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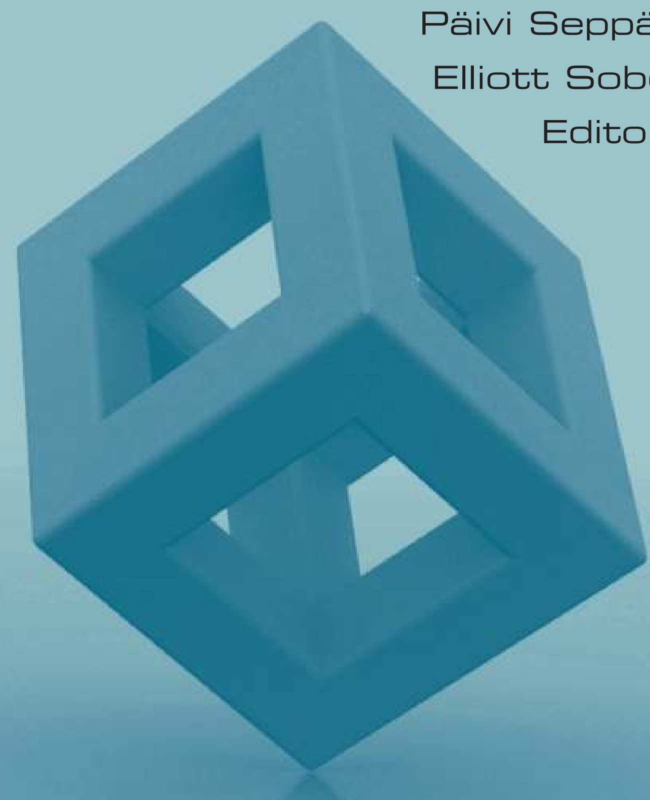

2 Models in geometry and logic: 1870-1920

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Abstract. Questions concerning the consistency of theories, and of the independence of axioms and theorems one from another, have been at the heart of logical investigation since the beginning of logic. It is well known that our own contemporary methods for proving independence and consistency owe a good deal to developments in geometry in the middle of the nineteenth century, and especially to the role of models in establishing the consistency of non-Euclidean geometries. What is less well understood, and is the topic of this essay, is the extent to which the concepts of consistency and of independence that we take for granted today, and for which we have clear techniques of proof, have been shaped in the intervening years by the gradual development of those techniques. It is argued here that this technical development has brought about significant conceptual change, and that the kinds of consistency- and independence-questions we can decisively answer today are not those that first motivated metatheoretical work. The purpose of this investigation is to help clarify what it is, and importantly what it is not, that we can claim to have shown with a modern demonstration of consistency or independence.

Keywords: independence, consistency, Hilbert, Beltrami, history of logic, non-Euclidean geometry.

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1 Introduction

In 1939, Ernest Nagel wrote:

> The sort of questions considered by meta-mathematics, e.g. the consistency and independence of axioms, have been discussed in antiquity and have been cultivated ever since by mathematicians in every age; but such problems have been clearly formulated and systematically explored only after pure geometry had been freed from its traditional associations with space, and only after its character as a calculus had been isolated from its applications. (Nagel, 1939, §74, 202-3.)

While there is no doubt a good deal of truth to this claim, the purpose of this paper is to suggest a way in which the characterization it gives us of the role of models in meta-mathematics is misleading. The aspect of Nagel's view that I want to question is the idea that with the increasing formalization of mathematical theories in the late 19th and early 20th centuries, we achieved at last a method, and tools, for the rigorous treatment of ancient questions. What I will suggest in what follows is that the questions we can now answer with our modern rigorous tools are not the same questions as those that arise for mathematical theories prior to the modern era. The questions we can now raise and answer, I will argue, have been shaped significantly by the tools we have developed. One result of this is that the notions of independence and consistency that we now take for granted are not those that went by these names prior to about 1900, and the clean methods we now have for demonstrating independence and consistency do not answer what in e.g. 1700, or even in 1850, would have been called by these names.[1]

2 1900

We begin by looking at the state of the art of independence proofs in the penultimate year of the nineteenth century. Here our example is David Hilbert's 1899 *Foundations of Geometry*, in which we find a clear and systematic application of the technique that was becoming, at this point, the standard approach to demonstrations of independence.[2] As Felix Klein puts it in 1908, describing what he calls the "modern theory" of geometric axioms:

> In it, we determine what parts of geometry can be set up without using

[1] On the role of geometry in the early development of modern logic, see Webb (1985).

[2] See Hilbert (1899). Hilbert was not alone in using the technique to significant effect at this time. Its application can be seen as well in the work of the Italian school (see e.g. Peano, 1894, Padoa, 1901, Pieri, 1898; for discussion of Pieri's work see Marchisotto and Smith, 2007) and in closely-related work by e.g. Veblen (1904) and Dedekind (1888).

> certain axioms ...
> As the most important work belonging here, I should mention Hilbert's [1899].
> (Klein, 1908, as quoted in Birkhoff and Bennett, 1988, p. 185.)

Hilbert's work in the *Foundations of Geometry* monograph involves a clear and careful axiomatization of Euclidean geometry, together with a consistency proof for the whole, and a series of independence proofs that demonstrate the connections of logical entailment holding between the various parts of the edifice. Hilbert characterizes both consistency and independence here in terms of the relation of logical deducibility: a set of axioms is *consistent*, he tells us, if "[I]t is impossible to deduce from them by logical inference a result that contradicts one of them" [§9], and a geometric axiom or theorem is *independent* of a collection thereof if it cannot be deduced from that collection.

