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Critical Studies/Book Reviews

LEO CORRY. ***David Hilbert and the Axiomatization of Physics (1898–1918).*** Dordrecht: Kluwer Academic Publishers, 2004. ISBN 1-4020-2777-X (cloth), 1-4020-2778-8 (e-book). Pp. xvii + 513.

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This book is a wonderful resource for historians and philosophers of mathematics and physics alike, not just for Hilbert's own work in physics, but also because Corry sets Hilbert in context, bringing out the people with whom Hilbert had contact, describing their work and possible links with Hilbert's work, and describing the activities going on around Hilbert. The historical thesis of this book is that Hilbert worked on a wide range of issues in physics for a period lasting more than two decades, employing and developing his axiomatic approach throughout. One conclusion that follows from this is that Hilbert's 1915–1917 work relating to Einstein's General Theory of Relativity was a natural continuation of Hilbert's pre-existing interests and activities, and not a one-off foray into foreign territory.[1]

Of especial interest to philosophers of mathematics are two further theses. Corry stresses that for Hilbert geometry is an empirical science, and related to this argues first, that Hilbert intends the axiomatic method to be used in enhancing our understanding of the *content* of a given theory via relating the results of the axiomatic investigation back to the intuitive content of the axioms; and, second, that to understand Hilbert's axiomatic approach in mathematics we must pay serious attention to his work in physics.

Corry also hopes to show 'the significant and unique contribution of Hilbert to certain important developments in twentieth-century physics' (p. 3).[2] In the end, this assessment of Hilbert's contribution to physics is far from clear cut: the two cases where Hilbert goes into the details of a physical theory show him lacking feel for what is important *physically* with respect to that theory. Nevertheless, philosophers and historians of

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[1] Since the time of publication of Corry's book, English translations of key texts relevant to this work have been published in [Renn and Schemmel, 2007]. These include Hilbert's two notes on the Foundations of Physics [1915; 1917].

[2] Page references are to Corry's book unless otherwise noted.

physics will find a great deal to interest them in the story of Hilbert's involvement in physics, and in the details Corry provides with respect to the other characters appearing in this story.

These are the main themes that run throughout the book, and they are supported by a richly woven text that is clearly written and well structured. There are nine chapters, the last being an 'Epilogue', along with seven extremely useful appendices listing Hilbert's lectures in physics, and so forth. Each chapter contains helpful 'summaries', although the reader should beware that these often contain new material.

Those familiar with Corry's articles on Hilbert and the foundations of physics, spanning the best part of the last decade, will not be surprised by the main lines of argument. Where the book draws on these earlier publications they are sometimes followed very closely, but for the most part this material has been re-worked, developed, and supplemented by new material, in addition to being integrated into a unified whole. The result is a comprehensive study that, in various ways, offers more than can be found in the publications that pre-date the book.

I

Corry begins his story in the early nineteenth century, with a chapter that discusses Hilbert's early career, and provides relevant context with respect to major figures and contemporary themes in mathematics. This chapter also reveals that Hilbert's involvement with physics began early. Corry notes that the young Hilbert studied physics thoroughly, using a textbook that sought a reduction of all physics to mechanics and which also sought to mathematize physics. Mere perusal of Appendices 2 and 3, listing Hilbert's courses, seminars, and lectures on physics, already indicates the breadth and duration of Hilbert's involvement with physics. Evidence of Hilbert's long-term interest in physics can also be gleaned from the long list of physicists he invited to Göttingen. These include Poincaré (1909), Lorentz (1910, 1913), Debye (1913), Nernst (1913), Sommerfeld (1913), Einstein (1915), Mie (1917), and Planck (1918). From 1912 he employed an assistant charged with keeping him up-to-date with developments in physics.

According to Corry, Hilbert's axiomatic method is already present in embryonic form in his work on algebraic invariants. By the time of a review article for the International Congress of Mathematicians in 1893, Hilbert distinguished three stages of development of a mathematical theory: naïve, formal, and critical. The axiomatic method is needed at the critical stage, 'precisely as a means to analyze already established theories, to criticize their basic assumptions, and to elucidate their logical deductive structure' (p. 20). Among the many definite and possible influences Corry mentions are Carl Neumann and Heinrich Hertz. Corry argues that although Hilbert never explicitly cites Neumann, central themes from Neumann's treatment of mathematical physical theories (such as in his discussion of 'the principles underlying the Galileo Newton theory of motion' in his Leipzig inaugural lecture (p. 51)) recur in Hilbert's lectures on physics (pp. 53–54). Corry shows that Heinrich Herts's work on the foundations of mechanics had a profound influence on Hilbert's axiomatic conception, including the general methodology of Hilbert's axiomatic approach, its application to all of natural science and not solely to geometry, the status and role Hertz attributed to his principles, and the goal of logical clarity and removal of contradictions (see especially Chapter 1 (section 1.3.3) and Chapter 2 (pp. 86–88)).[3] Throughout Chapter 1, Corry aims to show that Hilbert's interest in physics is present and relevant during the early evolution of his thoughts on the axiomatic method. Looking ahead just a few years to 1897, we find Hilbert teaching his first advanced seminar in physics (on mechanics, first jointly with Klein (1897–98) and then by himself (winter 1898–99)), and discussing in this seminar the role of axiomatization in natural science (p. 90).

