The Empiricist Roots of Hilbert's Axiomatic Approach

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1. Introduction

Hilbert's work on logic and proof theory—among the latest stages in his long and fruitful scientific career—appeared almost two decades after the publication of the epoch-making *Grundlagen der Geometrie*. In spite of the time span separating these two phases of his intellectual development, and given the centrality of the axiomatic approach to both, one might tend to consider the two as different manifestations of one and the same underlying conception. In particular, one runs the risk of examining the *Grundlagen* as an early expression of the ideas developed in Hilbert's work on proof theory. A close historical examination of these works, however, brings to light important differences between them.

In the present article I analyze an often overlooked aspect of the background to Hilbert's work on the foundations of geometry, namely, the empiricist elements that inform it. In order to do so, I describe works of three scientists, published in the late decade of the nineteenth century, which address the question of the role of 'first principles' in physics. The footprints of these works may be then clearly identified in Hilbert's early axiomatic work, and in particular in the *Grundlagen*. This analysis helps understanding the proper context of ideas from which the axiomatic approach originally arose, and it thus opens the way to a more comprehensive analysis of the historical background behind the development of Hilbert's proof theory.

2. Heinrich Hertz

The first book I want to discuss here is Heinrich Hertz's *The Principles of Mechanics Presented in a New Form*. Published posthumously in 1894, the *Principles* is an attempt to clarify the conceptual content and structure of mechanics, seen from an innovative and very original perspective, i.e., leaving the concept of force outside the whole presentation of the discipline. However, in spite of its specificity, Hertz's concern with the appopriate way to approach this particular area of physics was but an expression of his attitude towards what he saw as a more general kind of deficiency affecting other domains of research. His treatment of mechanics implied a more general perspective, from which all physical theories should, in his view, be reexamined. This perspective was explicitly discussed in the introduction to the book—a text that has become widely known and has been thoroughly analyzed in the literature[1]. Generalizing from the problems associated with the concept of force, Hertz wrote:

> Weighty evidence seems to be furnished by the statements which one hears with wearisome frequency, that the nature of force is still a mystery, that one of the chief problems of physics is the investigation of the nature of force, and so on. In the same way electricians are continually attacked as to the nature of electricity. Now, why is it that people never in this way ask what is the nature of gold, or what is the nature of velocity? Is the nature of gold better known to us than that of electricity, or the nature of velocity better than that of force? Can we by our conceptions, by our words, completely represent the nature of anything? Certainly not. I fancy the difference must lie in this. With the terms "velocity" and "gold" we connect a large number of relations to other terms; and between all these relations we find no contradictions which offends us. We are therefore satisfied and ask no further questions. But we have accumulated around the terms "force" and "electricity" more relations than can be completely reconciled amongst themselves. We have an obscure feeling of this and want to have things cleared up. Our confused wish finds expression in the confused question as to the nature of force and electricity. But the answer which we want is not really an answer to this question. It is not by finding out more and fresh relations and connections that it can be answered; but by removing the contradictions existing between those already known, and thus perhaps by reducing their number. When these painful contradictions are removed, the question as to the nature of force will not have been answered; but our minds, no longer vexed, will cease to ask illegitimate questions.[2]

Hertz described physical theories as "images" (*Bilder*) that we form for ourselves of natural phenomena. He proposed three criteria to evaluate among several possible images of the same phenomenon: permissibility, correctness, and appropriateness. An image is permissible, according to Hertz, if it does not contradict

1. For recent discussions see Baird et al. 1997; Lützen 1995.
2. Hertz 1956, 7-8. In what follows, all quotations refer to this English translation.

the laws of thought. A permissible image is correct for Hertz if its essential relations do not contradict the relations of external things. In fact, Hertz actually defined an image by means of the requirement that its "necessary consequents .. in thought are always the images of the necessary consequents in nature of the things pictured. (p. 1)"

But given two permissible and correct images of one and the same thing, it is by considering the appropriateness of each that Hertz proposed to assess their relative value. The appropriateness of an image comprises two elements: distinctness and simplicity. By the former, Hertz understood the ability to picture the greatest possible amount of "the essential relations of the object." Among various pictures of the same object, the "simpler" one is that which attains this distinctness while including the smaller number of empty relations. Hertz deemed simpler images more appropriate (p. 2); he used this last criterion directly to argue that his own presentation of mechanics was better than existing ones, since, by renouncing the concept of force, it provided a "simpler" image. In general, however, both distinctness and simplicity are far from being straightforwardly applicable criteria.

Hertz also made clear what he understood by "principles" in his work. Although the word had been used by other authors with various meanings, he meant by it any propositions or systems of propositions from which the whole of mechanics can be "developed by purely deductive reasoning without any further appeal to experience (p. 4)." Different choices of principles would yield different images of mechanics.

Hertz's own presentation of mechanics, as it is well known, uses only three basic concepts: time, space, mass; as already said, he was trying to eliminate forces from his account of mechanics. He thought that this concept, especially as it concerns forces that act at a distance, was artificial and problematic. He thought, moreover, that many physicists, from Newton on, had expressed their embarrassment when introducing it into mechanical reasoning, though no one had done anything to overcome this situation (pp. 6-7).

