STUDIES IN LOGIC, GRAMMAR AND RHETORIC 4 (17) 2001

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LOGICAL CONSEQUENCE AND THE LIMITS OF FIRST-ORDER LOGIC

1. Introduction

Logic as a formal mathematical theory is interesting in itself. It poses problems, like any other body of knowledge. Some of them, especially questions concerning axiomatization, consistency, completeness (in various senses of the term), decidability, etc. are more specific with respect to logic (and mathematics to some extent) than in the context of other theoretical systems. These questions are in principle independent of any application of logic to other branches of science. If logic is conceived in this manner, we speak about *logica docens*. On the other hand, logicians always claim that the main task of logic consists in governing intellectual activities. Thus, *logica utens* (logic in use) is deduction, indispensable device of mind, particularly in various reasonings. Leaving aside traditional prescriptions, usually considered as stemming from logic and helping us in processes of defining or classifying, the main aim of logical theory is to codify the rules of deductive proofs. These rules should be stated formally and effectively. In particular, checking whether a proof is correct or not should be subjected to mechanical or algorithmic procedures. However, the rules of deductive proofs must guarantee that they lead from true premises to true conclusions, that is, block deductive derivations of falsehoods from truth. We have here an important difference between deduction and induction. The label "correct deduction" is in fact pleonastic, unless it points out that a given deduction was more complex than the proof required, for instance, that is employs unnecessary premises or proceeds indirectly instead of directly. Yet too complicated deduction is still deduction, if any. Induction may be correct, despite starting with true premises and resulting with false conclusions, if its rules are preserved (of course, I am conscious that speaking about rules of induction is a delicate matter, but we think, for instance, that inductive

ISBN 83-89031-06-X ISSN 0860-150X

reasoning is correct if it was performed as carefully as possible). Incorrect induction is still induction, although incorrect deduction is not a deduction.

According to the contemporary view about logic, the codification of the rules of proof is a matter of syntax, although semantics investigates what it means that logical rules do not pass from true premises to false conclusions. On the other hand, we expect that syntax and semantics interplay in such a way that syntactically stated principles of inference have their semantic counterparts. To be more explicit, provability as a syntactic phenomenon displays the semantic fact that logical consequence is an operation that preserves truth. The question whether it is so or not, is known as the completeness problem (I will com back to this issue in section 5 below). Roughly speaking, a logic **LOG** is (semantically) complete if and only if the sets of its truths (logically valid sentences, tautologies) coincide with the set of its theorems (provable sentences). More strictly, the completeness problem consists in the question whether all tautologies are provable, and the problem whether provable sentences are tautological constitutes the soundness (adequacy) question. The latter is much easier to be solved, because it is sufficient to check whether axioms are tautologies, but the rules lead from tautologies to tautologies. The real completeness problem is a much more difficult topic. It is clear when we consider another formulation of the completeness theorem (equivalent with the formerly given), namely that every consistent set of sentences has a model. Since constructing models of theories is not a straightforward matter, it shows how complex the completeness problem can be.

It is quite understandable that logicians expected decidability of the methods of deductive proofs. At the first sight, it is even strange that things could be different. The concept of proof as formalized by our familiar logic, is recursive. Since any string of formulas can be mechanically checked whether it is a proof or not, it could well motivate the claim that the property of provability is decidable. Great philosophers or mathematicians, like Lebniz or Hibert were convinced about decidability of provability. This expectation was showed (Church, Turing) to be a dream. Thus even a semantically complete logic does not need to be decidable: although we can know that every tautology is provable, it does not imply that we have a mechanical procedure which tells us whether an arbitrary formula is provable or not. Things look differently in particular systems. Propositional calculus is semantically complete and decidable, but first-order predicate logic is semantically complete but undecidable. Going further, arithmetic is semantically incomplete (unless we employ a primitive trick consisting in taking all arithmetical truths as axioms) and undecidable.

Discovering theorems displaying various properties and limitations of formal systems became a great achievement of logicians of 20th century. So-called limitative theorems (Gödel – incompleteness of arithmetic, unprovability of consistency; Tarski – undefinability of truth; Church – undecidability of first-order quantification theory, undecidability of arithmatic; Skolem – the existence of non-standard models of first-order theories) threw a new light at formal systems and their properties. On the other hand, the crystal vision of logic as an instrumentarium of deduction, formerly propagated by people, like Frege, Russell or Hilbert, became obsolete. The question "What is logic?" appears much more complicated than the pioneers of logic were inclined to think. The problem is what we should preserve as essential properties of logic. Completeness?, decidability?, or something else, perhaps a considerable expressibility. It is an interesting philosophical question. I will try to exhibit some of its aspects by focusing on first-order logic and its properties as related to concepts of provability and logical consequence. Of course, it requires a comparison of first--order logic (**FOL**) with higher-order systems. One guiding idea directs my considerations. Although we are free to a great extent in adopting this or convention governing the use of the word "logic", it basically has nothing to do with properties of formal systems. They are complete or incomplete, decidable or undecidable, have a great expressive power or not, etc. entirely independently of our terminological decisions and inclinations concerning the usage of words. It should be remembered in any discussion about the nature of logic.

2. Some remarks about the main foundational projects of 20th century

The standard account of the history of mathematical logic is like that. Leibniz was a great forerunner of it, but his ideas were not understood and probably they could not be properly appreciated at the time. Then, Boole came and began the algebraic tradition in logic which was continued by people, like Peirce and Schröder. The genius of Frege changed everything. Firstly, Frege established the proper succession of logical systems, which starts with propositional calculus and goes to quantification theory. Secondly, he also projected and elaborated logicism in details, a project of *logica magna* (one of Leibniz's dreams), which could cover the whole of mathematics. Unfortunately, Frege's system was damaged by discovery of the Russell antinomy. Russell tried to save logicism by the theory of types.