Chapter 2 concerns axiomatization in the early part of Hilbert's career, and brings to the fore the relationship Hilbert saw between experience and the axioms of geometry, and the status of geometry as a natural science. Quoting from Hilbert's lecture notes for his 1893/94 lectures on non-Euclidean geometry, Corry writes:

> the axioms of geometry express observations of facts of experience, which are so simple that they need no additional confirmation by physicists in the laboratory. (p. 85)

For Hilbert, the process of axiomatization transforms the natural science of geometry into a pure mathematical science, and the same can be done for the other natural sciences. When we are proving something, be that in mathematics or in physics, we should separate out what can be demonstrated logically from the contributions made by intuition. The first objective of the axiomatic method is to make just such a separation, thereby enabling the investigation of what follows from given axioms, independently of how the axioms are to be connected up with experience. Contrasting Hilbert with Dedekind, Corry argues that Hilbert's motivation for his axiomatic method is methodological, as opposed to epistemological (p. 101).

The link between the axioms and experience is the basis of Corry's argument for the view that the goal of Hilbert's axiomatic method lies not in the results of the axiomatic investigation itself, within the pure

[3] The connection to Hertz went via Hermann Minkowski (see pp. 86–87), whose influence is the subject of Chapter 4 (on which see Section II, below).

mathematical science, but in what we learn when we return with these results in hand to the natural science from which we began:

> If we consider the main requirements he stipulated for axiomatic systems—completeness, consistency, independence, and simplicity—there should be no reason *in principle* why a similar analysis could not be applied 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 himself never followed this course nor encouraged others to do it. The definition of systems of abstract axioms and the kind of axiomatic analysis described above was meant to be carried out always retrospectively, and only for 'concrete' *well-established and elaborated* mathematical entities. (p. 98–99)

This point also informs Corry's reading of the famous interchange between Frege and Hilbert following the publication of the *Grundlagen der Geometrie* (see pp. 111–115). In response to Frege's allegation that Hilbert was turning geometry 'into a purely logical science', Corry writes:

> But it is essential to recall that for Hilbert, as for Pasch before him, *the axioms themselves* are not detached from spatial intuition, but rather are meant to fully capture it and account for it. Thus, contrary to Frege's characterization, Hilbert's aim was to detach the *deduction* (but only the deduction) of geometrical theorems from spatial intuition, *i.e.* to avoid the need to rely on intuition when deriving the theorems from the axioms. But at the same time, by choosing correct axioms that reflect spatial intuition, Hilbert was aiming, above all, at strengthening the effectiveness of geometry as the science—the *natural* science, one should say—of space. (p. 113)

Indeed, Corry emphasizes that Hilbert explicitly stated that his motivations as a whole were different from Frege's, born out of the difficulties encountered while working with mathematical theories, especially in physics, such as questions about consistency arising from the addition of new postulates. The axiomatic method, for Hilbert, is a tool for better understanding mathematical theories, rather than an end in itself.

Curious throughout this discussion, however, is the absence of any references to the philosophy of mathematics literature. Corry references only the Frege–Hilbert letters themselves, and four pages of an article by Rowe (concerning Hilbert's attitude towards Frege, rather than the philosophical issues at stake). In fact, Corry would find somewhat congenial company in some of the literature, such as in [Hallett, 1990], where careful attention is played to the role of experience in Hilbert's approach, and the importance of physics is clearly recognized, to name but one obvious example.[4]

Later, Hilbert expanded the role of the axiomatic method. In 1913, he taught on the theory of the electron, in which he discussed the search for principles that would unify physics, and once again (as in 1905) gave a course in which the axiomatization of physical theories was at the fore. Corry emphasizes that Hilbert by now presented the axiomatic method not only as allowing the investigation of other logically possible theories, but also as a means for developing theories that might turn out to be physically interesting. Hilbert stressed the provisional nature of the axiomatic investigation of theories that are far from being complete, but also asserted the value of making explicit all the assumptions at work in a theory, and the relationships between them, even while the theory remains incomplete. It is this methodology that is clearly at work in Hilbert's two forays into the details of specific physical theories, in 1912 and 1915 (see below).