In principle, Hertz's criticism of the traditional approach to mechanics concerned neither its correctness nor its permissibility, but only its appropriateness. Yet he also allowed room for changes in the status of correctness in the future. In criticizing the role played by force in the traditional image of mechanics, Hertz stressed that the problems raised by the use of this concept are part of our representation of this image, rather than of the essence of the image itself. This representation had simply not attained, in Hertz's view, scientific completeness; it failed to "distinguish thoroughly and sharply between the elements in the image which arise from the necessity of thought, from experience, and from arbitrary choice (p. 8)." A suitable arrangement of definitions, notations, and basic concepts would certainly lead to an essential improvement in this situation. This improvement in presentation, however, would also allow the correctness of the theory to be evaluated in the face of later changes in the state of knowledge. Hertz thus wrote:

> Our assurance, of course, is restricted to the range of previous experience: as far as future experience is concerned, there will be yet occasion to return to the question of correctness. To many this will seem to be excessive and absurd caution: to many physicists it appears simply inconceivable that any further experience whatever should find anything to alter in the firm foundations of mechanics. Nevertheless, that which is derived from experience can again be annulled by experience. This over-favorable opinion of the fundamental laws must obviously arise from the fact that the elements of experience are to a certain extent hidden in them and blended with the unalterable elements which are necessary consequences of our thought. Thus the logical indefiniteness of the representation, which we have just censured, has one advantage. It gives the foundation an appearance of immutability; and perhaps it was wise to introduce it in the beginnings of the science and to allow it to remain for a while. The correctness of the image in all cases was carefully provided for by making the reservation that, if need be, facts derived from experience should determine definitions or viceversa. In a perfect science such groping, such an appearance of certainty, is inadmissible. Mature knowledge regards logical clearness as of prime importance: only logically clear images does it test as to correctness; only correct images it compares as to appropriateness. By pressure of circumstances the process is often reversed. Images are found to be suitable for a certain purpose; are next tested in their correctness; and only in the last place purged of implied contradictions. (Hertz 1956, 10)

Hilbert mentioned Hertz's ideas in the framework of his own work on the foundation as early as 1894, while still in Königsberg. In 1893 Hilbert had announced a course on the foundations of geometry. Although this was not the first time he lectured on this topic, he adopted in its preparation, for the first time, an axiomatic perspective inspired on the model put forward by Pasch in a famous textbook published back in 1882.[1] Hilbert followed Pasch in viewing the application of the axiomatic approach as a direct expression of a naturalistic approach to geometry, rather than as opposed to it: the axioms of geometry—Hilbert wrote in the notes—express observations of facts of experience, which are so simple that they need no additional confirmation by physicists in the laboratory.[2] But as it happenned, only one student registered for Hilbert's 1893 course, and it was actually taught only in 1894. When revising his lecture notes of 1893 for teaching in 1894, Hilbert added some remarks from which we learn that in the meantime he became acquainted with the ideas discussed in Heinrich Hertz's textbook

1. On Pasch, see Contro 1976, 284-289.
2. Hilbert 1893-4, 10.

on *The Principles of Mechanics*, published that same year. Hilbert explained the possible role of axioms in elucidating the foundations of geometry, while alluding to Hertz's characterization of a "correct" scientific image (*Bild*) or theory. Thus Hilbert wrote:

> Nevertheless the origin [of geometrical knowledge] is in experience. The axioms are, as Hertz would say, images or symbols in our mind, such that consequents of the images are again images of the consequences, i.e., what we can logically deduce from the images is itself valid in nature. (Hilbert 1893-4, 10)

It is hard to know to what extent Hilbert actually read and understaood the ideas developed by Hetz in his book. Be that as it may, it is certain that his acquaintance with these ideas came to him through his good friend Hermann Minkowski (1864-1909). Minkowski, who together with Adolf Hurwitz (1859-1919) constituted Hilbert's closest circle since their years as students in Königsberg, had spent three semesters in Bonn, and then returned to that city after receiving his doctorate in Königsberg in 1885. MInkowski stayed in Bonn until 1894, and became involved in both mathematical and experimental physics.[1] During this period of his life, Minkowski's greatest scientific source of inspiration came from Hertz[2] and he certainly must have communicated this enthusiasm to Hilbert as well.

Hertz's book was widely praised following its publication in 1894, yet the actual impact on physical research of Hertz's approach to mechanics was perhaps far less than the interest it aroused.[3] The positive reactions often associated with the publication of Hertz's *Principles*, and the belated attraction it has exerted on historians and philosophers, should not mislead us to believe that the idea of axiomatizing physical disciplines was a widely accepted one, or became so after Hertz. Although an overall account of the evolution of this idea throughout the nineteenth century and its place in the history of physics seems yet to be unwritten, one should stress here that axiomatization was seldom considered a main task of the discipline. Nevertheless, it is worth discussing here briefly the ideas of two other German professors, Carl Neumann and Paul Volkmann, who raised interesting issues concerning the role of axioms in

1. See Rüdenberg and Zassenhaus (eds.) 1973, 39-42, and Hilbert 1909, 355.
2. At least according to Hilbert's report (Hilbert 1909, 355).
3. See Lützen 1995, 76-83.

physical science (one of them writing before Hertz's *Principles*, the second one after). Since their ideas are visibly echoed in Hilbert's work, a brief discussion of Neumann's and Volkmann's writings will help set up the background against which Hilbert's ideas concerning the axiomatization of physics arose.

3. Carl Neumann

Carl Neumann (1832-1925) was the son of the Königsberg physicist Franz Neumann. At variance with the more experimentally-oriented spirit of his father's work, Carl Neumann's contributions focused on the mathematical aspects of physics, particularly on potential theory, the domain where he made his most important contributions. His career as professor of mathematics evolved in Halle, Basel, Tübingen and Leipzig.[1] Neumann's inaugural lecture in Leipzig in 1869 discussed the question of the principles underlying the Galileo-Newton theory of motion. Neumann addressed the classical question of absolute vs. relative motion, examining it from a new perspective based on a philosophical analysis of the basic assumptions behind the law of inertia. The ideas introduced by Neumann in this lecture, and the ensuing criticism of them, inaugurated an important trend of critical examination of the basic concepts of dynamics—a trend of which Ernst Mach was also part—which helped to prepare the way for the fundamental changes that affected the physical sciences at the beginning of this century.[2]

Neumann opened his inaugural lecture of 1869 by formulating what he considered to be the universally acknowledged goal of the mathematical sciences: "the discovery of the least possible numbers of principles (notably principles that are not further explicable) from which the universal laws of empirically given facts emerge with mathematical necessity, and thus the discovery of principles *equivalent* to those empirical facts."[3] Neumann intended to show that the principle of inertia, as usually formulated, could *not* count as one such basic principle for mechanics. Rather "it must be dissolved into a fairly large number of partly fundamental principles, partly

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1. See DiSalle 1993, 345; Jungnickel & McCormmach 1986, Vol. 1, 181-185.
2. This trend is discussed in Barbour 1989, Chp. 12.
3. Neumann 1870, 3. I will refer here to the translation Neumann 1993.

definitions dependent on them. The latter include the definition of rest and motion and also the definition of *equally long time intervals*." Neumann's reconsideration of these fundamental ideas of Newtonian mechanics was presented as part of a more general discussion of the aims and methods of theoretical physics.

