TARSKI AND LEŚNIEWSKI ON LANGUAGES WITH MEANING VERSUS LANGUAGES WITHOUT USE

*A 60th Birthday Provocation for Jan Woleński**

Göran Sundholm
Leyden University

It is a moot question whether Jan Woleński himself knows how many articles he has written. 300? 350? It is, however, a fact true and certain that a considerable part of these articles deals with the ins and out of the history of Polish logic. I have not digested all of his *oeuvre*, but I have read quite a lot. In my opinion, Jan Woleński has written no finer article than

'Mathematical Logic in Poland 1900–1939: People, Circles, Institutions.'[1]

It constitutes a harmonious blend of historical analysis, novel archival material, and criticism, the whole being spiced with his own special brand of nationalistic Polish propaganda; the resulting mixture is very attractive indeed. In particular, we get novel insights concerning the complex relationship between two of the giants of the Lvov-Warsaw school, to wit Stanisław Leśniewski (26/3 1886–13/5 1939) and Alfred Tarski (14/1 1901–27/10 1983).[2] This was not the first time that Woleński commented on these matters: in the (sub-)section 'Leśniewski and Tarski' of his joint paper with Peter Simons *De Veritate* we read:

> [T]he mutual acknowledgements which Tarski and Leśniewski make to one another in their works show a reserve and carefulness of expression, which seem to go beyond even Polish standards of formal courtesy, and suggests a certain prickliness in their personal and professional relationship.[3]

*I am indebted to *dottoressa* Arianna Betti, of Genoa University, and presently EU Huygens Fellow at Leyden, for help with Polish source material, as well as to Dr. M. van Atten, Utrecht University, who came to my aid in tracking the original German text of Tarski (1930). (These affiliations held in 2000 when the present paper was written.)

J. Hintikka, T. Czarnecki, K. Kijania-Placek, T. Placek and A. Rojszczak†(eds.),
Philosophy and Logic. In Search of the Polish Tradition, 109–128.

The later paper provides material that puts into perspective and makes understandable this very prickliness.

About a decade ago I treated of the use of expressions in material supposition versus metamathematical naming, and, in the course of so doing, I had occasion to note the Tarski-Leśniewski *contretemps*.[4] My remarks on that occasion, though, went unnoticed, perhaps not unreasonably so, owing to the fact that they were buried deep inside a paper ostensibly dealing with Wittgenstein's *Tractatus*.[5] The matter is, however, not without some general interest for the philosophy of logic, and, in particular, the history of Polish logic.[6] Since the topic is one that might not be without its attraction for my friend Jan Woleński, and my views do not completely coincide with his, I am happy to avail myself of the present opportunity to return to the matter once again, and now in his honour.

1. Logician's Obligations

The turn of the decade 1930–31 constitutes a definite watershed in the development of modern logic from Frege onwards. Until then the foremost task of any logician worth his salt was to design a formal system that was adequate to the needs of, say, at least, mathematical analysis. This, however, was not all. The formal system must not be just a formal *Spielerei* in the sense of Frege's Jena colleague Johannes Thomae. On the contrary, it must be an *interpreted* formal system, where the primitive notions have been given careful meaning explanations, in such a way that its axioms, and primitive modes of inference, are thereby made intuitively evident, without further deductive ado.[7] It must be stressed that the immediacy in question is conceptual, but need not be temporal at all. On the contrary, it can be quite arduous to obtain the insight that a judgement is axiomatic, or that a mode of inference is immediate, and so 'eines Beweises weder fähig noch bedürftig'.[8] When such a judgement (or inference) is evident, it has to be *self*-evident, not, of course, in the sense that it should be "obvious" or "trivial", but in the sense that its evidence rests upon nothing else than what is available in the formulation of the judgement (or inference) in question. Reflection on the conceptual resources used is the only means for obtaining the insight in question, and, dependent upon the matter at hand, this might be quite hard a nut to crack.

