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GEOMETRY, INTUITION AND EXPERIENCE: FROM KANT TO HUSSERL*

In his famous celebratory lecture 'Geometry and Experience' held before the Prussian Academy of Science in Berlin in 1921, Einstein raised the puzzle:

> How is it possible that mathematics as a product of human thought, independent of all experience, fits reality so well?[1]

And Einstein immediately offered a short answer to the difficult question he had posed by pointing out firmly that:

> Insofar as the sentences of mathematics are related to reality, they are not certain, and insofar as they are certain, they are not related to reality.

This answer is not only typical of Einstein's way of reasoning, it is also in accord with the dogma of Logical Empiricism[2] according to which there are two and only two sources of knowledge, logic and experience. According to this principle, geometrical knowledge – and hence geometry – has to be attributed either to logic or to experience. But this poses a problem, because geometry (as a non-empirical discipline) is usually considered to be a part of mathematics and not a part of science, as Einstein himself (along with many modern physicists) had proposed. Einstein solves the apparently insoluble problem in simple way by splitting geometry into two epistemologically distinct parts: (i) a "pure axiomatic geometry" and (ii) a "practical geometry". The first belongs exclusively to the discipline of *formal logic*, the second is obviously a natural science. "We can regard it as the oldest branch of physics" (Einstein 1921, p. 6).

Einstein, of course, did not rest content with this laconic answer but tried to explain and justify it. In the course of this justification Einstein refers to the new axiomatics, more precisely, to Hilbert's "axiomatic-method". Immediately after the quotation just given, he continues:

> Full clarity about the matter, it seems to me, first became widespread through that trend in mathematics, which is known by the name "axiomatics". The progress achieved by axiomatics consists namely in this: through it the *Logical-Formal* became cleanly separated from the material or intuitive content: only the Logical-Formal is, according to axiomatics, the object of mathematics and not the intuitive or any other *content* with which the Logical-Formal is linked.[3]

Although Einstein uses the traditional term "axiomatics", it is obvious that he has Hilbert's *axiomatic method* in mind,[4] because in the succeeding two paragraphs he confronts the *older* with the *newer* interpretation of axioms by giving an example from geometry. This example makes it clear

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beyond doubt that Einstein is referring to Hilbert's *axiomatic method*, or what is taken to be under this label:

Geometry deals with things, which are denoted by the words "straight line", "point", etc . . . .Knowledge or intuition of these things is not presupposed, but only the validity of th[os]e equally pure formal axioms, . . . The axioms first define the things with which the geometry deals. For this reason, Schlick, in his book about epistemology, calls the axioms very strikingly "implicit definitions".[5]

Now, it is not without a certain irony that Einstein refers precisely to Hilbert's axiomatic method when he tries to *justify* his strict separation of geometry into two basically distinct disciplines, because, to my knowledge, Hilbert never made such a distinction between logical-formal and intuitive-contentful geometry. On the contrary, as I will show, Hilbert always adhered to the *unity* of geometry and intuiton or, in other words, to the unity of axiomatic and contentful geometry. To avoid a possible misunderstanding, this does not mean that Hilbert did not make an epistemological distinction between different kinds of geometry, or, more properly speaking between different ways of dealing with geometry.[6] However, Hilbert does not make this distinction in the way Einstein does. Hence something must be wrong with Einstein's appeal to Hilbert, and thus with his understanding of Hilbert's axiomatic method.

In my view, it is Einstein's sympathy with the philosophy of Logical Empiricism which is behind his misinterpretation of Hilbert's axiomatic method. According to the logical empiricists, we have only two sources of knowledge, logic and experience. Unfortunately, geometry does not fit into this clean picture. Hence, what would be more natural than to separate geometry into two aspects (aspects which then do fulfil the requirement of the two sources of knowledge) and to claim that this separation is best captured by Hilbert's axiomatic method. Of course, geometrical expressions like 'point', 'straight-line' etc. must then be taken as *uninterpreted* in order to avoid any question of incompatibility between their mathematical and physical meaning. But this is just the interpretation which logical empiricists had favoured for Hilbert's axiomatic procedure in his 'Festschrift' *Grundlagen der Geometrie* from 1899. And this interpretation is just what Einstein refers to when he tries to justify his distinction between two kinds of geometry. Evidence for this is Einstein's reference to Schlick's "implicit definitions"?[7]

What is so misleading about Einstein's "bifurcated" view of geometry? What bothers me is not the patent circularity in Einstein's justification for splitting geometry into two distinct parts, but rather something quite different:

First and foremost: Einstein overlooks the special character of geometry as a uniform discipline lying precisely between logic and experience. Instead, he deepens the puzzle of the successful application of geometry to reality since the separation of geometry into a pure formal theory and a physical science invokes the question: What has the second to do with the first? The answer, offered by Einstein, that physical geometry is obtained from formal geometry by "coordinating" physical objects to its meaningless terms,[8] is really no answer at all, because the coordination procedure presupposes that we have understood in advance the geometrical meaning of expressions like "rigid body". (Otherwise the question would arise of why, for example, we do not coordinate rubber bands with the formal notion of "rigid body".) In other words, the coordination procedure supposes a solution to precisely the puzzle which has to be solved.

My second objection is more of a fear, namely, that Einstein lends authority to the misinterpretation of Hilbert's axiomatic method by the logical empiricists. This in and of itself is merely an historical accident, perhaps of little interest to the philosopher today. But it becomes *philosophically relevant* if Hilbert's view of geometry itself offers a reasonable way of solving the problem posed by geometry, that is, the problem of explaining its extremely successful application to nature while emphasizing its genuine *mathematical* status. This is precisely, what I want to show in this paper. It is, of course, not an easy task. I will proceed in three stages.

First, I confront the reader with a set of metatheoretical claims made by Hilbert about geometry, claims which look flagrantly incoherent. Nevertheless, I will explain why these claims are not incoherent, contrary to first impressions. This I will do in the second stage by comparing Hilbert's views to those of Kant. Because this is insufficient to resolve the puzzle of applicability, I go one step further and relate Hilbert's views with those of Husserl. This will be done in the third and final section.

HILBERT'S META-GEOMETRICAL CONVICTIONS

When Hilbert took up geometry around 1890 he did this in a very peculiar, unparalleled way, a way which is quite dffferent from the fairytale which logical empiricists later circulated concerning the axiomatic method. Although Hilbert's views about geometry are still in flux in 1891, he has already one fundamental conviction: geometry is, unlike number-theory and algebra, not a branch of logic but rather "the doctrine of the properties of space"[9] and this implied for Hilbert that it rests on intuition and experience:

> The results of these domains (number theory, algebra, function theory) can be achieved by pure thinking.... Geometry, however, is completely different. I can never fathom the properties of space by mere thinking, just as little as I can 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 thinking, but is rather given to me through my senses. Therefore I require my senses for the establishment of its properties. I require intuition and experiment, just as with the establishment of physical laws.[10]

None the less geometry is a pure mathematical discipline, and in this respect it is distinguished from mechanics and all other natural sciences, which are, of course, empirical disciplines and for this reason not branches of pure mathematics. This sounds odd, particularly in the light of the previous citation. It is, however, an integral part, not only of Hilbert's early but also his later, more mature convictions. These can be summarized in three statements:

(1) Geometry is the most perfect natural science.[11]
(2) Geometry is (or has become) a pure mathematical science.[12]
(3) The task of geometry is the logical analysis of our spatial intuition.[13]

Taken together, the assertions appear rather incoherent, if not contradictory. But Hilbert maintained consistency among these doctrines by pursuing a philosophy different from Frege's logicism, from Poincaré's geometrical conventionalism, from logical positivism, and last but not least from neo-Kantianism to which he had some inclinations. But before I outline what is behind his position, let me mention another triple of distinction which Hilbert made early on, and which points in the direction, in which he tried to solve the puzzle of geometry as a science falling between logic and experience.