We cannot discuss the details of Hilbert's work in physics without first mentioning the 'sixth problem' in Hilbert's 1900 list of problems. Corry argues that the sixth problem is a natural part of Hilbert's developing axiomatic approach, and that his work relating to Einstein's General Theory of Relativity in 1915 is a natural continuation of this project. The first six problems on Hilbert's list concern 'the foundations of the mathematical sciences' [Hilbert, 1902, p. 455]—Hilbert himself groups them together as such—and Corry stresses that they are linked through the axiomatic approach that Hilbert advocates. For the first two problems this is explicit, whereas for problems 3–5 Corry writes (p. 105): 'although not explicitly formulated in axiomatic terms, they address the question of finding the correct relationship between specific assumptions and specific, significant geometrical facts.' Corry emphasizes that the fifth problem, concerning the foundations of geometry, involves consideration of the free mobility of rigid bodies and as such, he argues, provides a natural link into the sixth problem, concerning the axiomatization of physics, whose statement Hilbert begins as follows [Hilbert, 1902, p. 454]:

> 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*.

[4] As a recent article in this journal [Blanchette, 2007] illustrates, the discussion in philosophy of mathematics continues concerning what was at stake in, and what lessons we should take from, this exchange between Hilbert and Frege.

The main conclusion concerning the sixth problem that Corry wishes us to take away is this:

> For all its differences and similarities with other problems on the list, the important point that emerges from the above account is that the sixth problem was in no sense disconnected from the evolution of Hilbert's early axiomatic conception. Nor was it artificially added in 1900 as an afterthought about the possible extensions of an idea successfully applied in 1899 to the case of geometry. Rather, Hilbert's ideas concerning the axiomatization of physical science arose simultaneously with his increasing enthusiasm for the axiomatic method and they fitted naturally into his overall view of pure mathematics, geometry and physical science—and the relationship among them—at that time. (p. 110)

Corry emphasizes that while Hilbert had already stopped working in the area of some of his problems by 1900, and never worked on others, the sixth problem was one that received continuing attention from Hilbert and his collaborators and students.

Chapter 3 covers the period 1900–1905, culminating in Hilbert's 1905 lectures on the axiomatization of physics. In the first part of this chapter, Corry stresses Hilbert's empiricist standpoint with respect to the sciences, including arithmetic, and discusses the role accorded by Hilbert to axiomatization in the development of the sciences. He discusses Hilbert's involvement with physics during this period, including the range of physics topics on which Hilbert taught, and the place given to axiomatization in these lectures, along with the 1905 weekly seminar on theories of the electron (which involved Minkowski and other Göttingen professors). Corry then turns his attention to the 1905 lectures, which covered seven topics: mechanics, thermodynamics, probability calculus, kinetic theory of gases, insurance mathematics, electrodynamics, and psychophysics. These lectures show Hilbert actively engaged in central topics in physics, in great mathematical detail. In the case of mechanics, Hilbert states that we can make different choices about which axioms to put at the top (a variational principle, the Euler-Lagrange equations, energy conservation, and so forth), but at stake are two things: (1) finding the *simplest* things to take as axioms, and (2) finding out how far we can get in deriving mechanics if we start from certain axioms. The discussion of the axiomatization of mechanics concludes with Hilbert 'pondering' (p. 149) the unification of mechanics with electrodynamics.

In this chapter, Corry makes vivid that the general approach adopted by Hilbert in 1915 already existed for him in 1905. Indeed, Hilbert's application of the axiomatic method to *gravitation* is not new in 1915: in 1905 Hilbert discussed Newtonian gravitation in his mechanics lectures (see

p. 151). According to Corry, these 1905 lectures represent the culmination of one period of Hilbert's work in physics, and the beginning of a new period.

Indeed, despite all this activity in physics, it is not until 1912 that we get Hilbert's first publications on physical topics, beginning with a paper on the 'mathematical foundations of the kinetic theory of gases' in which Hilbert gave a mathematical solution to the Boltzmann equation, 'followed by a series of similar works on the foundations of elementary theory of radiation' (p. 229). These publications arose out of a course that Hilbert gave on radiation theory, and represent his first venture into the physical details of a theory. Chapter 5 provides a detailed account of Hilbert's lecture courses on physical topics during the period 1910–1914, and in addition to radiation theory these included mechanics, kinetic theory, the structure of matter, and the axiomatization of physics. In discussing kinetic theory, Hilbert expressed his concerns about the justification for using probabilistic methods, raising issues that continue to challenge philosophers of physics today. Hilbert's involvement with kinetic theory was far from superficial, and concerned physical as well as mathematical issues. As evidence for this, Corry lists some of the topics discussed in a seminar Hilbert organized with Erich Hecke, that include the ergodic hypothesis and its consequences, Brownian motion, Hilbert's theory of gases, and the theory of chemical equilibrium. In his course on radiation theory, Hilbert focused on Kirchhoff's law of emission and absorption, and the investigation of the assumptions needed to derive the core content of the law. It was this topic which he pursued in publications.