Frege retained this Aristotelian conception of axioms and proofs, in spite of his having abandoned a great deal or even most of Aristotelian logic.[9] He certainly overturned the traditional account of logical consequence—what follows from what—by replacing the "subject copula predicate" [S is P] logical form, with his own mathematized "function applied to argument" [$P(a)$] form; this novelty allowed for the analysis of a much richer variety of inferences and the traditional pattern simply faded out. In his theory of proof, or perhaps better, of *demonstration*, though, Frege did not depart from the Aristotelian paradigm of the *Posterior Analytics*, as witnessed by the Preface to *Begriffsschrift* and the early paragraphs of the *Grundlagen* that contain the (essentially Aristotelian) conditions on a proper *Begründung*. In the opening sections of his *Grundgesetze der Arithemetik*, Bd. I, Frege sets out the basic notions of the revised conceptual notation and attempts to lay down their meaning in such a way that every regular Name of his formal language will refer and every derivable thesis of the formal system will be a Name of *das Wahre*, that is, the common "truth-value" of all true propositions. The enterprise culminates in the §§ 29–31 that were intended to provide a secure foundation for the logical derivation of the mathematical laws that largely occupies the rest of the work. Alas, in spite of the ingenuity that Frege showed in his variations on the Aristotelian theme, he was ultimately shipwrecked on the rock of Russell's paradox. Thus, something must be wrong in the elaborate details of the attempted proof of referentiality in §31.

The next attempt in the grand tradition, namely Peano's *Formulaire*, must, with the benefit of hindsight, be considered a non-starter. Peano was a great, possibly unsurpassed, designer of logical notation, but lack of philosophical sophistication and semantical acumen mars his work and makes it unfit for foundational service at the highest level.

In comparison with Peano, the *Principia Mathematica* of Whitehead and Russell, while still confirming to the Aristotelian foundationalist pattern, is considerably more successful, both conceptually and in terms of impact. Indeed, Peano's influence, to a very large extent was transmitted through their more or less wholesale take-over of Peanesque notation. In some respects, though, the *Principia Mathematica* constitutes a retrograde step in comparison with Frege's previous attempt.[10] Its syntactic deficiencies were so great that volume II had to open with a 'Prefatory Statement of Symbolic Conventions', written by Whitehead, in order to restore some of the damage wrought in Volume I.[11] Concerning the semantical level, on the other hand, the authors were explicitly aware that three of their "axioms" so-called had not been given an appropriate underpinning in terms of meaning-explanations, to wit those of Reducibility and Infinity, as well as Zermelo's controversial Axiom of Choice or, in their terms, the "Multiplicative Axiom". One of Wittgenstein's avowed aims in the Tractatus was to improve upon the "old logic" of Frege and Russell. To this end he did supply a novel semantics, with a concomitant notion of proposition, which, he held, could achieve their principal aims, when put into proper perspective. Also the work of Frank Plumpton Ramsey falls squarely within this British tradition of emending the *Principia Mathematica*.

Two rival movements challenged the supremacy of the Frege-Russell—"logicist"—version of the Aristotelian foundationalist paradigm. First there was the fairly local, Amsterdam-centred intuitionism of Brouwer that, until now, had not been cast into a formalized mould, even though some of its criticisms of the classical means of procedure were clearly concerned with content. Indeed, the unrestricted use of the law of excluded middle constitutes a case of empty formalism that does not provide for clear content in the theorems proved nor does it ensure that the corresponding constructions can be executed. At Göttingen, on the other hand, the school gathered around Hilbert attempted to supply the (meta)mathematical details required for a mathematical counterpart to positivistic instrumentalism in science:

The verifiable consequences check out.

This is where uninterpreted formal languages made their triumphant entry into foundational studies. Indeed, it was Hilbert's discovery that "real" theorems with verifiable content, that is, in the present case, theorems concerning (free-variable) equations between simple computable functions, when established with the aid of "*ideal*" non-verifiable, but *verifiably consistent* means, could also be established without such means. In other words, the positivist slogan means that the ideal should be "conservative" over the real. In such a fashion, then, even after the onslaught of Frege, the formalist school of Thomae and others was given the opportunity of a second innings at Göttingen. In particular, the problem of content that so beset the logicists with respect to the controversial three "axioms" is elegantly side-stepped. At the level of content Hilbert deals only with propositions that are verifiable, which those axioms are not. Accordingly, they will face the tribunal of content only mediately via the demand for verifiable consistency. Hilbert rejects the unrestricted foundationalist demand for individual content through meaning-explanations, and confines it solely to verifiable propositions. In this fashion a foundation for the standard practice of classical mathematics would nevertheless be secured, since all its verifiable consequences would, indeed, verifiably check out.

