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The Emergence of First-Order Logic

1. Introduction

To most mathematical logicians working in the 1980s, first-order logic is the proper and natural framework for mathematics. Yet it was not always so. In 1923, when a young Norwegian mathematician named Thoralf Skolem argued that set theory should be based on first-order logic, it was a radical and unprecedented proposal.

The radical nature of what Skolem proposed resulted, above all, from its effect on the notion of categoricity. During the 1870s, as part of what became known as the arithmetization of analysis, Cantor and Dedekind characterized the set $\mathbb{R}$ of real numbers (up to isomorphism) and thereby found a categorical axiomatization for $\mathbb{R}$. Likewise, during the 1880s Dedekind and Peano categorically axiomatized the set $\mathbb{N}$ of natural numbers by means of the Peano Postulates.[1] Yet in 1923, when Skolem insisted that set theory be treated within first-order logic, he knew (by the recently discovered Löwenheim-Skolem Theorem) that in first-order logic neither set theory nor the real numbers could be given a categorical axiomatization, since each would have both a countable model and an uncountable model. A decade later, Skolem (1933, 1934) also succeeded in proving, by the construction of a countable nonstandard model, that the Peano Postulates do not uniquely characterize the natural numbers within first-order logic. The Upward Löwenheim-Skolem Theorem of Tarski, the first version of which was published as an appendix to (Skolem 1934), made it clear that no axiom system having an infinite model is categorical in first-order logic.

The aim of the present article is to describe how first-order logic gradually emerged from the rest of logic and then became accepted by mathematical logicians as the proper basis for mathematics—despite the opposition of Zermelo and others. Consequently, I have pointed out where a logician used first-order logic and where, as more frequently occurred, he employed some richer form of logic. I have distinguished between a logician's use of first-order logic (where quantifiers range only over individuals), second-order logic (where quantifiers can also range over sets or relations), ω-order logic (essentially the simple theory of types), and various infinitary logics (having formulas of infinite length or rules of inference with infinitely many premises).

It will be shown that several versions of second-order logic (sometimes including an infinitary logic) were common before *Principia Mathematica*. First-order logic—stripped of all infinitary operations—emerged only with Hilbert in (1917), where it remained a subsystem of logic, and with Skolem in (1923), who treated it as *all* of logic.

During the nineteenth and early twentieth centuries, there was no generally accepted classification of the different kinds of logic, much less an acceptance of one kind as the correct and proper one. (Infinitary logic, in particular, appeared in many guises but did not begin to develop as a distinct branch of logic until the mid-1950s.) Only very gradually did it become evident that there is a reasonable such classification, as opposed merely to the cornucopia of different logical systems introduced by various researchers. Likewise, it only became clear over an extended period that in logic it is important to distinguish between syntax (including such notions as formal language, formula, proof, and consistency) and semantics (including such notions as truth, model, and satisfiability). This distinction led, in time, to Gödel's Incompleteness Theorem (1931) and thus to understanding the limitations of even categorical axiom systems.

2. Boole: The Emergence of Mathematical Logic

Before the twentieth century, there was no reason to believe that the kind of logic that a mathematician used would affect the mathematics that he did. Indeed, during the first half of the nineteenth century the Aristotelian syllogism was still regarded as the ultimate form of all reasoning. When the continuous development of mathematical logic began in 1847, with the publication of George Boole's *The Mathematical Analysis of Logic*, Aristotelian logic was treated as one interpretation of a logical calculus. Like his predecessors, Boole understood logic as "the laws of thought," and so his work lay on the boundary of philosophy, psychology, and mathematics (1854, 1).

After setting up an uninterpreted calculus of symbols and operations, Boole then gave it various interpretations—one in terms of classes, one in terms of propositions, and one in terms of probabilities. This calculus was based on conventional algebra suitably modified for use in logic. He pursued the analogy with algebra by introducing formal symbols for the four arithmetic operations of addition, subtraction, multiplication, and division as well as by using functional expansions. Deductions took the form of equations and of the transformation of equations. The symbol v played the role of an indefinite class that served in place of an existential quantifier (1854, 124).

Eventually, a modified version of Boole's logic would become propositional logic, the lowest level of modern logic. When Boole wrote, however, his system functioned in effect as all of logic, since, within this system, Aristotelian syllogistic logic could be interpreted.

Boole influenced the development of logic by his algebraic approach, by giving a calculus for logic, and by supplying various interpretations for this calculus. His algebraic approach was a distinctly British blend, following in the footsteps of Peacock's "permanence of form" with its emphasis on the laws holding for various algebraic structures. But it would soon take root in the United States with Peirce and in Germany with Schröder.

3. Peirce and Frege: Separating the Notion of Quantifier

A kind of logic that was adequate for mathematical reasoning began to emerge late in the nineteenth century when two related developments occurred: first, relations and functions were introduced into symbolic logic; second, the notion of quantifier was disentangled from the notion of propositional connective and was given an appropriate symbolic representation. These two developments were both brought about independently by two mathematicians having a strong philosophical bent—Charles Sanders Peirce and Gottlob Frege.

Nevertheless, the notion of quantifier has an ancient origin. Aristotle, whose writings (above all, the *Prior Analytics*) marked the first appearance of formal logic, made the notions of "some" and "all" central to logic by formulating the assertoric syllogism. Despite the persistence for over two thousand years of the belief that all reasoning can be formulated in syllogisms, they remained in fact a very restrictive mode of deduction.

What happened circa 1880, in the work of Peirce and Frege, was not that the notion of quantifier was invented but rather that it was separated from the Boolean connectives on the one hand and from the notion of predicate on the other.

Peirce's contributions to logic fell squarely within the Boolean tradition. In (1865), Peirce modified Boole's system in several ways, reinterpreting Boole's + (logical addition) as union in the case of classes and as inclusive "or" in the case of propositions. (Boole had regarded $A + B$ as defined only when A and B are disjoint.)

Five years later, Peirce investigated the notion of relation that Augustus De Morgan had introduced into formal logic in (1859) and began to adapt this notion to Boole's system:

> Boole's logical algebra has such singular beauty, so far as it goes, that it is interesting to inquire whether it cannot be extended over the whole realm of formal logic, instead of being restricted to that simplest and least useful part of the subject, the logic of absolute terms, which, when he wrote [1854], was the only formal logic known. (Peirce 1870, 317)

Thus Peirce developed the laws of the relative product, the relative sum, and the converse of a relation. When he left for Europe in June 1870, he took a copy of this article with him and delivered it to De Morgan (Fisch 1984, xxxiii). Unfortunately, De Morgan was already in the decline that led to his death the following March. Peirce did not find a better reception when he gave a copy of the article to Stanley Jevons, who had elaborated Boole's system in England. In a letter of August 1870 to Jevons, whom Peirce described as "the only active worker now, I suppose, upon mathematical logic," it is clear that Jevons rejected Peirce's extension of Boole's system to relations (Peirce 1984, 445). Nevertheless, Peirce's work on relations eventually found wide currency in mathematical logic.

It was through applying class sums and products (i.e., unions and intersections) to relations that Peirce (1883) obtained the notion of quantifier as something distinct from the Boolean connectives. By way of example, he let l_{ij} denote the relation stating that i is a lover of j. "Any proposition whatever," he explained,

> is equivalent to saying that some complexus of aggregates and products of such numerical coefficients is greater than zero. Thus,
>
> $$\Sigma_i \Sigma_j l_{ij} > 0$$
>
> means that something is a lover of something; and

$$\Pi_i \Sigma_j l_{ij} > 0$$

means that everything is a lover of something. We shall, however, naturally omit, in writing the inequalities, the > 0 which terminates them all; and the above two propositions will appear as

$$\Sigma_i \Sigma_j l_{ij} \text{ and } \Pi_i \Sigma_j l_{ij}$$

(1883, 200-201)

When Peirce returned to the subject of quantifiers in (1885), he treated them in two ways that were to have a pronounced effect on the subsequent development of logic. First of all, he defined quantifiers (as part of what he called the "first-intentional logic of relatives [relations]") in a way that emphasized their analogy with arithmetic:

> Here, in order to render the notation as inconical as possible, we may use Σ for *some*, suggesting a sum, and Π for *all*, suggesting a product. Thus $\Sigma_i x_i$ means that x is true of some one of the individuals denoted by i or
>
> $$\Sigma_i x_i = x_i + x_j + x_k + \text{etc.}$$
>
> In the same way,
>
> $$\Pi_i x_i = x_i x_j x_k \text{ etc.}$$
>
> If x is a simple relation, $\Pi_i \Pi_j x_{ij}$ means that every i is in this relation to every j, $\Sigma_i \Pi_j x_{ij}$ that some one i is in this relation to every j. . . . It is to be remarked that $\Sigma_i x_i$ and $\Pi_i x_i$ are only *similar* to a sum and a product; they are not strictly of that nature, because the individuals of the universe may be innumerable. (1885, 194-95)

Thus in certain cases Peirce regarded a formula with an existential quantifier as an infinitely long propositional formula, for example the infinitary disjunction

$A(i)$ or $A(j)$ or $A(k)$ or . . . ,

where i, j, k, etc. were names for all the individuals in the universe of discourse. An analogous connection held between "for all i, $A(i)$" and the infinitary conjunction

$A(i)$ and $A(j)$ and $A(k)$ and. . . .