Hilbert's method of demonstrating non-deducibility is as follows: Given a set AX of axioms, and a statement (perhaps another axiom) A, we begin by uniformly re-interpreting the geometric terms ("point," "line," "lies-on," etc.) in AX and in A, in terms of objects and relations given by a different theory, in this case a theory R of real numbers and collections thereof. We then note that, as re-interpreted, each of the sentences AX, together with any sentence deducible from them, expresses a theorem of R. Finally, the negation $\neg A$ of the target sentence A also expresses a theorem of R, which, assuming the consistency of R, guarantees that A itself does not express a theorem of R. Still assuming the consistency of R, then, we have a guarantee that A is not deducible from AX.

The consistency of AX, in the sense of the non-deducibility of a contradiction from it, is demonstrable similarly, again assuming the consistency of the background theory R. As Hilbert says,

> From these considerations, it follows that every contradiction resulting from our system of axioms must also appear in the arithmetic related to the domain [of the background theory]. (Hilbert, 1899, §9.)

An important point to note about the interpretation-theoretic technique used by Hilbert here is that it presupposes that the relation of deducibility in question is "formal" in the sense that it is unaffected by the reinterpretation of geometric terms. It is this that guarantees that the sentences deducible from AX will express theorems of R under the reinterpretation, given only that the members of AX do. But the deducibility relation is not, for Hilbert in 1899, "formal" in the sense of "syntactically specified;" there is no formal language at this point, and no explicit specification of logical principles. We will use the term "semi-formal" for such a relation. The first thing, then, that Hilbert's interpretations (or models) shows is that a given sentence is not semi-formally deducible from a collection of sentences.

Hilbert's models also show, importantly, the *satisfiability* of the conditions implicitly defined by the collections of sentences in question. Given a collection $AX \cup \neg A$ of sentences whose geometric terms appear schematically, a Hilbert-style reinterpretation on which each member of that collection expresses a truth about constructions on the real numbers demonstrates the satisfiability of the condition defined by the collection. Equivalently, it demonstrates that the condition defined by AX can be satisfied without satisfying the condition defined by A.[3]

Hilbert's technique, then, demonstrates the *independence* of a given sentence from a collection of sentences in two different senses. Taking as an example the question of the independence of Euclid's parallel postulate (PP) from the remainder of the Euclidean axioms (EU) for the plane, the two senses, with our labels introduced, are:

- $Independence_D$: (PP) is not (semi-formally) deducible from (EU);
- $Independence_S$: The condition defined by $(EU) \cup \neg(PP)$ is satisfiable.

Independence$_S$ is the stronger of the two notions, though they are extensionally equivalent in the setting of an ordinary first-order language.[4]

3 Frege

Gottlob Frege's work, in the same period, focuses on a notion of independence that's distinct from both of the relations demonstrable via Hilbert's technique. For Frege, *independence* is a relation not between sentences but between *thoughts*, i.e. between the kinds of things expressible by fully-interpreted sentences. Each thought, as Frege understands it, has a determinate subject-matter: thoughts about geometric objects and relations are entirely distinct from thoughts about collections of real numbers. Hence the re-interpretation of sentences along Hilbert's lines will result in the assignment to those sentences of different thoughts. Finally, logical connections between thoughts, connections like dependence and independence, provability and consistency, are sensitive as Frege sees it to the contents of the simple terms in the sentences used to express those thoughts. Hence Hilbert's re-interpretation strategy amounts, from Frege's point of view, to shifting attention from the geometric thoughts in which one was originally interested to an entirely different collection of thoughts, a collection whose logical properties are no guide to those of the original target thoughts. As a result, Frege takes it that Hilbert's technique is unsuccessful in demonstrating consistency and independence. In Frege's words,

[3]This understanding of consistency in terms of satisfiability was the more central concern for e.g. Dedekind and Veblen.

[4]The equivalence is given by the completeness of first-order logic. Hilbert's setting is that of natural language without strictly-defined relations of deducibility or satisfiability, so the question of the extensional relationship between the two independence relations is imprecise. The expressive richness of that language, however, is well beyond that of (what was to become) first-order logic, giving the second relation, in that setting, a narrower extension than the first.

> Mr. Hilbert appears to transfer the independence putatively proved of his pseudo-axioms to the axioms proper ... This would seem to constitute a considerable fallacy. And all mathematicians who think that Mr. Hilbert has proved the independence of the real axioms from one another have surely fallen into the same error.[5] (Frege, 1906, p. 402.)

Or, as we might more calmly put it, the relation that Frege calls "independence" is neither of the relations, also reasonably known by that name, demonstrable via Hilbert's technique. For Frege, the parallels postulate is independent of the other axioms of Euclid iff it isn't provable from those axioms (and in this he agrees with Hilbert), but the notion of *proof* that Frege works with is a very rich one: the question of whether a given thought is provable from others can turn on non-trivial conceptual analyses of the components of those thoughts.[6] Hence a sentence A can be Independent_D and even Independent_S of a set AX of sentences while the thought $\tau(A)$ expressed by A fails to be independent in Frege's sense of the set $\tau(AX)$ of thoughts expressed by the members of AX. We introduce a term for this third kind of independence:

- $Frege-independent$: The thought $\tau(A)$ is not provable, in Frege's rich sense, from the set $\tau(AX)$ of thoughts.

To fix ideas with a vivid example: consider the sentences

(BET_A) Point B lies on a line between points A and C.