But for this mainstream trinity of logicism, intuitionism and formalism, also lesser deities had joined the fray. Even at the Göttingen Helicon, a stronghold of Hilbertian formalism, deviant voices were heard, for instance that of Moses Schönfinkel, a Russian émigré who devised a variable-free logical calculus that was later taken up, in various guises, by other Göttingen students, to wit the Americans Haskell B. Curry and Alonzo Church.

Of these, Curry, for sure, shared the formalist inclinations of the Göttingen school. His (1951) *Outlines* that were written already in 1939 surpass even Hilbert in their formalist ardour and take an even more severe stand on content than did their Göttingen predecessors. Church, on the other hand, belongs firmly in the opposite camp. The fruits of his labour as a Post-Doctoral Fellow bears the telling title *A Set of Postulates for the Foundation of Logic* [my underlining G. S.]. In Church's work, an unequivocally foundationalist stance is clearly visible, even at the height of the metamathematical era.

The unsurpassed sixty-page introduction to his *Introduction to Mathematical Logic* from (1956) reads like a wistful longing back to the long gone, pre-metamathematical days of logic a quarter of a century earlier when proof *in* a system, rather than proof about a system, still held sway.

The rumblings of the coming revolution were faintly heard. In response to a prize question that was posed by the Dutch *Wiskundig Genootschap*, Arend Heyting (1930), a few years earlier one of Brouwer 's few doctoral students, and now a secondary school-teacher at Enschede, offered an explicit formulation of *intuitionistic* laws of logic. However, even though his logic was formalized, Heyting did not go metamathematical, but sided with content. In other papers from the same time he and Kolmogoroff canvassed a notion of proposition that made evident the axioms and laws of inference of Heyting's formalism.[12] Thoralf Skolem had returned yet again to the famous—*metamathematical!*—theorem that now bears his name coupled with that of Löwenheim. The standard text-book of Hilbert and Ackermann, which inaugurates the metamathematical era in logic, appeared in 1928, and a year later Carnap's *Abriß*, which, however, was still looking back towards interpreted formal systems in use. Zermelo (1930) put set theory of a much firmer footing by providing (more or less natural) models for the system that now bears his name and that of Fraenkel. In the works of Gödel, finally, metamathematics came of an age, and, through the superb craftsmanship of Paul Bernays, its early results received a fitting codification in the monumental Hilbert-Bernays (1934–1939).

The formal systems that constitute the bread and butter of the logician's steady fare no longer provide tools for research. Instead they are converted into the very objects of foundational study. In particular, the formal languages of metamathematics are no longer languages in use, whether actual or potential, but *are designed for mention only*. Metamathematical expressions so-called do not express anything, but, on the contrary, they are expressed using real expressions. This distinction, between formal languages with meaning for foundational use versus formal "languages" for metamathematical study only, is clearly related to the van Heijenoort-Hintikka distinction between Logic as Language versus Logic as Calculus/Language as the Universal Medium versus Language as Calculus.[13] From the point of view of content, this distinction, between language for use versus language for mention only, might be said to capture the core of the van Heijenoort-Hintikka distinction. A metamathematician, with his different perspective, would perhaps not agree. I, for one, am happy to applaud, and join, Jaakko Hintikka's continuing efforts in stressing the *importance* of the Van Heijenoort-Hintikka distinction as a basic absolute presupposition in the development of twentieth century logic. We would, however, choose different sides of the dichotomy: I opt for Logic as Language and Hintikka, surely, would opt for Logic as Calculus. I strongly suspect that also Jan Woleński, as staunch an advocate as any of the accomplishments of the Polish metalogical school, would join him in that choice.

From now on, say after the advent of recursion theory in 1936, but certainly after World War II, it is possible to be a mathematical logician *without any foundational interest or motivation*. The main branches of mathematical logic do not any longer contribute to foundational study at all, but have become, more or less mainstream, straightforward mathematics. Model theory and recursion theory, in particular, speedily became autonomous branches of mathematics.