Here, and in the writings of those who later followed Peirce's approach (such as Schröder and Löwenheim), the syntax was not totally distinct from the semantics because a particular domain, to which the quantifiers were to apply, was given in advance. When this domain was infinite, it was natural to treat quantifiers in such an infinitary fashion, since, for a finite domain of elements i_1 to i_n, "for some i, $A(i)$" reduced to the finite disjunction

$$A(i_1) \text{ or } A(i_2) \text{ or} \ldots \text{or } A(i_n),$$

and "for all i, $A(i)$" reduced to the finite conjunction

$$A(i_1) \text{ and } A(i_2) \text{ and} \ldots \text{and } A(i_n).$$

Thus, unlike the logic of Peano for example, the logic that stemmed from Peirce was not restricted to formulas of finite length.

A second way in which Peirce's treatment of quantifiers was significant occurred in what he called "second-intensional logic." This kind of logic permitted quantification over predicates and so was one version of second-order logic.[2] Peirce used this logic to define identity (something that can be done in second-order logic but not, in general, in first-order logic):

> Let us now consider the logic of terms taken in collective senses [second-intensional logic]. Our notation ... does not show us even how to express that two indices, i and j, denote one and the same thing. We may adopt a special token of second intention, say 1, to express identity, and may write 1_{ij}.... And identity is defined thus:
>
> $$1_{ij} = \Pi_k (q_{ki} q_{kj} + \bar{q}_{ki} \bar{q}_{kj}).$$
>
> That is, to say that things are identical is to say that every predicate is true of both or false of both.... If we please, we can dispense with the token q, by using the index of a token and by referring to this in the Quantifier just as subjacent indices are referred to. That is to say, we may write
>
> $$1_{ij} = \Pi_k (x_{ki} x_{kj} + \bar{x}_{ki} \bar{x}_{kj}).$$
>
> (1885, 199)

In effect, Peirce used a form of Leibniz's principle of the identity of indiscernibles in order to give a second-order definition of identity.

Peirce rarely returned to his second-intensional logic. It formed chapter 14 of his unpublished book of 1893, *Grand Logic* (see his [1933, 56-58]). He also used it, in a letter of 1900 to Cantor, to quantify over relations while defining the less-than relation for cardinal numbers (Peirce 1976, 776). Otherwise, he does not seem to have quantified over relations. Moreover, what Peirce glimpsed of second-order logic was minimal. He appears never to have applied his logic in detail to mathematical problems, except in (1885) to the beginnings of cardinal arithmetic—an omission that contrasts sharply with Frege's work.

The logic proposed by Frege differed significantly both from Boole's system and from first-order logic. Frege's *Begriffsschrift* (1879), his first publication on logic, was influenced by two of Leibniz's ideas: a *calculus ratiocinator* (a formal calculus of reasoning) and a *lingua characteristica* (a universal language). As a step in this direction, Frege introduced a formal language on which to found arithmetic. Frege's formal language was two-dimensional, unlike the linear languages used earlier by Boole and later by Peano and Hilbert. From mathematics Frege borrowed the notions of function and argument to replace the traditional logical notions of predicate and subject, and then he employed the resulting logic as a basis for constructing arithmetic.

Frege introduced his universal quantifier in such a way that functions could be quantified as well as arguments. He made essential use of such quantifiers of functions when he treated the Principle of Mathematical Induction (1879, sections 11 and 26). To develop the general properties of infinite sequences, Frege both wanted and believed that he needed a logic at least as strong as what was later called second-order logic.

Frege developed these ideas further in his *Foundations of Arithmetic*, where he wrote of making "one concept fall under a higher concept, so to say, a concept of second order" (1884, section 53). A "second-order" concept was analogous to a function of a function of individuals. He quantified over a relation in the course of defining the notion of equipotence, or having the same cardinal number (1884, section 72)—thereby relying again on second-order logic. In his article "Function and Concept" (1891), he revised his terminology from function (or concept) of second order to function of "second level" (*zweiter Stufe*). Although he discussed this notion in more detail than he had in (1884), he did not explicitly quantify over functions of second level (1891, 26-27).

Frege's most elaborate treatment of such functions was in his *Fundamental Laws of Arithmetic* (1893, 1903). There quantification over second-level functions played a central role. Of the six axioms for his logic, two unequivocally belonged to second-order logic:

(1) If $a = b$, then for every property f, a has the property f if and only if b has the property f.
(2) If a property $F(f)$ of properties f holds for every property f, then $F(f)$ holds for any particular property f.

Frege would have had to recast his system in a radically different form if he had wanted to dispense with second-order logic. At no point did he give any indication of wishing to do so. In particular, there was no way in which he could have defined the general notion of cardinal number as he did, deriving it from logic, without the use of second-order logic.

In the *Fundamental Laws* (1893), Frege also introduced a hierarchy of levels of quantification. After discussing first-order and second-order propositional functions in detail, he briefly treated third-order propositional functions. Nevertheless, he stated his axioms as second-order (not third-order or ω-order) propositional functions. Frege developed a second-order logic, rather than a third-order or ω-order logic, because, in his system, second-level concepts could be represented by their extensions as sets and thereby appear in predicates as objects (1893, 42). Unfortunately, this approach, when combined with his Axiom V (which was a second-order version of the Principle of Comprehension), made his system contradictory—as Russell was to inform him in 1902.

Although Frege introduced a kind of second-order logic and used it to found arithmetic, he did not separate the first-order part of his logic from the rest. Nor could he have undertaken such a separation without doing violence to his principles and his goals.[3]

4. Schröder: Quantifiers in the Algebra of Logic

Ernst Schröder, who in (1877) began his research in logic within the Boolean tradition, was not acquainted at first with Peirce's contributions. On the other hand, Schröder soon learned of Frege's *Begriffsschrift* and gave it a lengthy review.[4] This review (1880) praised the *Begriffsschrift* and added that it promised to help advance Leibniz's goal of a universal language. Nevertheless, Schröder criticized Frege for failing to take account of Boole's contributions. What Frege did, Schröder argued, could be done more perspicuously by using Boole's notation; in particular, Frege's two-dimensional notation was extremely wasteful of space.

Three years later Frege replied to Schröder, emphasizing the differences between Boole's symbolic language and his own: "I did not wish to represent an abstract logic by formulas but to express a content [*Inhalt*] by written signs in a more exact and clear fashion than is possible by words" (1883, 1). As Frege's remark intimated, in his logic propositional functions carried an intended interpretation. In conclusion, he stressed that his notation allowed a universal quantifier to apply to just a *part* of a formula, whereas Boole's notation did not. This was what Schröder had overlooked in his review and what he would eventually borrow from Peirce: the separation of quantifiers from the Boolean connectives.

Schröder adopted this separation in the second volume (1891) of his *Lectures on the Algebra of Logic*, a three-volume study of logic (within the tradition of Boole and Peirce) that was rich in algebraic techniques applied to semantics. In the first volume (1890), he discussed the "identity calculus," which was essentially Boolean algebra, and three related subjects: the propositional calculus, the calculus of classes, and the calculus of domains. When in (1891) he introduced Peirce's notation for quantifiers, he used it to quantify over all subdomains of a given domain (or manifold) called 1: "In order to express that a proposition concerning a domain x holds. . . *for every domain* x (in our manifold 1), we shall place the sign Π_x before [the proposition]. . ." (1891, 26). Schröder insisted that there is no manifold 1 containing all objects, since otherwise a contradiction would result (1890, 246)—a premonition of the later set-theoretic paradoxes.

Unfortunately, Schröder conflated the relations of membership and inclusion, denoting them both with ⋹. (Frege's review (1895) criticized Schröder severely on this point.) This ambiguity in Schröder's notation might cast doubt on the assertion that he quantified over all subdomains of a given manifold and hence used a version of second-order logic. In one case, however, he was clearly proceeding in such a fashion. For he defined $x = 0$ to be $\Pi_a(x ⋹ a)$, adding that this expressed "that a domain x is to be named 0 *if and only if* x is included in every domain a. . ." (1891, 29). In other cases, his quantifiers were taken, quite explicitly, over an infinite sequence of domains (1891, 430-31). Often his quantifiers were first-order and ranged over the individuals of a given manifold 1, a case that he treated as part of the calculus of classes (1891, 312).

Schröder's third volume (1895), devoted to the "algebra and logic of relations," contained several kinds of infinitary and second-order propositions. One kind, a_0, was introduced by Schröder in order to discuss Dedekind's notion of chain (a mapping of a set into itself). More precisely, a_0 was defined to be the infinite disjunction of all the finite iterations of the relative product of the domain a with itself (1895, 325). Here Schröder's aim (1895, 355) was to derive the Principle of Mathematical Induction in the form found in (Dedekind 1888).

In the same volume Schröder made his most elaborate use of second-order logic, treating it mainly as a tool in "elimination problems" where the goal was to solve a logical equation for a given variable. He stated a second-order proposition

$$\Pi_u(\bar{u}_{hk} + \bar{u}_{hl}) = 1'_{lk}$$

that (following Peirce) he could have taken to be the definition of identity, but did not (1895, 511). However, in what he described as "a procedure that possesses a certain boldness," he considered an infinitary proposition that had a universal quantifier for uncountably many (in fact, continuum many) variables (1895, 512). Finally, in order to move an existential quantifier to the left of a universal quantifier (as would later be done in first-order logic by Skolem functions), he introduced a universal quantifier subscripted with relation variables (and so ranging over them), and then expanded this quantifier into an infinite product of quantifiers, one for each individual in the given infinite domain (1895, 514). This general procedure would play a fundamental role in the proof that Löwenheim was to give in 1915 of Löwenheim's Theorem (see section 9 below).

