect […] is not reflected in the mathematical equations’ (Hilbert, 1992, 71),\textsuperscript{81} that ‘the cause of the foregoing [is] the effect of the subsequent’ (Hilbert, 1992, 71).\textsuperscript{82} It is this last assumption that impelled Hilbert to further investigate not only the ‘meaning of ‘time-relationality [‘des Zeitverhältnisses’] for causality’ (Hilbert, 1992, 71),\textsuperscript{83} but also the question of whether physics is marked by a certain direction of time – and, if so, what this implies for its treatment of irreversible physical processes. Because these processes have ‘become actual in physics […] especially since the formulation of the law of increasing entropy by Clausius [which has been] explained […] as a […] probability law’\textsuperscript{84} (Hilbert, 1992, 72, my emphasis), Hilbert, in chapter eight of *Natur und Mathematisches Erkennen*, returned to the issue of probability with the aim of coming to grips with its (conditional) meaning for physics at large.\textsuperscript{85}

### 3.2.1.1 Probability, irreversibility and time-reversal covariance

Reflecting on the first two of the three abovementioned assumptions, Hilbert remarked that even though the laws of nature – formulated, as they are, in terms of differential equations – are generally covariant, they are dependent on ‘repetitions’ (‘*Wiederholungen*’) and ‘similarities’ (‘*Ähnlichkeiten*’) in nature.

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\textsuperscript{78} Hilbert justified these two points with reference to the widespread idea of the pre-established harmony between being and thought (Pyenson, 1985).

\textsuperscript{79} ‘[D]ie Einfachheit der Naturgesetze bedeutet […] noch keineswegs die Einfachheit der Naturgeschehens’ (Hilbert, 1992, 69).

\textsuperscript{80} ‘[U]nter gleichen Umständen [geschieht] stets Gleiches’ (Hilbert, 1992, 63).

\textsuperscript{81} ‘[D]as Verhältnis von Ursache und Wirkung […] kommt ja in den mathematischen Gleichungen gar nicht zum Ausdruck’ (Hilbert, 1992, 71).

\textsuperscript{82} ‘[D]ie Ursache das frühere [ist] die Wirkung das spätere’ (Hilbert, 1992, 71).

\textsuperscript{83} ‘[D]ie Bedeutung des Zeitverhältnisses für die Kausalität’ (Hilbert, 1992, 71).

\textsuperscript{84} Hilbert spoke of the question of the (ir)reversibility of physical processes which, ‘insbesondere seit der Aufstellung des Gesetzes von der Vermehrung der Entropie durch Clauius in der Physik aktuell geworden ist [und] als ein […] Wahrscheinlichkeitsgesetz erklärt [ist]’ (Hilbert, 1992, 71).

\textsuperscript{85} Hilbert wrote that ‘[w]e have to come to a somewhat more precise understanding of the meaning of the notion of probability for physics’ (‘[W]ir müssen […] uns […] mit der Bedeutung des Wahrscheinlichkeitsbegriffs für die Physik etwas näher befassen’) (Hilbert, 1992, 72).
Hilbert then introduced the more specific distinction between ‘repetitions under similar circumstances’ (‘Wiederholungen gleicher Umstände’) and ‘repetitions of (average) sizes’ (‘Wiederholungen derselben Grössenbestimmungen’) – the latter of which is clarified in the following example: ‘[T]he fixed stars all have approximately the same size and also about the same speed; all the grains of sand are almost similar; bodies consist of a large amount of almost similar molecules [etc]’ (Hilbert, 1992, 73). Hilbert wrote that in so far as this kind of repetition is wholly independent of ‘the validity and also of the criteria of the discoverability of the differential equations’, it is to be understood as a ‘feature of nature’ (‘Eigenschaft der Natur’ – i.e. as ‘a very special, remarkable fact of experience of such a fundamental character that it gives occasion for very new methods and theories in physics that are indispensable for the theoretical mastery of the appearances’, namely ‘the law of chance’ (‘Das Gesetz des Zufalls’) of probability theory. Given that the ‘probability laws’ (‘Wahrscheinlichkeitsgesetze’) are, thus, a necessary component of a complete description of reality in the form of an axiomatized ‘framework of concepts’ (‘Fachwerk von Begriffen’) the problem becomes that of explaining how they relate to the ‘real laws of nature’ (‘eigentliche Naturgesetze’).

After discussing a rather straightforward case of equiprobability, Hilbert summed up the features that distinguish probability laws from the laws of nature. Firstly, they cannot lay claim to the ‘exceptionless validity’ that characterizes the laws of nature – and this in so far as they cannot, secondly, predict the results to be explained, thirdly, prove that ‘what happens in almost all cases will actually be observed’ or, fourthly, rule out exceptions. If Hilbert, notwithstanding these defects, acknowledged that the differential equations

86 ‘[D]ie Fixsterne haben alle ungefähr dieselbe Grösse und auch ungefähr die gleichen Geschwindigkeiten; die Körner des Sandes sind einander nahezu gleich; die Körper bestehen au seiner grossen Zah; von nahezu gleichen Molekülen [etc]’ (Hilbert, 1992, 73).