(BET_C) Point B lies on a line between points C and A.

For Hilbert, a model can immediately show that (BET_C) is independent of (BET_A), in both of the relevant senses: (BET_C) is not semi-formally deducible from (BET_A), and the condition defined by (BET_A) $\cup$ $\neg$(BET_C) is satisfiable.[7] For Frege on the other hand, a model can show no such thing. Though Frege does not discuss this example, it is compatible with his views that the thoughts expressed by sentences (BET_A) and (BET_C) are the same thought, and hence provable immediately from one another. For an alternative example close to Frege's heart: each of the Dedekind-Peano axioms for number theory is Independent_D and Independent_S from the others, but this straightforwardly-demonstrable fact is in no tension with Frege's logicist thesis, according to which the thoughts expressed by those axiom-sentences are not Frege-independent of one another.

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[5] The "pseudo-axioms" as Frege calls them are Hilbert's partially-interpreted sentences; the "real axioms" are thoughts about points, lines, and planes.

[6] Rigorous deduction, for Frege as for Hilbert, cannot make reference to the meanings of non-logical terms. But the demonstration that a given thought τ is provable from a collection Σ of thoughts can (and, in the logicist project, regularly does) involve non-trivial analysis of τ and/or of Σ *en route* to the expression of those thoughts in the sentences that will appear in the rigorous deduction. See Blanchette (1996, 2012).

[7] This is a simplification of Hilbert's more-interesting result that the biconditional [(BET_A) iff (BET_C)] is independent of the other axioms of order. See Hilbert (1899, §10).

4 The Parallels Postulate

The paradigm independence question prior to the twentieth century was the question of the provability of Euclid's parallels postulate from the remainder of Euclid's axioms. The firm conviction, by the end of the nineteenth century, that the parallels postulate is not provable from the rest of Euclid rested in large part on the construction of - as we now put it - "models of non-Euclidean geometry." But the canonical early models, and the lessons drawn from them, were in interesting ways different both from the later Hilbert-style models, and from their descendants, the models that we take for granted today. We begin with a sketch of some aspects of the well-known history, in order to make some observations about the early use of models.

In the middle of the 18th century, J. H. Lambert famously examined the independence question by working out some of the fundamental implications of $(\neg PP)$, the negation of the parallels postulate.[8] Working in the paradigm set by Saccheri, who divided the alternatives to the parallels postulate (itself called the "first hypothesis") into the *second hypothesis*, in accordance with which the internal angles of a triangle would sum to more than two right angles, and the *third hypothesis*, according to which the angle-sum would be less than that of two right angles, Lambert notes that the second hypothesis holds of the triangles drawn on the surface of an ordinary sphere.[9] He also, intriguingly, suggests that the third hypothesis would hold of triangles drawn on the surface of a sphere with imaginary radius (i.e. a radius whose length is a multiple of i), if such a figure were possible. Lambert does not, however, treat the possibility of such instances as a reason to suspect that the parallels postulate is independent, but continues to search for a proof of (PP) from the remainder of Euclid.

That Lambert does not see in the behavior of arcs on a (real or imaginary) sphere any indication that the parallels postulate might be independent of the rest of Euclid is not entirely surprising: arcs on a real sphere are not infinite; they also violate the Euclidean principle that two points determine a unique line. Most importantly for our purposes, there is also a relatively clear sense in which the sides of the triangle-like figures drawn on a sphere are not "straight:" the fact that the internal angles of such a figure sum to more than two right angles is in no tension with the principle that *real* triangles will have an angle sum equal to that of two right angles. As Katherine Dunlop has put it, regarding Lambert's view of the question "whether the principles that hold of figures on [the surface of a sphere] constitute a theory that is genuinely comparable to Euclid's":

> Lambert appears to share the consensus view that they do not. It was not news, in the second half of the 18th century, that Euclid's parallel

[8] See Lambert (1786). For discussion, see Ewald (1996b).

[9] Saccheri's division was actually couched in terms of the angle-sums of quadrilaterals rather than of triangles; the two issues are equivalent.

> axiom did not hold of arcs on a sphere. But Lambert's contemporaries did not regard the arcs as lines. ... He clearly does not take the fact that the second hypothesis is satisfied on a spherical surface to show that it could belong to geometry after all. (Dunlop, 2009, p. 47.)

In short: the sides of spherical triangles are not lines. Therefore the fact that *they* behave "non-Euclideanly" gives no reason to suppose that *lines* might.

Similar concerns arguably attach to realizations of the third hypothesis, according to which two straight lines in a plane can converge without intersecting. Lambert's view is that this kind of asymptotic approach, discussed here with respect to two posited lines CD and AD, is contrary to the idea of straight lines:

> Whoever at this point objects that CD could perhaps approach AD asymptotically (like, for instance, the hyperbola and other asymptotic bent lines) in my opinion changes what the logicians call the *statum quaestionis*, or he deviates from Euclid .... I do not see how in the representation of *straight* lines objections about hyperbolas can be made. (Lambert, 1786, §3.)

Similarly, "[T]he *idea* that AD, CF are *straight* lines ... cannot coexist with the idea of an *asymptotic approach*. (Lambert, 1786, §10.)

So a surface whose geodesics approach asymptotically, of the kind now familiar as a model of hyperbolic geometry, could not have been seen from Lambert's point of view as a surface truly described by the negation of the parallels postulate: the only way to describe such a surface from this viewpoint is as a perfectly well-behaved Euclidean object whose geodesics are not lines.