In the 19th century, three distinct ways of dealing with geometry coexisted: (1) *analytical* geometry, (2) *intuitive* geometry (in particular *projective* geometry) and (3) *axiomatic* geometry. This distinction was quite common, and Hilbert more or less accepted it. This in itself is not very interesting. What is interesting, however, are the different *roles* or functions which Hilbert assigned to the three types of geometry:

(1) Analytical geometry is of no significance for the foundations of geometry; it presupposes the theory of real numbers as already established in advance and reduces then the meaning of geometrical expressions to analysis through the coordination of points with real numbers; its main significance lies in the application of geometry to the (natural) sciences.[14]
(2) Projective geometry is the most natural and appropriate way to represent the facts of our intuition and their complex relations. But according to Hilbert it lacks the logical clarity and conceptual distinctness geometry should entail as the most fundamental discipline of all natural sciences.[15]
(3) The clarity is to be supplied by the third approach, axiomatic geometry, as Hilbert calls rather misleadingly the logical analysis of our spatial intuition and its representation in logical deductive form. The significance of the axiomatic approach is exclusively of *epistemological* nature: its first goal is to separate the *logical* relations from the *descriptive* or intuitional content of geometry; its second aim is to arrange the intuitional content in such a form that every geometrical fact or phenomena can be logically deduced from the axioms.

This is, of course, a very rough and preliminary description of the axiomatic method, but it suffices to indicate the way, in which Hilbert wanted to prove the consistency of his three basic convictions and on this foundation to solve the puzzle of geometry.

The first point to be noted is that, according to Hilbert, the axiomatic method is only *one* approach to geometry among others. This means two things:

(i) The axiomatic method has no closer relation to geometry than, say, the approach by analytical means. Different approaches to geometry are possible.

(ii) The axiomatic method has no internal kinship to geometry. In any case it is not restricted to geometry; it can be applied very well to other sciences too.

The last point is particularly useful in grasping why the conflict between the claim that geometry is a natural science and the claim that geometry belongs to pure mathematics is only an apparent one: The axiomatic method is designed particularly to answer one specific epistemological question: Where does the border run between logical and non-logical, contentful thinking? In this respect, however, mathematics (including arithmetic) differs not *in principle* from physics (including geometry): both have a non-logical, intuitive content. (They differ with regard to the question where that content comes from; but this issue cannot be settled by axiomatic means; it must be tackled in a quite different way.) Consequently, neither are pure logical sciences.

But what's with the undeniable difference between mathematics and physics and where does geometry belong, once this difference is clearly established? Here, we must be careful not to make wrong distinctions. Geometry, taken as the science of space in which all external things occur, is doubtless a *natural science*. But it differs from physics by the special circumstance that we have an *implicit* knowledge of all the relevant facts, which belong to geometry. To state the same point more cautiously: In geometry it is extremely improbable (but, of course, not logically impossible) that new facts will come to light, facts which we did not know before. In this and only in this sense geometry is a mathematical discipline in distinction to physics, which depends on new facts brought to light by future experiments.

Given all the facts, it is then the task of the mathematician to analyze and organize the facts in such a way that their logical dependence and independence becomes as clear and distinct as possible. In order to avoid a possible misunderstanding I ought to reiterate the point that the difference between geometry and physics is not a difference in aim. Physics also aims at a clear and distinctive axiomatic presentation. However, in physics we are not as sure as in geometry that we already know all the basic facts: and for this reason the clarity and distinctness to be obtained in physics is not the same as that in geometry. But the difference is only one of degree and not of principle. Let me end this point with the remark that Hilbert's stance was not refuted (as one might suspect) by the rise of special and general relativity. Only the very improbable had happened: completely new and unexpected facts had become known, facts which to some extent also involved geometry.

Until now I have only explained why, according to Hilbert, geometry is at the same time a natural science as well as a mathematical discipline. But I have not explained the *epistemological* reasons for this view. These can't be revealed by the axiomatic method, because they touch the question of the sources of our geometrical knowledge, whereas the axiomatic method takes the facts as given, irrespective of where they come from. Claim (3) expresses Hilbert's basic conviction that our geometrical knowledge is closely connected with our spatial intuition. This claim would be completely harmless under the conditions that Hilbert had meant by "spatial intuition" our empirical impressions of material things in space, and second, had understood by "geometry" any geometry whatsoever. If this were the case, we could choose the 'right' geometry (Euclidean or non-Euclidean), which fits with the facts as we know them from our empirical impressions. In other words, claim (3) would be logically compatible with claim (1) and claim (2); the only difficulty would be to find a reasonable explanation as to why geometry as a mathematical theory fits so extraordinarily well with the empirical data, and in this respect we could fraternize with the logical empiricists. But, of course, we know already that Hilbert does not, for he denies both conditions.[16]

Later writings (to which I will come in the last section) make unmistakebly clear two points: first, that Hilbert means by "intuition" just pure intuition in Kant's *transcendental* sense as a "condition of the possibility of experience", and second that he means exactly Euclidean geometry when he speaks simply of 'geometry' in an epistemological context. This, however, leads inevitably to the question of the *relative consistency* of claim (3) with claims (1) and (2). How can the assertion (3) that it is the task of geometry to analyse our spatial intuition, be reconciled with claims (1) and (2) (that geometry is a physical as well as a mathematical science) if the geometry of our spatial intuition is the Euclidean geometry? This seems to be absurd.

In order to see that Hilbert's position is not absurd but, on the contrary, very reasonable, (indeed much more reasonable than any position I know Weyl's and Carnap's included),[17] we have to go a long way and to compare Hilbert's view on geometry first with the transcendental aesthetic of Kant and second with Husserl's phenomenology. The Kant part will be divided in three steps:

As a kind of prelude I will first compare Hilbert's philosophy of mathematics (Hilbert's label for arithmetic) with that of Kant for the simple reason that, in this terrain, the agreement between both thinkers is maximal. Next, I turn to geometry. Here, the agreement is of a lesser degree, although still remarkable. Yet a certain disagreement occurs, which cannot be removed. In the last section I show how this disagreement forces Hilbert to distance himself in parts from Kant and to move towards a position very similar to Husserl's so called phenomenology, in particular his later conception of "Lebenswelt", regarding the relation of intuition, geometry and experience. This will be investigated more closely in the final section.

HILBERT AND KANT ON ARITITMETIC AND INTUITION

Let me begin with arithmetic. There is not space here to compare Hilbert's position in all its details and ramifications with Kant's, but only in certain points of immediate interest.[18] There are in particular three aspects in respect to which the agreement is almost perfect.

First, both are equally convinced that arithmetic is in need of an intuitive or, as Hilbert also calls it, an extra-logical foundation. For Kant this is a common place; for Hilbert, however, usually dubbed a 'formalist', this has been denied more than once. For this reason I quote a rather long passage from Hilbert's 'New Foundations of Mathematics', which supports my assertion and which re-occurs in almost all publications of Hilbert regarding the foundations of mathematics after 1922:

> As we have seen, abstract operations with general extensions of concepts and contents have turned out to be insufficient and unreliable. Instead, as a precondition for the application of logical deductions and the performance of logical operations, something must already be given to us in our faculty of representation: certain extra-logical discrete objects, which are intuitively present as an immediate experience prior to all thinking. To ensure that logical conclusions are reliable, it is necessary that these objects can be surveyed completely in all their parts, and their occurence, their distinction, and their succession is something immediately intuitively present to us in conjunction with the objects as something that cannot be reduced to something else.[19]

Second, it seems that Kant and Hilbert agree that the intuitive foundations of arithmetic are the extra-logical, discrete objects like the 'fingers of my hand' (Kant) or the "strokes on paper" (Hilbert), which can be surveyed completely in all their parts. Kant speaks in this context of 'symbols in concreto',[20] which we use to demonstrate intuitively the validity of an equation like $7 + 5 = 12$ without any appeal to other propositions or concepts. Of course, the similarity of expression does not imply that Hilbert and Kant mean the same when they maintain that arithmetic rests on intuition. Indeed, it is not even quite clear what Kant precisely means by "intuition" in the arithmetical context. But one negative demarcation can be stated, which shows how closely related Kant's and Hilbert's views indeed are.

