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# HILBERT'S PROGRAM TO AXIOMATIZE PHYSICS (IN ANALOGY TO GEOMETRY) AND ITS IMPACT ON SCHLICK, CARNAP AND OTHER MEMBERS OF THE VIENNA CIRCLE

## INTRODUCTION

In recent years the works of Friedman, Howard and many others[1] have made obvious what perhaps was always self-evident. Namely, that the philosophy of the logical empiricists was shaped primarily by Einstein and his invention of the theory of relativity, whereas Hilbert and his *axiomatic approach* to the exact sciences had comparatively little impact on the logical empiricists and their understanding of science – if they had any effect at all. This is in one respect quite astonishing, insofar as Einstein himself confessed 1921 in his famous lecture before the Prussian Academy of Science that "without it [the axiomatic point of view] it would have been impossible for me to propound the theory of relativity".[2] Hence the simple question arises: why didn't the logical empiricists pay more attention to Hilbert and his axiomatic point of view, than they in fact did? It is an aim of this paper to answer this question and in part to *correct* this one-sided view in the hypothetical or contra-factual sense that the logical empiricists would have done better, if they had paid somewhat more attention to Hilbert and his axiomatic approach to science.

In order to avoid a serious misunderstanding, I should add that I don't blame the logical empiricists for this failure, at least not primarily, but first and foremost Hilbert himself. He, in the first place, failed to make his position sufficiently clear, and did not make much effort to promote his views beyond the narrow circle of mathematical physics in Göttingen. Still a further remark before I get started. If I speak of the logical empiricists, I have primarily Schlick, Reichenbach and (the early) Carnap in mind, and not so much the other members of the Vienna Circle and the Berlin school.

First I'll point out what the logical empiricists, starting with Schlick, assimilated from Hilbert's work, both the early in geometry as well as the later one on the new foundations of mathematics, and how they understood this. Next I'll come to the more important point, which ideas and crucial aspects of Hilbert's work they missed [more or less involuntarily] and, most important, which misapprehensions and wrong conclusions resulted from this failure. Once we have



*M. Heidelberger and F. Stadler (eds.), History of Philosophy and Science,* 213–224.

clarified the misapprehensions, in particular, *why* they are misapprehensions, the way is free to recognise, what a more reasonable philosophy of science should look like and how it could have developed, if one had taken Hilbert's axiomatic point of view more seriously and carefully. The philosophical view that emerges from this I'll call "recursive epistemology".

## SCHLICK'S RECEPTION OF HILBERT'S *FOUNDATIONS OF GEOMETRY*

Basically there were only two topics in Hilbert's extensive work on the foundations of mathematics and physics, which received, at least, a certain minimum of attention in the philosophical considerations of the logical empiricists regarding the development of a new brand of "logical empiricism". The first is connected with Hilbert's book on the "Foundations of Geometry" from 1899, and the second with Hilbert's so called "formalism" with respect to a proper foundation of mathematics developed during the twentieth of the last century. (One can already see: In the twenty years between 1900 and 1920 there is nothing in the work of Hilbert and his disciples, which the logical empiricists regarded as interesting or worthwhile to consider. I'll come back to this crucial point.) Yet first let me consider the *Foundations of Geometry* and inquire, which topic or aspect was estimated as important by the logical empiricists?

If we were not accustomed with the answer, we could hardly believe it. Instead of taking the true mathematical *achievements* of this work as important (such as the proof of the *independence* of the Archimedean Axiom from the remaining axioms of Euclid or the invention of the 'axiom of completeness'[3] in Euclidean geometry) Schlick, in the same vein as Frege, picked up a side issue, the so called "implicit definitions". Of course, from Schlick's epistemological point of view this was an important issue. It enabled him to *separate* Hilbert's representation of geometry not only from the suspicious 'pure *spatial intuition*' of Kant, but also from *'physical space'* and its empirical treatment by the theory of relativity. Instead, geometry became an object of mere logical stipulations of formal concepts and relations without any intuitive or physical content.[4] Furthermore, once the separation of 'abstract geometry' from intuition and physical space was established, Schlick was able to explain why (in his view) Poincaré was right and Reichenbach wrong. Poincaré insisted, in conscious opposition to Reichenbach's empiricism regarding physical geometry, on the *conventional* character of physical geometry in principle. This means, we are free to choose a certain geometry not only with respect to the space-time of special relativity, but also and in particular with respect to the geometry of space-time in general relativity[5]. Here comes a rather long quotation from Schlick's *Allgemeine Erkenntnislehre*, which corroborates my claim. Notice, that the AE was published 1918, three years after Einstein had announced his general theory of relativity.