5. Hilbert: Early Researches on Foundations

During the winter semester of 1898-99, David Hilbert lectured at Göttingen on Euclidean geometry, soon publishing a revised version as a book (1899). At the beginning of this book, which became the source of the modern axiomatic method, he briefly stated his purpose:

> The following investigation is a new attempt to establish for geometry a system of axioms that is *complete* and *as simple as possible*, and to deduce from these axioms the most important theorems of geometry in such a way that the significance of the different groups of axioms and the scope of the consequences to be drawn from the individual axioms are brought out as clearly as possible. (Hilbert 1899, 1)

Hilbert did not specify precisely what "complete" meant in this context until a year later, when he remarked that the axioms for geometry are complete if all the theorems of Euclidean geometry are deducible from the axioms (1900a, 181). Presumably his intention was that all *known* theorems be so deducible.

On 27 December 1899, Frege initiated a correspondence with Hilbert about the foundations of geometry. Frege had read Hilbert's book, but found its approach odd. In particular, Frege insisted on the traditional view of geometric axioms, whereby axioms were justified by geometric intuition. Replying on 29 December, Hilbert proposed a more arbitrary and modern view, whereby an axiom system only determines up to isomorphism the objects described by it:

> You write: "I call axioms propositions that are true but are not proved because our knowledge of them flows from a source very different from the logical source, a source which might be called spatial intuition. From the truth of the axioms it follows that they do not contradict each other." I found it very interesting to read this sentence in your letter, for as long as I have been thinking, writing, and lecturing on these things, I have been saying the exact opposite: if the arbitrarily given axioms do not contradict each other with all their consequences, then they are true and the things defined by the axioms exist. For me this is the criterion of truth and existence. (Hilbert in [Frege 1980a], 39-40)

Hilbert returned to this theme repeatedly over the following decades.

Frege, in a letter of 6 January 1900, objected vigorously to Hilbert's claim that the consistency of an axiom system implies the existence of a model of the system. The only way to prove the consistency of an axiom system, Frege insisted, is to give a model. He argued further that the crux of Hilbert's "error" was in conflating first-level and second-level concepts.[5] For Frege, existence was a second-level concept, and it is precisely here that his system of logic, as found in the *Fundamental Laws*, differs from second-order logic as it is now understood.

What is particularly striking about Hilbert's axiomatization of geometry is an axiom missing from the first edition of his book. There his Axiom Group V consisted solely of the Archimedean Axiom. He used a certain quadratic field to establish the consistency of his system, stressing that this proof required only a denumerable set. When the French translation of his book appeared in 1902, he added a new axiom that differed fundamentally from all his other axioms and that soon led him to try to establish the consistency of a nondenumerable set, namely the real numbers:

> Let us note that to the five preceding groups of axioms we may still adjoin the following axiom which is not of a purely geometric nature and which, from a theoretical point of view, merits particular attention:
>
> Axiom of Completeness
>
> To the system of points, lines, and planes it is impossible to adjoin other objects in such a way that the system thus generalized forms a new geometry satisfying all the axioms in groups I-V. (Hilbert 1902, 25)

Hilbert introduced his Axiom of Completeness, which is false in first-order logic and which belongs either to second-order logic or to the metamathematics of his axiom system, in order to ensure that every interval on a line contains a limit point. All the same, he had some initial reservations, since he added: "In the course of the present work we have not used this 'Axiom of Completeness' anywhere" (1902, 26). When the second German edition of the book appeared a year later (1903), his reservations had abated, and he designated the Axiom of Completeness as Axiom V2; so it remained in the many editions published during his lifetime.

Hilbert's initial version of the Axiom of Completeness, which referred to the real numbers rather than to geometry, stated that it is not possible to extend $\mathbb{R}$ to a larger Archimedean ordered field. This version formed part of his (1900a) axiomatization of the real number system. There he asserted that his Axiom of Completeness implies the Bolzano-Weierstrass Theorem and thus that his system characterizes the usual real numbers.

The fact that Hilbert formulated his Axiom of Completeness in his (1900a), completed in October 1899, lends credence to the suggestion that he may have done so as a response to J. Sommer's review, written at Göttingen in October 1899, of Hilbert's book (1899).[6] Sommer criticized Hilbert for introducing the Archimedean Axiom as an axiom of continuity—an assumption that was inadequate for such a purpose:

> Indeed, the axiom of Archimedes does not relieve us from the necessity of introducing explicitly an axiom of continuity, it merely makes the introduction of such an axiom possible. Thus, for the whole domain of geometry, Professor Hilbert's system of axioms is not sufficient. For instance,. . .it would be impossible to decide geometrically whether a straight line that has some of its points within and some outside a circle will meet the circle. (Sommer 1900, 291)

In effect, Hilbert met this objection with his new Axiom of Completeness. It is unclear why he formulated this axiom as an assertion about maximal models rather than in a more mathematically conventional way (such as the existence of a least upper bound for every bounded set).

That same year Hilbert gave his famous lecture, "Mathematical Problems," at the International Congress of Mathematicians held at Paris. As his second problem, he proposed that one prove the consistency of his axioms for the real numbers. At the same time he emphasized three assumptions underlying his foundational position: the utility of the axiomatic method, his belief that every well-formulated mathematical problem can be solved, and his conviction that the consistency of a set S of axioms implies the existence of a model for S (1900b, 264-66). When he gave this address, his view that consistency implies existence was only an article of faith—albeit one to which Poincaré subscribed as well (Poincaré 1905, 819). Yet in 1930 Gödel was to turn this article of faith into a theorem, indeed, into one version of his Completeness Theorem for first-order logic.

In 1904, when Hilbert addressed the International Congress of Mathematicians at Heidelberg, he was still trying to secure the foundations of the real number system. As a first step, he turned to providing a foundation for the positive integers. While discussing Frege's work, he considered the paradoxes of logic and set theory for the first time in print. To Hilbert these paradoxes showed that "the conceptions and research methods of logic, conceived in the traditional sense, do not measure up to the rigorous demands that set theory makes" (1905, 175). His remedy separated him sharply from Frege:

> Yet if we observe attentively, we realize that in the traditional treatment of the laws of logic certain fundamental notions from arithmetic are already used, such as the notion of set and, to some extent, that of number as well. Thus we find ourselves on the horns of a dilemma, and so, in order to avoid paradoxes, one must simultaneously develop both the laws of logic and of arithmetic to some extent. (1905, 176)

This absorption of part of arithmetic into logic remained in Hilbert's later work.

Hilbert excused himself from giving more than an indication of how such a simultaneous development would proceed, but for the first time he used a formal language. Within that language his quantifiers were in the Peirce-Schröder tradition, although he did not explicitly cite those authors. Indeed, he regarded "for some x, $A(x)$" merely as an abbreviation for the infinitary formula

$$A(1) \text{ o. } A(2) \text{ o. } A(3) \text{ o. } \ldots ,$$

where o. stood for "oder" (or), and analogously for the universal quantifier with respect to "und" (and) (1905, 178). Likewise, he followed Peirce and Schröder (as well as the geometric tradition) by letting his quantifiers range over a *fixed* domain. Hilbert's aim was to show the consistency of his axioms for the positive integers (the Peano Postulates without the Principle of Mathematical Induction). He did so by finding a combinatorial property that held for all theorems but did not hold for a contradiction. This marked the beginning of what, over a decade later, would become his proof theory.

Thus Hilbert's conception of mathematical logic, circa 1904, embodied certain elements of first-order logic but not others. Above all, his use of infinitary formulas and his restriction of quantifiers to a fixed domain differed fundamentally from first-order logic as it was eventually formulated. When in 1918 he began to publish again on logic, his basic perspective did not change but was supplemented by *Principia Mathematica.*

6. Huntington and Veblen: Categoricity

At the turn of the century the concept of the categoricity of an axiom system was made explicit by Edward Huntington and Oswald Veblen, both of whom belonged to the group of mathematicians sometimes called the American Postulate Theorists. Huntington, while stating an essentially second-order axiomatization for $\mathbb{R}$ by means of sequences, introduced the term "sufficient" to mean that "there is essentially *only one* such assemblage [set] possible" that satisfies a given set of axioms (1902, 264). As he made clear later in his article, his term meant that any two models are isomorphic (1902, 277).

In 1904 Veblen, while investigating the foundations of geometry, discussed Huntington's term. John Dewey had suggested to Veblen the use of the term "categorical" for an axiom system such that any two of its models are isomorphic. Veblen mentioned Hilbert's axiomatization of geometry (with the Axiom of Completeness) as being categorical, and added that, for such a categorical system, "the validity of any possible statement in these terms is therefore completely determined by the axioms; and so any further axiom would have to be considered redundant, even were it not deducible from the axioms by a finite number of syllogisms" (Veblen 1904, 346).

After adopting Veblen's term "categorical," Huntington made a further observation:

> In the case of any categorical set of postulates one is tempted to assert the theorem that if any proposition can be stated in terms of the fundamental concepts, either it is itself deducible from the postulates, or

else its contradictory is so deducible; it must be admitted, however, that our mastery of the processes of logical deduction is not yet, and possibly never can be, sufficiently complete to justify this assertion. (1905, 210)

Thus Huntington was convinced that any categorical axiom system is deductively complete: every sentence expressible in the system is either provable or disprovable. On the other hand, Veblen was aware, however fleetingly, of the possibility that in a categorical axiom system there might exist propositions true in the only model of the system but unprovable in the system itself.[7] In 1931 Gödel's Incompleteness Theorem would show that this possibility was realized, in second-order logic, for *every* categorical axiom system rich enough to include the arithmetic of the natural numbers.

7. Peano and Russell: Toward *Principia Mathematica*

In (1888) Guiseppe Peano began his work in logic by describing that of Boole (1854) and Schröder (1877). Frege, in a letter to Peano probably written in 1894, described Peano as a follower of Boole, but one what had gone further than Boole by adding a symbol for generalization.[8] Peano introduced this symbol in (1889), in the form $a \supset_{x,y,\ldots} b$ for "whatever x, y, ... may be, b is deduced from a." Thus he introduced the notion of universal quantifier (independently of Frege and Peirce), although he did not separate it from his symbol $\supset$ for implication (1889, section II).