Corry discusses two critical responses that Hilbert received to this published work, from Ernst Pringsheim and Max Planck. Pringsheim's criticism 'forced Hilbert to produce, for the first time ever, a thorough and detailed discussion of the logical interdependence of the axioms he had suggested for a physical theory. In all previous known instances he had left such discussion at a very general level. . . . It was only here that he carried out this analysis in detail.' (p. 256) In other words, this is the first time that Hilbert follows through in detail with his application of the axiomatic method to a real physical theory, and the outcome is, I think, revealing. Both Planck's and Pringsheim's objections indicate that, to some extent, Hilbert missed the point of what was at stake physically. Corry (p. 266) quotes Born on what Hilbert was trying to do and why Pringsheim misunderstood him, and why his criticisms were unfair to Hilbert. He says that Hilbert was seeking to uncover the assumptions underlying the work of the physicists, and to investigate whether they are partially superfluous or mutually contradictory, whereas the physicists misinterpreted the term 'axiom' as meaning 'definitive truth'. Yet this cannot be the whole story. In going to the level of detail forced on him by Pringsheim's and Planck's criticisms, Hilbert displayed his lack of grasp of what is important physically within a theory: he took as given what physicists deemed in need of demonstration (for example), and he specialized and idealized to cases that didn't include those of physical interest.

In his concluding remarks on Hilbert's work on kinetic and radiation theory, Corry acknowledges that Hilbert's contributions in these fields may not have been as significant as Hilbert held them to be, and immediately goes on to suggest that, nevertheless, he had a significant influence via the work of others, including several students. It seems to me that this is not enough if we are to arrive at an evaluation of the significance of Hilbert's work in physics. What we seem to have, rather, is a dilemma: when Hilbert stays at a high level of generality, the value of his axiomatic method is unclear—the proof of the pudding is in the eating; but when he tries to work the project through in detail for a specific case, his lack of feel for what matters physically gets cast into sharp relief. One question is: does the same fate await him when he makes his second detailed foray into physics, in 1915?

Before addressing that question, we first need to follow another thread in Corry's story, leading to Hilbert's use of Mie's theory in 1915. One of Corry's subsidiary themes is that early on Hilbert was committed to a mechanical reductionist picture, this being made increasingly explicit in his lectures between 1910 and 1912, but that this shifted to an electromagnetic reductionist position in 1913. Although Hilbert never explained this shift, Corry attempts to understand (in Chapter 5) when, why, and how it happened.[5] He writes:

> The increasing mathematical difficulty inherent in the treatment of physical disciplines based on the atomist hypothesis, and above all of kinetic theory, was a main factor behind Hilbert's decision to abandon the theory as a possible foundational standpoint. An additional factor that most probably came to strengthen this move was his recent study of current work on radiation theory, from which Hilbert learnt about the crucial implications of Planck's quantum hypothesis on the classical conceptions of the structure of matter and of energy. (p. 231)

By the end of 1913 Hilbert had shifted to treating electrodynamics as foundational. In his 1913/14 lectures he noted the desirability of explaining gravitation on the basis of 'the electromagnetic field and the Maxwell equations, together with some auxiliary hypotheses, such as the existence of rigid bodies' (p. 283). This shows that Hilbert's use of the Mie theory of electromagnetism in his 1915 work on the foundations of physics, as a candidate for being part of the foundations of physics, has a history

[5] Chapter 4, concerning Minkowski, is discussed below in Section II.

in Hilbert's work, and is closely and deeply tied to his ongoing work on physics. Indeed, by the end of Chapter 5, Corry has achieved what he claims: Hilbert's 1915 work on electromagnetism and gravitation has been shown to be a natural continuation of a long-standing area of active interest.

Chapter 6 is entitled 'Einstein and Mie: Two pillars of Hilbert's unified theory'. The discussion of Einstein draws heavily on secondary sources from recent Einstein scholarship, and brings nothing new to the table. It serves its purpose, however, in providing the essential Einstein background to Hilbert's 1915 work on electromagnetism and gravitation. The section on Mie is based on Corry's own previously published work.