Kant does not mean, as is usually stated, that time is *the* characteristic form of intuition in arithmetic. This opinion, which has been propagated mainly by Brouwer and his intuitonistic school, is to my mind not correct and has been refuted by Kant himself. Time is, according to Kant, only typical for the phenomena of motion (or change in general) but not for arithmetic. "Pure mathematics", says Kant in his dissertation, "considers space in geometry, time in pure mechanics".[21] Although, counting takes place *in* time, like any other human activity, this fact in itself is no sufficient reason to turn arithmetic into a temporal science. In geometry, too, the drawing of a line takes time, and yet the characteristic form of intuition in geometry is space and not time. Essential for arithmetic as the doctrine of magnitudes is exclusively the reference to discrete singular objects like the fingers of my hand or the beats of a clock; whether they are space-like or time-like is irrelevant. Precisely in this point, Hilbert and Kant agree: the epistemological foundation for arithmetic is not time as such but its reference to extralogical objects. Of course, these objects only occur either in space, or in time, or in the union of both. (This, by the way, is the reason why they are extralogical objects.) Hence, arithmetic is not possible without at least one of these forms of intuition, although which one is a contingent matter.

Third, Hilbert and Kant agree that arithmetic begins with (or perhaps better, has its origin in) absolutely concrete, singular propositions like "$2 + 3 = 5$" or "$3 > 2$" and not with general propositions or laws, as indeed most of the other sciences do. This agreement is particularly remarkable, because it shows that Kant and Hilbert share a certain peculiar view of arithmetic, which as far as I know nobody else affirms. What follows from this idiosyncratic view?

According to Hilbert, the propositions involved express "matters of fact" and not e.g., logical truths or conceptual relations. Viewed in this way, arithmetic originates from concrete facts and not from universal laws. This is extremely important for the foundations of arithmetic, because it permits us to distinguish two different domains of arithmetical propositions: (i) the domain of singular propositions expressing the simple facts, and (ii) the domain of universal and existential propositions about numbers and their properties and relations. The first domain is unproblematic in the twofold sense that its propositions obey the principle of excluded middle and are, taken together, obviously consistent. The second domain is, in contrast to the first, more problematic, because its propositions express no facts, are neither true nor false, but *ideal* assumptions, whose sole purpose is to explain, via deduction, and to unify and simplify the propositions of the first domain.[22] For this reason, they cannot be asserted as true, but only assumed hypothetically as *axioms*, and their internal consistency has to be proved separately. In other terms, the epistemological status and function of both types of propositions, of singular propositions and axioms, is basically different. It is precisely this difference, which Hilbert has in mind, when he says:

When number theory is carried out in this [intuitive] way, there are no axioms, and so no contradictions of any kind are possible. Indeed, we have concrete signs as objects, we operate with them and make content-ful statements about them.... But certain, the whole of mathematics can-not be presented in this way. Already by the transition to higher arithmetic and algebra, for example, when we want to arrive at assertions about infinitely many numbers or functions, this contentual way of proceeding fails. Since, for infinitely many numbers we cannot write down number signs or introduce [appropriate] abbreviations.[23]

This means that, first in the second infinite domain of arithmetical propositions, axioms become *essential*, because we cannot talk about infinite totalities of numbers or functions without introducing some ideal assumptions like the validity of the "tertium non datur" or general and existential claims.[24] In the restricted domain, however, in which we deal only with intuitively given, finite sets of concrete numbers, the axioms are only *means* of simplifying and unifying the set of singular propositions and can in principle be dismissed. In his later work, *The Logical Foundations of Mathematics*, Hilbert explains more closely his view regarding the relation between the first and second domain, in particular, why the axioms with the ideal assumptions can be dismissed, at least in principle, in favour of the intuitively given finite domain of numbers:

By means of the axioms 1. to 10. we obtain easily all the positive integers and the numerical equations, which hold for these. Also, through this beginning, we can achieve elementary number theory by means of 'finite' logic through purely intuitive considerations to which belong recursion and intuitive induction for the finite totalities with which we are presented: without utilizing any objectionable or problematic inferential procedure. The provable formulas, which can be achieved by this standpoint, all have the character of the finite, i.e. the thoughts, whose representations they are, can also be obtained by contentually and immediate consideration of finite totalities *without any axioms*.[25]

In the intuitively given finite domain of singular propositions about numbers the role of the axioms is obviously only that of a *tool*, but, of course, an extremely useful tool, which it is worthwhile to study for its own sake. That Kant with respect to arithmetic advocates essentially the same view, I cannot develop here in all its necessary details. I can, however, quote two passages, which confirm my claim. The first passage is from the *Critique*. In Chapter II about the 'System of all Principles of Pure Understanding', Kant argues (in the context of the Axioms of Intuiton) with respect to propositions about numbers (*quantitas*) and their relation to axioms as follows:

As regards magnitude (*quantitas*), that is, as regards the answer to be given to the question, "What is the magnitude of a thing?" there are no axioms in the strict meaning of the term: although there are a number of propositions which are synthetic and immediately certain (*indemonstrabilia*). The propositions, that if equals be added to equals the wholes are equal, and if equals be taken from equals the remainders are equal, are analytic propositions. [Consequently, they are not] axioms, [for these] have to be *a priori* synthetic propositions. On the other hand, the evident propositions of numerical relation are indeed synthetic, but are not general like those of geometry, and cannot, therefore, be called axioms but only numerical formulas. The assertion that $7 + 5$ is equal to 12 is not an analytic proposition. But although the proposition is synthetic, it is also only singular.[26]

There is a second, quite similar remark in a letter to the mathematician Johann Schulz of 25 November 1788, which shows that Kant was not muddled, but on the contrary quite clear about the essential point that finite arithmetic needs no axioms:

> Arithmetic certainly has no *axioms*, because, properly speaking, it has no *quantum* – i.e., no object of intuition as magnitude – for its object, but merely quantity, i.e. a concept of a thing in general through the determination of magnitude. It has, however, postulates, i.e. immediately certain practical judgements .... The judgement 3 + 4 = 7 seems to be merely a theoretical judgement ... the +[denotes] a kind of synthesis through which a third number is to be found out of two given ones, a task which is neither in need of a solution procedure nor of a proof. Hence the judgement is a postulate.[27]

Although the difference between quantum and quantitas is difficult to grasp and requires detailed commentary, the main point, the non-axiomatic character of elementary number theory, is clear, I think. Therefore let us turn immediately to geometry.

INTUITION AND GEOMETRY

Regarding geometry, one should expect a basically different relation between Hilbert and Kant than in the case of arithmetic. Whereas in the latter case, the agreement can be, and indeed is, almost perfect because in arithmetic no dramatic revolutions had intervened since Kant's time, the situation in geometry is completely different. Kant's view that geometry is based on pure intuition seems to be refuted once and for all by the emergence of non-Euclidean geometries during 19th century and by their successful application to nature in the theories of special and general relativity. In any case thus or very similar argues M. Friedman in his recent book *Kant and the Exact Sciences*.[28] It is surprising, therefore, to hear that Hilbert himself did not share this opinion, that, on the contrary, he conceded a certain legitimacy to Kant's transcendental philosophy not only in regard to arithmetic but also with respect to geometry, and that precisely in spite of the scientific developments just referred to. This does not mean, of course, that Hilbert agreed with Kant in every respect: on the contrary, in the case of geometry the differences are greater and more fundamental than in the case of arithmetic. Let me first explain the two most fundamental agreements and then stress the differences.