87 ‘[D]er Gültigkeit und auch von den Bedingungen der Auffindbarkeit der Differentialgleichungen’ (Hilbert, 1992, 73).


89 ‘[W]as in fast allen Fällen eintritt auch tatsächlich beobachtet wird’ (Hilbert, 1992, 75).
expressing the laws of nature have to be combined with probability laws, he is quick to point out that the ‘enormous mathematical difficulties [that this] engenders’ are related to the philosophical problem of the ‘fundamental permissibility of the probability-theoretical elucidation of physical laws’. Hilbert attempted to confront this twofold (mathematical cum physical) problem by distinguishing between the mathematical ‘content’ and the epistemological status of probability statements; these give quantitative expression to the assumption that ‘what almost always happens, is actually constantly found’, but it simultaneously holds that probability statements in physics are neither of the level of objective laws of nature nor of mathematical statements that are presumably right (see Hilbert, 1993, pp. 76-77; Hilbert, 1920, p. 1; Hilbert, 1922, p. 103). More in specific, there are mathematical statements, such as that \(2\sqrt{2}\) is an irrational number, which are presumably right in the purely mathematical sense of being not yet proven and there are probabilistic statements which are presumably right in a non-mathematical sense. It was on the basis of this distinction that Hilbert hoped to resolve the ‘unclarities’ (‘Unklarheiten’) and ‘mistakes’ (‘Irrtumme’) connected to the difference between those explanations of nature (‘Naturerklärungen’) expressed in terms of the laws of chance and those expressed in differential equations.

It is at this point that Hilbert returned to the initial starting point for his discussion of the meaning of probability, namely that of the tension between the fact that, on the one hand, in so far as the principle of general covariance allows for coordinate transformations that correspond to the reversal of time, it implies time-reversal covariance of the laws of nature and, on the other hand, a direction of time is implied not only in irreversible processes found in

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90 The whole passages reads as follows: ‘Die enormen mathematischen Schwierigkeiten, welche hieraus entspringen, warden vielfach nich beachtet, und es ist eine verbreitete Meinung, dass die heute noch bestehende Unvollkommenheit in der Begründung der kinetischen Gastheorie ganz auf philosophischem Gebiet liege, nämlich die grundsätzliche Zulässigkeit der wahrscheinlichkeitstheoretischen Deutung physikalischer Gesetze betreffen’ (Hilbert, 1992, 76).

91 ‘[D]as, was fast immer statthat, [wird] in Wirklichkeit stets angetroffen’ (Hilbert, 1921 [2009], 389).

92 Reflecting on the abovementioned example, Hilbert had it that ‘er ist seinem Inhalte nach eine rein mathematische Behauptung, die sich nur wegen der entgegenstehenden mathematischen Schwierigkeiten vorläufig nicht beweisen lässt’ (Hilbert, 1922, p. 103).
nature, but also in the third assumption (the causality principle accounting for Cauchy determination) of the new physics mentioned earlier. Hilbert solved the conflict between time-reversal covariance and everyday perceptions (as it first arose in the context of Boltzmann’s famous $H$-theorem) by means of the argument that irreversibility arises solely from their application of probabilistic statements and is, thus,\(^{93}\) not inherent in the fundamental laws of physics – i.e. ‘not an objectively existing law in nature’ (‘kein objektives in der Natur bestehendes Gesetz’ (Hilbert, 1922, p. 104). On the contrary, Hilbert argued that irreversibility is a result of the ‘anthropomorphic point of view’ (‘antropomorphen Standpunkt’) connected to the selection of initial- and boundary conditions (‘Anfangs- und Randbedingungen’) accompanying the probabilistic description of the irreversible behavior observable in the (time)-evolution of macro-states.\(^{94}\) Here, it is interesting to observe, with regard to both the status of ‘anthropocentric irreversibility’ reflected upon in this sub-section and the content of the following section (section 4), that Hilbert attributed to (the new) quantum mechanics the achievement of guaranteeing the complete reversibility of the elementary processes of physics (see Hilbert, 1922, p. 104).

3.2.2 **Hilbert's definition of probability as an 'accessorial principle'**

Hilbert concluded his discussion of the probabilistic origin, so to say, of irreversibility and the idea of a privileged direction of time with the following statement:

‘[T]he fundamental principle of probability theory, [has it] that what almost always happens is actually constantly found [...] When we look at this fact in this [particular] context, realized by nature, as a mark of a direction of time [...] I would like to regard this as a feature of our

\(^{93}\) Hilbert wrote that the explanations of irreversible processes on the basis of the statistical method the asymmetry concerning the past and future arise merely as a result of the choice of initial-states and -conditions.