However, this construction was not satisfactory even for Russell himself, who found some of its elements, like the axiom of reducibility, dubious. On the other hand, the theory of types disappointed other people not because of its details, but quite principally. Other foundational projects arose, namely Brouwer's intuitionism and Hilbert's formalism. All foundational projects had advantages and disadvantages. Logicism was either incomplete or based on artificial assumptions (the mentioned axiom of reducibility). Intuitionism cut classical mathematics too much. Formalism was promising but Gödel's and Church's results devastated it considerably.

I do not want to suggest that these facts did not happen. Also I do not underestimate works of Gödel, Tarski or Church, which brought the real revolution and done in the frameworks of *Principia Mathematica*. Yet the history of mathematical logic and the foundations of mathematics the above outlined scenario suggests, does not exhibit the whole truth. First of all, it overestimates the relevance of antinomies. What matters here is not only that some antinomies (the paradox of Burali-Forti) were earlier discovered than Russell's, but that the problem of the set of all set that are not elements of themselves was known to Zermelo and probably Hilbert, and did not alarm them. Zermelo's way out consisted in an axiomatization of set theory, which precluded dangerous sets. However, one can argue that, due to the common practice of axiomatizing of mathematical theories at the break of 19th and 20th century, set theory would be captured by an axiomatic system, even if no antinomies were discovered. Hilbert's case was even more explicit. His demand that consistency of mathematics should be effectively proved was explicitly articulated before Russell announced his famous paradox. The same concerns Hilbert's slogan that in mathematics there is no room for *non ignorabimus*. Finally, Brouwer's protest against epistemology of classical mathematics is also conceivable independently of antinomies. Of course, since antinomies appeared, all three great foundational projects (logicism, intuitionism an formalism) had to propose devices to avoid them, but, with exception of logicism, it was their secondary task.

Also, I think, that the consequences of Gödel's incompleteness theorems did not concern formalism only. Look at the definition of logic in Frege and Russell. Roughly speaking, it says that theorems of logic, including mathematics as reducible to logic, are provable by purely logical methods plus definitions in terms of a few very primitive concepts (in particular, the membership relation). Since the logicist identifies logical truths and logical theorems, the definition of logic says that all logical truths are provable by purely logical means. However, due to the first incompleteness theorem, it is impossible, because we have true sentences, but unprovable by logic (see Woleński 1995, 1995a).

The matter of intuitionism is perhaps less evident, but consider the following reasoning. The intuitionist demands that all mathematical theorems must be proved by constructive methods. Whatever it means, constructive methods have to avoid the principle of excluded middle. The second incompleteness theorem says that consistency of arithmetic is not provable in arithmetic itself. Now, it is reasonable to say that constructive methods should not go beyond arithmetic. Since Peano arithmetic is interpretable in Heyting arithmetic (Peano arithmetic formalized in logic without the principle of excluded middle via the Gödel translation: $\neg\neg A$ is a theorem of **HA** iff A is a theorem of **PA**; the symbol $\neg$ stands for the intuitionistic negation), the latter is also subjected to the second Gödel theorem. Now let CON(**HA**) abbreviate "**HA** is consistent". Due to the Gödel result, CON(**HA**) is not provable in **PA**. Should the intuitionist prove that **HA** is consistent? I say "yes". The intuitionist says that existence means consistency + the method of construction. Thus, consistency is a necessary, though not sufficient condition of existence in the intuitionistic sense, and this is a reason that the intuitionist should be able to prove that his basic theory, namely arithmetic, is consistent. Perhaps one will remark that something improper was smuggled in the above reasoning, namely that CON(**HA**) is to be provable by the intuitionist. In fact, if we inverse the link between **PA** and **HA**, we obtain that if A is not a theorem of **PA**, then $\neg\neg A$ is not a theorem of **HA**. Hence, $\neg\neg$CON(**HA**) is not a theorem of **HA**. The reasoning that leads to the result is of course classical. A possible counterattack of the intutionist is that we must distinguish $\neg\neg$CON(**HA**) and COH(**HA**). The former is too weak in order to capture the consistency of **HA** in the intuitionistic sense, but the latter also is defective as formalized by classical devices and it does not express the intuitive (intuitionistic) consistency of **HA**. We know, the intuitionist continues, that **HA** is consistent, because it is a true manisfestation of the Mathematician's Mind. I consider this way out as begging the question. It is really strange to say that consistency is a matter of intuitive faith, but everything else in mathematics is subjected to constructive proofs. The situation seems rather like this: either the intuitionist is not able to express CON(HA) in a rigorous way or he or she cannot constructively prove it (in fact, $\neg\neg$CON(**HA**) expresses something very close to the requirement of consistency; so if this formula is not intutionistically provable, a stronger one is unprovable too). No horn of this dilemma is nice.