Peano's work gave no indication of levels of logic. In particular, he expressed "x is a positive integer" by $x \varepsilon N$ and so felt no need to quantify over predicates such as N. On the other hand, his Peano Postulates were essentially a second-order axiomatization for the positive integers, since these postulates included the Principle of Mathematical Induction. Beginning in 1900, Peano's formal language was adopted and extended by Bertrand Russell, and thereby achieved a longevity denied by Frege's.

When he became an advocate of Peano's logic, Russell entered an entirely new phase of his development. In contrast to his earlier and more traditional views, Russell now accepted Cantor's transfinite ordinal and cardinal numbers. Then in May 1901 he discovered what became known as Russell's Paradox and, after trying sporadically to solve it for a year, wrote to Frege about it on 16 June 1902. Frege was devastated. Although his original (1879) system of logic was not threatened, he realized that the system developed in his *Fundamental Laws* (1893, 1903a) was in grave danger. There he had permitted a set ("the extension of a concept," in his words) to be the argument of a first-level function; in this way Russell's Paradox arose in his system.

On 8 August, Russell sent Frege a letter containing the first known version of the theory of types—Russell's solution to the paradoxes of logic and set theory:

> The contradiction [Russell's Paradox] could be resolved with the help of the assumption that ranges [classes] of values are not objects of the ordinary kind: i.e., that $\phi(x)$ needs to be completed (except in special circumstances) either by an object or by a range of values of objects [class of objects] or a range of values of ranges of values [class of classes of objects], etc. This theory is analogous to your theory about functions of the first, second, etc. levels. (Russell in [Frege 1980a], 144)

This passage suggests that the seed of the theory of types grew directly from the soil of Frege's *Fundamental Laws*. Indeed, Philip Jourdain later asked Frege (in a letter of 15 January 1914) whether Frege's theory was not the same as Russell's theory of types. In a draft of his reply to Jourdain on 28 January, Frege answered a qualified yes:

> Unfortunately I do not understand the English language well enough to be able to say definitely that Russell's theory (*Principia Mathematica* I, 54ff) agrees with my theory of functions of the first, second, etc., levels. It does seem so.[9]

In 1903 Russell's Paradox appeared in print, both in the second volume of Frege's *Fundamental Laws* (1903a, 253) and in Russell's *Principles of Mathematics*. Frege dealt only with Russell's Paradox, whereas Russell also discussed in detail the paradox of the largest cardinal and the paradox of the largest ordinal. In an appendix to his book, Russell proposed a preliminary version of the theory of types as a way to resolve these paradoxes, but he remained uncertain, as he had when writing to Frege on 29 September 1902, whether this theory eliminated *all* paradoxes.[10]

When Russell completed the *Principles*, he praised Frege highly. Nevertheless, Russell's book, which had been five years in the writing, retained an earlier division of logic that was more in the tradition of Boole, Peirce, and Schröder than in Frege's. There Russell divided logic into three parts: the propositional calculus, the calculus of classes, and the calculus of relations.

In 1907, after several detours through other ways of avoiding the paradoxes,[11] Russell wrote an exposition (1908) of his mature theory of types, the basis for *Principia Mathematica.* A type was defined to be the range of significance of some propositional function. The first type consisted of the individuals and the second of what he called "first-order propositions": those propositions whose quantifiers ranged only over the first type. The third logical type consisted of "second-order prepositions," whose quantifiers ranged only over the first or second types (i.e., individuals or first-order propositions). In this manner, he defined a type for each finite index *n*. After introducing an analogous hierarchy of propositional functions, he avoided classes by using such propositional functions instead. Like Peirce, he defined the identity of individuals *x* and *y* by the condition that every first-order proposition holding for *x* also holds for *y* (1908, sections IV-VI).

Thus the theory of types, outlined in (Russell 1908) and developed in detail in *Principia Mathematica,* included a kind of first-order logic, second-order logic, and so on. But the first-order logic that it included differed from first-order logic as it is now understood, among other ways, in that a proposition about classes of classes could not be treated in his first-order logic. As we shall see, this privileged position of the membership relation was later attacked by Skolem.

One aspect of the logic found in *Principia Mathematica* requires further comment. For Russell and Whitehead, as for Frege, logic served as a foundation for all of mathematics. From their perspective it was impossible to stand outside of logic and thereby to study it as a system (in the way that one might, for example, study the real numbers). Given this state of affairs, it is not surprising that Russell and Whitehead lacked any conception of a metalanguage. They would surely have rejected such a conception if it had been proposed to them, for they explicitly denied the possibility of independence proofs for their axioms (Whitehead and Russell 1910, 95), and they believed it impossible to prove that substitution is generally applicable in the theory of types (1910, 120). Indeed, they insisted that the Principle of Mathematical Induction cannot be used to prove theorems *about* their system of logic (1910, 135). Metatheoretical research about the theory of types had to come from those schooled in a different tradition. When the consistency of the simple theory of types was eventually proved, without the Axiom of Infinity, in (1936), it was done by

Gerhard Gentzen, a member of Hilbert's school, and not by someone within the logicist tradition of Frege, Russell, and Whitehead.

Russell and Whitehead held that the theory of types can be viewed in two ways—as a deductive system (with theorems proved from the axioms) and as a formal calculus (1910, 91). Concerning the latter, they wrote:

> Considered as a formal calculus, mathematical logic has three analogous branches, namely (1) the calculus of propositions, (2) the calculus of classes, (3) the calculus of relations. (1910, 92)

Here they preserved the division of logic found in Russell's *Principles of Mathematics* and thereby continued in part the tradition of Boole, Peirce, and Schröder—a tradition that they were much less willing to acknowledge than those of Peano and Frege.

Russell and Whitehead lacked the notion of model or interpretation. Instead, they employed the genetic method of constructing, for instance, the natural numbers, rather than using the axiomatic approach of Peano or Hilbert. Finally, Russell and Whitehead shared with many other logicians of the time the tendency to conflate syntax and semantics, as when they stated their first axiom in the form that "anything implied by a true elementary proposition is true" (1910, 98).

Principia Mathematica provided the logic used by most mathematical logicians in the 1910s and 1920s. But even some of those who used its ideas were still influenced by the Peirce-Schröder tradition. This was the case for *A Survey of Symbolic Logic* by C. I. Lewis (1918). In that book, which analyzed the work of Boole, Jevons, Peirce, and Schröder (as well as that of Russell and Whitehead), the logical notation remained that of Peirce, as did the definition of the existential quantifier Σ_x and the universal quantifier Π_x in terms of logical expressions that could be infinitely long:

> We shall let $\Sigma_x \phi x$ represent $\phi x_1 + \phi x_2 + \phi x_3 + \ldots$ to as many terms as there are distinct values of x in ϕ. And $\Pi_x \phi x$ will represent $\phi x_1 \times \phi x_2 \times \phi x_3 \times \ldots$ to as many terms as there are distinct values of x in ϕx. . . . The fact that there might be an infinite set of values of x in ϕx does not affect the theoretical adequacy of our definitions. (Lewis 1918, 234-35)

Lewis sought to escape the difficulties that he found in such an infinitary logic by reducing it to the finite:

> We can assume that any law of the algebra [of logic] which holds *whatever finite* number of elements be involved holds for any number of elements whatever. . . . This also resolves our difficulty concerning the possibility that the number of values of x in ϕx might not be even denumerable. . . . (1918, 236)

He made no attempt, however, to justify his assertion, which amounted to a kind of compactness theorem.

A second logician who introduced an infinitary logic in the context of *Principia Mathematica* was Frank Ramsey. In 1925 Ramsey presented a critique of *Principia*, arguing that the Axiom of Reducibility should be abandoned and proposing instead the simple theory of types. He entertained the possibility that a truth function may have infinitely many arguments (1925, 367), and he cited Wittgenstein as having recognized that such truth functions are legitimate (1925, 343). Further, Ramsey argued that "owing to our inability to write propositions of infinite length, which is logically a mere accident, $(\phi).\phi a$ cannot, like $p.q$, be elementarily expressed, but must be expressed as the logical product of a set of which it is also a member" (1925, 368-69). It appears that Ramsey was not concerned with infinitary formulas per se but only with using them in his heuristic argument for the simple theory of types. Yet his proposal was sufficiently serious that Gödel later cited it when arguing against such infinitary formulas (Gödel 1944, 144-46).

8. Hilbert: Later Foundational Research

Hilbert did not abandon foundational questions after his 1904 lecture at the Heidelberg congress, though he published nothing further on them for more than a decade. Rather, he gave lecture courses at Göttingen on such questions repeatedly—in 1905, 1908, 1910, and 1913. It was the course given in the winter semester of 1917-18, "Principles of Mathematics and Logic," that first exhibited his mature conception of logic.[12]

That course began shortly after Hilbert delivered a lecture, "Axiomatic Thinking," at Zurich on 11 September 1917. The Zurich lecture stressed the role of the axiomatic method in various branches of mathematics and physics. Returning to an earlier theme, he noted how the consistency of several axiomatic systems (such as that for geometry) had been reduced to a more specialized axiom system (such as that for $\mathbb{R}$, reduced in turn to the axioms for $\mathbb{N}$ and those for set theory). Hilbert concluded by stating that the "full-scale undertaking of Russell's to axiomatize logic can be seen as the crowning achievement of axiomatization" (1918, 412).