Neumann focused on the reduction of all phenomena of celestial motion to inertia and gravitational attraction, as a noteworthy instance of the proper working of physical theories. Inertia and gravitational attraction, however, while fulfilling their reductionist task properly, remained themselves unexplained. Neumann compared this kind of reduction to the one known in geometry, wherein the science of triangles, circles and conic sections "has grown in mathematical rigor out of a few principles, of axioms, that are not further explicable and that are not any further demonstrable." He was thus placing mechanics and geometry on the same side of a comparison, the second side of which was represented by logic and arithmetic; the results attained in these latter domains—as opposed to those of geometry and mechanics—"bear the stamp of *irrevocable certainty*", that provides "the guarantee of an *unassailable truth*." The non-explanatory character of mechanics and geometry, Neumann stressed, cannot be considered as a flaw of these sciences. Rather, it is a constraint imposed by human capacities.

The principles to which physical theories are reduced not only remain unexplained, said Neumann, but in fact one cannot speak of their being correct or incorrect, or even of their being probable or improbable. The principles of any physical theory can only be said to have *temporarily* been confirmed; they are *incomprehensible* (*unbegreiflich*) and *arbitrary* (*willkürlich*). Neumann quoted Leibniz, in order to explain his point: nature should indeed be explained from established mathematical and physical principles, but "the *principles themselves* cannot be deduced from the laws of mathematical necessity."[1] Thus, in using the terms *arbitrary* and *incomprehensible*, Neumann was referring to the limitations of our power of reasoning. Always relying on basically Kantian conceptions, he contrasted the status of the choice of the principles in the physical sciences to the kind of necessity that guides the choice of mathematical ones. This is what their arbitrariness means. Neumann was clearly not implying that physical theories are simply formal deductions of any

1. Neumann 1993, 361. The reference is to Leibniz *Mathematische Schriften*, part 2, Vol. 2 (Halle 1860, p. 135.

arbitrarily given, consistent system of axioms devoid of directly intuitive content. Rather they have very concrete empirical origins and interpretations, but, given the limitations of human mental capacities, their status is not as definitive as that of the principles of logic and arithmetic.

Neumann concluded the philosophical section of his lecture by reformulating the task of the physicist in the terms discussed before: to reduce physical phenomena...

> ...to the fewest possible arbitrarily chosen principles—in other words, to reduce them to the fewest possible things remaining *incomprehensible*. The greater the number of phenomena encompassed by a physical theory, and the smaller the number of inexplicable items to which the phenomena are reduced, the more perfect is the theory to be judged.

He expressed the hope that his analysis may have shown that “mathematical physical theories in general must be seen as subjective constructions, originating with us, which (starting from arbitrarily chosen principles and developed in a strictly mathematical manner) are intended to supply us with the most faithful pictures possible of the phenomena.”[1] Following Helmholtz, Neumann claimed that any such theory could only claim objective reality—or at least general necessity—if one could show that its principles “are the *only possible ones*, that no other theory than this one is conceivable which conforms to the phenomena.” However, he deemed such a possibility as lying beyond human capabilities. Nevertheless—and this is a point that Hilbert will also stress time and again in his own attempts to axiomatize physical domains—the constant re-examination of principles and of their specific consequences for the theory is vital to the further progress of science. Neumann thus concluded:

> High and mighty as a theory may appear, we shall always be forced to render a precise account of its principles. We must always bear in mind that these principles are something *arbitrary*, and therefore something *mutable*. This is necessary in order to survey wherever possible what effect a change of these principles would have on the entire edifice (*Gestaltung*) of a theory, and to be able to introduce such a change at the right time, and (in a word) that we may be in a position to preserve the theory from a *petrification*, from an *ossification* that can *only* be deleterious and a *hindrance* to the advancement of science.[2]

Hilbert never directly cited Neumann’s inaugural lecture, or any other of his publications, but it seems fair to assume that Hilbert knew about Neumann’s ideas from very early on. Together with Rudolf Alfred Clebsch (1833-1872), Neumann founded the *Mathematische Annalen* in 1868 and coedited it until 1876,[3] and was

1. Neumann 1870, 22 (1993, 367).
2. Neumann 1870, 23 (1993, 368).

surely a well-known mathematician. Moreover, in 1885, when Hilbert spent a semester in Leipzig, Neumann was one of two professors of mathematics there, and the two must have met, the young Hilbert listening to the older professor. In any case, many of Neumann's conceptions recurrently appear in Hilbert's discussions of physical theories: the reduction of theories to basic principles, the provisory character of physical theories and the ability to reformulate theories in order to meet new empirical facts, the affinity of geometry and mechanics. Neumann had a lifelong concern with the ongoing over-specialization of mathematics and physics, and with their mutual estrangement, which he considered detrimental for both. He believed in the unity of the whole edifice of science and in constant cross-fertilization among its branches.[1] These are also central themes of Hilbert's discourse on mathematics and physics. Neumann's concerns as described here illuminate, if not directly the early roots of Hilbert's conceptions, then at the very least, the proper context in which the emergence of Hilbert's axiomatic method should be considered.

4. Paul Volkmann

Paul Volkmann (1856-1938) spent his whole career in Königsberg, where he completed his dissertation in 1880, and was appointed full professor in 1894.[2] In the intimate academic atmosphere of Königsberg, Hilbert certainly met Volkmann on a regular basis, perhaps at the weekly mathematical seminar directed by Lindemann. Although it is hard to determine with exactitude the nature of his relationship with Hilbert and the extent and direction of their reciprocal influence, looking at Volkmann's conception of the role of axiomatic treatments in science can certainly illuminate the atmosphere in which Hilbert was working and within which his own axiomatic conception arose.