Due to various circumstances, mainly works of Hilbert, Brouwer, Heyting, Gödel, Tarski and Skolem, the new situation in logic and the foundations of mathematics consisted in replacing old positions, heavily burdened by philosophical assumptions, by three new schemes: set theoretical (akin to logicism), proof theoretical (akin to formalism) and constructive (akin to intuitionism). I will take the first one as the point of reference. According to the set theoretical foundational project, mathematics is not reducible to logic, but to set theory. This brought the question concerning the limits of logic. Considering the further development, the question is to be reduced to another one: what is *the* logic? First-order or higher-order logic? If we add formalism to this business, we encounter another problem, namely the relation between proof and proof or, more generally, syntax and semantics. The Hilbert program was in fact based on a hope that all mathematical questions are solvable by finite (or combinatorial) syntactic methods. In Poland, due to Tarski, set theoretical methods resulted with the rise of rigorous semantics done by exact mathematical methods. I regard the Gödel incompleteness theorems and the Tarski undefinability theorem as signs of the limitations of syntax over semantics. These results so deeply changed the foundational scenario that we can properly speak about the semantic revolution (see Woleński 1999), which produced a new style of thinking in logic, the foundations of mathematics and philosophy. When we look at the interplay between syntax and semantics in formal theories, a natural question that arises is this: how to characterize constructions in which syntactic and semantic descriptions coincide? Perhaps we should answer that logic is the domain in which syntax and semantics are equivalent. What about constructivism? Well, it has its own merits because it is always good to know what the limitations of constructive methods are, that is, what can be constructively proved and what require other methods. However, constructivism is not directly involved in the problem of how syntax is related to semantics, because the latter is clearly non-constructive. So I will not touch constructivism in my further remarks. I will assume, somehow dogmatically, that constructive or effective procedures do not exceed primitive recursive arithmetic. This view seems to be a reasonable minimal understanding what should be included into the domain of constructive methods.

I take semantic revolution as being of the utmost importance. Yet, I understand other preferences, in particular pointing out that the most revolutionary work was given by Turing and consisted in elaborating the concept of computability. The reason for this view is obvious because of the significance of Turing's ideas for computer science which, their technological effects and their influence on philosophy, particularly the philosophy of mind. It is highly probable that logic in the 21^{th} century will be dominated by the needs of theoretical informatics. On the other hand, I am inclined to think that ideas advanced by Turing still belong to syntax. If I can prophet something, I will do it by saying that sooner or later the Tarski--style computer semantics will become equally important as it is in logic of the second half of 20^{th} century. Even if not, let us look at the semantic revolution as a historical fact.

3. The rise of first-order logic (see Moore 1980, Moore 1988)

Famous (and less famous) axiomatizations at the end of the 19^{th} century, namely Dedekind's (number theory), Peano's (number theory) and Hilbert's (geometry) were second-order, due to axioms, like induction (Dedekind, Peano) and completeness (of real numbers) (Hilbert). Frege's logic was also second-order, and the system of *Principia* covered what is presently known as ω-logic. Nobody at that time made any difference between first--and higher-order logic. The situation changed about 1915 when Löwenheim proved his famous result, later improved by Skolem, about satisfiability of first-order formulas in the domain of natural numbers. What is interesting in this context is that Löwenheim was strongly influenced by Schröder and the algebraic tradition in logic. So, at least in this respect, this tradition became something more that only a historical blind path of the development of logic. Another important fact, which helped to see the difference between first and second-order logic consisted in the change of formalization of set theory. Zermelo's system distinguishes sets and their elements in this way that there are some objects (atoms), which are not sets. It invites elementary quantification (over atoms) and non-elementary one (over sets, their families, etc.). Von Neumann recommended (in the twenties) another approach, based on the principle that everything is a set. This principle, somehow at odds with ordinary intuition, but natural in the realm of mathematics, enabled to formalize the set theory as an elementary (first--order) construction.

First-order system logic was consciously extracted from the whole body of logic by Hilbert (see Hilbert-Ackermann 1928, Hilbert 1928). It is sometimes said that Hilbert accepted the view that all mathematical theories can be formalized in first-order language (the Hilbert thesis; see Pogorzelski 1994, p. 170). Historically speaking, this view is false. Hilbert never said something like that. He only came to the conclusion that all deductive rules of inference can be formalized in first-order logic. It was the reason that he became interested in first-order logic and its properties. In particular, Hilbert stated as an open problem the question whether first--order logic is (semantically) complete. The positive answer given by Gödel in 1930 was an essential contribution to Hilbert problem because it assured that every universally valid formula of first-order logic was provable. Hilbert had to be very happy that a semantic concept (validity) was reducible to a syntactic one, namely provability. Partial successes (Bernays, Schöfinkel, Ackermann, Gödel, Behmann) in attempts to solve the decision problem also looked promising. However, the hopes concerning the final solution of the decision problem became destroyed after 1936, when Church proved the undecidability of first-order logic. Yet, elementary logic remained the most secure logical quantification system, due to its completeness and, in the course of time, it attracted many logicians (including Tarski and Gödel). Also the *opus magnum* of Hilbert and Bernays (see Hibbert-Bernays 1934-1939) seems to favour first-order logic for its "hard" properties, like consistency, completeness or effective syntax. However, I do not know any explicit statement of Hilbert, Gödel or Tarski suggesting that logic should be reduced to **FOL**. In general, every textbook of mathematical logic, published since the 40s of the 20th century extracts **FOL** as a separate and basic order logic. It was Quine (see Quine 1970) who made the claim that the logic should be identified with **FOL**. This claim, rather philosophical than purely logical, began to be extensively discussed in the last twenty five years (see Westerståhl 1976, papers in Barwise-Feferman 1985, Shapiro 1991, papers in Shapiro 1996).