The 1917 course treated the axiomatic method as applied to two disciplines: geometry and mathematical logic. When considering logic, Hilbert emphasized not only questions of independence and consistency, as he had in the Zurich lecture, but also "completeness" (which, however, was handled as in his [1899]). Influenced by *Principia Mathematica*, he treated propositional logic as a distinct level of logic. But he deviated from Russell and Whitehead by offering a proof for the consistency of propositional logic. After discussing the calculus of monadic predicates and the corresponding calculus of classes, he turned to first-order logic, which he named the "functional calculus." Stating primitive symbols and axioms for it, he developed it at some length. In conclusion, he noted:

> With what we have considered thus far [first-order logic], foundational discussions about the calculus of logic come to an end—if we have no other goal than formalizing logical deduction. We, however, are not content with this application of symbolic logic. We wish not only to be in a position to develop individual theories from their principles purely formally but also to make the foundations of mathematical theories themselves an object of investigation—to examine in what relation they stand to logic and to what extent they can be obtained from purely logical operations and concepts. To this end the calculus of logic must serve as our tool.
>
> Now if we make use of the calculus of logic in this sense, then we will be compelled to extend in a certain direction the rules governing the formal operations. In particular, while we previously separated propositions and [propositional] functions completely from objects and, accordingly, distinguished the signs for indefinite propositions and functions rigorously from the variables, which take arguments, now we permit propositions and functions to be taken as logical variables in a way similar to that for proper objects, and we permit signs for indefinite propositions and functions to appear as arguments in symbolic expressions. (Hilbert 1917, 188)

Here Hilbert argued, in effect, for a logic at least as strong as second-order logic. But his views on this matter did not appear in print until his book *Principles of Mathematical Logic*, written jointly with Ackermann, was published in 1928. In that book, which consisted largely of a revision of Hilbert's 1917 course, he expressed himself even more strongly:

> As soon as the object of investigation becomes the foundation of. . .mathematical theories, as soon as one wishes to determine in what relation the theory stands to logic and to what extent it can be obtained from purely logical operations and concepts, then the extended calculus [of logic] is essential. (Hilbert and Ackermann 1928, 86)

In the 1917 course (and again in the book), this extended functional calculus permitted quantification over propositions and included explicitly such expressions as

(X)(if X, then X or X).

Hilbert cited the Principle of Mathematical Induction as an axiom that, if fully expressed, requires a quantifier varying over propositions. Likewise, he defined the identity relation in his extended logic in a manner reminiscent of Peirce: two objects x and y are identical if, for every proposition P, P holds of x if and only if P holds of y (1917, 189-91). Finally, he treated set-theoretic notions (such as union and power set) by means of quantifiers over propositions.

What, in 1917, was Hilbert's extended calculus of logic? At first glance it might appear to be second-order logic. Yet he stated his preliminary version of the extended calculus in a way that permitted a function of propositional functions to occupy an argument place for individuals in a propositional function—an act that is illegitimate in second-order logic and that, as he knew, gave rise to Russell's Paradox. Hilbert used this paradox to motivate his adoption of the ramified theory of types as the definitive version of his extended calculus. After indicating how the real numbers can be constructed via Dedekind cuts by means of the Axiom of Reducibility, Hilbert concluded his course: "Thus we have shown that introducing the Axiom of Reducibility is the appropriate means to mold the calculus of levels [the theory of types] into a system in which the foundations of higher mathematics can be developed" (1917, 246).

In the 1917 course Hilbert explicitly treated first-order logic (without any infinitary trappings) as a subsystem of the ramified theory of types:

> In this way is founded a new form of the calculus of logic, the "calculus of levels," which represents an extension of the original functional calculus [first-order logic], since this is contained in it as a theory of first order, but which implies an essential restriction as compared with our previous extension of the functional calculus. (1917, 222-23)

The same statement appeared verbatim in (Hilbert and Ackermann 1928, 101). Thus in 1917, and still in 1928, Hilbert treated first-order logic as a subsystem of all of logic (for him, the ramified theory of types), regarding set theory and the Principle of Mathematical Induction as incapable of adequate treatment in first-order logic (1917, 189, 200; Hilbert and Ackermann 1928, 83, 92).

On the occasion of his Zurich lecture, Hilbert invited Paul Bernays to give up his position as *Privatdozent* at the University of Zurich and come to Göttingen as Hilbert's assistant on the foundations of arithmetic. Bernays accepted. Thus he became Hilbert's principal collaborator in logic, serving first as official note-taker for Hilbert's 1917 course.

In 1918 Bernays wrote a *Habilitationsschrift* in which was proved the completeness of propositional logic. This work was influenced by (Schröder 1890), (Frege 1893), and *Principia Mathematica*. For the first time the completeness problem for a subsystem of logic was expressed precisely. Bernays stated and proved that "every provable formula is a valid formula, and conversely" (1918, 6). In addition he established the deductive completeness of propositional logic as well as what was later called Post completeness: no unprovable formula can be added to the axioms of propositional logic without giving rise to a contradiction (1918, 9).

Hilbert published nothing further on logic until 1922, when he reacted strongly against the claims of L. E. J. Brouwer and Hermann Weyl that the foundations of analysis were built on sand. As a countermeasure, Hilbert introduced his proof theory (*Beweistheorie*). This theory treated the axiom systems of mathematics as pure syntax, distinguishing them from what he called metamathematics, where meaning was permitted. His two chief aims were to show the consistency of both analysis and set theory and to establish the decidability of each mathematical question (*Entscheidungsproblem*)—aims already expressed in his (1900b).

Hilbert began by proving the consistency of a very weak subsystem of arithmetic, closely related to the one whose consistency he had established in 1904 (1922, 170). He claimed to have a proof for the consistency of arithmetic proper, including the Principle of Mathematical Induction. He considered this principle to be a second-order axiom, as he had previously and did subsequently.[13] Likewise, he introduced not only individual variables (*Grundvariable*) but variables for functions and even for functions of functions (1922, 166).

To defend mathematics against the attacks of Brouwer and Weyl, Hilbert intended to establish the consistency of mathematics from the bottom up. Having shown that a subsystem of arithmetic was consistent, he hoped soon to prove the consistency of the whole of arithmetic (including mathematical induction) and then of the theory of real numbers. Thus Hilbert was thinking in terms of a sequence of levels to be secured successively. In a handwritten appendix to the lecture course he gave during the winter semester of 1922-23,[14] eight of these levels were listed. Analysis occurred at level four, and higher-order logic was considered in part at level five.

Already in (1922, 157), Hilbert had spoken of the need to formulate Zermelo's Axiom of Choice in such a way that it becomes as evident as $2 + 2 = 4$. In (1923) Hilbert utilized a form of the Axiom of Choice as the cornerstone of his proof theory, which was to be "finitary." He did so as a way of eliminating the direct use of quantifiers, which he regarded as an essentially infinitary feature of logic: "Now where does there appear for the first time something going beyond the concretely intuitive and the finitary? Obviously already in the use of the concepts '*all*' and '*there exists*'" (1923, 154). For finite collections, he noted, the universal and existential quantifiers reduce to finite conjunctions and disjunctions, yielding the Principle of the Excluded Middle in the form that $\sim(\forall x)A(x)$ is equivalent to $(\exists x)\sim A(x)$ and that $\sim(\exists x)A(x)$ is equivalent to $(\forall x)\sim A(x)$. "These equivalences," he continued,

> are commonly assumed, without further ado, to be valid in mathematics for infinitely many individuals as well. In this way, however, we abandon the ground of the finitary and enter the domain of transfinite inferences. If we were always to use for infinite sets a procedure admissible for finite sets, we would open the gates to error. . . . In analysis . . . the theorems valid for finite sums and products can be translated into theorems valid for infinite sums and products only if the inference is secured, in the particular case, by convergence. Likewise, we must not treat the infinite logical sums and products
>
> $A(1) \;\&\; A(2) \;\&\; A(3) \;\&\; \ldots$
>
> and
>
> $A(1) \vee A(2) \vee A(3) \vee \ldots$
>
> in the same way as finite ones. My proof theory . . . provides such a treatment. (1923, 155)

Hilbert then introduced what he called the Transfinite Axiom, which he regarded as a form of the Axiom of Choice:

$A(\tau A) \rightarrow A(x).$

The intended meaning was that if the proposition $A(x)$ holds when x is τA, then $A(x)$ holds for an arbitrary x, say a. He thus defined $(\forall x)A(x)$ as $A(\tau A)$ and similarly for $(\exists x)A(x)$ (1923, 157).

Soon Hilbert modified the Transfinite Axiom, changing it into the ε-axiom:

$$A(x) \rightarrow A(\varepsilon_x(A(x))),$$

where ε was a universal choice function acting on properties. The first sign of this change occurred in handwritten notes that Hilbert prepared for his lecture course on logic during the winter semester of 1922-23;[15] the new ε-axiom appeared in print in (Ackermann 1925) and (Hilbert 1926). Hilbert had used the Transfinite Axiom, acting on number-theoretic functions, to quantify over number-theoretic functions and to obtain a proof of the Axiom of Choice for families of sets of real numbers (1923, 158, 164). Likewise, Ackermann used the ε-axiom, acting on number-theoretic functions, to quantify over such functions while trying to establish the consistency of number theory (1925, 32).