Neither Lambert nor his contemporaries has the idea of a mathematical theory as providing an implicit definition of a structure-type, or of the mathematical terms in a theory as place-holders for the elements of satisfying structures. So the idea of curved lines as appropriate candidates for filling such a place, i.e. as objects satisfying implicitly-defined conditions, can make no sense from this perspective. Also lacking at this point is the idea that the proof of sentences one from another is unaffected by the reinterpretation of the geometric terms in those sentences. This idea, as natural as it seems now, requires an understanding of the mathematical language as peculiarly well-behaved in various ways, including a stratification into defined and primitive terms in such a way that none of the mathematical content resides in implicit connections between the contents of those terms. In short, neither of the views essential to the idea of reinterpretation as a method for proving independence, i.e. the idea of axioms as implicit definitions and the idea of rigorous proof as surviving reinterpretation, is part of the standard conception of mathematical theories in the middle of the eighteenth century.

The idea of geometry as a reinterpretable theory rather than a doctrine of space, and the idea that good principles of proof should survive re-interpretation, were both helped along by the success of projective geometry and its duality principles in the early

19th century: it is irresistible to view the counterpart theorems obtained by switching "point" with "line" etc. in the projective setting as also obtained by coordinated re-interpretations of those terms. The idea that an axiom and its dual are in some sense "the same" axiom under different interpretations is the beginning of a certain broadening in the understanding of the role of the term "line" within axioms. Also critical in this progression toward the loosening of the conception of geometric axiom was Riemann's reconceptualization of geometry as a theory of arbitrary n-dimensional manifolds, of which space itself forms merely a particular instance.[10]

The final piece of background to mention before looking at the role of models in the middle of the nineteenth century is the work of Lobachevsky. Following up on Saccheri's third hypothesis, Lobachevsky had provided in 1840 an elegant and deep theory involving the denial of the parallels postulate, and according to which the angle-sum of triangles is less than two right angles (Lobachevsky, 1840). The demonstration that one can go as far as Lobachevsky goes in developing the non-Euclidean theory, without encountering contradiction, provided compelling reason to think that the behavior of parallel lines might not, after all, be dictated by the rest of Euclid.

Against this backdrop, the middle of the nineteenth century saw the construction of surfaces deliberately understood as "models" of non-Euclidean geometry in the sense of containing line-like entities which, often under an alternative understanding of such central notions as that of distance along the surface, satisfied the postulates of two-dimensional Lobachevskian geometry, which we will call "L." Beltrami provides, for example, a metric on the open unit circle that satisfies L when its open chords are taken as lines, and another for that circle when selected arcs play the role of lines (Beltrami, 1868a, 1868b). In each case, the line-like entities are finite on the Euclidean measure, but not on the imposed metric; triangles formed by these entities are just what one would expect (enclosed, in the ordinary Euclidean sense, by three chords or arcs), and the triangles' angle-measure is on the imposed metric essentially what it 'actually' is on the Euclidean metric, summing in each case to less than two right angles. The simple geometric relations between points and lines (the intersection of lines, the lying of a point on a line) are, similarly, understood in the ordinary Euclidean way.

The most significant of the constructions at this point is Beltrami's celebrated pseudosphere and the cover he constructs on it, a surface of constant negative curvature whose geodesics play the role of lines in L.[11] In this case, the line-like entities are very much like lines: they are "really" infinite, i.e. infinite on the ordinary Euclidean metric, and as a result behave in the way that lines on an infinite Euclidean plane would, given an appropriate warping of that plane. Because the curvature of the surface is constant, it

[10] See Riemann (1873). For discussion of the mathematical background to the broadening of the concept of line, and especially the central role of the use of analysis in this reconceptualization of geometry, see Gray (1979).

[11] For discussion of the construction, see Stillwell (1996a). For discussion of the place of Beltrami's work in the history of the independence claim, see Scanlan (1988), Stump (2007).

satisfies the principle of the free mobility of figures on the plane, allowing the important proof-technique of superposition. The surface is, in short, just what one needs in order to provide a vivid depiction of what lines would be like if space had a constant negative curvature: they would be just as described by Lobachevsky.

The close analogy between the geodesics on the constructed surface and the lines of Euclidean geometry is crucial to the role played by the surface in Beltrami's thought. As he puts it,

> The most essential figure in elementary geometry is the straight line. Its specific character is that of being completely determined by two points, so that two lines which pass through the same points coincide throughout their extension. ...
>
> Now this characteristic ... is not peculiar to straight lines in the plane; it also holds (in general) for geodesics on a surface of constant curvature. ... [T]he surfaces of constant curvature, and only these, have the property analogous to that of the plane, namely: given two surfaces of constant and equal curvatures, in each of which there is a geodesic, the superposition of the two surfaces at two points of the geodesic causes them to coincide (in general) along its whole extension.
>
> It follows that, except in the case where this property is subject to exceptions, the theorems of planimetry proved by means of the principle of superposition and the straight line postulate, for plane rectilinear figures, also hold for figures formed analogously on a surface of constant curvature by means of geodesics. (Beltrami, 1868a, pp 8-9 of Stillwell, 1996b.)[12]

That is to say: the figures on this surface analogous to triangles will have all of those properties provable of Euclidean triangles with the exception of those that depend on the parallels postulate; the geodesics will share the corresponding portion of the Euclidean properties of lines, and so on. And that the geodesics are described by L establishes Beltrami's central point, that the surface is a "substrate for" Lobachevsky's geometry, so that this theory is not idle. (Beltrami, 1868a.)