4. The rise of metamathematics and formal semantics

It is self-understandable that the Hilbert program with its demands of solvability of every mathematical problem (including the consistency of mathematics) by finitary devices resulted with a vital interest concerning properties of formal mathematical systems. It does not mean that this question was entirely overlooked by other logicians. Russell and Whitehead clearly saw that consistency and completeness (adequacy) are fundamental properties of logical systems. This is well documented by the following fragment of *Principia Mathematica* (Whitehead-Russell 1910, p. 12).

> "The proof of a logical system is its adequacy and completeness. That is: (1) the system must embrace among its deductions all those propositions

which we believe to be true and capable of deduction from logical premisses alone, though possibly they may require some slight limitation in the form of an increased stringency of enunciation; and (2) the system must lead to no contradictions, namely by pursuing our inferences we must never be led to assert both p and not-p, i.e. both "$\vdash p$" and "$\vdash \neg p$" cannot legitimately appear."

However, the authors of *Principia Mathematica* did not think that consistency and completeness could be solved by applying exact mathematical methods. Post went further and proved formally (Post 1921) that propositional calculus was complete. He clearly saw that the problem of completeness is about propositional calculus but not a question, which could be solved within the system developed in *Principia Mathematica* (Post 1921, p. 21-22; page-reference to reprint):

"We here wish to emphasize that the theorems of this paper are *about* the logic of propositions but are *not included* therein. More particularly, whereas the propositions of 'Principia' are *particular* assertions introduced for their interest and usefulness in later portions of thr work, those of the present paper are about the set of *all* such possible assertions."

On the other hand, there is nothing in Post, which would testify that he saw the completeness problem as an example of a wider project of the foundational research. It was Hilbert who consciously and systematically initiated such a program. He established metamathematics as a mathematical field. More specifically, metamathematics in the Hilbertian sense consisted in investigating formal, that is, logical and mathematical systems by so called finitary methods.

Hilbert's idea of metamathematics became popular in Poland. In fact, the Warsaw school of logic (*Ł*ukasiewicz, Leśniewski, Tarski, Lindenbaum and others) were doing some metamathematical work in the twenties of the 20$^{\text{th}}$ century, probably independently of any knowledge about the Hilbert project. In the end of the 20s, Tarski published two papers (Tarski 1930, Tarski 1930a) in which he defined and systematised many metamathematical concepts with a full consciousness that he followed Hilbert and his school (Tarski 1930a, p. 60; page-reference to English translation):

"The deductive disciplines constitute the subject-matter of the *methodology of the deductive sciences*, which today, following Hilbert, is usually called *metamathematics*, in much sense in which spatial entities constitute the subject-matter of geometry and animals that of zoology. [...]. Strictly speaking metamathematics is not to be regarded as a single theory. For the purpose of invetigating each deductive discipline a special metadiscipline should be constructed. The aim is *to make precise the meaning of a series of important metamathematical concepts* which are common to the special metadisciplines, *and to establish the fundamental properties of these concepts*. One result of this approach is that some concepts which can be defined on the basis of special metadiscplines will here be regarded as primitive concepts and characterized by a series of axioms."

Tarski did not remark, however, that his work was fairly different from that of Hilbert at a very basic point. The main difference is that Tarski did not restrict metamathematical methods to finitary ones. A famous Lindenbaum's lemma, which appeared in Tarski 1930 for the first time, namely the statement that every consistent set of sentences has a maximally consistent extension is perhaps the best exemplification of a metamathematical and non-constructive theorem. Tarski, after years, stressed that it was a crucial point that Warsaw school was more liberal in the repertoire of sound mathematical methods as metamathematical devices (Tarski 1954, p. 713; page-reference to reprint):

"As an essential contribution of the Polish school to the development of metamathematics one can regard the fact that from the very beginning it admitted into mathematical research all fruitful methods, whether finitary or not. Restriction to finitary methods seems natural in certain parts of metamathematics, in particular in the discussion of consistency problems, though even here these methods may be inadequate. At present it seems certain, however, tha exclusive adherence to these methods would prove a great handicap in the development of metamathematics."

Why did Polish logicians (with some exceptions, like Chwistek or Leśniewski) admit infinitary methods? It was due to the way of looking at set theory and its controversial problems, for instance the status of the axiom of choice. Perhaps this way of thinking is best represented by two following quotations, very similar in their content, although separated by a few decades (Sierpiński 1965, p. 94; Tarski 1962, p. 124; page-reference to reprint; in order to clarify the phrase "separated by a few decades", I note that Sierpiński's view was expressed by him in the twenties of 20$^{\text{th}}$ century):

"Still, apart from our personal inclination to accept the axiom of choice, we must take into consideration, in any case, its role in the Set Theory and in the Calculus. On the other hand, since the axiom of choice has been questioned by some mathematicians, it is important to know which theorems are proved with its aid and to realize exact point at which the proof has been based on

> the axiom of choice; for it has frequently happened that various authors have made use of axiom of choice in their proof without being aware of it. And after all, even if no one questioned the axiom of choice, it would not be without interest to investigate which proofs are based on it, and which theorems can be proved without its aid – this, as we know, is also done with regard to other axioms."

> "We would of course fully dispose of all problems involved [that is, concerning the existence of inaccessible cardinals – J. W.], if we decide to enrich the exiom system of set theory by including (on a permanent basis so to speak) a statement which precludes the existence of "very large cardinals", e.g. by a statement to a effect that every cardinal $> \omega$ is strongly incompact. Such a decision, however, would be contrary to what is regarded by many as one of the main aims of research in the foundations of set theory, namely, the axiomatization of increasingly large segments of "Cantor's absolute'. Those who share these attitude are always ready to accept new "constructions principles", new axioms securing the existence of new classes of "large" cardinals (provided they appear to be consistent with old axioms), but are not prepared to accept any axioms precluding the existence of such cardinals – unless this is done on a strictly temporary basis, for the restricted purpose of facilitating the metamathematical discussion of some axiomatic system of set theory."