\(^{94}\) In line with the remark in the foregoing footnote, Hilbert, for instance, noted that the chosen ‘initial- and boundary conditions [are] not found in reality’ (‘Anfangs- und Randbedingungen [warden] nicht [...] in der Wirklichkeit vorgefunden’) (Hilbert, 1992, 81).
thought [or of] our comprehension instead of as an object[ive] feature of nature’ (Hilbert, 1923 [2009], 389, f. 44).

If Hilbert, in *Natur und Mathematische Erkennen*, had not definitely made up his mind as to the precise characterization of probability, in the second part of the 1923 lecture ‘Grundsätzliche Fragen der modernen Physik’ he wrote that ‘I [Hilbert] would like to call everything that has to be added to the world equations in order to understand the events in inanimate nature “accessorial” for short’ and, then gave, ‘constancy’ (‘Konstanz’), ‘stability’ (‘Stabilität’), ‘periodicity’ (‘Periodicität’), and the assumption of the applicability of the principles of probability as examples. Hilbert explained the notion of ‘accessorial’ by comparing its epistemological role within physics prior and posterior to general relativity:

‘[That] we need initial conditions and constraints in order to obtain a determinate solution of the world equations [...] means that these equations only allow us to predict future events if we know enough about the present state of affairs. In this respect, the world equations resemble Newton’s equations [...] But there is an important difference [which] becomes visible if we ask: Do we need accessory laws of nature? The answer in the case of the ‘world equations’ is that we do not, but in Newton’s case the answer is that we do’ (Majer & Sauer, 2006, 219).

The reason for this difference is that the world equations – representing, as they do, the axiomatic foundation for all laws of nature – ‘permit propositions about the present state of nature without the support of accessorical laws, whereas Newton’s equations do not’ (Majer & Sauer, 2006, 220).

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96 ‘Ich [Hilbert] möchte Alles, was noch zu den Weltgleichungen hinzugefügt werden muss, um die Geschehnisse in der leblosen Natur zu verstehen, kurz accessorisch nennen’ (Hilbert, 1923 [2009], 408).
That ‘there are no real accessorial laws of nature’ (Hilbert, 1923 [2009], 415)\(^{97}\) in the new physics does not mean that it does not stand in need of so-called ‘accessorial principles’ – such as the applicability of probability. In fact, these principles account for the very possibility of applying the world equations to nature. Importantly, however, Hilbert was of the opinion that the ‘accessorial principles’ ‘do not have the math\[ematical\] character of new equations, but are of a general character, [and] are connected to our thinking as such and [to] our attitude towards nature’\(^{98}\) (Hilbert, 1923 [2009], 416). In other words, they are accessorial ideas of the human mind which belong to the ‘anthropomorphic’ realm of (the conditions of) possible physical experience in so far as they have no ‘reference in inanimate nature’ (Majer & Sauer, 2006, 220) whatsoever.

4. **Fourth period. The implicit definition of probabilities through the quantum mechanical axioms**

Hilbert first touched upon the ‘old quantum theory’ of, among others, Bohr in his 1912 course on radiation theory, but it was only some ten years later that he would explicitly consider this theory in his ‘Mathematische Grundlagen der Quantentheorie’ (Hilbert, 1922/1923 [2009]) and in a ‘Vorlesung’ on ‘Statistical Mechanics’ of 1922 (Hilbert, 1922, pp. 78-96) and some fourteen years later that he would devote himself to the new quantum mechanics of Werner Heisenberg, Max Born, Pascual Jordan, Paul Dirac, Erwin Schrödinger and Norbert Wiener in his ‘Mathematische Methoden der Quantentheorie’ (Hilbert, 1927/1927 [2009]). It is important to observe that around the year 1922 Hilbert introduced quantum theory in the context of the ‘paradoxes’ and ‘contradictions’ in the foundations of the statistical mechanics and the ‘discrepancies with experience’ of the theories of Maxwell, Boltzmann and Willard Gibbs (see Hilbert, 1922, p. 78). The discovery of these problems were said to provide the impetus for (‘gab den Anstoss zu’ (ibid., p. 78))\(^{99}\) the ‘most modern

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\(^{97}\) ‘[E]s \[gibt\] keine eigentlichen accessorischen Naturgesetze’ (Hilbert, 1923 [2009], 415).


\(^{99}\) For example, Hilbert wrote that the ‘paradoxen Ergebnisse der klassischen Theorie [der] erste Anstoss zur Einführung der Quantentheorie’ (Hilbert, 1922, pp. 96-97).
extension of statistical thoughts, for quantum theory'. Given that the classical problems were due, first and foremost, to the assumption of equiprobability ('Aequipartitionssatz'), the superiority ('Überlegenheit') of the quantum theory followed from the fact that its discrete structure implied the dismissal of the validity of this subjective 'Satz'.