We might ask what, exactly, constructions like Beltrami's were understood to show in this period about the parallels postulate and its connection to the rest of Euclidean geometry. The answer, arguably, is that there is no single precise lesson that was drawn from these constructions, aside from the general view that they showed, in some sense, the coherence of L and hence the independence (in some sense) of the parallels postulate. In 1868, axiomatic theories were still not understood as providing implicit definitions, and there was no well-developed sense of proof as semi-formal, or of the mathematical language as usefully subject to arbitrary reinterpretation. The

[12] The "straight line postulate" is the postulate that two points completely and uniquely determine a line.

constructed “models” of L were consequently not tools for demonstrating the modern notions of Independence$_D$ or of Independence$_S$. To get a sense of what the models were in fact taken to show, we look briefly here at the lessons drawn by Helmholtz and Poincaré.

To begin with, Helmholtz continues in Beltrami’s mold to emphasize the importance of the analogy between Euclidean lines and the geodesics on curved surfaces, and the corresponding analogies between the figures constructed in each domain. As he puts it, speaking specifically of Gauss’s surfaces:

> The difference between plane and spherical geometry has long been evident, but the meaning of the axiom of parallels could not be understood till Gauss had developed the notion of surfaces flexible without dilatation and consequently that of the possibly infinite continuation of pseudospherical surfaces. Inhabiting a space of three dimensions . . . we can represent to ourselves the various cases in which beings on a surface might have to develop their perception of space . . . (von Helmholtz, 1876, p. 308.)

and

> These remarks will suffice to show the way in which we can infer from the known laws of our sensible perception the series of sensible impressions which a spherical or pseudospherical world would give us, if it existed. In doing so we nowhere meet with inconsistency or impossibility ... We can represent to ourselves the look of a pseudospherical world in all directions . . . Therefore it cannot be allowed that the axioms of our geometry depend on the native form of our perceptive faculty, or are in any way connected with it. (von Helmholtz, 1876, p. 319.)

In short, as Helmholtz sees it, these geometric constructions show that Kant is wrong. The pseudospherical models demonstrate that a non-Euclidean “world” is representable to us, with the consequence that Euclidean space is not uniquely determined by our representational capacities.

Despite having a radically different picture of the nature of geometrical truth, Poincaré shares essentially this reaction to the demonstrative value of geodesics on curved surfaces. Having claimed that Beltrami has shown via his pseudosphere that no contradiction is deducible from Lobachevsky’s geometry, Poincaré continues:

> This he has done in the following manner: [I]magine beings without thickness living on [a surface of constant curvature] . . . These surfaces . . . are of two kinds: - Some are of *positive curvature*, and can be so deformed as to be laid on a sphere. . . . Others are of negative curvature. M. Beltrami has shown that the geometry of these surfaces is none other than that of Lobachevsky. (Poincaré, 1892, p. 405.)

The result of this demonstration is, as Poincaré sees it, similarly anti-Kantian:

> [W]e ought . . . to inquire into the nature of geometrical axioms. Are they synthetic conclusions a priori, as Kant used to say? They would appeal to us then with such force, that we could not conceive the contrary proposition, nor construct on it a theoretical edifice. There could not be a non-Euclidean geometry. (Poincaré, 1892, pp. 406-7.)

That there *is* in fact a non-Euclidean geometry, as demonstrated by the combination of Lobachevsky and Beltrami, shows in Poincaré's view the conceivability of that contrary science, and hence the falsehood of the claim that Euclid's geometry is synthetic a priori.

It is worth noting that this late-19th-century inference from the existence of Beltrami-style surfaces to the possibility or the conceivability of non-Euclidean space is by no means a trivial one. There is nothing about those surfaces that conflicts with Euclid: they are constructed within a purely Euclidean framework. And unless the actual behavior of the geodesics on a pseudosphere is taken to indicate the conceivable or possible behavior of lines, their satisfaction of L fails to tell us anything about the coherence of non-Euclidean surfaces or spaces. The question whether geodesics should be taken as such representatives is not a technical question but a conceptual one: Helmholtz and Poincaré, in keeping with the emerging confidence of the late 19th century in the richness and safety of the non-Euclidean framework, take it this way; those with a more conservative understanding of the nature of a line could in principle reject the inference, just as Lambert did with the sides of spherical triangles. That the geodesics on the pseudosphere did successfully play this representative role is due both to a certain loosening of the concept of *line* in the intervening century, and to the recognizably line-like character of the geodesics themselves.

The importance of the representative capacity of the curved surface, i.e. the representability of lines on a plane by geodesics on that surface, is emphasized by Poincaré in his commentary on just how such a surface undermines the claim that Euclid's axioms are synthetic a priori. In a truly synthetic a priori science like arithmetic, claims Poincaré, there can be no such representation of an alternative possibility:

> [L]et us take a true synthetical a priori conclusion; for example, the following: - If an infinite series of positive whole numbers be taken, . . . there will always be one number that is smaller than all the others. . . . Let us next try to free ourselves from this conclusion, and, denying [this proposition], to invent a false arithmetic analogous to the non-Euclidean geometry. We will find that we cannot . . . . (Poincaré, 1892, p. 406.)