Metamathematics in the understanding of Hilbert and (early Tarski) was restricted to syntactic matters. If we say that metamathematics is "about" mathematics and deals with its subject-matter by mathematical methods, nothing prevents us to add formal semantics to metamathematical investigations. Adjective "formal" is important here, because only formal semantics is done by mathematical methods. Since I described the development of semantics elsewhere, I restrict here to the facts concerning formal semantics. Two conceptions of semantics have to be distinguished. The best way to see the difference consists in an appeal to Frege's distinction between sense and reference, although he did not invent it in order to clarify various ways of semantic thinking. Traditionaly, the linguists considered semantics as devoted to studies about meanings (senses) and their changes. This understanding attracted also many philosophers, who often worried about meanings of expressions. The further development of formal semantics, at least that important for mathematical logic, gave priority to referential issues.

The Löwenheim-Skolem theorem and the Gödel completeness theorem are early semantic results. Post probably did not observe that his completeness theorem had an explicit semantic flavour. Neither Löwenheim, nor Skolem, nor Gödel defined semantic concepts, which they used, for instance domain, validity or satisfaction. These ideas were understood in their writings as it was practicized in the ordinary mathematical parlance of that time. Gödel himself clearly appreciated the importance of semantic methods. After years, he documented it in the following way with respect to the concept of truth (quoted after Wang 1996, p. 242):

> "[...] it should be noted that the heuristic principle of my construction of undecidable number theoretical proposition in the formal systems of mathematics is the highly transfinite concept of 'objective mathematical truth', as opposed to demonstrability [...] which with it was generally confused before my own and Tarski's work. Again, the use of this transfinite concept eventually leads to finitary provable results, fo example, the general theorems about the existence of undecidable propositions in consistent formal systems. [...]. A similar remark applies to the concept of mathematical truth, where formalists considered formal demonstrability to be an *analysis* of the concept of mathematical truth and, therefore, were of course not in position to *distinguish* the two."

Gödel even accused Skolem for being insensitive to non-finitistic methods (in fact, to semantic matters) and thought that it prevented the latter to prove the completeness theorem (quoted after Wang 1974, p. 7-8):

> "The completeness theorem, mathematically, is indeed an almost trivial consequence of Skolem 1922. However, the fact is that, at that time, nobody (including Skolem himself drew this conclusion (neither from Skolem 1922 nor, as I did, from similar consideration of his own. [...] This blindness (or prejudice [...]) of logicians is indeed surprising. But I think the explanation is not hard to find. It lies in widespread lack, at that time, of the required epistemological attitute toward metamathematics and toward non-finitary reasoning."

Yet, as I already mentioned, Gödel himself also did not define semantic notions. Why? It seems that Gödel did not believe that they are subjected to rigorous mathematical treatment, although he always considered semantic intuitions as very powerful heuristic pathways.

It was Tarski who introduced semantics as a part of metamathematics in his seminal treatise on the concept of truth in formalized languages. The success of formal semantics was rooted in four factors. First, the fear of semantic antinomies had to be overcome. Perhaps it was more important in Poland than in other countries, because Polish logicians with their considerable philosophical inheritance were more sensitive to the problem of antinomies than their mostly mathematical colleagues in other countries. Secondly, a new conception of logic (see van Heijenoort 1967) had to replace the old one. This new conception considered logic as a reinterpretable calculus, contrary to the looking at logic as a language (as the universal medium; see Hintikka 1989, Kusch 1989, Woleński 1997a). The second conception, shared by Frege, to some extent by Russell and very radically by Wittgenstein, prevented any serious talk about relations between language and its referential relations to something else. In particular, it makes the distinction between language and metalanguage (crucial for semantics) simply meaningless. Quite contrary, logic conceived as reinterpretable calculus naturally suggested that language referred to something which was dependent on interpretation. Thirdly, language and its referential relations had to be dressed in a mathematical manner. It was done by recursive definition of language as a set of sentences and assuming that the concepts of interpretation and satisfaction, which establish the link between language and its subject matter, are also recursive. Thus, syntax and semantics became compositional. Sometimes it is regarded as a too restrictive approach, which does not fit intensional language, but the way of overcoming meanders of intensionality is unclear until now. Fourthly, semantics required infinitary methods. It is remarkable that the first mention of satisfiability by Tarski appeared in his paper on definable real numbers, that is, on the occasion of considering problems of descriptive set theory, which makes heavy use of infinitary methods. If we look at circumstances associated with the development of formal semantics, nothing is peculiar as compared with syntax. Both semantics and syntax use infinitary methods and both are compositional. What is then the difference between them? The fundamental question is this: is it possible to define semantic relations by syntactic machinery? The general answer, well motivated by basic metamathematical results, is: no (see Woleński 1997 for further philosophical comment about the relation between syntax and semantics). I claim that **FOL** is the only logic in which syntax and semantic are parallel in the way which can be called "logical", but this qualification must be somehow restricted even in this case.