Mie sought a unification of mechanics and electrodynamics in which electrons would emerge as 'no more than singularities in the ether'. Nevertheless, Mie's electromagnetic theory of matter differed from earlier attempts to develop an 'electromagnetic world-view' in that he adopted the principle of relativity as one of the three explicitly stated assumptions at the basis of his theory (in addition to which he also assumed energy conservation). Within the framework of these assumptions, Mie sought to identify a 'world-function' depending on electromagnetic quantities deriving from Maxwell's equations. The dynamical equations of the theory are then to be arrived at from this world-function via application of a stationarity principle within the calculus of variations.[6] While Mie was unable to find a world-function that led to the electron, he stated that 'the possibility cannot be denied, on the other hand, that one such function exists' (quoted on p. 303). In the third instalment of his proposal (November 1912), Mie discussed gravitation, in which he offered an intriguing 'principle of relativity of the gravitational potential', according to which, in regions of constant gravitational potential, the effect of the gravitational potential could be made as small as you like by a 'rescaling of all other dynamic variables' (p. 305). Corry ends the section noting the serious problems that Mie's theory faced, including those that could not have been foreseen at the time. Einstein, however, dismissed Mie's attempted gravitational theory from the start. Following a talk in 1913, and in response to a question from Mie himself, Einstein is reported to have said that since the theory did not satisfy the principle of equivalence, he had not studied it in detail. Mie, for his part, rejected Einstein's approach in print in 1913. Corry highlights three main criticisms:

1. Mie criticized the limited covariance of the Einstein–Grossmann *Entwurf* theory;
2. Mie claimed that his 'principle of relativity of the gravitational potential' offered a better understanding of the invariance properties of the theory than Einstein's 'generalized principle of the relativity of motion'; and

[6] See pp. 302–303, and see also fn 70 on p. 334 concerning terminology.

3. Mie maintained that the use of tensors introduces unnecessary difficulties (unlike in his own scalar theory).

On the first of these, Mie was completely vindicated; and on the second, we can hardly say that Mie was wrong to find Einstein's 'generalized principle of relativity of motion' problematic as an interpretation of the significance of widening the covariance properties of the field equations. The first criticism was also important for Hilbert, as we shall discuss below. However, there is an important intermediary figure between Mie's theory and Hilbert's attempt at a unified theory. Corry writes (p. 309) that 'it was only through Born's reformulation of the theory, and perhaps through his personal mediation, that Hilbert adopted it as one of the central pillars of the unified foundation of physics that he was about to develop'. Born not only presented Mie's theory in Göttingen, but sought to clarify the mathematical structure, in which he (1) stressed the role of variational techniques; (2) used a generalized framework that is 'tensorial in spirit'; and (3) did not explicitly talk of the ether, not even in his discussion of energy conservation. According to Corry, Born emphasized that Mie's approach did not depend on any specific hypotheses concerning the nature of physical phenomena, and this would have made it look very attractive for Hilbert's purposes. This claim sits a little uncomfortably, perhaps, with a claim made earlier (in Chapter 3, p. 182), where Corry states that Hilbert's commitment to Mie's electromagnetism in 1915 represents a change from his position in his 1905 lectures on the axiomatization of physics, where Hilbert insisted on the separation of the physical from the logical structure of the theory.

Born explicitly omitted gravitation from his version of Mie's theory, and 'Born and Hilbert seem to have simply ignored this part of the theory in the framework of their discussions' (p. 316). Hilbert did not attempt to generalize Mie's scalar theory of gravitation into a tensorial version. For the gravitational part of his theory, he drew inspiration from Einstein.

Chapter 7 is devoted to Hilbert's work on a unified theory of electromagnetism and gravitation during the period 1915–16, and his close involvement with Einstein during the final months of the latter's creation of the General Theory of Relativity, culminating in the Einstein Field Equations at the end of November. Corry begins with Einstein's visit to Göttingen in the summer of 1915, speculating on who may have attended his lectures, and what the content of those lectures may have been (Corry notes in particular that 'Einstein most probably repeated the details of the "hole argument" and the problems related with the degree of covariance of the equations in his theory' (p. 323)). Corry paints a positive picture of the personal relationship between Einstein and Hilbert, extending to their political views, but drawing on the available evidence deems it unlikely that the two men were in direct contact between Einstein's Göttingen visit and his letter to Hilbert of November 7. The chapter includes a detailed review of the extant correspondence between the two men during November 1915, in the context of their manuscripts and publications from this time.

By this point in the book, two things are clear. First, Hilbert's 1915 work on the Foundations of Physics is a natural continuation of his longstanding interest in physics. Indeed, he lectured on the structure of matter in 1915 (this also being the topic of a seminar which he ran jointly with Minkowski, Debye, and Born over many years). Second, and relatedly, his goals are his own, and quite distinct from those of Einstein. As Corry (p. 333) stresses: 'one should not lose sight of the fact that Hilbert's main focus of interest all along this period was the problem of the structure of matter, seen from an electrodynamic, reductionist point of view, and that his gradually increasing interest in Einstein's ideas was ancillary to this main problem.' Corry persuasively argues that, in his first communication on the Foundations of Physics, Hilbert sets up a system of axioms for physics *in general*, with the field equations being derived as 'a kind of by-product embedded within a much broader argument' (p. 333). The first two axioms are 'Mie's axiom of the world-function' (*i.e.*, the postulation of a function depending on specified parameters, from which the dynamical equations are to be derived via a stationarity principle), and the 'Axiom of general invariance'. Corry writes (p. 333): 'This time, Hilbert thought he had accomplished for physics what he had done for geometry in 1899, or at least he repeatedly declared so.' In Chapter 8 Corry claims that new light is shed on Hilbert's 1917 lecture 'Axiomatic thinking' when the background of his work in foundations of physics is taken into account.