In 1923 Hilbert stressed not only that his proof theory could give mathematics a firm foundation by showing the consistency of analysis and set theory but also that it could settle such classical unsolved problems of set theory as Cantor's Continuum Problem (1923, 151). In his (1926), Hilbert attempted to sketch a proof for the Continuum Hypothesis (CH). Here one of his main tools was the hierarchy of functions that he called variable-types. The first level of this hierarchy consisted of the functions from $\mathbb{N}$ to $\mathbb{N}$—i.e., the number-theoretic functions. The second level contained functionals—those functions whose argument was a number-theoretic function and whose value was in $\mathbb{N}$. In general, the level $n+1$ consisted of functions whose arguments was a function of level n and whose value was in $\mathbb{N}$. He permitted quantification over functions of any level (1926, 183-84). In (1928, 75) he again discussed his argument for CH, which he explicitly treated as a second-order proposition. This argument met with little favor from other mathematicians.

From 1917 to about 1928, Hilbert worked in a variant of the ramified theory of types, one in which functions increasingly played the principal role. But by 1928 he had rejected Russell's Axiom of Reducibility as dubious—a significant change from his support for this axiom in (1917).

Instead, he asserted that the same purpose was served by his treatment of function variables (1928, 77). He still insisted, in (Hilbert and Ackermann 1928, 114-15), that the theory of types was the appropriate logical vehicle for studying the theory of real numbers. But, he added, logic could be founded so as to be free of the difficulties posed by the Axiom of Reducibility, as he had done in his various papers. Thus Hilbert opted for a version of the simple theory of types (in effect, ω-order logic).

Hilbert's co-workers in proof theory used essentially the same system of logic as he did. Von Neumann (1927) gave a proof of the consistency of a weak form of number theory, working mainly in a first-order subsystem. However, he discussed Ackermann's work involving both a second-order ε-axiom and quantification over number-theoretic functions (von Neumann 1927, 41-46). During the same period, Bernays discussed in some detail "the extended formalism of 'second order'," mentioning Löwenheim's (1915) decision procedure for monadic second-order logic and remarking how questions of first-order validity could be expressed by a second-order formula (Bernays and Schönfinkel 1928, 347-48).[16]

In (1929), Hilbert looked back with pride and forward with hope at what had been accomplished in proof theory. He thought that Ackermann and von Neumann had established the consistency of the ε-axiom restricted to natural numbers, not realizing that this would soon be an empty victory. Hilbert posed four problems as important to his program. The first of these was essentially second-order: to prove the consistency of the ε-axiom acting on number-theoretic functions (1929, 4). The second was the same problem for higher-order functions, whereas the third and fourth concerned completeness. The third, noting that the Peano Postulates are categorical, asked for a proof that if a number-theoretic sentence is shown consistent with number theory, then its negation cannot be shown consistent with number theory. The fourth was more complex, asking for a demonstration that, on the one hand, the axioms of number theory are deductively complete and that, on the other, first-order logic with identity is complete (1929, 8).

Similarly, in (Hilbert and Ackermann 1928, 69) there was posed the problem of establishing the completeness of first-order logic (without identity). The following year Gödel solved this problem for first-order logic, with and without identity (1929). His abstract (1930b) of this result spoke of the "restricted functional calculus" (first-order logic without identity) as a subsystem of logic, since no bound function variables were permitted.

Although the Completeness Theorem for first-order logic solved one of Hilbert's problems, Gödel soon published a second abstract (1930c) that threatened to demolish Hilbert's program. This abstract gave the First Incompleteness Theorem: in the theory of types, the Peano Postulates are not deductively complete. Furthermore, Gödel's Second Incompleteness Theorem stated that the consistency of the theory of types cannot be proved in the theory of types, provided that this theory is ω-consistent. This seemed to destroy any hope of proving the consistency of set theory and analysis with Hilbert's finitary methods.

Probably motivated by Gödel's incompleteness results, Hilbert (1931) introduced a version of the ω-rule, an infinitary rule of inference.[17] In his last statement on proof theory in 1934, Hilbert attempted to limit the damage done by those results. He wrote of

> the final goal of knowing that our customary methods in mathematics are utterly consistent.Concerning this goal, I would like to stress that the view temporarily widespread—that certain recent results of Gödel imply that my proof theory is not feasible—has turned out to be erroneous. In fact those results show only that, in order to obtain an adequate proof of consistency, one must use the finitary standpoint in a sharper way than is necessary in treating the elementary formalism. (Hilbert in [Hilbert and Bernays 1934], v)

9. Löwenheim: First-Order (Infinitary) Logic as a Subsystem

It was Leopold Löwenheim, aware of *Principia Mathematica* but firmly placed in the Peirce-Schröder tradition, who first established significant metamathematical results about the semantics of logic. His 1915 result, the earliest version of Löwenheim's Theorem (every satisfiable sentence has a countable model), is now considered to be about first-order logic. As will become evident, however, in one sense Löwenheim in 1915 was even further from first-order logic than Hilbert had been in 1904.

In an unpublished autobiographical note, Löwenheim remarked that shortly after he began teaching at a *Gymnasium* (about 1900) he "became acquainted with the calculus of logic through reviews and from Schröder's books" (Löwenheim in [Thiel 1977], 237). His first published paper (1908) was devoted to the solvability of certain symmetric equations in Schröder's calculus of classes, and he soon turned to related questions in Schröder's calculus of domains (1910, 1913a) and of relations (1913b).

In 1915 there appeared Löwenheim's most influential article, "On Possibilities in the Calculus of Relations." His later opinion of this article, as expressed in unpublished autobiographical notes, was ambivalent:

> I. . . have pointed out new paths for science in the field of the calculus of logic, which had been founded by Leibniz but which had come to a deadlock. . . . On an outing, a somewhat grotesque landscape stimulated my fantasy, and I had an insight that the thoughts I had already developed in the calculus of domains might lead me to make a breakthrough in the calculus of relations. Now I could find no rest until the idea was completely proved, and this gave me a host of troubles. . . . This breakthrough has scarcely been noticed, but some other breakthroughs which I made in my paper "On Possibilities in the Calculus of Relations" were noticed all the same. This became the foundation of the modern calculus of relations. But I did not take much pride in this paper, since the point had only been to ask the right questions while the proofs could be found easily without ingenuity or imagination. (Löwenheim in [Thiel 1977], 246-47)

Löwenheim's article (1915) continued Schröder's work (1895) on the logic of relations but made a number of distinctions that Schröder lacked, the most important being between a *Relativausdruck* (relational expression) and a *Zählausdruck* (individual expression). Löwenheim's definitions of individual expression and relational expression (1915, 447-48) differed from those for first-order and second-order formula in that, following Peirce and Schröder, such expressions were allowed to have a quantifier for each individual of the domain, or for each relation over the domain, respectively. In effect, Löwenheim's logic permitted infinitely long strings of quantifiers and Boolean connectives, but these infinitary formulas occurred only as the expansion of finitary formulas, to which they were equivalent in his system.

In (1895) Schröder had used a logic that was essentially the same as Löwenheim's, namely, a logic that was second-order but permitted quantifiers to be expanded as infinitary conjunctions (or disjunctions). After distinguishing carefully between the (infinitary) first-order part of his logic and the entire logic, Löwenheim asserted that all important problems of mathematics can be handled in this (infinitary) second-order logic.

Apparently, his discovery of Löwenheim's Theorem was motivated by the two ways in which Schröder's calculus of relations could express propositions: (1) by means of quantifiers and individual variables and (2) by means of relations with no individual variables (cf. the combinatory logic developed by Schönfinkel [1924] and later by Curry). Whereas Schröder regarded every proposition of form (1) as capable of being "condensed" (that is, written in form (2)) because he permitted individuals to be interpreted as a certain kind of relation, Löwenheim dispensed with this interpretation. He established that not all propositions can be condensed, by showing that condensable first-order propositions could not express that there are at least four elements.

After treating condensation, Löwenheim next gave an analogous result for denumerable domains: For an infinite domain M (possibly denumerable and possibly of higher cardinality), if a first-order proposition is valid in every finite domain but not in every domain, then the proposition is not valid in M. This was his original statement of Löwenheim's Theorem. His proof used a second-order formula stating that

$$(\forall x)(\exists y)\phi(x,y) \longleftrightarrow (\exists f)(\forall x)\phi(x,f(x)),$$

where f was a function variable, and he regarded this formula (for the given domain M) as expandable to an infinitary formula with a first-order existential quantifier for each individual of M (1915, section 2). His next theorem stated that, since Schröder's logic can express that a domain is finite or denumerable, then Löwenheim's Theorem cannot be extended to Schröder's (second-order) logic.

Some historians of mathematics have regarded Löwenheim's argument for his theorem as odd and unnatural.[18] But his argument appears so only because they have considered it within first-order logic. Although Löwenheim's Theorem holds for first-order logic (as Skolem was to show), this was not the logic in which Löwenheim worked.

10. Skolem: First-Order Logic as All of Logic

In 1913, after writing a thesis (1913a) on Schröder's algebra of logic, Skolem received his undergraduate degree in mathematics. He soon wrote several papers, beginning with (1913b), on Schröder's calculus of classes. During the winter of 1915-16, Skolem visited Göttingen, where he discussed set theory with Felix Bernstein. By that time Skolem was already acquainted with Löwenheim's Theorem and had seen how to extend it to a countable set of formulas. Furthermore, Skolem had realized that this extended version of the theorem could be applied to set theory. Thus he found what was later called Skolem's Paradox: Zermelo's system of set theory has a countable model (within first-order logic) even though this system implies the existence of uncountable sets (Skolem 1923, 232, 219).