There is of course no difficulty in providing a model, in a modern sense, for the negation of the principle Poincaré describes here, the least-number principle. Simply interpret "less-than" via the greater-than relation; alternatively, take "positive whole number" to be interpreted by the negative integers. Poincaré's claim is that we "cannot" represent *the positive whole numbers* as failing to satisfy the least-number principle; his point is that a collection that fails this principle is not the positive whole numbers. Lines, on the other hand, *can* be represented faithfully as failing to satisfy the parallels postulate. What we get from Beltrami's surface, on this account, is a demonstration that Lobachevsky's geometry is just as coherent as is Euclid's, not in the sense of (semi-) formal deductive consistency (which holds just as well in the case of the "false arithmetic"), but in the sense that each provides a coherent description of space.[13]

In general: the construction of Beltrami-style models of L demonstrated the independence of the parallels postulate in a sense quite different from those later notions of Independence$_D$ and Independence$_S$, which notions could not in any event have been made sense of in the setting of a traditional understanding of geometry and its language. The independence-claim in question was instead the less rigorously-demarcated idea of the coherence of a space whose lines behave non-Euclideanly. The role of the constructed surface in this project is to depict a genuine possibility for space itself. Hence the kind of independence shown is very strong: from the claim that it's possible for lines to satisfy most of Euclid without satisfying the parallels postulate, we conclude that one cannot prove the latter from the former, even if one employs proof procedures much richer and more content-sensitive than are the formal or semi-formal deductive principles favored after the turn of the twentieth century. The result is strong enough to establish not just Independence$_D$ and Independence$_S$, but arguably also the Frege-independence of the parallels postulate.

But while the representational strategy just described proves a strong result, an important weakness of the strategy is that its scope of applicability is severely limited. There is no way via appeal to representative surfaces to demonstrate, for example, the independence of (BET$_A$) from (BET$_C$), since no construction of points on a line-like element will satisfy one of the between-ness claims without satisfying the other. And as Poincaré points out above, no such representative strategy can successfully depict a situation in which the natural numbers fail the least-number principle. More generally, there is no straightforward way to extend the strategy beyond the scope of a small part of geometry, that part in which the statement to be demonstrated independent is one whose negation can be represented via recognizable analogues of lines and figures.

5 The turn of the century

In addition to the developments mentioned above concerning the recognition of duality principles in projective geometry and the Riemannian generalization of the scope of geometry, the path from Beltrami to Hilbert turns on closely-related developments concerning mathematical languages and their subject-matter. Here the developments are of both a mathematical and a logical kind. In the first category is the development

[13] For Poincaré, of course, unlike Helmholtz, the further lesson to be drawn is that the choice between the two is purely conventional: they describe the same empirical possibility for space.

of the view of mathematical theories not as statements about determinate collections of entities, but instead as collections of sentences that characterize multiply-instantiable structural properties. This development opens the way for a conception of consistency as satisfiability, and hence of independence as Independence$_S$. On the more purely-logical side is the increasingly-important idea near the end of the twentieth century of proof as characterizable in terms of semi-formal (or later purely formal) principles of sentential deduction, an idea that made Independence$_D$ a natural and central concern.

The modern strategy in place by the turn of the twentieth century, the Hilbert-style method described above, is both much more broadly applicable (since it can be applied to any relatively well-regimented axiomatic theory) and more rigorous than is the kind of appeal to constructed surfaces made by Helmholtz and Poincaré. There is no need for a Hilbert-style model to contain recognizable analogues of elements of an original subject-matter, and there is no conceptual question to be raised about whether the model depicts a conceivable arrangement of the target objects and properties. As long as the sentences in question are true as re-interpreted, and the rules of proof preserve truth under the reinterpretation, Independence$_S$ and Independence$_D$ are immediate. Poincaré's objections about the impossibility of representing false synthetic a priori theories have no purchase in this setting, and Lambert's concerns about the representability of lines by non-lines are irrelevant both to the newly-conceived notions of independence, and to their modern demonstration.

6 The formalization of logic

In the period of roughly 1880 to 1905, it became standard to apply to mathematical axiom-systems a handful of fundamental "meta-theoretical" questions, including those of completeness (in various senses), consistency, and mutual independence. Independence demonstrations for axioms of number theory, geometry, and analysis were familiar by the end of this period, and proceeded in essentially the Hilbert-style way.[14]

Between 1905 and 1915, the Hilbert school became increasingly interested in the development of systems of axioms for logic itself, and specifically in the formal, i.e. syntactic, specification of such systems. The natural question to raise at this point is whether the independence-proving techniques that are clearly useful in mathematical settings can be applied to systems of pure logic. Can one, for example, demonstrate the independence of logical axioms one from another using the modern technique of interpretation-style models?

[14]For discussion of these developments, see Awodey and Reck (2002), Mancosu, Zach, and Badesa (2009), Zach (1999).

Bertrand Russell, asked this question in 1909, responds as follows:

> I do not prove the independence of primitive propositions in logic by the recognized methods; this is impossible as regards principles of inference, because you can't tell what follows from supposing them false: if they are true, they must be used in deducing consequences from the hypothesis that they are false, and altogether they are too fundamental to be treated by the recognized methods. (Russell to Jourdain, April 1909, as reported in Grattan-Guinness, 1977, p. 117.)

A similar sentiment appears in *Principles of Mathematics*, and again in *Principia Mathematica*. (Russell, 1903, §17; Russell and Whitehead, 1910-1913, *1.)