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3. See Tobies & Rowe (eds.) 1990, 29.

1. See Jungnickel & McCormmach 1986, Vol. 1, 184-185.

2. See Jungnickel & McCormmach 1986, Vol. 2, 144-148; Olesko 1991, 439-448; Ramser 1974.

Volkmann was very fond of discussing epistemological and methodological issues of physics, but his opinions on these issues could be very variable. Concerning the role of axioms or first principles in physical theories, he moved from ignoring them altogether (Volkmann 1892), to emphatically denying their very existence (Volkmann 1894), to stressing their importance and discussing at length the principles of mechanics in an elementary textbook published in 1900. This book was intended as a thorough defense of the point of view that all of physics can be reduced to mechanics. Volkmann acknowledged in his book the influence of Hertz and of Boltzmann, but at the same time he believed that these physicists had paid excessive attention to the mathematics, at the expense of the physical content behind the theories.

In the introduction to his 1900 textbook, Volkmann warned his students and readers that his lectures were not a royal road, comfortably leading to an immediate and effortless mastery of the system of science. Rather, he intended to take the reader a full circle around, in which the significance of the foundations and the basic laws would only gradually be fully grasped in the course of the lectures. Volkmann adopted this approach since he considered it to mimic the actual doings of science. Volkmann illustrated what he meant by comparing the development of science to the construction of an arch. He wrote:

> The conceptual system of physics should not be conceived as one which is produced bottom-up like a building. Rather it is like a thorough system of cross-references, which is built like a vault or the arch of a bridge, and which demands that the most diverse references must be made in advance from the outset, and reciprocally, that as later constructions are performed the most diverse retrospections to earlier dispositions and determinations must hold. Physics, briefly said, is a conceptual system which is consolidated retroactively. (Volkmann 1900, 3-4)

This retroactive consolidation is the one provided by the first principles of a theory. That is, the foundational analysis of a scientific discipline is not a starting point, but rather a relatively late stage in its development. This latter idea is also central to understanding Hilbert's axiomatic conception. In fact, the edifice metaphor itself was one that Hilbert was to adopt wholeheartedly and to refer to repeatedly throughout his career when explaining his conception. In his Paris 1900 address (see below), Hilbert already alluded to this metaphor, but only later did he use it in the more articulate way put forward here by Volkmann. More importantly, the role assigned by Volkmann to the axiomatic analysis of a theory was similar to Hilbert's, not only for physical theories, but also for geometry.

Volkmann's epistemological discussion stressed a further point that is also found at the focus of Hilbert's own view: science as a product of the dialectical interaction between the empirical world and the world of thought. Given the inherent limitations of human intellect one can attain only a subjective comprehension of experience, which is of necessity flawed by errors. The aim of science is to eliminate these errors and to lead to the creation of an objective experience. This aim is achieved with the help of first principles, which open the way to the use of mathematics to solving physical problems. Once the mathematical foundations of a discipline are laid, a dialectical process of interaction between subjective perception and objective reality begins. A constant reformulation and adaptation of ideas will help to close the unavoidable gap between these two poles (Volkmann 1900, 10). Volkmann's account here, as will be seen below, also matches to a large extent Hilbert's own views. But of greater interest is the fact that according to Volkmann, the principles involved in this process are of three kinds: axioms (or postulates), hypotheses, and natural laws.

Volkmann's treatment of these three categories is not very clear or concise, yet it seems to have tacitly conveyed a very significant classification that also Hilbert would allude to when putting forward specific systems of axioms for physical theories. Its essence may be grasped through the examples that Volkmann gave of the three kinds of principles. As examples of postulates or axioms, he mentioned the principle of conservation of energy and the Galileo-Newton inertia law. Among hypotheses, the undulatory nature of light (whether elastic or electromagnetic), and an atomistic theory of the constitution of matter. Among natural laws: Newton's gravitation laws and Coulomb's law.

Very roughly, these three kinds of propositions differ from one another in the generality of their intended range of validity, in the degree of their universal acceptance, and in the greater or lesser role played in them by intuitive, as opposed to conceptual, factors. Thus, the axioms or postulates concern science as a whole, or at least a considerable portion of it, they are universally or very generally accepted, and they can predominantly be described as direct expressions of our intuition (*Anschauung*). Natural laws stand at the other extreme of the spectrum, and they are predominantly conceptual. Physical hypotheses stand in between. They express very suggestive images that help us to overcome the limitations of the senses, leading to the formulation of more precise relations. Volkmann's axioms cannot be directly proved or disproved through measurement. Only when these postulates are applied to special fields of physics and transformed into laws, can this be done. The more an axiom is successfully applied to particular domains of physics, without leading to internal contradiction, the more strongly it is retrospectively secured as a scientific principle.[1]

It is not our concern here to evaluate the originality or fruitfulness of these ideas of Volkmann. Nor, I think, is it possible to establish the exact scope of their influence on Hilbert's own thought. Rather, I have described them in some detail in order to fill out the picture of the kind of debate around the use of axioms in physics that Hilbert witnessed or was part of. Still, in analyzing in some detail Hilbert's axiomatization of particular domains of physics, we will find clear echoes of Volkmann's ideas. It should also be stressed, that in his 1900 book, Volkmann cited Hilbert's *Grundlagen* as a recent example of a successful treatment of the ancient problem of the axioms of geometry (p. 363).