Nevertheless, Skolem did not lecture on this result until the Fifth Scandanavian Congress of Mathematicians in July 1922. When it appeared in print the following year, he stated that he had not published it earlier because he had been occupied with other problems and because

> I believed that it was so clear that the axiomatization of set theory would not be satisfactory as an ultimate foundation for mathematics that, by and large, mathematicians would not bother themselves with it very much. To my astonishment I have seen recently that many mathematicians regard these axioms for set theory as the ideal foundation for mathematics. For this reason it seemed to me that the time had come to publish a critique. (Skolem 1923, 232)

It was precisely in order to establish the relativity of set-theoretic notions that Skolem proposed that set theory be formulated within first-order logic. At first glance, given the historical context, this was a strange suggestion. Set theory appeared to require quantifiers not only over individuals, as in first-order logic, but also quantifiers over sets of individuals, over sets of sets of individuals, and so on. Skolem's radical proposal was that the membership relation ε be treated not as a part of logic (as Peano and Russell had done) but like any other relation on a domain. Such relations could be given a variety of interpretations in various domains, and so should ε. In this way the membership relation began to lose its privileged position within logic.

Skolem made Löwenheim's article (1915) the starting point for several of his papers. This process began when Skolem used the terms *Zählausdruck* and *Zählgleichung* in an article (1919), completed in 1917, on Schröder's calculus of classes. But it was only in (1920) that Skolem began to discuss Löwenheim's Theorem, a subject to which he returned many times over the next forty years. Skolem first supplied a new proof for this theorem (relying on Skolem functions) in order to avoid the occurrence in the proof of second-order propositions (in the form of subsubscripts on Löwenheim's relational expressions [Skolem 1920, 1]). Extending this result, Skolem obtained what became known as the Löwenheim-Skolem Theorem (a countable and satisfiable set of first-order propositions has a countable model). Surprisingly, he did not even state the Löwenheim-Skolem Theorem for first-order logic as a separate theorem but immediately proved Löwenheim's Theorem for any countable conjunction of first-order propositions. His culminating generalization was to show Löwenheim's Theorem for a countable conjunction of countable disjunctions of first-order propositions. Thus, in effect, he established Löwenheim's Theorem (and hence the Löwenheim-Skolem Theorem) for what is now called $L_{\omega_1,\omega}$.[19]

Nevertheless, when in (1923) Skolem published his application of Löwenheim's Theorem to set theory, he stated the Löwenheim-Skolem Theory explicitly for first-order logic and did not mention any of his other generalizations of Löwenheim's Theorem for an infinitary logic. Never again did he return to the infinitary logic that he had adopted from Schröder and Löwenheim. Instead he argued in (1923), as he would for the rest of his life, that first-order logic is the proper basis for set theory and, indeed, for all of mathematics.

The reception of the Löwenheim-Skolem Theorem, insofar as it concerned set theory, was mixed. Abraham Fraenkel, when in (1927) he reviewed Skolem's paper of 1923 in the abstracting journal of the day, the *Jahrbuch über die Fortschritte der Mathematik*, did not mention first-order logic but instead held Skolem's result to be about Schröder's calculus of logic. Fraenkel concluded from Skolem's Paradox that the relativity of the notion of cardinal number is inherent in *any* axiomatic system. Thus Fraenkel lacked a clear understanding of the divergent effects of first-order and second-order logic on the notion of cardinal number.

In 1925, when John von Neumann published his axiomatization for set theory, he too was unclear about the difference between first-order and second-order logic. He specified his desire to axiomatize set theory by using only "a finite number of purely formal operations" (1925, section 2), but nowhere did he specify the logic in which his axiomatization was to be formulated. His concern, rather, was to give a finite characterization of Zermelo's notion of "definite property." At the end of his paper, von Neumann considered the question of categoricity in detail and noted various steps, such as the elimination of inaccessible cardinals, that had to be taken to arrive at a categorical axiomatization. After remarking that the axioms for Euclidean geometry were categorical, he observed that, because of the Löwenheim-Skolem Theorem,

> no categorical axiomatization of set theory seems to exist at all.... And since there is no axiom system for mathematics, geometry, and so forth that does not presupppose set theory, there probably cannot

> be any categorically axiomatized infinite systems at all. This circumstance seems to me to be an argument for intuitionism. (1925, section 5)

Yet von Neumann then claimed that the boundary between the finite and infinite was also blurred. He exhibited no awareness that these difficulties did not arise in second-order logic.

The reason for Fraenkel's and von Neumann's confusion was that, circa 1925, the distinction between first-order logic and second-order logic was still unclear, and it was equally unclear just how widely the Löwenheim-Skolem Theorem applied. Fraenkel had spoken of "the uncertainty of general logic" (1922, 101), and Zermelo added, concerning his 1908 axiomatization of set theory, that "a generally recognized 'mathematical logic', to which I could have referred, did not exist then—any more than it does today, when every foundational researcher has his own logistical system" (1929, 340)

What is surprising is that Gödel seemed unclear about the question of categoricity when he proved the Completeness Theorem for first-order logic in his doctoral dissertation (1929). Indeed, he made certain enigmatic comments that foreshadow his incompleteness results:

> Brouwer, in particular, has emphatically stressed that from the consistency of an axiom system we cannot conclude without further ado that a model can be constructed. But one might perhaps think that the existence of the notions introduced through an axiom system is to be defined outright by the consistency of the axioms and that, therefore, a proof has to be rejected out of hand. This definition,...however, manifestly presupposes the axiom that every mathematical problem is solvable. Or, more precisely, it presupposes that we cannot prove the unsolvability of any problem. For, if the unsolvability of some problem (say, in the domain of real numbers) were proved, then, from the definition above, it would follow that there exist two non-isomorphic realizations of the axiom system for the real numbers, while on the other hand we can prove that any two realizations are isomorphic. (1929, section 1)

In his paper Gödel wrote of first-order logic, but then he introduced a notion, categoricity, that belongs essentially to second-order logic. Thus it appears that the distinction between first-order logic and second-order logic, insofar as it concerns the range of applicability of the Löwenheim-Skolem Theorem, remained unclear even to Gödel in 1929.

In (1929) Zermelo responded to the criticisms of his notion of "definite property" by Fraenkel, Skolem, and von Neumann. Zermelo chose to define "definite property," in effect, as any second-order propositional function built up from = and ε—although he did not specify the axioms or the semantics of his second-order logic. But in (1930) he was well aware that this logic permitted him to prove that any standard model of Zermelo-Fraenkel set theory without urelements consists of V_α for some level of Zermelo's cumulative type hierarchy if and only if α is a strongly inaccessible ordinal. This result holds in second-order logic, but it fails in first-order logic—as later shown in (Montague and Vaught 1959).

Zermelo was dismayed by the uncritical acceptance of "*Skolemism*, the doctrine that *every* mathematical theory, and set theory in particular, is satisfiable in a *countable model*" (1931, 85). He responded by proposing a powerful infinitary logic. It permitted infinitely long conjunctions and disjunctions (having any cardinal for their length) and no quantifiers—what is now called the propositional part of $L_{\infty,\omega}$. This was by far the strongest infinitary logic considered up to that time.

Zermelo was also dissatisfied with Gödel's Incompleteness Theorem, and wrote to him for clarification. There followed a spirited exchange of letters between the old set-theorist and the young logician. Zermelo, like Skolem and many others, failed to distinguish clearly between syntax and semantics. On the other hand, Zermelo insisted in these letters on extending the notion of proof sufficiently that every valid formula would be provable. In his last published paper (1935), he extended the notion of rule of inference to allow not only the ω-rule but well-founded, partially ordered strings of premises of any cardinality.

Zermelo's proposal about extending logic fell on deaf ears.[20] Gödel, though he adopted Zermelo's cumulative type hierarchy, formulated his own researches on set theory within first-order logic. Indeed, Gödel's constructible sets were the natural fusion of first-order logic with Zermelo's cumulative type hierarchy.

In December 1938, at a conference in Zurich on the foundations of mathematics, Skolem returned again to the existence of countable models for set theory and to Skolem's Paradox (1941, 37). On this occasion he emphasized the relativity, not only of set theory, but of mathematics as a whole. The discussion following Skolem's lecture revealed both interest in and ambivalence about the Löwenheim-Skolem Theorem, especially when Bernays commented:

> The axiomatic restriction of the notion of set [to first-order logic] does not prevent one from obtaining all the usual theorems...of Cantorian set theory.... Nevertheless, one must observe that this way of making the notion of set (or that of predicate) precise has a consequence of another kind: the interpretation of the system is no longer necessarily unique.... It is to be observed that the impossibility of characterizing the finite with respect to the infinite comes from the restrictiveness of the [first-order] formalism. The impossibility of characterizing the denumerable with respect to the nondenumerable in a sense that is in some way unconditional—does this reveal, one might wonder, a certain inadequacy of the method under discussion here [first-order logic] for making axiomatizations precise? (Bernays in [Gonseth 1941], 49-50)

Skolem objected vigorously to Bernays's suggestion and insisted that a first-order axiomatization is surely the most appropriate.

In 1958, at a colloquium held in Paris, Skolem reiterated his views on the relativity of fundamental mathematical notions and criticized Tarski's contributions:[21]

> It is self-evident that the dubious character of the notion of set renders other notions dubious as well. For example, the semantic definition of mathematical truth proposed by A. Tarski and other logicians presupposes the general notion of set. (Skolem 1958, 13)

In the discussion that followed Skolem's lecture, Tarski responded to this criticism:

> [I] object to the desire shown by Mr. Skolem to reduce every theory to a denumerable model.... Because of a well-known generalization of the Löwenheim-Skolem Theorem, every formal system that has an infinite model has a model whose power is any transfinite cardinal given in advance. From this, one can just as well argue for excluding denumerable models from consideration in favor of nondenumerable models. (Tarski in [Skolem 1958], 17)

11. Conclusion

As we have seen, the logics considered from 1879 to 1923—such as those of Frege, Peirce, Schröder, Löwenheim, Skolem, Peano, and Russell—were generally richer than first-order logic. This richness took one of two forms: the use of infinitely long expressions (by Peirce, Schröder, Hilbert, Löwenheim, and Skolem) and the use of a logic at least as rich as second-order logic (by Frege, Peirce, Schröder, Löwenheim, Peano, Russell, and

Hilbert). The fact that no system of logic predominated—although the Peirce-Schröder tradition was strong until about 1920 and *Principia Mathematica* exerted a substantial influence during the 1920s and 1930s—encouraged both variety and richness in logic.