(1) The most important agreement is, by far, the common conviction that geometry is based on intuition. This is not only explicitly stated by Hilbert at the beginning of his 'Festschrift' (and all the other early lectures) about the foundations of geometry,[29] but also stressed by Hilbert in later years as well, after relativity theory had been established. He opens the 'Festschrift' with a quotation from Kant's *Critique*, which, to my mind, has to be taken very seriously, indeed literally, because it expresses, as I will show, Hilbert's epistemological point of view very accurately, and this not only with respect to geometry but to all theoretical sciences:

So fängt denn alle menschliche Erkenntnis mit Anschauung an, geht von da zu Begriffen und endigt mit Ideen. [A702/B730][30]

And immediately in the very first paragraph of the Introduction Hilbert says: "The designated task amounts to the logical analysis of our spatial intuition". But as late as 1930 Hilbert in his essay 'Naturerkennen und Logik' emphasized his basic agreement with Kant:

Thus the most general and fundamental idea of Kantian epistemology retains its significance: namely, the philosophical problem of determining that intuitive attitude a priori and thereby of investigating the conditions of the possibility of all conceptual knowledge and of all experience.[31]

In view of this and numerous similar passages, I think it is impossible to dismiss Hilbert's reference to Kant simply as a *façon de parler* or to pass over it in silence as most philosophers and mathematicians now do. On the contrary, it would be important to know how Hilbert arrived at this opinion and even more important to understand how it was possible for him to hold this view without being in contradiction with the development of modern geometry and its successful application in physics. But before I take up this challenge, I would like to discuss a further common ground between Hilbert and Kant.

(2) In his (1918) lecture on *Space and Time*, Hilbert, referring to the relationship between geometry and space, states clearly and unequivocally:

The exact systematic collection of the properties of space, the investigation of the logical relations between them and the development of the consequences which result from them form the [proper] content of Euclidean geometry.[32]

At first glance, this in fact is a rather surprising statement from Hilbert, since Euclidean geometry seems to be granted a *peculiar* status. However, for the moment, I would like to leave it at this and reserve the solution of this riddle for the conclusion. This statement interests me here for a different reason.

If you take this statement together with the statement about spatial intuition in the 'Festschrift', then this means: Euclidean geometry is *the* theory of space, its properties and logical relations, as we "intuitively" grasp it respectively as we represent it in our sensible imagination, that is, as a system of possible relations between things, in particular their shape and relative positions.[33] But Kant determines the relation between geometry, space and external intuition in nearly the same way. In *The Transcendental Exposition of the Concept of Space* he says: "Geometry is a science which determines the properties of space synthetically, and yet a priori. What, then, must be our representation of space in order that such knowledge of it may be possible?" And the answer is: "It must in its origin be intuition; for from a mere concept no propositions can be obtained which go beyond the concept – as happens in geometry" [*Critique*

B40/41]. This means that, according to Kant, Euclidean geometry is a *synthetic* a priori science of space precisely because it is the theory of our external, object-related intuition. This in turn is nothing other than our representation of space as a system of things qua *external appearance*. If one ignores for the moment the possible difference between space "as a system of things qua external appearances" and space as a "system of the possible relative positions of things", then the epistemological relation between geometry, intuition and space for both Hilbert and Kant is exactly the same: Euclidean geometry is the theory of our external intuition and this in turn is simply our representation of space as a system X, be it "the things qua external appearances", be it "the possible relative positions of things".

Thus it seems, as if I would like to suggest that Hilbert intended a "return to Kant". But that is not the case. In order to convince the reader, I simply have to explain the remaining fundamental difference between Hilberts and Kant's interpretation of geometry as the science of space respectively of our spacial intuition. I will do this in two steps which appear to be rather different but which in fact are closely related.

(3a) For Kant, geometry – quite unlike arithmetic – is in essence an *axiomatic* science, which means that it proceeds from *general statements* or principles instead of concrete immediately certain judgements or postulates as Kant names them. From these general principles we derive singular propositions by way of 'constructing' the geometrical concepts in (pure) intuition. For Hilbert, geometry is different. Hilbert views geometry epistemologically as closely related to arithmetic: both are based on *facts*, indeed on facts of our finite intuition, i.e., singular judgements about "intuitively given, extra-logical objects", like "straight line *a* is twice as long as straight line *b*" and similar statements. In this sense Hilbert remarks in his 'Festschrift':

> We can divide the axioms of geometry into five groups; each one of these groups expresses certain interrelated fundamental facts of our intuition.[34]

At first glance, this quotation seems to contradict my interpretation that, according to Hilbert, geometry is founded on facts of our intuition, because Hilbert obviously starts from axioms and not from singular propositions as in arithmetic. The apparent contradiction is, however, easily resolved: In arithmetic, too, we have axioms, but only as an "instrument" while we remain in the finite. Hence, if we transfer the relationship between facts and axioms in arithmetic to geometry, then we can, at least in principle, do without axioms in geometry, as long as we remain in an intuitive-finite domain. This in turn means that the axiomatic representation first becomes essential in geometry when one transcends the finite and steps into infinity. This, unquestionably, does take place in Euclidean geometry. The infinity is, however, in both cases, arithmetic as well as Euclidean geometry, the result of an "idealisation" or, as Hilbert puts it:

merely [an] *idea* – if one means by an idea, in Kant's terminology, a concept of reason, which transcends all experience and by which the concrete becomes completed in the sense of a totality".[35]

This means that in geometry, as in arithmetic, the axiomatic representation only serves as a means to simplify and unify the relations in the finite by including the regulative idea of infinity. If this interpretation is correct, then it implies a number of additional, important differences between Hilberts and Kant's views on geometry. For now, I will deal only with one difference, that which is most fundamental for the further course of the argumentation.

(3b) If it is correct that both Hilbert and Kant share the view that Euclidean geometry is an immediate expression of our spatial intuition, then this in turn has the unexpected consequence, based on Hilbert's argument for the ideality of Euclidean geometry, that our outer intuition in itself already involves ideal elements. And this is exactly what Hilbert maintains, because although space is *unlimited*, it is not *infinite* (at least not infinite because it is unlimited) for the simple reason that the two concepts are *logically independent*, as Hilbert points out: "Unlimitedness and finiteness do not exclude each other, as can be seen from the example of a two-dimensional spherical surface".[36] On the contrary, the assertion of the infinity of space is a genuine conceptual *extension* of the assertion of its unlimitedness. This view brings Hilbert for the first time into genuine conflict with Kant. More precisely, Hilbert thinks that Kants conclusion from the unlimitedness of space to its infinity is a logical mistake:

Up to the time of Kant and beyond, there was no doubt about the infinity of space. But this opinion rests on an error in thinking. From the fact that beyond a segment of space, always more space is present only the unlimitedness of space can be [logically] deduced but definitely not its infinity.[37]

This confronts us with the difficult question: Why does Hilbert retain Kant's view that Euclidean geometry is the only correct or appropriate theory of our intuition of space when, at the same time, he criticizes Kant's view of space to rest on a serious error of logical reasoning. This seems to be rather inconsequential! For a long time, I had no answer to this question. But now I believe that I have found the correct answer, at least in principle. In the final chapter I will explain the outlines of the answer.