5. Physics and Geometry in Hilbert's Early Courses

In the early years of his career in Königsberg, Hilbert taught several courses on geometry. Beginning with the first of these courses, Hilbert consistently presented geometry as *natural* science in which—contrary to other mathematical domains—sensorial intuition plays a decisive role. This position, which we have already seen espoused by Carl Neumann, is explicitly manifest in the following, significant passage taken from the introduction to a course taught in 1891:

> Geometry is the science that deals with the properties of space. It differs essentially from pure mathematical domains such as the theory of numbers, algebra, or the theory of functions. The results of the latter are obtained through pure thinking ... The situation is completely different in the case of geometry. I can never penetrate the properties of space by pure reflection, much as I can never recognize the basic laws of mechanics, the law of gravitation or any other physical law in this way. Space is not a product of my reflections. Rather, it is given to me through the senses.[2]

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1. For more details, see Volkmann 1900, 12-20. On pp. 78-79, he discusses in greater detail Newton's laws of motion and the universal law of gravitation as examples of principles and laws of nature respectively.

2. The German original is quoted in Toepell 1986, 21. Similar testimonies can be found in many other manuscripts of Hilbert's lectures. Cf., e.g., Toepell 1986, 58.

As already mentioned, in his courses of 1893/94, the axiomatic approach became Hilbert's preferred way of presenting geometry. Between 1891 and 1899, Hilbert became involved with certain problems connected with the foundations of projective geometry, which at that time were receiving considerable attention from mathematicians such as Hermann Wiener and Friedrich Schur. Hilbert's celebrated *Grundlagen der Geometrie* represents the culmination of his efforts in addressing these problems, and the axiomatic presentation plays a major role in Hilbert's original contribution to them.[1] All the while, the influence of foundational discussions on physics, although certainly to a lesser degree than the geometrical issues at stake, is clearly noticeable throughout.

Thus for instance, in 1894 Hilbert articulated for the first time the kind of requirements he expected from an adequate axiomatic system to fulfill. He did so, as was already said, by referring to Hertz's definition of an "image", and he formulated the requirements as follows:

> The problem can be formulated as follows: What are 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.[2]

Hilbert always made clear that this kind of analysis should in no sense be limited to geometry. What makes geometry especially amenable to a full axiomatic analysis is the very advanced stage of development it has attained, rather than any other specific, essential trait concerning its nature. In all other respects, geometry is like any other natural science. Hilbert thus stated that:

> Among the appearances or facts of experience manifest to us in the observation of nature, there is a peculiar type, namely, those facts concerning the outer shape of things. Geometry deals with these facts ... Geometry is a science whose essentials are developed to such a degree, that all its facts can already be logically deduced from earlier ones. Much different is the case with the theory of electricity or with optics, in which still many new facts are being discovered. Nevertheless, with regards to its origins, geometry is a natural science.[3]

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1. Toepell 1986 presents a detailed account of Hilbert's involvement with these issues, which prepared the way to the results and the approach adopted in the *Grundlagen*.
2. Quoted from the original in Toepell 1986, 58-59.
3. Quoted in Toepell 1986, 58.

It is the very process of axiomatization that transforms the natural science of geometry, with its factual, empirical content, into a pure mathematical science. There is no apparent reason why a similar process might not be applied to any other natural science. And in fact, in the manuscript of his lectures we read that "all other sciences—above all mechanics, but subsequently also optics, the theory of electricity, etc.—should be treated according to the model set forth in geometry."[1]

6. *Grundlagen der Geometrie* and its Aftermath

The *Grundlagen der Geometrie* appeared in 1899, containing the first full-fledged version of Hilbert's axiomatic treatment of geometry. A detailed analysis of the *Grundlagen* and of the geometrical issues addressed in it would be well beyond the scope of the present article.[2] I will therefore limit myself to an overview in which the empiricist elements of Hilbert's approach are rendered manifest.

Hilbert's axioms for geometry are formulated for three systems of undefined objects—"points", "lines" and "planes"—which establish mutual relations to be satisfied by these objects. The axioms are divided into five groups (axioms of incidence, of order, of congruence, of parallels and of continuity), but the groups have no pure logical significance in themselves. Rather they reflect Hilbert's actual conception of the axioms as an expression of our spatial intuition: each group expresses a particular way in which these intuitions manifest themselves.

Hilbert's requirement for independence of the axioms is the direct manifestation of the foundational concerns that directed his research. When analyzing independence, his interest focused mainly on the axioms of congruence, continuity and of parallels, since this independence would specifically explain how the various basic theorems of Euclidean geometry are logically interrelated. As we have seen, this requirement had already appeared—more vaguely formulated—in Hilbert's early lectures on geometry, as a direct echo of Hertz's demand for appropriateness. Hilbert's study of mutual independence focused on geometry itself rather than on the abstract relations embodied in the axioms; the *Grundlagen* was by no means a general study of the abstract relations between systems of axioms and their possible models. It is for this reason that Hilbert's original system of axioms was not—from the logical point of

1. Quoted in Toepell 1986, 94.
2. This is precisely a main contribution of Toepell 1986. See especially, pp. 143-236.

view—the most economical possible one. In fact, several mathematicians noticed quite soon that Hilbert's system of axioms, seen as a single collection rather than as five groups, contained a certain degree of redundancy.[1] Hilbert's own aim was to establish the interrelations among the groups of axioms rather than among individual axioms belonging to different groups.

Hilbert also required that the axioms be "simple". This requirement had also been explicitly put forward by Hertz; and it complements that of independence. It means, roughly, that an axiom should contain 'no more than a single idea.' This requirement is mentioned in Hilbert's introduction, but it was neither explicitly formulated nor otherwise realized in any clearly identifiable way in the *Grundlagen.* It was present, however, in an implicit way and remained here—as well as in other, later works—as an aesthetic desideratum for axiomatic systems, which was not transformed into a mathematically controllable feature.

Still another requirement is that of "completeness", which runs parallel to Hertz's demand for correctness.[2] Very much like Hertz's stipulation for correct images, Hilbert required from any adequate axiomatization that it should allow for a derivation of *all* the known theorems of the discipline in question. The axioms formulated in the *Grundlagen*, he claimed, would allow all the known results of Euclidean, as well as of certain non-Euclidean, geometries to be elaborated from scratch, depending on which groups of axioms were admitted.[3] Hilbert discussed in great detail the role of each of the groups of axioms in the proofs of two crucial results: the theorem of Desargues and

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1. Cf., for instance, Schur 1901. For a more detailed analysis of this issue see Schmidt 1933, 406-408. It is worth pointing out that in the first edition of the *Grundlagen* Hilbert stated that he intended to provide an independent system of axioms for geometry. In the second edition, however, this statement no longer appeared, following a correction by E.H. Moore (1902) who showed that one of the axioms may be derived from the others. See also Corry 1996, § 3.5; Torretti 1978, 239 ff.