5. First-order logic and its basic properties

There is an ambiguity in conceiving logic, even in the case of **FOL**. A more traditional account considers logic as the set of theorems derived from suitably adapted axioms by proper inference rules, for instance, modus ponens. According to another view, logic is a pair $\langle \boldsymbol{L}, Cn \rangle$, where $\boldsymbol{L}$ is a language and Cn is a consequence operation, which operates on $\boldsymbol{L}$. Under the second understanding, logic produces theorems from some assumptions dependent on the considered subject matter, for example, arithmetic or geography. On the other hand, logic as the set of theorems consists of propositions assumed to be tautologies, at least in the case of first-order logic. We can try to reconcile (at least, to some extent) both approaches to logic by the following way. Firstly, we introduce (first--order) Cn axiomatically by stipulating the following postulates (this way of introducing the consequence operation goes back to Tarski; see Tarski 1930, Tarski 1930a):

(C1) $\emptyset \leq \boldsymbol{L} \leq \aleph_0$

(C2) $X \subseteq CnX$

(C3) if $X \subseteq Y$, then $CnX \subseteq CnY$

(C4) $Cn\,CnX = CnX$

(C5) if $A \in CnX$, then $\exists Y \subseteq X \wedge Y \in \mathbf{FIN}(A \in CnY)$

(C6) if $B \in Cn(X \cup \{A\})$, then $(A \to B) \in CnX$, provided that A, B and element of X are sentences, that is, closed formulas.

(C7) if $(A \to B) \in CnX$, then $B \in Cn(X \cup \{A\})$

(C8) $Cn\{A, \neg A\} = \boldsymbol{L}$

(C9) $Cn\{A\} \cap Cn\{\neg A\} = \emptyset$

(C10) $A(t_i/x_i) \in Cn\{\forall x_i A\}$, if the term t_i is substitutable in A for x_i;

(C11) $(A \to \forall x_i B) \in Cn\{\forall x_i(A \to B)\}$, if x_i is not free in A;

(C12) $\forall x_i A \in Cn\{A\}$.

Then, we define logic by

(D) $\mathbf{LOG} = Cn\emptyset$.

I will not enter into a deper motivation for defining logic as the set of consequences of the empty set (see Woleński 1998 for a more extensive discussion discussion). At the moment the observation will suffice that (D) sees logic as independent of any particular assumptions, that is, connected with specific domains. In the case of first-order logic, (D) has an additional justification in the (weak) completeness theorem

(WCT) $A \in Cn\emptyset$ if and only if A is universally valid, that is true in all models.

Thus, assuming (D), the completeness theorem establishes the parity between derivability from the empty set and the universal validity. Since universality was always conceived as a basic property of logic, it gives a strong evidence for (D) as an intuitive definition of logic. The strong completeness theorem is important, when we understand logic as $\langle \boldsymbol{L}, Cn\rangle$. It is the statement

(SCT) $A \in CnX$ if and only if A is true in all models of the set X of sentences.

Of course, if we take the empty set instead of X, (SCT) becomes (WCT). Unfortunately, completeness does not select a logic uniquely, because it is a property of many formal systems. It is always possible to state axioms for Cn, that it will allow us to define the resulting systems as Cn. Some such systems are semantically complete, others not. Thus, semantic completeness as a property does not separate first-order logic from other candidates for being logics. Thus, we must look for an other characterization of first-order logic. One hint comes from the following theorem:

(NDC) **FOL** does not distinguish any extralogical content, that is, if something is provable in this logic about an individual object, property or relation denoted by a predicate letter, the same is also provable about any other object, property or relation.

This feature is certainly desirable, if we like to keep the intuition that logic is independent of specific subject-matters.

The recent and most popular characterization of first-order logic comes from the Lindström theorem (more strictly, one of theorems of this sort; note that this theorem applies above all to logic understood as $\langle \mathbf{B}, Cn\rangle$).

(L) A logic **LOG** is equivalent to **FOL** if and only if the following conditions hold:
- (a) **LOG** is effectively regular (its syntax is recursive, its formulas are finite);
- (b) **LOG** has Boolean connectives;
- (c) **LOG** is compact (it has a model if its every finite subset has a model);
- (d) **LOG** satisfies the downward Löwenheim-Skolem theorem (if it has an infinite model, it has a countable model).

This theorem says that **FOL** is the strongest logic, which jointly possesses properties (La)–(Ld). For instance, second-order logic is neither compact nor satisfies the Löwenheim-Skolem theorem

(L) is in its main part a semantic theorem and it has no clear syntactic counterpart, although the regularity of **FOL** and the Boolean character of its connectives are guaranteed by properties of Cn. If we have the completeness theorem, compactness as related to models, can be mapped on the finiteness of syntactic consistency. However, no simple syntactic counterpart of the downward Löwenheim-Skolem theorem is available. What is known is the Lindenbaum maximalization theorem for languages with uncountably many constants is equivalent to the downward Löwenheim-Skolem (see Surma 1968) theorem, but it goes beyond syntax of **FOL**. (L) is surprising, because it characterizes **FOL** by unexpected properties as basic. A more discussed matter concerns the consequences of (L) for the expressive power of **FOL**. Its compactness and (d) decide that many concepts important for mathematics, for instance, finiteness, are simply not definable in first-order languages. Moreover, first-order theories are automatically non-categorical, because, due to the Löwenheim-Skolem theorem, they have models, which differ in cardinalities. These properties of **FOL**, in particular, the limitations of its expressive power, are commonly regarded as disadvantages of first--order logic and reasons for favouring higher-order logics or logic with infinitely long formulas (see Shapiro 1991 for the first option and Barwise and Feferman 1985 for more wide spectrum of possibilities).