The 1926/1927 announced the need for an axiomatic presentation of the four different approaches to the new quantum theory as put forward in the years 1925-1926; the matrix mechanics of Heisenberg (and Born and Jordan), the $q$-calculus of Dirac, the wave-mechanics of Schrödinger and the operational (or operator) calculus of Born-Wiener (e.g. Lacki, 2000, section 2.2). This much was initiated by Hilbert in his 1928 paper 'Über die Grundlagen der Quantenmechanik', written with his physical assistants John von Neumann and Lothar Nordheim (Hilbert, Von Neumann & Nordheim, 1928), and further developed by Von Neumann in his *Mathematische Grundlagen der Quantenmechanik* of 1932 (e.g. Mehra & Rechenberg, 2000, chapter 3). Their contribution, essentially, consisted of the formal unification of several apparently separate formulations of the theory by means of the application of the axiomatic method – with this axiomatization being a source of discovery or performative presentation rather than retrospective systematization of the general theory (see Lacki, 2000, 297-298).

The physical starting point of Hilbert, Von Neumann and Nordheim’s paper was the acknowledgment, put forward with reference to ‘der gewöhnlichen Mechanik’ (Hilbert, Von Neumann & Nordheim, 1928, 2), of the omnipresence, so to say, of probability-relations (‘Wahrscheinlichkeitsrelationen’) within mechanical systems with certain degrees of freedom. After physical requirements have been put upon these probabilities postulating certain relations between the probabilities, an analytical apparatus is sought which, firstly, fulfills exactly these relations and, secondly, is physically interpretable only by the sake of the abovementioned requirements. This process is said to correspond to the axiomatization of geometry in which all ‘entities’ (points, lines etc.) and their relations are mathematically represented by mathematical entities the features of which can lead to geometrical statements in so far as they

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100 ‘[D]er modernsten Weiterbildung der statistischen Ideen, zur Quantentheorie’ (Hilbert, 1922, p. 78).

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are related in a way similar to the geometrical entities (ibid., 2-3). At the same time, the difficulties attached to quantum mechanics impelled Hilbert to reconsider his views on the axiomatization of physical theories in so far as in this case ‘the formalism has to some extent to lead the way’ (Lacki, 2000, 297) and the axiomatization proceeds backwards. More in specific, Hilbert emphasized that the analytical apparatus is put forward before the presentation of the axiomatic system and the basic physical relations (‘physikalischen Grundrelationen’) can be presented only through the interpretation of the formalism. And this implies that if the analytical apparatus is ‘unambiguously fixed’ (‘eindeutig festliegt’) the physical interpretation, of necessity, can and will be adjusted with a considerable amount of ‘freedom’ (‘Freiheit’) and arbitrariness (‘Willkühr’). Although Hilbert, Von Neumann and Nordheim admitted that their theory could not ground a complete axiomatics (‘eine vollständige Axiomatik begründen’), they did claim that it is ‘through the axiomatization that formerly somewhat vague concepts, such as probability et cetera, lose their mystical character, since they are implicitly defined by the axioms’. 101 Put differently, probability was mathematized in the axiomatization for quantum mechanics, but what probabilities are, or what their physical meaning consists of, still remained an open question.

5. Concluding remarks: brief summary of the sections

The goal of this paper was to provide an account of the place of probability theory within the development of Hilbert’s project of the axiomatization of physics that is able to make sense of the central observation that in the period 1900-1926 Hilbert eventually came to question the very possibility of achieving the goal of the mathematization of probability as a physical discipline in the way described in the famous ‘sixth problem’. Where Hilbert initially regarded probability as a mathematizable physical discipline (section 1) or a mathematical method (section 2), he eventually came to understand it as a non-mathematizable feature of thought (section 3) and finally (section 4) implicitly defined it via the axioms for quantum mechanics, albeit without a fixed physical interpretation. The paper’s analysis of Hilbert’s attempts, in the period between 1900-

101 ‘Durch die Axiomatisierung verlieren die vorher etwas vagen Begriffe, wie Wahrscheinlichkeit und so weiter, ihren mystischen Charakter, da sie dann durch die Axiome implizit definiert sind’ (Hilbert, Von Neumann & Nordheim, 1928, p. 3).
1926, to come to terms with the status of probability theory and the meaning of probability can hopefully not only articulate their fundamental role in his own contributions to the axiomatization of physics. But it could perhaps also shed an interesting light on accepted ideas both on the introduction of probability, as a branch of mathematics and mathematical method (Garber, 1973), into modern physics as well as on Hilbert as the towering proponent of the axiomatization of probability theory.
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