2. And, importantly, it should not be confused with the later, model-theoretical notion of completeness, which is totally foreign to Hilbert's early axiomatic approach.

3. Several important changes concerning the derivability of certain theorems appeared in the successive editions of the *Grundlagen.* I do not mention them here, as they are not directly relevant to the main concerns of this article.

the theorem of Pascal. Hilbert's analysis allowed a clear understanding of the actual premises necessary for coordinatizing projective geometry, that constitutded a major issue among those addressed by Hilbert as part of his involvement with the foundations of projective geometry.[1]

The question of the consistency of the various kinds of geometries was an additional concern of Hilbert's analysis, but, remarkably, it is not explicitly mentioned in the introduction to the *Grundlagen*. He addressed this issue in the *Festschrift* immediately after introducing all the groups of axioms and after discussing their immediate consequences. Seen from the point of view of Hilbert's later metamathematical research and the developments that followed it, the question of consistency appears as the most important one undertaken in the *Grundlagen*; but in the historical context of the evolution of his ideas it certainly was not. In fact, the consistency of the axioms is discussed in barely two pages, and it is not immediately obvious why Hilbert addressed it at all. It doesn't seem likely that in 1899 Hilbert would envisage the possibility that the body of theorems traditionally associated with Euclidean geometry might contain contradictions. Euclidean geometry, after all, was for Hilbert a natural science whose subject matter is the properties of physical space. Hilbert seems rather to have been echoing here Hertz's requirements for scientific theories, in particular his demand for the permissibility of images. Hilbert had repeatedly stressed in his lectures—following an idea of Hertz—that the axiomatic analysis of physical theories was meant to clear away any possible contradictions brought about over time by the gradual addition of new hypotheses to a specific theory. Although this was not likely to be the case for the well-established discipline of geometry, it might still happen that the particular way in which the *axioms* had been chosen in order to account for the theorems of this science led to statements that contradict each other. The recent development of non-Euclidean geometries made this possibility only more patent. Thus, Hilbert believed that in the framework of his system of axioms for geometry he could also easily show that no such contradictory statements would appear.

1. However, there were many subsequent corrections and additions, by Hilbert as well as by others, that sharpened still further the picture put forward by Hilbert in the first edition of the *Grundlagen.* A full account of the *Grundlagen* would require a detailed discussion of the differences between the successive editions. Toepell 1986, 252, presents a table summarizing the interconnections between theorems and groups of axioms as known by 1907.

These are, then, Hilbert's main requirements concerning the axiomatic systems that define geometry: completeness, consistency, independence, and simplicity. *In principle*, there should be no reason why a similar analysis could not apply for any given system of postulates that establishes mutual abstract relations among undefined elements arbitrarily chosen in advance and having no concrete mathematical meaning. But *in fact*, Hilbert's own conception of axiomatics did not convey or encourage the formulation of abstract axiomatic systems as such: his work was instead directly motivated by the need for better understanding of mathematical and scientific theories. In Hilbert's view, the definition of systems of abstract axioms and the kind of axiomatic analysis described above was meant to be carried out, retrospectively, for 'concrete', *well-established and elaborated* mathematical entities. In this context, one should notice that in the years immediately following the publication of the *Grundlagen*, several mathematicians, especially in the USA, undertook an analysis of the systems of abstract postulates for algebraic concepts such as groups, fields, Boolean algebras, etc., based on the application of techniques and conceptions similar to those developed by Hilbert in his study of the foundations of geometry.[1] There is no evidence that Hilbert showed any interest in this kind of work, and in fact there are reasons to believe that they implied a direction of research that Hilbert did not contemplate when putting forward his axiomatic program. It seems safe to assert that Hilbert even thought of this direction of research as mathematically ill-conceived.[2]

A well-known, immediate, reaction that followed the publication of the *Grundlagen* was an interchange of letter between Hilbert and Gottlob Frege, that has been frequently cited ever since.[3] Also here we find interesting traces of the empirical underpinnings of Hilbert's axiomatic approach, that have received relatively little attention. Thus for instance, in a letter in which Hilbert explained the different motivations between his and Frege's understanding of the role of axioms. Axiomatic research, Hilbert stated, was not for him an end in itself with inherent justification, but

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1. For instance Moore 1902a, Huntington 1902.

2. On the American postulationalists and Hilbert's response (or lack of it) to their works, see Corry 1996, § 3.5.

3. The relevant letters between Hilbert and Frege appear in Gabriel et al. (eds.) 1980, esp. pp. 34-51. For comments on this interchange see Boos 1985; Mehrtens 1990, 117 ff.; Peckhaus 1990, 40-46; Resnik 1974.

rather a tool to achieve a clearer understanding of mathematical theories. The need to undertake axiomatic analysis was forced upon him, as it were, by problems Hilbert had found in his day-to-day mathematical research. Thus in a letter dated December 29, 1899, Hilbert wrote to Frege:

> If we want to understand each other, we must not forget that the intentions that guide the two of us differ in kind. It was of necessity that I had to set up my axiomatic system: I wanted to make it possible to understand those geometrical propositions that I regard as the most important results of geometrical enquiries: that the parallel axiom is not a consequence of the other axioms, and similarly Archimedes' axiom, etc. ... I wanted to make it possible to understand and answer such questions as why the sum of the angles in a triangle is equal to two right angles and how this fact is connected with the parallel axiom.[1]