It is not difficult to see the problem as Russell sees it. Suppose (with Russell) that the basic idea of an independence proof is to assume some collection of claims to be true, assume a further claim to be false, and then check to see whether any contradiction follows. As applied to the independence of the parallels postulate, the idea is that we are assured by the coherence of the non-Euclidean surface that no contradiction follows from supposing that the parallels postulate is false while the rest of Euclid is true. The role of the model on this picture is not that of re-interpretation to show Independence$_D$ or Independence$_S$; it is the earlier idea of a representation of a situation in which the axioms in question are true and false respectively. Given this understanding of an independence-proof and of the role of models, it is clear that one cannot, just as Russell says, apply the technique to principles of logic. To assume a handful of logical axioms true and a target logical axiom false is already to engage in contradiction. And there is no sense in which such a collection of sentences, i.e. one including the negation of a principle of propositional logic, can be taken to describe a coherent situation. In short, and as Russell notes, the principles of logic - to which we must appeal when demonstrating independence in the arenas of geometry or analysis - are "too fundamental to be treated by" such a method.

Nevertheless, there is clearly something wrong with Russell's idea that the "recognized method" at the time of his writing cannot be applied to systems of logic. By as early as 1905, Hilbert had already been lecturing on the use of arithmetical interpretations to show the mutual independence of axioms for propositional logic. And by 1918, Bernays uses just such interpretations to demonstrate the mutual independence of some of Russell's own propositional axioms (Bernays, 1918).[15]

Bernays' technique for demonstrating the independence of an axiom A from axioms $A_1 \ldots A_n$ is essentially as follows: We give a systematic assignment of values (e.g. the numbers 1, 2, 3) to every sentence of the language, in such a way that, for example: each of $A_1 \ldots A_n$, together with every sentence deducible from these via the specified inference rules, is assigned the value 1; A itself is not assigned 1. The immediate conclusion is that A is not deducible from $A_1 \ldots A_n$.

[15] For discussion of Bernays 1918, see Mancosu et al. (2009).

The first thing to note about this strategy is that, *contra* Russell, it does not involve supposing that A is false. It also doesn't involve the representation of a state of affairs that in any sense satisfies or exemplifies $A_1 \ldots A_n$; there is no need to try to make sense of the presumably-incoherent idea of a state of affairs in which an instance of an axiom of propositional logic is false. What is demonstrated by Bernays' method is simply the non-deducibility, now in an entirely *formal* - i.e. syntactic - sense, of the formula A from the collection $A_1 \ldots A_n$ of formulas.

The connection between the method of Hilbert 1899 and Bernays 1918 can be brought out as follows. First of all, both methods are instances of what we can call the *arbitrary valuation strategy*: That strategy, applied to a formula A and a collection $A_1 \ldots A_n$ of formulas, is to assign values to formulas in such a way that, for a designated value V, V is assigned to each of $A_1 \ldots A_n$ and to each formula *deducible* from them, but is not assigned to A. The two coordinated differences between Hilbert 1899 and Bernays 1918 are (i) the kind of values employed, and (ii) the nature of the deducibility relation.

For Hilbert 1899, the value V is in each case the value "expresses a theorem of B under interpretation I," where B is a background theory (typically a theory of constructions on the real numbers) and I is the interpretation of the formulas in question via the subject-matter of B. Deducibility is a relation not explicitly specified, but understood in terms of self-evident principles of inference, subject to the constraint that the principles be semi-formal, holding independently of the interpretations of the geometric terms appearing in those formulas. The critical fact about deducibility assumed throughout is that anything deducible from a formula that expresses a theorem of B is also a formula that expresses a theorem of B. This assumed feature of deducibility is the guarantee that V is preserved by deducibility. The guarantee that the target formula A *lacks* value V, i.e that A does not express a theorem of B under interpretation I, is given by the facts that (a) by design, $\neg A$ expresses a theorem of B under I, and (b) B is, by assumption, itself consistent.

No such valuation, and no such account of deducibility, can work in the setting of independence proofs for principles of pure logic. First of all, the designated value V must in this setting be one that is not automatically had by all truths of logic, since it is precisely a truth of logic that we will want to demonstrate lacks V. So V cannot be the value "expresses a theorem of theory B under interpretation I," for any B or I. The relation of deducibility, in addition, cannot be merely a generally-understood notion of (semi-formal) provability, since any such notion will count the formulas of pure logic as deducible from everything. Bernays' method rests on the existence of a syntactically-specified relation of deducibility, with respect to which it is not trivially true that each principle of logic is deducible from everything. It also rests on the choice of a targeted value V that has nothing to do with the "interpretation," in any ordinary sense, of the formulas in question: i.e. nothing to do with the idea of those formulas as expressing truths and falsehoods about either the intended or an alternate subject-matter.

With respect to the question, then, of whether Russell is right that the "recognized method" is not applicable to questions of independence in systems of logic, the answer will turn on what exactly we take to fall under the scope of “recognized method." Taking that method to be the very broad strategy we’ve called the “arbitrary valuation” strategy, Russell is wrong: the method works, as Bernays shows. Taking, on the other hand, that method to be the more narrowly-construed instance of that technique in which valuations are understood in terms of re-interpretations into an assumed-consistent background theory, then Russell is right: we cannot interpret the language in such a way that axiom-sentences of propositional logic express the negations of theorems of a consistent background theory. Finally: given Russell’s own, old-fashioned way of understanding the “recognized method,” as involving the supposition of the falsehood of the target axiom and a subsequent question about the consistency of the result, the method is clearly not applicable to principles of logic, for the reasons Russell himself gives.