GEOMETRY AND THE THEORY OF RELATIVITY: HILBERT'S MOVE TO HUSSERL

The core of the answer can be found in two lecture series on the genesis and development of the *special* and *general tbeory of relativity*, which Hilbert held in the winter term 1918/19 and the summer term 1921. The first is his lecture on 'Space and Time' [S&T], already mentioned, and the second his lecture about 'Grundgedanken der Relativitätstheorie' [GGRT], which, although two years apart, are very closely related in content. The first develops the objective, experimental reasons for the transition from the classical conceptions of space and time first to the special and then to the general relativistic conception of spacetime. The second reflects the *necessity* of this development in a more philosophical manner, a manner which is of particular significance here. Hilbert's view, in brief, is this:

There are *two* conceptions of space-time, a traditional one, rooted in the "Lebenswelt", and a scientific view which is coupled to the development of physics. I chose the term "Lebenswelt" quite deliberately because Hilbert means something similar to what Husserl describes with this expression in his book *Die Krisis der Europäischen Wissenschaften und die Transzendentale Phänomenologie*, when he distinguishes between the common conception of space and time of the *everyday life* and the *scientific* conception of space and time, in particular the four-dimensional spacetime of the theory of relativity.

In his lectures Hilbert again and again emphasizes that our classical conception of space and time as characterized by Euclidean geometry and the Galileo-group of transformations is, in the practical sense, fully correct, and not only that, but even *after* the development of the theory of relativity, the classical conception is the accepted basis of all our *practical* actions. Morever this conception was in a certain way (which will be characterized more closely in a moment) the epistemic *presupposition* for the development of the theory of relativity, without which the detection of facts which finally forced scientists to create and adopt the theory of relativity would have been quite impossible. To support my claim let me quote only two of many sections from the GGRT:

> Thus we have listed all the essential features of the old conception of space, time and motion. But before we turn to our second question, it is still absolutely necessary to bring to mind how excellent this conception of space-time has proved to be. As far as natural sciences and their applications are concerned, we find that everything is based on this conception. And in this construction everything fits together perfectly. Even the boldest speculations of physicists and astronomers are brilliantly confirmed in the minutest detail so that one can say that the experiences of everyday life, our practice and custom, the traditional intuition and the most demanding sciences were in complete agreement and most pleasant harmony with each other. (p. 20)
>
> This prevailing conception of space-time was the framework within which our entire knowledge of nature, in particular of mechanics, physics and astronomy, this rich, expansive, manifold and diversely specified material fits perfectly. And what is more, this conception of space-time did not develop from science and not at all from reflection. On the contrary we use the concepts space and time in *daily life* in this way and experience them continually anew so that they are familiar to us and we feel comfortable with them.
>
> So it happened that the laws of geometry and the geometrical theory of motion in which this conception of space-time finds its precise expression and which represents the permanent tools, the ABC of the physicist so to speak, were simultaneously viewed as something preceding all physics which can be recognized without experiment by intuitive reasoning.

Thus the theory of the sum of the angles in a triangle or the theory of the addition of velocities are just as valid and with the same degree of certainty as the laws of arithmetic. (GGRT pp. 30–31)[38]

I think, these quotations testify that Hilbert's view regarding classical space-time theory indeed is closely related to Husserl's concept of "Lebenswelt", especially since Husserl too conceives of space-time, according to which we act in daily life, as being for the most part independent of the historical development of the sciences. Since it existed long *before* the development of modern science, and it remained quite exactly the same, even *after* the development of the theory of relativity. So far, so good.

Yet, a trivial objection could be raised to this interpretation: namely, Husserl wrote *The Crisis of the European Sciences* in the mid-thirties whereas Hilbert gave his lectures in 1918 and 1921 respectively. Hence, Hilbert could not have known Husserl's conception of "Lebenswelt". But the objection is no objection at all, because in the first place Hilbert *could* have developed his particular view quite independently of Husserl. In this case we would have a mutually independent creation of related epistemological views, because it is almost certain that Husserl did not know Hilbert's lectures on relativity.[39] However, the situation is actually more interesting. Not only had Husserl already developed the nucleus of his later distinction between science and 'Lebenswelt' in 1905 in his 'Five Lectures about The Idea of Phenomenology', but he had developed it probably in full awarness of most of Hilbert's work on geometry.[40] And this last point is the crucial one, because (as we now know) Hilbert made the distinction in question already in his early work on geometry. In his first lecture series on geometry in 1891, as a 'Privatdozent' in Königsberg, Hilbert already remarked: "Indeed, the oldest geometry[41] also arises from the intuition of things in space, as it is offered up by daily life, and, like every science in its beginnings, had posed problems of a practical nature".[42] Of course, this might be seen as a sloppy remark, but it is not, and on the contrary it is intended as a serious claim that the epistemic origin of Euclidean geometry becomes evident from the fact that Hilbert repeats it at the beginning of his lectures on 'The Foundations of Euclidean Geometry' in 1898. Here he says: "This [Euclidean] geometry is in a way the geometry of daily life. It is the basis of all consideration of nature and natural science".[43]

It is not my intention to maintain that Husserl was indeed influenced by Hilbert's view in this respect. Although we know that Husserl had close contact with Hilbert in the years shortly before and after the turn of the century (it was Hilbert who had initiated Husserl's call to Göttingen) I will leave aside the thorny question of who influenced whom. Instead I want to point out a striking similarity between Husserl's and Hilbert's epistemological point of view with respect to mathematics as whole, not only of geometry but also of arithmetic. Both were *finitists* with respect to the question of where the certainty of our mathematical knowledge comes from. And yet, at the same time, both believed in *Cantor's paradise*, that is, in the legitimacy of the (actual) *infinite*. This attitude posed for both thinkers a serious problem, namely: how can the *transition* from the finite to the infinite be justified? In Hilbert's terms: how can the infinite be justified on the basis of the finite? In spite of this common problem it is not unlikely that both thinkers finally came up with a similar solution. In fact, in Husserl's case we have ample evidence that his late distinction between the sciences and the *Lebenswelt* was intended to solve precisely this problem. But what does the solution look like?

To answer this question is in Husserl's case not an easy task, particularly not with respect to geometry, because Husserl himself never published a detailed proposal how Euclidean geometry, which obviously takes space as infinite, is constituted in a phenomenological manner on the basis of our *finite* intuition rooted in *Lebenswelt*. Aside from some suggestions in *Ding und Raum* (1907) and *Ideen* (1913), he seems to have delegated the tricky task to his students E. Stein and O. Becker. The best I can do in this respect, is to refer the reader to Becker's excellent work 'Beiträge zur phänemenologischen Begründung der Geometrie und ihrer physikalischen Anwendungen' (1922). Yet one qualifying remark is required. However sceptical the reader may be about Becker's phenomenological justification of Euclidean geometry (a scepticism, which I share because the so-called *Wesensanalyse* of the phenomenal space relies too much on psycho-physical considerations about the subject apprehending space) the reader should not underestimate Becker's phenomenological approach as epistemologically naive or inferior to Weyl's *infinitesimal* analysis of space, as M. Friedman suggests.[44] Two simple reasons can be raised against this estimation. In the first place, Weyl's "infinitesimal" analysis of space is itself problematic, both from a physical perspective and from Hilbert's axiomatic point of view, because it presupposes the validity of Euclidean geometry in the infinitesimal small and, hence, takes as granted what is, properly speaking, the task of the axiomatic approach, namely an investigation of the dependence and independence of the different axioms which together amount to Euclidean geometry. Second, and much more important, there is a different reading of the phenomenological constitution of Euclidean space, which makes perfect good sense from a physical as well as methodological point of view, and this is Hilbert's "transcendental" approach to Euclidean geometry, to which I now turn.