Formal languages lacking content entered the foundations of mathematics only through Hilbert's philosophical preconceptions: he wanted to secure (the practice of) classical mathematics precisely by side-stepping content, without having to bother about detailed justification in terms of meaning-explanations for the individual expressions of his "languages". Kurt Gödel, was able to refute the Hilbert's philosophy decisively, by means of mathematical proof.[14] This he did by taking the idea that the (meta)mathematical expressions are objects of mathematical study literally in a strict sense: notoriously, he even converted the expressions into numbers, the most prototypical of mathematical objects.[15] In the course of his epoch-making work, Gödel created such interesting mathematics, that the concomitant philosophical disaster was forgotten, or perhaps not even noticed. As a consequence, the bandwagon of mathematical logic, metamathematically construed, rolled on ever further, with the result that even today, seventy years later, the languages without content are still with us, in spite of the fact that their *raison d'être* was obviated virtually at the outset. Recently, though, computer science has brought back interpreted languages into focus: after all, programming languages will not serve their purpose without proper interpretation.

2. Probing Principal Problems of Polish Prickliness

The complex relationship between Stanisław Leśniewski and Alfred Tarski cannot be properly evaluated without a background awareness of the tension between the above two paradigms, namely, the logic-in-use tradition of Frege and others, and the metamathematical tradition of Hilbert and others. Leśniewski, the older man by some fifteen years, began his research directly before World War I, and was very much established in the former paradigm. Indeed, during the golden age of logic, it is arguable, the foundationalist standpoint in logic received no better formulation than in the works of Stanisław Leśniewski. By background and training, he was a philosopher, and it was reflection on philosophical themes that provided the main impetus for his work. Today it is by far too little known.[16] The supremacy of first-order predicate calculus, with the ensuing (metamathematical) model-theoretic semantics, has eclipsed completely the logical virtues for which he stood. Leśniewski gave a powerful, albeit longwinded, formulation of a foundationalist perspective in the peroration to a high-profile, international presentation of his work:

> Da ich keine Vorliebe für verschiedene "Mathematikspiele" habe, welche darin bestehen, daß man nach diesen oder jenen konventionellen Regeln verschiedene mehr oder minder malerische Formeln aufschreibt, die nicht notwendig sinnvoll zu sein brauchen oder auch sogar, wie es einige der "Mathematikspieler" lieber haben möchten, notwendig sinnlos sein sollen,—hätte ich mir nicht die Mühe der Systematisierung und der vielmaligen skrupulösen Kontrollierung der Direktiven meines Systems gegeben, wenn ich nicht in die Thesen dieses Systems eienen gewissen ganz bestimmten, eben diesen und nicht einen anderen, Sinn legen würde, bei dem für mich die Axiome des Systems [...] eine unwiderstehliche intuitive Geltung haben. Ich sähe keinen Widerspruch darin, [...], daß ich eben deshalb beim Aufbau meines Systems einen ziemlich radikalen "Formalismus" treibe, weil ich ein verstockter "Intuitionist" bin: indem ich mich beim Darstellen von verschiedenen deduktiven Theorien bemühe, in einer Reihe sinnvoller Sätze eine Reihe von Gedanken auszudrücken, [...], welche ich "intuitiv" für mich bindend betrachte, kenne ich keine wirksamere Methode, den Leser mit meinen "logischen Intuitionen" bekannt zu machen, als die Methode der "Formalisierung" der darzulegenden deduktiven Theorien, die jedoch keineswegs unter dem Einfluss solch einer "Formalisierung" aufhören, *aus lauter sinnvollen Sätzen zu bestehen, welche für mich intiutive Geltung* haben.[17]

Alfred Tarski, on the other hand, was a mathematician, and a brilliant one at that, with prominent results, quite early on, often obtained in collaboration with other mathematicians. The notorious Banach-Tarski "paradox" from (1924), concerning the decomposition of the sphere, and his famous joint paper (1926) with Adolf Lindenbaum, which states, without proof, more than a hundred propositions of set theory, readily spring to mind. Nevertheless, in spite of his being primarily a mathematician, Tarski got his doctorate under Leśniewski, whose only PhD student he was, and in early works Tarski gave detailed contributions to the development of Leśniewski's system.[18] Leśniewski must have had a very high opinion of Tarski's ability: Woleński reports that Leśniewski had the habit of claiming that a 'hundred percent of my doctoral students are geniuses'.[19]