A second, relevant example that can be mentioned here concerns the kind of difficulties reported by Hilbert as having motivated the development of his axiomatic outlook. These difficulties were found by Hilbert mainly in *physical*, rather than mathematical theories. Hilbert's explanations here show a clear connection to similar concerns expressed by Hertz in stressing the need to analyze carefully the addition of ever new assumptions to physical theories, so as to avoid possible contradictions. They also help us to understand many of Hilbert's later endeavours in physics. In the same letter of December 29, he wrote:

> After a concept has been fixed completely and unequivocally, it is on my view completely illicit and illogical to add an axiom—a mistake made very frequently, especially by physicists. By setting up one new axiom after another in the course of their investigations, without confronting them with the assumptions they made earlier, and without showing that they do not contradict a fact that follows from the axioms they set up earlier, physicists often allow sheer nonsense to appear in their investigations. One of the main sources of mistakes and misunderstandings in modern physical investigations is precisely the procedure of setting up an axiom, appealing to its truth (?), and inferring from this that it is compatible with the defined concepts. One of the main purposes of my *Festschrift* was to avoid this mistake.[2]

In a different passage of the same letter, Hilbert commented on the possibility of replacing the basic objects of an axiomatically formulated theory by a different system of objects, provided the latter can be put in a one-to-one, invertible relation with the former. In this case, the known theorems of the theory are equally valid for the second system of objects. Concerning physical theories, Hilbert wrote:

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1. Quoted in Gabriel et al. (eds.) 1980, 38.

2. Quoted in Gabriel et al. (eds.) 1980, 40. The question mark "(?)" appears in the German original (after the word "Wahrheit").

> All the statements of the theory of electricity are of course valid for any other system of things which is substituted for the concepts magnetism, electricity, etc., provided only that the requisite axioms are satisfied. But the circumstance I mentioned can never be a defect in a theory [footnote: it is rather a tremendous advantage], and it is in any case unavoidable. However, to my mind, the application of a theory to the world of appearances always requires a certain measure of good will and tactfulness: e.g., that we substitute the smallest possible bodies for points and the longest possible ones, e.g., light-rays, for lines. At the same time, the further a theory has been developed and the more finely articulated its structure, the more obvious the kind of application it has to the world of appearances, and it takes a very large amount of ill will to want to apply the more subtle propositions of [the theory of surfaces] or of Maxwell's theory of electricity to other appearances than the ones for which they were meant ...[1]

Hilbert's letters to Frege show very clearly, then, the direct motivation of his axiomatic point of view. That point of view in no sense involved either an empty game with arbitrary systems of postulates nor a conceptual break with the classical entities and problems of mathematics and empirical science. Rather it sought an improvement in the mathematician's understanding of the latter.

One last aspect of Hilbert's work, directly connected with the *Grundlagen* and at the same time with empirical roots of his conceptions, concerns the famous list of 23 problems presented by Hilbert in 1900. These problems cover a wide range of mathematical disciplines, some of which included Hilbert's own fields of activities. Among them, one which has traditionally attracted relatively little attention of mathematicians and historians is the sixth problem, which Hilbert formulated as follows:

> The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank are the theory of probabilities and mechanics. (Hilbert 1902, 454)

Hilbert mentioned several existing works as examples of what he had in mind here: the fourth edition of Mach's *Die Mechanik in ihrer Entwicklung*, Hertz's *Principles*, Boltzmann's 1897 *Vorlesungen über die Principien der Mechanik*, and also Volkmann's 1900 *Einführung*. Boltzmann's work offered a good example of what axiomatization would offer. Boltzmann had indicated, though only schematically, that limiting processes could be applied, starting from an atomistic model, to obtain the

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1. Quoted in Gabriel et al. (eds.) 1980, 41. I have substituted here "theory of surfaces" for "Plane geometry", which was the English translator's original choice. In the German original the term used is "Flächentheorie."

laws of motion of continua. Hilbert thought it convenient to go in the opposite direction also, i.e., to derive the laws of motions of rigid bodies by limiting processes, starting from a system of axioms that describe space as filled with continuous matter in varying conditions. Thus one could investigate the equivalence of different systems of axioms, an investigation which Hilbert considered of the highest theoretical importance.

Hilbert's mention of Volkmann here is anything but a coincidence. In fact, in a course taught in Göttingen in 1905 on the axiomatic method, Hilbert developed for the first time in any detail what he had in mind when he formulated the sixth problem of his list. A close analysis of this course (which can not be undertaken here for lack of space)[1] shows very clearly that the ideas developed by Volkmann concerning the role of first principles in physics, and the various kinds of principles that in his view may arise when analyzing a theory in axiomatics terms, are directly reflected in Hilbert's application of his axiomatic appproach to a wide range of physical theories. Obviously, then, when formulating the sixth problem in 1900, Volkmann's outlook—which Hilbert had come to know while still a colleague of Volkmann's at Königsberg—was a main influence, which certainly informed the background of Hilbert's axiomatic approach to geometry as well.

7. Concluding Remarks

The publication of the *Grundlagen* had an enormous impact on many domains of mathematics over the decades to come. Over that time, Hilbert's own conception of the axiomatic definition and treatment of mathematical theories, and of the essence of the axiomatic method, underwent a complex evolution that deserve a close, separate discussion in itself. Clearly there is a significant distance separating his early axiomatic work and, say, his work on proof theory. Unfortunately, we can not deal with all these developments here, but, from what was said above, the empiricist element that informs the early development of Hilbert's axiomatic approach is manifest enough. It can be added, moreover, that this empiricist element never disappeared altogether from the basis of Hilbert's approach to mathematics at large, and in particular from his views of the role of the axiomatic method. This element—very often overlooked in accounts of Hilbert's 'philosophy of mathematics'—should not be lost from sight when trying to understand the roots and scope of his work in proof theory.

1. But see Corry 1997.

BIBLIOGRAPHY

BAIRD, D. ET AL. (EDS.)

1995 *Heinrich Hertz: Classical Physicist, Modern Philosopher*, Boston, Kluwer (Forthcoming).

BARBOUR, J.

1989 *Absolute or Relative Motion. A Study from a Machian Point of View of the Discovery and the Structure of Dynamical Theories*, Cambridge, Cambridge University Press.