To begin with, I have to answer the question: in what sense is the conception of space-time as rooted in the *Lebenswelt* a presupposition for the development of the special theory of relativity?[46] For the sake of brevity, I can only indicate the direction, in which the correct answer lies.[47] Only on the basis of the traditional conception of space-time was it possible to increase the precision of experimental arrangements and measuring methods to such an extent that one could prove by means of very sophisticated optical experiments that Galileo's theorem for the addition of velocities is *false*, strictly speaking, or at least only approximatively valid. Yet, because the addition theorem is an immediate logical consequence of the classical conception of space-time, hence, this too had to be rejected and a far more general theory put in its place: the special theory of relativity. (The adjective 'special' is very misleading because the *principle of relativity* as formulated in the special theory of relativity covers *many more phenomena* than Galileo's corresponding principle in classical mechanics. The latter was defined only for macroscopic bodies like ships and planets but not for light-rays, whereas the principle of relativity in the "special" theory covers also the propagation of light (as well as other electromagnetic waves).)

Two opposite objections can be raised against this "dialectical" procedure: The first says that one cannot presuppose *false* assumptions like the traditional conception of space-time, for this is to presuppose nothing at all. The second maintains that the procedure as characterized by Hilbert is illogical, or even worse, that it entails a *vicious circle* because it destroys the assumptions (of classical space and time), on which basis the experiments and measurements were performed. Consequently, classical space-time cannot simply be suspended and another theory, special relativity, put into its place. Both objections are unconvincing. First, physicists have certainly used classical space-time conceptions; indeed, all experiments until recently were designed and performed on this basis. Second, there is just no serious circularity or illogicality in the procedure indicated. On the contrary it is a paradigm of logical reasoning, and therefore I suspect that the error is on the side of the opponents; probably they were mislead by the *several* meanings of the terms 'assumption' and 'presuppose'. But, whatever the error is, I will concede this much: the logical meaning of the term 'presupposition' has not been sufficiently analysed and logically explained: In what sense does the special theory of relativity presuppose the classical conception of space-time and, hence, of Euclidean geometry?

In order to answer this question, I will compare Hilbert's conception of "presupposition" with Kant's transcendental conception of Euclidean space as one of the two aesthetic conditions of the possibility of experience. In particular I will ask: how compatible is the distinction between *two* conceptions of space (one anchored in the *Lebenswelt*, the other the result of 'scientific' investigations) with Kants *undivided* conception of space as one of the forms of pure intuition which is at the same time, and this is quite essential for Kant's transcendental consideration, one of the "conditions for the possibility of experience"? The answer bears certain "dialectical" features in that it confirms Kant's conception in one respect, yet contradicts it in another, and both together are combined in a third phenomenologically modified position.

First, the positive aspect of distinction which confirms Kant: For Kant, Euclidean geometry was a synthetic a priori science of space exactly because space is only the *subjective* form of external intuition. The latter for Kant, however, means nothing less than that space is a formal character of the external senses and determines how the subject is "to be affected by the objects and thus obtaining an immediate representation, that is, intuition of them" [B 41]. Thus it is completely consequential when Kant says: "The constant form of this receptivity: which we term sensibility, is a necessray condition of all the relations in which objects can be intuited as outside us; and if we abstract from these objects, it is a pure intuition, and bears the name of space" [B43/A27].

This subjective interpretation of space as a form of representation or intuition of external objects is taken up by Hilbert in the distinction between "*lebensweltlicher*" and scientific conceptions of space (viz., of space-time) when he, like Kant, supposes that the conception of space-time, which is rooted in the *Lebenswelt* is a kind of *anthropological constant* which is, for the most part, independent of respective developments in science. Just as today we still distinguish between "up" and "down", in spite of the Copernican revolution, we also retain the classical conception of space-time in everyday life despite the acceptance of the theory of relativity. The anthropological constraint on the everyday conception of space-time is, however, for Hilbert precisely the prime reason why he distances himself from Kant in one very specific aspect.

Whereas for Kant the *subjective* forms of sensibility are simultaneously the *objective* conditions of the perception of objects and thus the central conditions for the possibility of experience, for Hilbert this is definitely not the case. On the contrary, the goal of science is to free our cognition from the mere subjective conditions of sensibility and to try to attain an *objective*, observer independent cognition. Hilbert includes, however, among the subjective conditions of sensibility not only the secondary qualities such as colours, sounds and smells, but also space and time as the two forms of human intuition rooted in the *Lebenswelt*. This is the deeper reason why Hilbert differentiates between *science* and *Lebenswelt*, between a *everyday* conception of space and time and only views the former, not the latter, as objective:

> Hitherto, the objectification of our view of the processes in nature took place by emancipation from the subjectivity of human sensations. But a more far reaching objectification is necessary, to be obtained by emancipating ourselves from the *subjective* moments of human *intuition* with respect to space and time. This emancipation, which is at the same time the high-point of scientific objectification, is achieved in Einstein's theory, it means a radical elimination of *anthropomorphic* slag, and leads us to that kind of description of nature which is *independent* of our senses and intuition and is directed purely to the goals of objectivity and systematic unity.[48]

Although this statement is clear and quite unmistakable in its insistence on separating the *subjective* intuiton of space and time from their *objective* structure (as represented in our best physical theories), two questions must to be raised which are not answered by Hilbert's statement itself. First, what does Hilbert understand by space and time "objectively" if they are not the subjective forms of human intuition? And, second, why then do we need the classical conceptions of space and time in physics at all if they are merely our *subjective* forms of intuition, and do not correctly represent the objective structure? Let me take the second question first.

The answer, in principle, has already been given. We need the classical conception of space-time in order to design the bulk of physical experiments and to measure continuous quantities like distance, velocity etc., and in particular to separate *forces* from mere kinematical changes. However, the presupposition of the classical conception of space-time for the possibility of measurement is not an *absolute* one, as it is in Kant's transcendental epistemology. It is, rather, of a *contingent* and anthropomorphic nature, connected to human cognitive powers in the sense that we as *finite* beings can only recognize the world by making certain *ideal* assumptions regarding the structure of time and space. However,we have to bear in mind precisely that these *are* ideal assumptions. Thus, classical space-time is not presupposed as something *real*, but only as something *ideal*, in fact as a regulative principle (or idea) in Kant's sense. Once this distinction is made, any illusion as to the absoluteness of the presupposition vanishes. This in turn means that we must be prepared to *change* our ideal assumptions regarding space and time as and when this becomes *necessary* for the sake of physics, as was indeed the case at the turn of the century.

This brings me back to the first question regarding the *objective* structure of space and time. To this question, neither Hilbert nor anybody else has anything like a *final* answer: for any answer given will depend on the historical development, and the present state, of science. There can be no recognition of the objective structure of space and time without some ideal presuppositions, and these presuppositions may well turn out, in the light of developing science to be false, thus leading to further presuppositions, and so on.

Against this line of argument it could be objected that we are never *forced* to change an ideal assumption with respect to geometry, and that, in particular, Euclidean geometry can be maintained come what may. Poincaré's geometric conventionalism amounts to something like this, in particular that Euclidean geometry would have nothing to fear from future experience. It is, therefore, of interest to note that both Hilbert and Husserl (through his disciple Becker) vehemently opposed Poincaré's conventionalism. Hilbert's reasons can be briefly explained as follows. According to Helmholtz, our knowledge of geometry is essentially based on the notion of the *free mobility of rigid bodies* in space. But this, however, singles out only the spaces of 'constant curvature'. For this reason, Poincaré could quite correctly maintain that, provided space has constant curvature, the choice of an appropriate geometry is a matter of taste or convention, and that therefore the choice of Euclidean geometry is irrefutable by experiments with rigid bodies. However, according to Hilbert, the situation changes dramatically and fundamentally with the new mechanics introduced by the special theory of relativity. Here the classical conception of space and time has to be abandoned irrevocably because Galileo's law of the additivity of velocities is *contradicted* by the body of experiments available at the time. More precisely, the *impossibility* of rigid bodies in the sense that there is no body which "has the same shape and size when stationary and when in motion"[49] contradicts the Euclidean conception of space as defended by Poincaré. This is so because if rigid bodies existed *infinite* velocities would be possible, (something ruled out by the special theory of relativity). Thus, for Hilbert, the *special* theory of relativity *already* shows Poincaré's conventionalism to be wrong, and this long before the advent of the general theory of relativity.