Fortunately, most of Tarski's philosophically relevant writings were collected, and translated, by J. H. Woodger, in the well-known *Logic, Semantics, Metamathematics* from 1956.[20] The very fact, though, that Tarski is and has been read mainly in collections, be they English, French or Polish, and not as originally published, has allowed an interesting circumstance concerning the relationship between Leśniewski and Tarski to remain hidden: in 1929–30, at the time when Tarski began his work on *Der Wahrheitsbegriff*, his sense of identification with Leśniewski was very strong indeed. Alone among academic works known to me, bar one, Leśniewski's *Grundzüge* open in a very peculiar way. After the title and the name of the author we get, not, as one would expect, an introduction, but a two-page list of bibliographical references set out in an overly precise—some would even say *neurotic*—manner:

> Bei bibliographischen Berufungen werden unten folgende Abkürzungen gebraucht:
> "Ajdukiewicz$_1$" für "*Przegląd Filozoficzny*. Jahrbuch 29 (für das Jahr 1926). Heft III–IV. 1927 Kazimierz Ajdukiewicz. *Voraussetzungen der traditionellen Logik*" (polnisch).

etc.

The complete list of references looks unmistakably characteristic, and has a strange beauty of its own, owing to the use of wide spacing for proper names.[21] The only other piece known to me that proceeds after the same fashion is Tarski (1935), which opens with a list of contents, followed by exactly similar bibliographical references set out with quotation marks both on the abbreviations and what they abbreviate, while using wide spacing for proper names.[22] In my opinion, this is no coincidence, but, undoubtedly, a reflection of His Master's Voice. Tarski's writings around this time provide further evidence that, as far as *foundational* matters are concerned, he was indeed a disciple of Leśniewski:

> Zum Schluß sei bemerkt, daß die Voraussetzung eines bestimmten philosophischen Standpunktes zu der Grundlegung der Mathematik bei den vorliegenden Ausführungen nicht erforderlich ist. Nur nebenbei erwähne ich deshalb, daß meine persönliche Einstellung in diesen Fragen im Prinzip mit dem Standpunkt übereinstimmt, dem S. Leśniewski in seinen Arbeiten über die Grundlagen der Mathematik einen prägnanten Ausdruck gibt und den ich als "intuitionistische Formalismus" bezeichnen würde.[23]

At this point Tarski further refers to the lengthy passage from Leśniewski (1929, p. 78) that was quoted above, thus making it abundantly clear to whom we owe the deliberately provocative turn of phrase: the ironic sneer *intuitionistic formalism* is not Tarski's own, but that of Leśniewski. In the next footnote, Tarski feels obliged to swear fealty to Leśniewski yet again:

> Anstatt "sinnvolle Aussagen" könnte auch "regelmässig konstruierte Aussagen" gesagt werden. Wenn ich das Wort "sinnvoll" gebrauche, so geschieht das, um meiner übereinstimmung mit der oben erwähnten Richtung des intuitionistischen Formalismus auch äusserlich einen Ausdruck zu geben.[24]

In 1929, at the beginning of his work on *Der Wahrheitsbegriff*, we find Tarski true to his Leśniewskian calling. In its conception, and philosophy of its early parts, Tarski follows his master. Concerning *§1. Der Begriff der wahren Aussage in der Umgangssprache* he states:

> Die Bemerkungen, die ich in diesem Zusammenhang vorbringen werde, sind zum grössten Teil nicht das Resultat meiner eigener Untersuchungen: es finden

> in ihnen die Anschauungen Ausdruck, die St. Leśniewski in seinen Vorlesungen an der Warschauer Universität [...], in wissenschaftlichen Diskussionen und in privaten Gesprächen entwickelt hat; insbesondere betrifft dies fast alles, was ich über die Ausdrücke in Anführungszeichen und die semantischen Antinomien sagen werde.[25]

The Polish version was published in 1933, but is was presented to the Warsaw Society of Sciences and Letters, by Jan Łukasiewicz, already on March 21, 1931, or so Tarski informs us.[26] All in all, when his work began in 1929, and for a couple of years to come, Tarski was outwardly committed to Leśniewski's foundational views.