Euclidean geometry has served as the geometry of everyday life, and until a short time ago it seemed to provide the proper foundation for all the purposes of natural science. The new physics, however, in one of its boldest and most beautiful moves, has concluded from the Einsteinian Theory of Gravitation that we cannot make do with the Euclidean metrical determinations if we wish to describe nature with the greatest accuracy and ba means of the simple laws. According to the theory, a different geometry must be used at each place in the world, a geometry that depends on the physical state (the gravitational potential) at that place. On the basis of Einstein's latest work it is likely that world space as a whole can best be viewed as endowed with approximately "spherical" properties (thus a finite, although of course also unbounded).

It cannot be emphasized too much that we are not *compelled* to conceive of space in accordance with a theory of this kind. No experience can prevent us from retaining Euclidean geometry if we insist on doing so. But then we do not obtain the simplest formulations of the laws of nature, and the system of physics as such becomes less satisfactory. ... The physical description of nature is not tied to any particular geometry and no intuition dictates that we must base such a description on the Euclidean axiom system as the only correct one, nor, of course, on any of the non-Euclidean systems either. We select – in the beginning instinctively, in more recent times deliberately – those axioms that lead to the simplest physical laws. In principle, however, we could have chosen other axioms if we were willing to pay the price of more complicated formulations of the laws of nature. Thus fundamentally the choice of axioms is left to our discretion.

And this means that they are *definitions.*

Our finding then is that geometry, not only as a pure conceptual science but also as the science of space, does not proceed from synthetic *a priori* propositions. Instead, it proceeds from conventions, that is, from implicit definitions.[6]

Of course, all this sounds quite familiar. Nonetheless, it would be very interesting to know how Schlick arrived at this peculiar result, which is, as Friedman has pointed out rightly[7], quite distinct from the usual brand of English empiricism. Although I have not the time to go into any details, one point seems obvious: Schlick's position is much more influenced by Einstein and Poincaré than by Hilbert and a careful reading of his texts. This is indirectly corroborated by Schlick himself, who, at the end of the section just quoted, praises Einstein for stating precisely the same insight into the *conventional* character of geometry[8], whereas Schlick never refers to Hilbert's "Foundations of Physics". This is, by all means, no accident, because Hilbert criticises Poincaré's conventionalism vehemently – and even includes in his critique the much-admired Einstein for his concordance with Poincaré.[9]

But more important than the question of philosophical alliances is in the present context the question whether Schlick's position, seen as a whole, was consistent. It was obviously not in agreement with Hilbert's views about geometry, which is immediately clear from Hilbert's uncompromising and repeated critique[10] of conventionalism. But it was also not consistent in itself. This can be recognised, if one analyses chapter 7 on 'Implicit Definitions' in AE carefully. Schlick begins this chapter with the usual story of modern 'post-Euclidean' geometry: the rejection of spatial intuition as a reliable source for the meaning of geometrical terms such as 'point', 'line', and 'plane' and the subsequent transformation of geometry into a purely *deductive* science – without any recursion to intuition. This brings him to Hilbert and his axiomatic presentation of geometry, whose real advantage he localises in that, what he calls – not Hilbert – the method of implicit definitions. What is the logical essence of this method? Schlick gives the following answer.