Let me return to Einstein's conception of the relationship between "geometry and experience", in particular his distinction of *two* kinds of geometry. As we have seen: Hilbert, too, differentiates between two kinds of geometry. So far Hilbert and Einstein agree. However, they do not agree on how and in what way the two geometries differ. In particular, they do not agree on the epistemological way the distinction is to be justified. Einstein suggests to divide geometry into a purely *logical-formal* geometry, i.e., one not interpreted at all, in particular not intuitively, and a *practical* geometry, i.e., one interpreted by coordinating real objects to geometrical terms. Hilbert does *not* suggest such a division; in particular, he doesn't assume that there is a pure logical-formal geometry as Einstein ascribes to him, and indeed this would be very questionable from an epistemological point of view. Hilbert takes both kinds of geometry as interpreted, but in a *different* way. The first, he thinks interpreted by recourse to intuition anchored in the *Lebenswelt*; the second, becomes interpreted through controlled experiments and precision measurements resulting in a certain way from a refinement and an objectification of the intuition rooted in the *Lebenswelt*. In the end, however, if we ignore the epistemologically different starting points, Hilbert and Einstein again agree that geometry is a *natural* science based on real experiments and measurements. Thus, similarly to Einstein, Hilbert can assert:

> Geometry is nothing but a branch of physics; in no way whatsoever do geometrical truths differ essentially from physical truths nor are they of a different nature.[50]

NOTES

* I take this opportunity to thank my colleagues at ZiF for their very helpful comments and criticisms during the academic year. For the preparation of this paper, I am especially indepted to Michael Hallett, who not only improved my English but also saved me from a number of errors.

[1] Einstein (1921, p. 3–4).

[2] Quine's: 'Two Dogmas of Empiricism' in *Philosophical Review* (1951) reprinted in *From a Logical Point of View*, Harper Torchbooks, New York.
[3] Einstein (1921, p. 4). The emphasis in the last sentence is mine; the expression 'Logical Formal' has been left almost the same as Einstein's original German term "das Logisch-Formale".
[4] This is Hilbert's own label for the "new" understanding of axioms, exemplified for the first time in his *Grundlagen der Geometrie* (1899). Whether the *Foundations of Geometry* really entail a "new" understanding of axioms [in oppositon to the traditional one] I will not discuss here. It suffices for my purpose to remark that the scientific community, in particular certain philosophers like Schlick, Carnap and Reichenbach, interpreted Hilbert's achievements in the *Grundlagen* very quickly in this particular way. Einstein is a further example of this trend, though, of course, a rather superb one.
[5] Einstein (1921, pp. 4–5).
[6] Indeed, Hilbert makes a clear distinction between *two* ways of recognising geometry, as we will see at the end of the paper. But this distinction is almost the contrary of Einstein's distinction between pure axiomatic and practical geometry.
[7] For the sake of clarity, I should add that the logical empiricists were not the only ones to interpret Hilbert in this formalist way: Frege did too. But there is an important difference between Frege and the empiricists: Whereas the latter agreed with this type of formalist reading, because it fitted so nicely with their epistemological views. Frege developed it in order to criticize Hilbert for his unclear notions of *axiom* and *definition*, and to reduce these to absurdity. Frege's polemic against Hilbert seems to me also unjustified and to rest on a dual misunderstanding. But I refrain from discussing this here.
[8] See Einstein (1921, pp. 5–6); the German word 'zuordnen' I have translated as 'coordination' although the latter does not capture precisely the sence of the action conveyed by the term "zuordnen".
[9] Hilbert (1891, p. 5ff).
[10] Hilbert (1891, pp. 6–7); Hilbert seems here to advocate a kind of 'logicicsm' with respect to arithmetic; this impression is, however, an illusion; it mainly results from the strong opposition of geometry and arithmetic; later he rejected the idea of logicism completely.
[11] Hilbert (1898a, p. 1); 'Geometrie ist die vollkommenste Naturwissenschaft'; see (1898).
[12] Hilbert (1898b, p. 2); 'Geometrie ist [dadurch] eine rein mathematische Wissenschaft [geworden]'; the words in brackets are later supplements.
[13] Hilbert (1899, p. 1) 'Die bezeichnete Aufgabe läuft auf die logische Analyse unserer räumlichen Anschauung hinaus'. See also (1898), (1898a) and (1902).
[14] See Hilbert (1898, p. 3ff).
[15] See Hilbert (1898, p. 5ff) and also (1891, p. 3ff).
[16] The ultimate reason is, of course, that Hilbert is immediately aware that the proposal of the logical empiricists, to split geometry into two parts, is no solution to the puzzle of geometry. On the contrary one must take geometry as a unity between mathematics and natural sciences and explain, why that is as it is, in order to solve the puzzle.
[17] See M. Friedman's essay in this volume. All three authors share the conviction that Euclidean geometry has a privileged position among the possible geometries; but they differ with respect to the reasons for its privilege. Carnap gives essentially an anthropomorphic explanation: the geometry of our intuition is indeed Euclidean, but it is valid only locally, in infinitesimally small regions, our close surroundings, so to speak. Weyl, on the other hand, gives a mathematical justification; he argues that the demand of continuous differentiability of the transformation functions for coordinates presupposes the validity of Euclidean metric in the 'infinitesimal small' (Weyl 1925, p. 12). Hilbert's justification of the privileged status of Euclidean geometry again differs from both.
[18] In particular I cannot offer a detailed account of the internal development of Hilbert's finitism, but have to take it in its mature form, as it was formulated for the first time in his 'Neubegründung der Mathemetik' (New Foundations of Mathematics) in 1922.
[19] Hilbert (1922, p. 162).

[20] Already in his 'Untersuchung über die Deutlichkeit der Grundsätze der natürlichen Theologie und Moral' (1764) Kant asserts: 'Mathematics considers in its solutions, proofs and consequences the general under the signs *in concreto*, philosophy [Weltweisheit] however, the general through the signs in abstracto' [AA 73]. But also later, in part II of the *Critique of Pure Reason* we find similar remarks: "Philosophy confines itself to universal concepts; mathematics can achieve nothing by concepts alone but hastens at once to intuition, in which it considers the concept in concreto" [B744/A716]. The last remark is particularly important because it shows that, according to Kant, there exists a very close connection between intuition and symbols in concreto. In intuition we consider a concept in concreto by constructing a concrete object which fits the concept, and in view of this object we prove the proposition in question.

[21] See the Inaugural Dissertation 'De mundi sensibilis atque intelligibilis', Section 12; compare this also with *Critique* B 49, vhere Kant explicitly states, "that the concept of change and with it the concept of motion [as change of position] is only possible in and by means of the representation of time": furthermore B 291/92 and *Prolegomena*, Section 10, as well as the letter to Schulz of 25 November, 1788.

[22] I use here a concept of explanation, which differs from the usual deductive-nomological conception of Hempel and Oppenheim, insofar as I judge it as essential that an explanation not only permits the logical deduction of singular propositions from law-like sentences, but also presents new and deeper reasons for the unity and order of a certain domain of facts and their objective relations to further domains. This "Tieferlegung der Fundamente" is, as Hilbert says, one of the main tasks of the axiomatic method.