However, during the next few years to come, relations between Tarski and Leśniewski went sour. From Jan Woleński, we know about Leśniewski's growing antipathy for Tarski, as witnessed by the shocking letter (by today's standards; I cannot speak for Warsaw 1935) from Leśniewski, writing from Zakopane to Twardowski, September 8, 1935.[27] It must be stressed, though, that, while the letter manifests strong distaste for Tarski as a person, even by this late date, Leśniewski still holds Tarski's ability as a researcher in as high regard as ever.

In the main body of *Der Wahrheitsbegriff* Tarski firmly adheres to the Husserl-Leśniewski doctrine of semantical categories:

> [Der] Begriff [der semantischen (oder Bedeutungs-) Kategorie], welcher von E. Husserl stammt, wurde durch Leśniewski in die Untersuchungen über die Grundlagen der deduktiven Wissenschaften eingeführt. Formal betrachtet, ist die Rolle dieses Begriffs bei dem Aufbau einer Wissenschaft analog der Rolle des Begriffs Typus im System *Principia Mathematica* von Whitehead und Russell; was aber seinen Ursprung und seinen Inhalt anbelangt, entspricht er (annäherungsweise) eher dem aus der Grammatik der Umgangssprache wohl bekannten Begriff des Redeteiles. Während die Typentheorie hauptsächlich als eine Art Vorbeugungsmittel gedacht war, das die deduktiven Wissenschaften vor eventuellen Antinomien bewahre sollte, dringt die Theorie der semantischen Kategorien so tief in die fundmentalen, die Sinnhaftigkeit der Ausdrücke betreffende Intuitionen hinein, dass es kaum möglich ist, sich eine wissenschaftliche Sprache vorzustellen, deren Aussagen einen deutlichen inhaltlichen Sinn besitzen, deren Bau jedoch mit der in Rede stehenden Theorie in einer ihrer Auffassungen nicht in Einklang gebracht werden kann.[28]

At this point Tarski cites a number of references, among which Leśniewski (1929, p. 14). His text, in fact, is little but a paraphrase of this passage:

> Im J. 1922 habe ich eine Konzeption der "semantischen Kategorien" skizziert, die mir diese oder jene einer jeden intuitiven Begründung für mich entbehrenden "Hierarchien der Typen" ersetzen sollten, und die, wenn ich überhaupt mit Sinn reden wollte, ich heute mich gezwungen fühlen würde anzunehmen, auch wenn keine "Antinomien" auf der Welt beständen. Indem meine Konzeption der "semantische Kategorien" in Bezug auf ihre theoretischen Konsequenzen in enger formaler Verwandschaft mit den bekannten "Theorien der logischen Typen" [.]

> blieb, knüpfte sie, was ihre intuitive Seite anbetrifft, eher den Faden der Tradition der "Kategorien" von Aristoteles, der "Redeteil" der traditionellen Grammatik und der "Bedeutungskategorien" von Herrn Edmund Husserl [.] an.

The doctrine of semantic categories had *einen wesentlichen Einfluss* on the structure of Tarski's work and on its results.[29] A truth-predicate, like any other predicate, has to be slotted into a category. Truth for a particular language, as defined by Tarski, will have an order exceeding that of the object language by at least one. For ("object"-) languages of finite order, this poses no particular problem, since ascent to a higher level is always possible. However, for languages of infinite order there is no room left for a truth predicate. Truth for an infinite-order language would have to be transcategorial, whence it is indefinable.

A determining feature of Tarski's article is that he defines truth only for formal languages, that is, *(künstlich konstruierte) Sprachen* [...] *in denen der Sinn jedes Ausdrucks durch seine Gestalt eindeuting bestimmt ist*.[30] First, in §1, a definition with respect to natural language is ruled out. In §2, he carefully describes the formal language of the *Klassenkalkül*, and gives the definition of its truth predicate in §3. In the sequel Tarski generalizes from the particular case and treats generally of languages of finite order in §4, where a truth definition is possible, and of infinite order in §5, where, as just noted, it is not, as long as one remains within the confines of the Husserl-Leśniewski doctrine of semantic categories. The generalization to other cases then the *allgemeine Klassenkalkül*, of course, demands discussion of the conditions that the languages in question have to satisfy in order that the Tarski techniques be applicable. In this connection he appends a long footnote of crucial importance for my present purposes. I expect that it was written fairly late in the course of his investigations, since, after all, at least one concrete instance of the truth definition has to be given before it makes sense to consider the conditions under which it generalizes. In the course of this footnote Tarski characterizes Leśniewski's formal languages in a most revealing way:

> Um die folgende Ausführungen in eine ganz präzise, konkrete und dabei genügend allgemeine Form zu kleiden, würde es genügen, als Gegenstand der Untersuchungen die Sprache irgend eines vollständigen Systems der mathematischen Logik zu wählen. Eine solche Sprache kann nähmlich als "universale" Sprache betrachtet werden, und zwar in dem Sinne, dass alle anderen formalisierten Sprachen—auch wenn man von Unterschieden "kalligraphischer" Natur absieht—entweder Bruchstücke von ihr sind oder sich aus jener Sprache bzw. aus ihren Bruchstücken durch Hinzufügung dieser oder jener Konstanten gewinnen lassen, wobei semantische Kategorien der betreffende Konstanten [...] *schon durch gewisse Ausdrücke der gegebenen Sprache repräsentiert* sind; die Anwesenheit oder Abwesenheit derartiger Konstanten übt, wie wir uns überzeugen werden, nur einen minimalen Einflüss auf die Lösung des uns interessierenden Problems aus. Nichdestoweniger konnte ich mich hier nicht entschliessen die Untersuchungen in der erwähnten Richtung zu konkretisieren,

und zwar aus folgenden Gründen. Dass einzige mir bekannte vollständige System der mathematischen Logik, dessen Formalisierung—im Gegensatz z. B. zum System Whitehead-Russell —keine Einwände zulässt und vollkommene Präzision aufweist, ist das von Leśniewski begründete System, das bisher in seiner Gänze noch nicht veröffentlicht worden ist [...]. Leider scheint mir dieses System wegen gewisser spezifischer Eigentümlichkeiten ein überaus undankbares Objekt für methodologische und semantische Untersuchungen zu sein. Die Sprache dieses Systems ist nicht als etwas potentiell "Fertiges" gedacht, sondern als etwas "Wachsendes": es sind nicht im vorhinein alle Zeichen und Sprachformen vorgesehen, welche in den Sätzen des Systems erscheinen können; dagegen sind genaue Regeln angegeben, welche in jedem Aufbaustadium des Systems seine sukzessive Bereicherung durch neue Ausdrücke und Formen ermöglichen; im Zusammenhang damit besitzen solche Termini wie "Aussage", "Folgerung", "beweisbarer Satz", "wahre Aussage" in Bezug auf das besprochene System keine absolute Bedeutung und müssen auf den jeweiligen aktuellen Zustand des Systems bezogen werden. Formal genommen würde es sogar schwer fallen, dieses System der allgemeinen [...] Charakterisierung der formalisierten deduktiven Wisssenschaften unterzuordnen. Um unter diesen Umständen das System Leśniewski's den Bedürfnissen der vorliegenden Untersuchungen anzupassen, müsste es einer recht gründlichen Umarbeitung unterzogen werden, was jedoch den Rahmen dieser Arbeit vollständig sprengen würde.[31]

Überaus undankbares Objekt für methodologische und semantische Untersuchungen—these are strong words, especially when applied to the cherished system of your *Doktorvater*. On what, if not methodological and semantical investigations pertaining to his system, had Leśniewski given his best energies for more than a decade? Even somone with an ego of smaller size than that of St. Leśniewski might take umbrage at these words. What their effect on him would have been in 1933, when the Polish version appeared, I can only begin to guess. If his behaviour, when faced with criticism from Wacław Sierpiński a couple of years earlier, more about which below, is anything to go by, Leśniewski's reaction to Tarski's very public apostasy, in a major work that was bound to attract attention in large measure, will have been nothing short of utter outrage.