A further important difference between the method as employed by Hilbert 1899 and its refinement in Bernays 1918 involves the kind and the strength of the independence claims thereby demonstrated. As above, Hilbert’s technique shows Independence$_D$: it shows that A is not deducible from $A_1 \ldots A_n$, where “deducible from" is the semi-formal relation described above. Similarly, Bernays’ technique shows that A is not deducible from $A_1 \ldots A_n$, where “deducible from" is now the rigorously-specified relation specific to a particular formal system. But Hilbert’s technique also demonstrates, as we’ve discussed, the stronger result of “Independence$_S$,”: it demonstrates that the condition implicitly defined by $\{A_1 \ldots A_n, \neg A\}$ is satisfiable. But Bernays’ method provides no such further result: no domain is exhibited that satisfies conditions implicity defined by the formulas in question. And indeed, the satisfiability claim in question makes no sense as applied to the formulas to which Bernays applies it: the axioms are not implicit definitions of structural conditions, and there is no sense to be made of a domain with respect to which some of those axioms express falsehoods.

The importance of Independence$_S$ is most vivid in the setting of the kind of structuralist approach to mathematical theories and axioms that was beginning to take hold at the end of the nineteenth century, and remains of central importance today. In Dedekind’s work, for example, the theory of natural-number arithmetic is the theory of any and all ordered collections of objects that satisfy the natural-number axioms, or equivalently the theory of any and all ω-sequences.[16] The role of each axiom on this conception is to provide a partial characterization of the type of structure in question. Given a collection of axioms $A_1 \ldots A_n$, the addition of a further axiom A would be redundant if every structured domain satisfying the former collection already satisfied the latter. The important independence relation, from this point of view, is the relation of non-redundancy in this sense, which is to say that it is a matter of the satisfiability of $\{A_1 \ldots A_n, \neg A\}$, i.e. of Independence$_S$.

Independence$_D$ is the relevant kind of independence if instead the goal of the axioms

[16]See Dedekind (1888).

is the deductive characterization of a body of truths. In this setting, A is redundant with respect to $A_1 \ldots A_n$ if A is deducible from $A_1 \ldots A_n$, and hence independent in the relevant sense if Independent$_D$ of those axioms. In a setting in which axioms are intended to provide both a deductive characterization of a theory and a definition of the structural characteristics of its domain, both kinds of independence are relevant; and as above, both are demonstrable via the construction of a model in the mode of Hilbert 1899. Once we move to Bernays 1918, however, the goal of the axioms is purely deductive (there being no sense in which the axioms of the propositional calculus define properties of structures), and the relevant kind of independence is just Independence$_D$.

7 Summing up

The idea with which we began was the traditional idea that the development of model-theoretic methods around the turn of the 20th century provided, at last, a rigorous way of answering old independence questions. The contrary proposal suggested here is that this is not quite the right way to view the developments of the period 1870 - 1920. Instead of a single notion of independence that's given increasingly-rigorous treatment, we have a handful of different independence questions, some of which are susceptible to rigorous treatment, and some of which are not. As our methods have changed, so too have the questions that we are in a position to ask (and answer).

Passing from 1870 to 1899 to 1918, we see the following three lines of development.

First, we see a gradual increase in rigor. By 1899, questions of independence are divorced from questions about the representability of the subject-matter (e.g. of lines by geodesics, of positive numbers by negative numbers), and linked to the more tractable notion of reinterpretation. By 1918, the appeal to an informal notion of provability is replaced by appeal to an explicitly-defined relation of formal deducibility.

Secondly, we see an increase in the scope of the methods. In 1870, the canonical independence-proof technique applies to the parallels postulate and to that small collection of geometric propositions whose negation can be represented as holding on something recognizably like a surface. By 1899, we have a technique that applies to all of geometry and arithmetic. And by 1918, the standard technique allows us to prove independence even of the axioms for formalized systems of logic.

But, thirdly, we see in this period a gradual decrease in the strength of the independence claims demonstrable by the emerging methods. In 1870, as understood e.g. by Helmholtz, a model establishes a strong modal claim, i.e. the claim that space might really be a certain way, and that we can conceive of its being that way. By 1899, the method exhibited in Hilbert's *Foundations of Geometry* makes no claim to establishing such a strong claim about the possible configuration of space or about our conception of it; the claims made via the new method are claims of non-deducibility and of the satisfiability of implicitly-defined conditions. Finally, the method employed by Bernays in 1918 provides us with clear demonstrations of non-deducibility; it is neither intended to provide, nor is it capable of providing, any modal results about the subject-matter of the theory or any conclusions about the satisfiability of structural conditions.

If the oldest versions of the question of the independence of the parallels postulate are questions whose positive answer would establish the possibility or conceivability of non-Euclidean space, then they are not the questions answerable via either the method of 1899 or the method of 1918. And if the questions asked of arithmetic and geometry by Deekind and Veblen have to do with the satisfiability of conditions implicitly defined by the axioms in question, then those questions, decisively answerable via the method of 1899, are not answerable via the method of 1918.[17] The weakest of our independence relations, that of pure non-deducibility in a rigorously-specified formal system, is also the cleanest, and the most crisply demonstrable. It is natural to take it to be, in some sense, a refinement of older and more inchoate questions about independence. But if the suggestions made here are accurate, then the gap between the independence-claims cleanly demonstrable via the modern methods and the independence-claims that originally motivated much geometric work prior to the end of the 19th century is quite large; and it is sufficiently large that we cannot take the newer methods to be merely cleaned-up ways of answering the old questions.

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[17] See Dedekind (1888), Veblen (1904).

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