I have some reservations toward criticism of **FOL** via (L). Although I agree that (L) is surprising, I guess (similarly Tharp 1975) that properties listed in (L) are important for understanding logic and they clarify some controversial issue. (L) does not mention the completeness theorem which seems crucial for the concept of logic. However, we can say that (L) shows that (WCT), if it is considered as a supplement of (D), must be understood in a way. More specifically, since the completeness phenomenon is much more general than the related property of **FOL**, and it is too wide as a mark of logic, (L), so to speak, restricts (WCT) to a proper shape. Let me explain the point in such a way. Compactness and the downward Löwenheim-Skolem theorem are consequences of the completeness theorem (in the version: every consistent set of first-order formulas has a model), but only in **FOL**. It is clear, because second-order logic is complete but it does not satisfy conditions (Lc) and (Ld). It suggests that completeness in second-order logic means something different than in the case of **FOL**. Formally, everything is similar: every consistent set of second-order formulas has a model. On the other hand, it is well-known that the Henkin proof of the completeness theorem for second-order logic, introduces some distinction in the class of all models. Thus, not every model of second-order logic is treated in the same way. Putting it in another way, extralogical elements are present in selecting second-order models. This fact, together with the lack of recursive axiomatization of second-order modes of inference (I mentioned it earlier), shows that second-order (a fortiori, higher-order too) logic has feature which are not proper for logic. Nothing like that occurs in the case of **FOL**. In particular, the Löwenheim-Skolem theorem says that all models of a first-order language are equal, independently of their cardinality. Hence, the definition of logic as the set of sentences true in all models and its equivalence with (D), has its full sense only in **FOL**. (WCT), (NDC) and (L) collectively taken exhibit fundamental properties of fist-order logic, in particular, contribute to a better understanding of its universality. If we insist that logic, should be universal, **FOL** satisfies this requirement to the greatest extent.

6. Final remarks

First-order logic certainly has serious expressive limitations. On the other hand, second-order logic, although richer in content, does not admit any recursive definition of its deductive machinery. Although the concept of logical (semantic) consequence is the same in all types of logic, it is not everywhere replaceable by effectively given proof-procedures. Now, one must decide what is expected from logic: a powerful expressive power or recursive production of the modes of inference, closely connected with the strict parallelism of syntax and semantics. The moral from my discussion is that both aims cannot be simultaneously achieved. Something must be chosen. As I already indicated, the strict parallelism of syntax and semantics is restricted even in the case of **FOL**. It is due to the fact that the completeness theorem for first-order logic, contrary to propositional calculus has no constructive proof. Thus, effective syntax is mapped into non-effective semantics and reversely, due to the use of methods, which are not constructive. Another point to be observed is that **FOL** has some extralogical aspects, at least two. Firstly, it is based on an assumption that something exists. Secondly, the identity predicate is somehow between logical and extralogical notions. On the one hand, first-order logic with identity obeys all principal metatheorems, which hold for **FOL** without identity, but, on the other hand, identity allows us to define numerical quantifiers (for example, "there are exactly two objects"), which are purely not logical items. I only note these points without entering into a deeper discussion of them.

Bibliography

Barwise J. – Feferman, S. 1985, *Model-Theoretic Logics*, edited by J. Barwise and S. Feferman, Springer Verlag, Berlin.

Hilbert, D. 1929, "Probleme der Grundlegung der Mathematik", in *Atti del Congresso internationale dei matematici, Bologna 3-10 settembre 1928*, Bologna 1929; reprinted in D. Hilbert, *Grundlagen der Geometrie*, 7th edition, Verlag und Druck, B. G. Teubner, Leipzig 1930, pp. 313-323.

Hilbert D. – Ackermann W. 1928, *Grundzüge der theoretischen Logik*, Verlag von Julius Springer, Berlin.

Hilbert, D. – Bernays, P. 1934-1939, *Grundlagen der Mathematik*, 2 vls., Springer, Berlin; 2nd ed., Springer Verlag, Berlin 1968-1970.

Hintikka, J. 1989, "Is Truth Ineffable?", in *Les Formes Actuelles du Vrai: Entretiens de Palermo*, edited by N. Scardonna, Enchiridion, Palermo, pp. 89-120; reprinted in J. Hintikka, *Lingua Universalis vs. Calculus Ratiocinator. An Ultimate Presupposition of Twentieth-Century Philosophy* (*Selected Papers*, v. 2), Kluwer Academic Publishers, Dordrecht 1997, pp. 20-45.

Kusch, M. 1989, *Language as Calculus vs. Language as Universal Medium. A Study in Husserl, Heidegger and Gadamer*, Kluwer Academic Publishers, Dordrecht.

Moore, G. H., 1980, "Beyond First-Order Logic: The Historical Interplay between Mathematical Logic and Set Theory", *History and Philosophy of Logic* 1, pp. 95-137; reprinted in Shapiro 1996, pp. 3-45.

Moore, G. H. 1988, "The Emergence of First-Order Logic", in *History and Philosophy of Modern Mathematics* (Minnesota Studies in the Philosophy of Science, v. XI), edited by W. Aspray and Ph. Kitcher, University Press of Minnesota, Minneapolis, pp. 95-139.

Pogorzelski, W. 1994, *Notions and Theorems of Elementary Formal Logic*, Warsaw University – Białystok Branch, Białystok.

Post, E. 1921, "Introduction to a General Theory of Elementary Propositions", *American Journal of Mathematics* 43, pp. 163-183; reprinted in E. Post, *Solvability, Provability, Definability. The Collected Works of Emil L. Post*, edited by M. Davis, Birkhäuser Verlag, Basel, pp. 21-43.