In response to his 1915 theory, Hilbert faced criticism for being physically naïve, and for not recognizing what Einstein was doing *physically*. Corry (p. 362) raises the question of 'Hilbert's actual understanding of all the physical issues involved'. This was Hilbert's second foray into the nitty-gritty details of physical theory, and it seems to me that the criticisms Hilbert faced are strongly reminiscent of those he faced in 1912, and that they raise exactly the same question about Hilbert's physical understanding of the theories with which he was dealing. This same issue arises again in the context of energy conservation in 1918, about which more below.

Corry has other criticisms to make of Hilbert, not all of which seem to me to be well founded. The first concerns Hilbert's claim that, in his theory, the electromagnetic phenomena are a consequence of the gravitational. Hilbert arrived at this result via a special case of a theorem later derived by Emmy Noether, and Corry asserts that the conclusion is not warranted by this theorem (pp. 337 and 391). However, the result that Hilbert in fact proved is that the gravitational field equations, in conjunction with the postulate of general covariance, yield four mutually independent combinations of (the Euler derivatives associated with) the electromagnetic field equations and their first derivatives. It is *this* that Hilbert offers as giving the 'exact mathematical expression' of his claim that electrodynamic phenomena depend on the gravitational phenomena (see [Brading and Ryckman, 2008, §3.3]). And this result *does* follow from Noether's second theorem, vindicating Hilbert.

Corry appears to endorse the view found in [Renn and Stachel, 1999], according to which Hilbert fell into a confusion about the physical significance of coordinate systems that had been expressed by Einstein in his so-called 'hole argument'. Corry speculates that

> there is no reason to believe that Hilbert raised any serious objections to the 'hole argument' as Einstein understood it, and to its implications for the limited kind of covariance that should be adopted in the desired theory. (p. 325)

Similarly, he later asserts,

> there is no evidence that during and after Einstein's visit to Göttingen, Hilbert raised any specific concern about the 'hole argument' and the limited degree of invariance of Einstein's theory as presented then. In fact, we do not know at all what his attitude was towards the 'hole argument' prior to November 1915. (p. 333)

This leads him to misinterpret the status of general covariance in Hilbert's theory, asserting that his 'Axiom of general invariance' (or, in modern terminology, of general covariance) was 'curtailed' by the addition of four non-covariant conditions, such that Hilbert's theory was *not* generally covariant, despite the axiom. It is true that Hilbert adds four non-covariant conditions to his theory, and that he arrives at these via energy conservation. The similarity with Einstein is striking, since he used energy conservation to restrict the covariance class of the equations of the *Entwurf* theory, and it is tempting to think that Hilbert's interpretation of the role of these four conditions is the same as Einstein's, and motivated by the causality concerns that Einstein expresses in his 'hole argument'. But this is *not* how Hilbert understood the place of causality, or the role of the four non-covariant conditions. Rather, in my opinion (see [Brading and Ryckman, 2008]), Hilbert maintained his adherence to the fundamental status of general covariance throughout, and the introduction of the four non-covariant conditions is associated in the first place with the mathematical desideratum of extracting a Cauchy-determinate structure from within a generally covariant theory, and in the second place with recovering our *experience* of causal order. In this regard, in his Second Communication on the Foundations of Physics [1917] Hilbert argued for the use of 'proper coordinate systems' that would ensure Cauchy determination and preserve the causal order in the face of general covariance. Corry asserts that this is 'one of several instances where Hilbert's treatment reflects an underlying attribution of some kind of physical meaning to the choice of coordinate systems' (p. 386). But it is easy to be misled here. The 'physical meaning' at issue for Hilbert is 'physical meaningfulness *for us*': according to Hilbert, causality is an anthropomorphic concept, and if we are to represent the world as physically meaningful *to us* then we must represent it as causally ordered. This, Hilbert believed, required the use of 'proper coordinate systems'. But none of this undermines his commitment to requiring that the underlying theory be generally covariant. Moreover, nothing here indicates that Hilbert attributed physical significance to coordinate systems. On the contrary, as Corry himself writes,

> a representation of natural phenomena, Hilbert said, can only be considered 'once and for all to be free of subjectivity and arbitrariness, if it is independent of the way in which the world-points are denoted (through coordinates) in it.' (p. 393)