> Inference can proceed *only* from *judgments* or statements. Hence when we utilize a concept in the business of thought, we employ none of its properties save the property that certain judgments *hold* with respect to the concept – for example, that the axioms hold for the primitive concepts of geometry. It follows that for a rigorous science, which engages in series of inferences, a concept is indeed *nothing more* that that concerning which certain judgments can be expressed. Consequently, this is also how the concept is to be defined.[11]

If we ignore for a moment some minor inaccuracies in the statement, Schlick's answer seems to be the following: Logical deductions have to start exclusively from *judgements*, that is from *true* propositions. At the same time, the basic concepts and relations of geometry have to be *defined* (that is endowed with a meaning) solely by means of some axioms (entailing the respective terms), but without any recursion to intuition or some other kind of presupposed meaning. It remains Schlick's secret how the axioms can be judged to be 'true', i.e. to be *valid* propositions of the concepts and relations in question, if *these* concepts and relations have no meaning prior to the definitions by the axioms. Truth of those axioms is, however, an indispensable presupposition – as Schlick himself confesses – in order to use the 'modus ponens' properly in logical deductions.[12] I would say, Schlick simply didn't understand Hilbert's *axiomatic* approach to geometry[13] and, consequently, also not to the natural sciences[14]. The latter point is the more important one and brings me promptly to the main question of this essay. Which of the significant ideas and aspects in Hilbert's foundational work were *not* recognised (or accepted) by the logical empiricists, and which misapprehensions resulted from this ignorance? Let me begin with a particular point and then continue with more general aspects. I presume the following.

## SOME OBVIOUS MISAPPREHENSIONS

It never occurred to the minds of the logical empiricists that Hilbert might take geometry as a "natural science", indeed as the "simplest and most perfect" of all natural sciences, as he had always stressed from his very first lecture in geometry in 1893 until his last lecture in 1927.[15] Unfortunately, the only place, where he did not stress that aspect, is his [published] book *Grundlagen der Geometrie*. At least to the extent, the logical empiricists have an excuse for their formalistic reading of Hilbert's *Foundation of Geometry*. However, I bet that even *if* Hilbert had published his opinion, the logical empiricist would have ignored it, just as they ignored Hilbert's second conviction regarding geometry, namely, I quote: "The designated task (the specification of the axioms of geometry) comes up to the logical analysis of our spatial intuition". This claim could impossibly be true according to the epistemological point of view the logical empiricists held with respect to the development of geometry during 19th century. Consequently, they ignored Hilbert's assertion completely faithful to the rule: what's not permitted cannot be possible either! I presume that both assertions, taken together, would have confused the logical empiricist entirely, because they were convinced that both assertions would contradict each other, that geometry as a *natural* science had to be *empirical* and that for this reason its axioms could impossibly rest on a logical analysis of spatial *intuition.* But Hilbert's point of view with respect to geometry is not as absurd [or plainly inconsistent] as it looks at first glance. On the contrary, it makes pretty much sense, if one distinguishes carefully, as Hilbert did, the two categories *natural* and *empirical* sciences, because this opens a solution to the *epistemological* problem that haunted the logical empiricists from the very beginning. How can we *know* the laws of the different kinds of geometry quasi *'a priori'*, i.e. before they have been testified by experiments? After all, the logical empiricists had missed a big opportunity to come to grips with the old problem of physical geometry along the ideas that Hilbert had offered. Instead, they made common cause with Poincaré and his – at their time already outdated – geometrical conventionalism.

Once on the wrong track the logical empiricists missed another, much more important aspect of Hilbert's work: his tireless efforts to extend the axiomatic method beyond the domain of geometry on the totality of physical sciences. Again the logical empiricists can be partially excused for this failure for two reasons. First, Hilbert didn't publish very much in this area, to say the least, and second because the few papers, which he in fact published, did not look very much like an axiomatic treatment of the physical field under investigation. But the logical empiricists cannot be excused completely, because, as far as I know, they never referred to any of Hilbert's papers in physics,[16] although two of the papers dealt with the Foundations of Physics, more properly speaking with the generalised field equations in the frame of general relativity. This was a topic, in which the logical empiricists usually demonstrated great interest. Consequently, the question arises: Why didn't they do just the same in Hilbert's case? I have no direct answer, only an indirect one. But before presenting it, let me quickly outline the main bulk of Hilbert's work in physics as one can find it in the Nachlass in Göttingen.

## OUTLINE OF HILBERT'S WORK IN PHYSICS

Hilbert's engagement in physics can be roughly distinguished into three periods. The first began in 1898 with a lecture on the principles of mechanics and lasted roughly until 1910. Note that the first lecture was held before *the Foundations of Geometry* were finished. The second period started about 1911, when Hilbert for the first time remoulded his lecture on continuum mechanics in order to account

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