Quine W. V. 1970, *Philosophy of Logic*, Prentice-Hall, Englewood Clifts; 2nd edition, Harvard University Press, Cambridge, Mass. 1986.

Shapiro, S. 1991, *Foundations without Foundationalism. A Case for Second--Order Logic*, Clarendon Press, Oxford.

Shapiro, S. 1996, *The Limits of Logic*, edited by S. Shapiro, Dartmouth, Aldershot.

Sierpiński, W. 1965, *Cardinal and Ordinal Numbers*, PWN – Polish Scientific Publishers, Warszawa.

Skolem, Th. 1922, "Einige Bemerkungen zur axiomatische Begründung der Mengenlehre", in *Proceedings of the 5th Scandinavian Mathematical Congress*, Helsinki 1922, pp. 217-232; reprinted in Th. Skolem, *Selected Works in Logic*, edited by J. E. Fenstad, Universitetsforlaget, Oslo 1970, pp. 137-152; English translation (by S. Bauer-Mengelberg in *From Frege to Gödel.* A Source Book in Mathematical Logic, 1879-1931, edited by J. van Heijenoort, Harvard University Press, Cambridge, Mass. 1967, pp. 290-301.

Surma, S. J. 1968, "Some Metamathematical Equivalents of the Axiom of Choice", *Prace z Logiki* 3, pp. 71-79.

Tarski, A. 1930, "Über einige fundamentale der Metamathematik", *Sprawozdania z Posiedzeń Towarzystwa Naukowego Warszawskiego* 23, 22-29; reprinted in Tarski 186, v. 1, pp. 313-320; English translation (by J. H. Wodger) "On Some Fundamental Concepts of Metamathematics", in Tarski 1956, pp. 30-37.

Tarski, A. 1930a, "Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften I", *Monatshefte für Mathematik und Physik* 37, pp. 361-304; reprinted in Tarski 1986, v. 1, pp. 347-390; English translation (by J. H. Woodger) "Fundamental Concepts of the Methodology of the Deductive Sciences", in Tarski 1956, pp. 60-109.

Tarski, A. 1954, [A Contribution to Discussion after the talk of P. Bernays, "Zur Beurteilung der Situation in der beweistheoretischen Forschung"], *Revue Internationale de Philosophie* 8, pp. 16-20; reprinted in Tarski 1986, v. 4, pp. 713-714.

Tarski, A. 1956, *Logic, Semantics, Metamathematics. Papers from 1923 to 1938*, Clarendon Press, Oxford; 2nd edition (edited by J. Corcoran), Hackett Publishing Company, Indianapolis 1983.

Tarski, A. 1962, "Some Problems and Results Relevant to the Foundations of Set Theory", in *Logic, Methodology and Philosophy of Science. Proceedings of the 1960 International Congress*, edited by E. Nagel, P. Suppes and A. Tarski, Stanford University Press, Stanford, pp. 125-135; reprinted in Tarski 1986, v. 4, pp. 115-125.

Tarski, A. 1986, *Collected Papers*, 4 vls., edited by S. R. Givant and R. McKenzie, Birkhäuser Verlag, Basel.

Tharp, L. 1975, "Which Logic is the Right Logic?", *Synthese* 31, pp. 1-21; reprinted in Shapiro 1996, pp. 47-67.

van Heijenoort, J. 1967, "Logic as Calculus and Logic as Language", *Synthese* 17, pp. 324-330; reprinted in J. van Heijenoort, *Selected Essays*, Bibliopolis, Naples 1985, pp. 11-16.

Wang, H. 1996, *A Logical Journey. From Gödel to Philosophy*, The MIT Press, Cambridge, Mass.

Westerståhl, D. 1976, *Some Philosophical Aspects of Abstract Model Theory*, Institutionen för Filosofi, Göteborh Universitet, Gothenburg.

Whitehead A. N. – Russell, B. 1910, *Principia Mathematica*, v. 1, Cambridge University Press, Cambridge; 2nd Ed., Cambridge University Press, Cambridge 1925.

Woleński, J. 1995, "Logicism and the Concept of Logic", in *Mathematik und Logik. Frege Kolloquium 1993*, edited by I. Max and W. Stelzner, Walter de Gruyter, Berlin, pp. 111-119.

Woleński, J. 1995a, "Logic and Mathematics", in *The Foundational Debate. Constructivity in Mathematics and Physics*, edited by W. De Pauli-Schimanovich, E. Köhler and F. Stadler, Kluwer Academic Publishers, Dordrecht, pp. 197-210.

Woleński, J. 1997, "Semantics as Transcendent(al)", in *Logica '96. Proceedings of the 10th International Symposium*, edited by T. Childers, P. Kolár and V. Svoboda, Filosofia, Praha, pp. 284-290.

Woleński, J. 1997a, "Husserl and the Development of Formal Semantics", *Philosophia Scientiae* l.4, pp. 151-158; reprinted in J. Woleński, *Essays in the History of Logic and Logical Philosophy*, Jagiellonian University Press, Kraków 1999, pp. 110-114.

Woleński, J. 1998, "Logic from a Metalogical Point of View", in *Logic at Work Essays Dedicated to the Memory of Helena Rasiowa*, edited by E. Orlowska, Physica-Verlag A Springer-Verlag Company, Berlin, pp. 25-35.

Woleński, J. 1999, "Semantic Revolution: Rudolf Carnap, Kurt Gödel, Alfred Tarski", in *Alfred Tarski and the Vienna Circle*, edited by J. Woleński and E. Köhler, Kluwer Academic Publishers, Dordrecht, pp. 1-15.

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