[23] Hilbert (1922, p. 164); German original: "Bei der solcherart [anschaulich] betriebenen Zahlentheorie gibt es keine Axiome, und so sind auch keinerlei Widersprüche möglich. Wir haben eben konkrete Zeichen als Objekte, operieren mit diesen und machen über sie inhaltliche Aussagen .... Aber freilich läßt sich nicht die ganze Mathematik auf solche Art erfassen. Schon beim Übertritt zur höheren Arithmetik und Algebra, z. B. wenn wir Behauptungen über unendlich viele Zahlen oder Funktionen gewinnen wollen, versagt jenes inhaltliche Verfahren. Denn für unendlich viele Zahlen können wir nicht Zahlzeichen hinschreiben oder [geeignete] Abkürzungen einführen".

[24] The "tertium non datur" says in this case: Either all numbers have a certain property E, or there is at least one number, which has the property non-E. See Majer (1993, p. 60).

[25] Hilbert (1923, p. 181); [Italics are mine]; original in German: "Auf Grund der Axiome 1. bis 10. erhalten wir leicht die ganzen positiven Zahlen und die für diese geltenden Zahlgleichungen. Auch läßt sich aus diesen Anfängen mittels 'finiter' Logik durch rein anschauliche Überlegungen, wozu die Rekursion und die anschauliche Induktion für vorliegende endliche Gesamtheiten gehört, die elementare Zahlentheorie gewinnen, ohne daß dabei eine bedenkliche oder problematische Schlußweise zur Anwendung gelangt. Die beweisbaren Formeln, die auf diesem Standpunkt gewonnen werden haben sämtlich den Charakter des Finiten, d.h. die Gedanken, deren Abbilder sie sind, können auch *ohne irgendwelche Axiome* inhaltlich und unmittelbar mittels Betrachtung endlicher Gesamtheiten erhalten werden".

[26] *Critique* [A164/B204].

[27] "Die Arithmetik hat freylich keine Axiome, weil sie eigentlich kein Quantum (d.i. keinen Gegenstand der Anschauung als Größe) sondern blos die Quantität, d.i. einen Begriff von einem Ding überhaupt durch Größenbestimmung zum Objecte hat. Sie hat aber dagegen Postulate d.i. unmittelbar gewisse practische Urtheile ... Das Urtheil $3 + 4 = 7$ scheint zwar ein blos theoretisches Urtheil zu sein ... das + [bezeichnet aber] eine Art der Synthesis aus zwei gegebenen Zahlen eine dritte zu finden und eine Aufgabe, die keiner Auflösungsvorschrift noch eines Beweises bedarf, mithin ist das Urtheil ein Postulat.

[28] See Friedman (1992, p. xii), where he argues that Kant's philosophy (of geometry) is refuted precisely because it was so extraordinarily well adapted to the exact sciences of his days, in particular to Newton's physics. Compare, however, his contribution to this volume, where he considers the possibility of a modification of Kant's philosophy into a more general abstract form, which is compatible with the exact sciences of our century.

[29] The 'Festschrift' was Hilbert's contribution to a volume published in honor of Weber and Gauss which later became published separately under the title 'Grundlagen der Geometrie'. It was the result of a number of lectures on the foundations of geometry, that Hilbert had held since 1881. See Hilbert (1891, 1893, 1898, 1898a, 1899, 1902).
[30] This quotation however, must not be misinterpreted as a motto for the *Grundlagen* of the following sort: It was Hilbert's intention to *eliminate* intuition from geometry in favour of concepts. (This is suggested by H. Stein in his otherwise very interesting essay 'Logos, Logic, and Logistique'.) This would contradict, not only Hilbert's *fundamental* tendencies to base mathematics on a secure finite domain of intuitively identifiable objects, but also deprive the geometrical concepts of all their spatial meaning and, hence, reduce them to mere formal notions, an idea to which Hilbert has strongly opposed. See Hilbert 'On the Infinite' and my paper 'Different Forms of Finitism', in which I defend Hilbert's finite point of view against modern tendencies to dissolve Hilbert's finitism.
[31] Hilbert (1930): "Damit behält also der allgemeinste Grundgedanke der Kantschen Erkenntnistheorie seine Bedeutung: nämlich das philosophische Problem, jene anschauliche Einstellung a priori festzustellen und damit die Bedingungen der Möglichkeit jeder begrifflichen Erkenntis und zugleich jeder Erfahrung zu untersuchen", p. 961.
[32] Hilbert (1918, p. 3). In the original German: "Die genaue systematische Zusammenstellung der Eigenschaften des Raumes, die Untersuchung der zwischen ihnen bestehenden logischen Beziehungen und die Entwicklung der Konsequenzen aus ihnen bildet den Inhalt der Euklidischen Geometrie".
[33] It is important to note that Hilbert does not refer to *rigid bodies* and their motions as the proper object of geometry, as for example Helmholtz, Lie and Poinceré typically do. Instead, he speaks only of the "spatial positions and relations of things", and this quite from the beginning, long before the advent of the special theory of relativity. (See his lectures (1891, 1893 and 1898).) In this way, he avoids the difficulty to built geometry on a physical notion, namely that of rigid bodies, which is, strictly speaking, incompatible with special relativity, because, as Hilbert remarks: "there is no rigid body in the sense that it would have the same shape and size when motionless and when in motion" (1918, p. 82). See Majer (1995) where I explain Hilbert's critique of Poincaré's conventionalism more closely.
[34] Hilbert (1899, p. 2).
[35] Hilbert (1925, p. 190).
[36] Hilbert (1933).
[37] Hilbert (1933); the section closes with the sentence just quoted above.
[38] Corresponding sections for the practical validity of the classical conception of space and time can also be found in the early *Raum und Zeit* on pages 8 and 15:

> If this common view of space is valid, then naturally all the propositions of [Euclidean] geometry must be confirmable by experience. Thus, for example, the sum of the angles in a triangle must alvays be 180°. And this is indeed really the case.

And a bit later:

> The conception of time thus presented must – if it is applicable – prove itself in reality and, in fact, it does confirm itself brilliantly. The fact that the lives of different people can intermesh so intricately is based on its application.

[39] Husserl had left Göttingen in 1908 long before Hilbert gave his lectures on relativity and the latter were not circulated very much – at least to my knowledge.
[40] By "most of Hilbert's work on geometry" I do not only mean the 'Festschrift' and the other published papers but also the lecture series on geometry in 1898 and 1902 which Hilbert gave in Göttingen after Husserl had established his contacts with Hilbert.
[41] The geometry in question is that of Euclid.
[42] See (1891, p. 7): the original in Gcrman: "In der That entspringt denn auch die älteste Geometrie aus dem Anschauen der Dinge im Raum, wie sie das *tägliche Leben* bietet,und, wie alle Wissenschaft am Anfang, hat sie Probleme vom praktischen Bedürfnis gestellt".

[43] See (1898, p. 2); the original in German: "Diese [euklidische] Geometric ist gewissermaßen die Geometrie des *täglichen Lebens*. Sie liegt aller Naturbetrachtung und aller Naturwissenschaft zu Grunde".
[44] Friedman; this issue, pp. 255ff.
[45] Schmidt (in his contribution to this volume) points out that the assumption that space has a *differentiable*, structure is from a micro-physical perspective no less problematic than the assumption of rigid rods and homogenous clocks.
[46] It suffices here to concentrate on the special theory of relativity because the general theory of relativity was only a theoretical extension of certain aspects that had already been prepared in special relativity. Taken in this way, the special theory of relativity is the logical presupposition for the general theory.
[47] The complete answer as well as its justification is explained in Hilbert's two lectures on relativity, to which I have to refer the reader for any closer inspection.
[48] Hilbert (1921, p. 13); (italics by the author).
[49] Hilbert (1918, p. 82): see also Majer (1995) where Hilbert's criticism of Poincaré's conventionalism is developed in greater detail.
[50] Hilbert (1930, p. 962). There is a very similar remark in Einstein (1921, p. 6): "Geometry, thus supplemented, is obviously a natural science; we can definitely view it as the oldest branch of physics".

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