We have no reason to doubt that Hilbert had identified this as the significance of general covariance by the fall of 1915, if not immediately following Einstein's lectures of that summer.[7]

In the concluding summary of Chapter 7, Corry claims that Hilbert gave up his own program and joined in the work on Einstein's General Theory of Relativity. He states that Hilbert 'stopped speaking of his own theory and began to speak about his contribution to general relativity'. This theme recurs in Chapter 8, where the presupposition is that already in 1916 Hilbert had abandoned his own theory and was working within Einstein's GTR. But no evidence is offered for this perspective. In discussing Hilbert's teaching in this area, labeling it 'Hilbert teaches GTR', Corry's presentation indicates that Hilbert was in fact teaching the wider framework provided by his own theory, and Einstein's theory within that context. For example, Hilbert's concern with Euclidean geometry and action-at-a-distance in physical theories led him to be interested in matter-free solutions of the Einstein field equations, and Corry locates this work as lying 'within GTR' (p. 380). Yet, as Corry rightly states, this concern 'arises naturally within Hilbert's approach' (*ibid.*), and there is no reason to suppose that Hilbert's interest in these solutions is connected with any departure from his own program. Indeed, as Corry goes on to say, the 'discussion of the Schwartzschild solution also led Hilbert back to the main starting point of his unified theory, namely, a consideration of the behavior of matter in space and time' (p. 381). And again, on p. 383, Corry stresses that while Hilbert in his lectures presented equations that are formally identical to the Einstein field equations, they have a different interpretation, since the energy-momentum tensor is purely electromagnetic. So why should we not believe that all of this discussion took place within Hilbert's own

[7] For further discussion see [Brading and Ryckman, 2008, §7].

program? There is nothing here to indicate otherwise. Similar criticisms apply to Corry's interpretation of Hilbert's Second Communication, for a detailed treatment of which see [Brading and Ryckman, 2008]. The concluding section of the chapter opens with the phrase: 'Hilbert's way to general relativity ... '. Corry is at pains to stress that Hilbert arrived at general relativity after a long journey within physics that began long before Einstein's 1915 visit to Göttingen. I think that Corry is right about the origin of Hilbert's journey, but wrong about its destination. Hilbert retained his own program of axiomatization: incorporating Einstein's results enabled him to develop his own program further, such as in the treatment of Euclidean geometry and action-at-a-distance, but those results should be understood as lying (for Hilbert) squarely within his own project. This is not in conflict with Corry's emphasis on Hilbert's strong support for GTR where, as Corry notes, in his lectures after 1916 Hilbert consistently credited Einstein with the theory, which he deemed 'the most important achievement of the human spirit ever'.

Chapter 8 covers the time period 1916–18, and includes the reactions of Mie and Einstein to Hilbert's theory. Corry shows that even someone as sympathetic to the project as Mie nevertheless misunderstood Hilbert's aims and motivations. As for Einstein, Corry writes that he 'essentially kept silent about Hilbert's work' (p. 375). Einstein was more publicly vocal in his criticisms of Weyl's 1918 unified theory of gravitation and electromagnetism. This theory has enjoyed a recent revival of interest among philosophers of physics, to whom Einstein's criticisms are well known. Less familiar are the criticisms due to Hilbert which Corry brings to light. Corry offers a somewhat unflattering interpretation of what motivated Hilbert's criticisms, and it would be interesting to revisit the criticisms and examine their intrinsic merits (or otherwise).

Corry shows that Hilbert's interests in physics continued to be wide-ranging well into the 1920s. The final chapter is entitled 'Epilogue', and contains much of interest, including an enlightening section on 'The culture of "nostrification" in Göttingen'. Hilbert's final publication in physics was on the axiomatization of quantum theory in 1927.

II

As already mentioned at the beginning of Section I, Corry's text is useful for the background it provides concerning whom Hilbert read, and with whom he was in contact. Among the supporting characters making an appearance in the story are three women. In Chapter 3 (p. 120), Anne Lucy Bosworth is mentioned as one of Hilbert's students who, along with Max Dehn and Georg Hamel, worked on issues in the foundations of geometry in the period following the publication of the *Grundlagen der*

Geometrie. The year of her birth is given as 1868, but no date is given for her death. However, a short biography of Anne Lucy Bosworth Focke now appears on the Agnes Scott College (Atlanta, Georgia) website devoted to women mathematicians,[8] where it is noted that she was Hilbert's first female PhD student, with a dissertation entitled 'Begründung einer vom Parallelenaxiome unabhängigen Streckenrechnung'. She died on May 15, 1907, of pneumonia.