First-order logic emerged as a distinct subsystem of logic in Hilbert's lectures (1917) and, in print, in (Hilbert and Ackermann 1928). Nevertheless, Hilbert did not at any point regard first-order logic as the proper basis for mathematics. From 1917 on, he opted for the theory of types—at first the ramified theory with the Axiom of Reducibility and later a version of the simple theory of types (ω-order logic). Likewise, it is inaccurate to regard what Löwenheim did in (1915) as first-order logic. Not only did he consider second-order propositions, but even his first-order subsystem included infinitely long expressions.

It was in Skolem's work on set theory (1923) that first-order logic was first proposed as all of logic and that set theory was first formulated within first-order logic. (Beginning in [1928], Herbrand treated the theory of types as merely a mathematical system with an underlying first-order logic.) Over the next four decades Skolem attempted to convince the mathematical community that both of his proposals were correct. The first claim, that first-order logic is all of logic, was taken up (perhaps independently) by Quine, who argued that second-order logic is really set theory in disguise (1941, 144-45). This claim fared well for a while.[22] After the emergence of a distinct infinitary logic in the 1950s (thanks in good part to Tarski) and after the introduction of generalized quantifiers (thanks to Mostowski [1957]), first-order logic is clearly not all of logic.[23] Skolem's second claim, that set theory should be formulated in first-order logic, was much more successful, and today this is how almost all set theory is done.

When Gödel proved the completeness of first-order logic (1929, 1930a) and then the incompleteness of both second-order and ω-order logic (1931), he both stimulated first-order logic and inhibited the growth of second-order logic. On the other hand, his incompleteness results encouraged the search for an appropriate infinitary logic—by Carnap (1935) and Zermelo (1935). The acceptance of first-order logic as one basis on which to formulate all of mathematics came about gradually during the 1930s and 1940s, aided by Bernays's and Gödel's first-order formulations of set theory.

Yet Maltsev (1936), through the use of uncountable first-order languages, and Tarski, through the Upward Löwenheim-Skolem Theorem and the definition of truth, rejected the attempt by Skolem to restrict logic to countable first-order languages. In time, uncountable first-order languages and uncountable models became a standard part of the repertoire of first-order logic. Thus set theory entered logic through the back door, both syntactically and semantically, though it failed to enter through the front door of second-order logic.

Notes

1. Peano acknowledged (1891, 93) that his postulates for the natural numbers came from (Dedekind 1888).

2. Peirce's use of second-order logic was first pointed out by Martin (1965).

3. Van Heijenoort (1967, 3; 1986, 44) seems to imply that Frege did separate, or ought to have separated, first-order logic from the rest of logic. But for Frege to have done so would have been contrary to his entire approach to logic. Here van Heijenoort viewed the matter unhistorically, through the later perspectives of Skolem and Quine.

4. There is a widespread misconception, due largely to Russell (1919, 25n), that Frege's *Begriffsschrift* was unknown before Russell publicized it. In fact, Frege's book quickly received at least six reviews in major mathematical and philosophical journals by researchers such as Schröder in Germany and John Venn in England. These reviews were largely favorable, though they criticized various features of Frege's approach. The *Begriffsschrift* failed to persuade other logicians to adopt Frege's approach to logic because most of them (Schröder and Venn, for example) were already working in the Boolean tradition. (See [Bynum 1972, 209-35] for these reviews, and see [Nidditch 1963] on similar claims by Russell concerning Frege's work in general.)

5. Frege pointed this out to Hilbert in a letter of 6 January 1900 (Frege 1980a, 46, 91) and discussed the matter in print in (1903b, 370-71).

6. The suggestion that Sommer may have prompted Hilbert to introduce an axiom of continuity is due to Jongsma (1975, 5-6).

7. Forder, in a textbook on the foundations of geometry (1927, 6), defined the term "complete" to mean what Veblen called "categorical" and argued that a categorical set of axioms must be deductively complete. Here Forder presupposed that if a set of axioms is consistent, then it is satisfiable. This, as Gödel was to establish in (1930a) and (1931), is true for first-order logic but false for second-order logic. On categoricity, see (Corcoran 1980, 1981).

8. (Frege 1980a, 108). In an 1896 article Frege wrote: "I shall now inquire more closely into the essential nature of Peano's conceptual notation. It is presented as a descendant of Boole's logical calculus but, it may be said, as one different from the others.... By and large, I regard the divergences from Boole as improvements" (Frege 1896; translation in 1984, 242). In (1897), Peano introduced a separate symbol for the existential quantifier.

9. (Frege 1980a, 78). Nevertheless, Church (1976, 409) objected to the claim that Frege's system of 1893 is an anticipation of the simple theory of types. The basis of Church's objection is that for "Frege a function is not properly an (abstract) object at all, but is a sort of incompleted abstraction." The weaker claim made in the present paper is that Frege's system helped lead Russell to the theory of types when he dropped Frege's assumption that classes are objects of level 0 and allowed them to be objects of arbitrary finite level.

10. Russell in (Frege 1980a, 147; Russell 1903, 528). See (Bell 1984) for a detailed analysis of the Frege-Russell letters.

11. Three of these approaches are found in (Russell 1906): the zigzag theory, the theory of limitation of size, and the no-classes theory. In a note appended to this paper in February 1906, he opted for the no-classes theory. Three months later, in another paper read to the London Mathematical Society, the no-classes theory took a more concrete shape as the

substitutional theory. Yet in October 1906, when the Society accepted the paper for publication, he withdrew it. (It was eventually published as [Russell 1973].) The version of the theory of types given in the *Principles* was very close to his later no-classes theory. Indeed, he wrote that "*technically*, the theory of types suggested in Appendix B [1903] differs little from the no-classes theory. The only thing that induced me at that time to retain classes was the technical difficulty of stating the propositions of elementary arithmetic without them" (1973, 193).

12. Hilbert's courses were as follows: "Logische Prinzipien des mathematischen Denkens" (summer semester, 1905), "Prinzipien der Mathematik" (summer semester, 1908), "Elemente und Prinzipienfragen der Mathematik" (summer semester, 1910), "Einige Abschnitte aus der Vorlesung über die Grundlagen der Mathematik und Physik" (summer semester, 1913), and "Prinzipien der Mathematik und Logik" (winter semester, 1917). A copy of the 1913 lectures can be found in Hilbert's *Nachlass* in the Handschriftenabteilung of the Niedersächsische Staats- und Universitätsbibliotek in Göttingen: the others are kept in the "Giftschrank" at the Mathematische Institut in Göttingen. Likewise, all other lecture courses given by Hilbert and mentioned in this paper can be found in the "Giftschrank."

13. See (Hilbert 1917, 190) and (Hilbert and Ackermann 1928, 83). The editors of Hilbert's collected works were careful to distinguish the Principle of Mathematical Induction in (Hilbert 1922) from the first-order axiom schema of mathematical induction; see (Hilbert 1935, 176n). Herbrand also realized that in first-order logic this principle becomes an axiom schema (1929; 1930, chap. 4.8).

14. The course was entitled "Logische Grundlagen der Mathematik."

15. "Logische Grundlagen der Mathematik," a partial copy of which is kept in the university archives at Göttingen. On the history of the Axiom of Choice, see (Moore 1982).

16. Bernays also wrote about second-order logic briefly in his (1928).

17. On the early history of the ω-rule, see (Feferman 1986).

18. See, for example, (van Heijenoort 1967, 230; Vaught 1974, 156; Wang 1970, 27).

19. The recognition that Skolem in (1920) was primarily working in $L_{\omega_1,\omega}$ is due to Vaught (1974, 166).

20. For an analysis of Zermelo's views on logic, see (Moore 1980, 120-36).

21. During the same period Skolem (1961, 218) supported the interpretation of the theory of types as a many-sorted theory within first-order logic. Such an interpretation was given by Gilmore (1957), who showed that a many-sorted theory of types in first-order logic has the same valid sentences as the simple theory of types (whose semantics was to be based on Henkin's notion of general model rather than on the usual notion of higher-order model).

22. See (Quine 1970, 64-70). For a rebuttal of some of Quine's claims, see (Boolos 1975).

23. For the impressive body of recent research on stronger logics, see (Barwise and Feferman 1985).

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Added in Proof:

In a recent letter to me, Ulrich Majer has argued that Hermann Weyl was the first to formulate first-order logic, specifically in his book *Das Kontinuum* (1918). This is too strong a claim. I have already discussed Weyl's role in print (Moore 1980, 110-111; 1982, 260-61), but some further comments are called for here.

It is clear that Weyl (1918, 20-21) lets his quantifiers range only over objects (in his Fregean terminology) rather than concepts, and to this extent what he uses is first-order logic. But certain reservations must be made. For Weyl (1918, 19) takes the natural numbers as given, and has in mind something closer to ω-logic. Moreover, he rejects the unrestricted application of the Principle of the Excluded Middle in analysis (1918, 12), and hence he surely is not proposing *classical* first-order logic. Finally, one wonders about the interactions between Hilbert and Weyl during the crucial year 1917. What conversations about foundations took place between them in September 1917 when Hilbert lectured at Zurich and was preparing his 1917 course, while Weyl was finishing *Das Kontinuum* on a closely related subject?