BOLTZMANN, L.

1897 *Vorlesungen ueber die Principien der Mechanik*, Leipzig, Verlag von Ambrosius Barth. (English Translation of the Introduction in Boltzmann 1974, 223-254.)

BOOS, W.

1985 “'The True' in Gottlob Frege’s „Über die Grundlagen der Geometrie”“, *Arch. Hist. Ex. Sci.* 34, 141-192.

CONTRO, W.

1976 “Von Pasch bis Hilbert”, *Arch. Hist. Ex. Sci.* 15, 283-295.

CORRY, L.

1996 *Modern Algebra and the Rise of Mathematical Structures*, Boston and Basel, Birkhäuser.

1997 “David Hilbert and the Axiomatization of Physics (1894-1905), *Arch. Hist. Ex. Sci.* 51 (2), 83-198.

DISALLE, R.

1993 “Carl Gottfried Neumann”, *Science in Context* 6, 345-354.

FREUDENTHAL, H.

1957 “Zur Geschichte der Grundlagen der Geometrie. Zugleich eine Bespreschung der 8. Auflage von Hilberts ‘Grudlagen der Geometrie’,” *Nieuw Archief voor Wiskunde* 4, 105-142.

GABRIEL, G. ET AL. (EDS.)

1976 *Gottlob Frege - Wissenschaftlische Briefwechsel*, *Hamburg*, Felix Meiner.

1980 *Gottlob Frege - Philosophical and Mathematical Correspondence*, Chicago, The Universoty of Chicago Press (Abridged from the German edition by Brian McGuiness and translated by Hans Kaal).

HERTZ, H.

1956 *The Principles of Mechanics Presented in a New Form*, New York, Dover (English translation of *Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt*, Leipzig (1894).)

HILBERT, D.

GA *Gesammelte Abhandlungen*, 3 vols., Berlin, Springer, (1932-1935; 2d ed. 1970).

1893-4 *Die Grundlagen der Geometrie*, SUB Göttingen, Cod Ms. D. Hilbert 541.

1899 *Grundlagen der Geometrie* (*Festschrift zur Feier der Enthüllung des Gauss-Weber-Denkmals in Göttingen*), Leipzig, Teubner.

1901 “Mathematische Probleme”, *Archiv f. Math. u. Phys.* 1, 213-237. (*GA* Vol. 3, 290-329.)

1902 “Mathematical Problems”, *Bull. AMS* 8, 437-479. (English by M.W. Newson)

1909 “Hermann Minkowski”, *Gött. Nach.* (1909) 72-101. (Repr. in *Math. Ann.* 68 (1910), 445-471.)

HUNTINGTON, E.V.

1902 “Simplified Definition of a Group”, *Bull. AMS* 8, 296-300.

JUNGNICKEL, C. AND R. MCCORMMACH

1986 *Intellectual Mastery of Nature - Theoretical Physics form Ohm to Einstein*, 2 Vols., Chicago, Chicago University Press.

LÜTZEN, J.

1995 "Renouncing Forces; Geometrizing Mechanics - Hertz's Principles of Mechanics", K_benhavns Universitet, Preprint.

MEHRTENS, H.

1990 *Moderne - Sprache - Mathematik,* Frankfurt, Suhrkamp.

MOORE, E.H.

1902 "Projective Axioms of Geometry", *Trans. AMS* 3, 142-158.

1902a "A Definition of Abstract Groups", *Trans. AMS* 3, 485-492.

NEUMANN, C.G.

1870 *Ueber die Principien der Galilei-Newton'schen Theorie,* Leipzig, Teubner.

1993 "On the Principles of the Galilean-Newtonian" (English translation by Gideon Freudenthal of Neumann 1870), *Science in Context* 6, 355-368.

OLESKO, K.M.

1991 *Physics as a Calling. Discipline and Practice in the Königsberg Seminar for Physics*, Ithaca, Cornell University Press.

PASCH, M.

1882 *Vorlesungen über neuere Geometry,* Leipzig, Teubner.

PECKHAUS, V.

1990 *Hilbertprogramm und Kritische Philosophie.Der G_ttinger Modell interdisziplin_rer Zusammenarbeit zwischen Mathematik und Philosophie,* G_ttingen, Vandenhoeck & Ruprecht.

RAMSER, L.

1974 "Paul Oskar Eduard Volkmann", *DSB* 14, 67-68.

RESNIK, M.

1974 "The Frege-Hilbert Controversy", *Philosophy and Phenomenological Research* 34, 386-403.

RÜDENBERG, L. AND H. ZASSENHAUS (EDS.)

1973 *Hermann Minkowski - Briefe an David Hilbert*, Berlin/New York, Springer.

SCHMIDT, E.

1933 "Zu Hilberts Grundlegung der Geometrie", in Hilbert *GA* Vol. 2, 404-414.

SCHUR, F.

1901 "Über die Grundlagen der Geometrie", *Math. Ann.* 55, 265-292.

TOBIES, R. AND D.E. ROWE (EDS.)

1990 *Korrespondenz Felix Klein-Adolph Mayer. Auswahl aus den Jahren 1871-1907*, Leipzig, Teubner.

TOEPELL, M.M.

1986 *Über die Entstehung von David Hilberts „Grundlagen der Geometrie"*, Göttingen, Vandenhoeck & Ruprecht.

TORRETTI, R.

1978 *Philosophy of Geometry from Riemann to Poincaré,* Dordrecht, Reidel.

VOLKMANN, P.

1892 "Ueber Gesetze und Aufgaben der Naturwissenschaften, insbesondere der Physik in formalen Hinsicht", *Himmel und Erde* 4, 441-461.

1894 "Hat die Physik Axiome?" (April 5, 1894), *Schriften der physikalisch-ökonomischen Gesellschaft zu Königsberg* 35, 13-22..

1900 *Einführung in das Studium der theoretischen Physik, insbesondere das der analytischen Mechanik mit einer Einleitung in die Theorie der Physikalischen Erkentniss*, Teubner, Leipzig.