Louise Lange appears in Corry's list of those who may have attended Einstein's Göttingen lectures in the summer of 1915. She was Hilbert's assistant for physics during 1913–16, and annotated the manuscript of Hilbert's lectures on statistical mechanics during the summer semester of 1914. Corry offers what biographical details he has been able to find in a footnote, but very little seems to be known about her. With this being such a crucial period of Hilbert's work in physics, it would be interesting to know more about her, and her work with Hilbert.

The most famous of the three women is Emmy Noether, who also receives a mention amongst those who may have attended Einstein's 1915 Göttingen lectures. The main discussion of her is later, when Corry addresses the issue of energy conservation as it was discussed in Göttingen in 1918. Here, Corry offers just a few brief remarks on the significance of her contribution, but for further details see [Brading, 2005]. In my opinion, the Göttingen discussions of energy conservation (which also included Einstein) bring up once again an issue raised above: the failure of the 'Göttingen mathematicians' to appreciate the *physical* significance of various mathematical features of the theories they were dealing with. Hilbert and Klein worried about the physical significance of energy conservation in generally covariant theories because of certain mathematical features of these theories, whereas Einstein rejected these concerns by focusing on the physical interpretation of the terms appearing in the relevant mathematical expressions.[9]

Hermann Minkowski (see pp. 86–87) appears regularly and prominently until his death in 1909. Hilbert and Minkowski were friends as students, and it was through Hilbert that Minkowski went to Göttingen in 1902. Following the publication of the *Grundlagen der Geometrie*, Minkowski warned Hilbert that his conception of axioms would get him into a 'tough fight' with the philosophers. In 1905, Hilbert and Minkowski were involved together in the weekly seminar on the electron theories, and in 1907 they ran a joint seminar on the equations of electrodynamics. Yet, Corry says, this collaboration between Minkowski and Hilbert has rarely been mentioned, let alone given any serious attention, in the assessment

[8] http://www.agnesscott.edu/LRIDDLE/WOMEN/focke.htm.

[9] For details of this exchange between Einstein, Hilbert and Klein, and the role played by Noether, see [Brading, 2005].

of their involvement with the special and general theories of relativity, respectively. Corry's assertion is that this mutual involvement is important for understanding the work of each in relativity theory. Corry claims that Minkowski's work in special relativity should be understood against the background of Hilbert's axiomatization program, and that this in turn sheds light on Hilbert's own work on gravitation and relativity in 1915. Chapter 4 focuses on Minkowski's work on special relativity (this chapter is strongly based on [Corry, 1997]), and the above claim is repeatedly made, but compared with the length of the chapter (40 pages), relatively little specific evidence is adduced in its favor. Perhaps the most important support is Corry's discussion of Minkowski's appendix to his 1908 paper. One of the goals of the axiomatic method is to check that when new hypotheses are added to a physical theory they do not lead to a contradiction within the theory. Of Minkowski's appendix, Corry writes:

> it was precisely in order to avoid the danger of such a possible contradiction in the framework of the recent, exciting developments in physics that Minkowski undertook this painstaking conceptual analysis of the ideas involved. In this final section, he explored in detail the consequences of adding the postulate of relativity to the existing edifice of mechanics, as well as its compatibility with the already established principles of the discipline. (p. 198)

This provides us with a direct link between Hilbert's axiomatic method and Minkowski's work on special relativity, and also between this work and Hilbert's later exploration of the consequences of combining a version of Mie's electrodynamical theory with an 'axiom' of general covariance.

According to Minkowski, while the 'postulate of relativity' should be taken as an axiom, it has wider scope than any particular given theory, and should also be taken as an axiom even for as yet unknown theories. This provides us with a second clear link with Hilbert, who had a long-standing interest in such special principles (as, for example, his axiom of continuity), and (although Corry does not say so at this point) in 1915 he applied himself to the 'as yet unknown' unified theory of gravitation and electromagnetism via an axiom of general covariance. Minkowski compared the postulate of relativity with the principle of energy conservation in this regard, as a principle extending beyond currently known theories. This leads Corry to make some brief suggestive remarks comparing Minkowski's and Einstein's views on the status of the principle of relativity. For Minkowski these principles lie at the base of an axiomatic treatment of any physical theory, whereas for Einstein these principles have a strongly heuristic character. While 'there are clear differences between Einstein's approach and Minkowski's axiomatic analysis of the postulate of relativity', Corry continues in a note that, 'On the other hand, Minkowski's axiomatic approach, and in particular his stress on universally valid principles in physics, strongly brings to mind Einstein's oft-quoted remarks on the differences between theories of principle and constructive theories' (pp. 220–221).

There is a great deal more that I could have chosen to comment on in this rich and provocative text. Philosophers will find many issues raised that are worthy of further investigation. That said, the points of disagreement that I have discussed here concern details. The overall thesis—of Hilbert's long-standing and wide-ranging interests in physics, and the deep linkage between this and the development of his axiomatic method—is, it seems to me, thoroughly persuasive.

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