Work on the problem of defining truth had led Tarski to disillusionment with the Leśniewskian framework: its conception of language proved unservicable for the kind of investigation that he envisaged. Alfred Tarski, the (1935) author of the *Nachwort*, could no longer accept even the doctrine of semantic categories:

Heute könnte ich den damals in dieser Frage vetretenen Standpunkt nicht mehr verteidigen.[32]

In the main body of the article (simple) type theory, rather than set theory, is used out, presumably since the set-theoretic (does not fit into the Husserl-Leśniewski hierarchy of semantic categories. In the *Nachwort*, Tarski abjures this theory and converts to set theory, to which he remained faithful throughout the rest of his career.

Why did Tarski part company with Leśniewskian foundationalism at this juncture? I will offer four kinds of considerations:

(1) ZEITGEIST

We must note that he was not alone in doing so. Almost everybody did. Church, early Quine, and Heyting are the only foundationalist die-hards that spring to mind in the younger generation of logicians. Among philosophers, Carnap, who had been squarely foundationalist in *Der logische Aufbau der Welt* (1928), as well as in his textbook (1929), fell, hook, line, and sinker for the metamathematical approach in his *Logische Syntax der Sprache* (1934). Tarski, furthermore, was conditioned towards metamathematics by previous activities. As was already noted, his work was mainly that of a (theorem-proving) mathematician. Inspection of S. Givant's (1986) Tarski bibliography yields that, prior to (1932), his publications were almost entirely mathematical in content and approach. Only the early doctoral dissertation (written under Leśniewski's direction!), with the two ensuing publications (1923), (1924), is logical foundationalist in approach. For the rest, mathematics only: well-ordered sets, finite sets, polygons, equivalents of the Axiom of Choice, decompositions of the sphere, cardinal arithmetic, the geometry of solids, measure theory, definability of sets are some of the topics Tarski dealt with during the 1920's.

(2) ŁUKASIEWICZ AND METAMATHEMATICS

Leśniewski was, of course, not the only influence on Tarski. In philosophy, Kotarbiński seems to have been at least as important, whereas among logicians there was also Łukasiewicz and his group. In contradistinction to Leśniewski, Łukasiewicz was not a foundationalist.[33] For him the main task of the logician was to explore the various possibilities of constructing "logics", that is, more or less artificial systems of logic, and in his case, especially systems for the *propositional* calculus.[34] Indeed, the survey that was collated by Tarski and Łukasiewicz (1930) is an early classic of metamathematics. It contains work that was carried out in Łukasiewicz's seminar on mathematical logic from 1926 onwards, by him, Tarski, Lindenbaum, Sobociński, and Wajsberg. This work, together with related articles by Tarski, belongs to an entirely different paradigm from that of Leśniewski: the problem of content has here receded very far into the background or is indeed entirely absent.[35]

(3) ZERMELO AND THE FOUNDATIONS OF SET THEORY

Most of Tarski's early mathematical works were devoted to problems of set theory, both general, for instance cardinal arithmetic, or descriptive, for instance, measure theory. In his "practical" approach to set theory, Tarski followed Wacław Sierpiński, the foremost Polish set theorist of his times: treat it like ordinary mathematics and do not bother too much about axioms and foundations.[36] In his early views on the *foundations* of set theory, on the other hand, he appears to have followed His Master's Voice, perhaps out of respect and conviction, but possibly also for want of something more congenial. In the excerpts from previous works of his that follow the bibliography in the *Grundzüge*, Leśniewski states his *credo* of content in the form of some barbed remarks contra Zermelo's set theory, which, at the time of writing, lacked intuitive models and was axiomatic only in Hilbert's hypothetico-deductive sense:

> Die architektonisch raffinierte Konstruktion des Herrn Ernst Zermelo[2] {[2] Mathematische Annalen. 65 Band. 1908. E. Zermelo. Untersuchungen über die Grundlagen der Mengenlehre I.} führt in die "Mengenlehre" eine Reihe von Verboten ein, die, einer intuitiven Begründung entbehrend, auf die Verdrängung der "Antinomien" aus der Mathematik hinzielen. Die Frage, ob [...] die "Mengenlehre" des Herrn Zermelo jemals zum Widerspruch führen wird, ist eine vollkommen gleichgültige Frage vom Gesichtspunkte der Zustände einer auf die Wirklichkeit gerichteten intellektuellen Mühsal, die aus einer unwiderstehlichen intuitiven Notwendigkeit des Glaubens an die "Wahrheit" gewisser Voraussetzungen und an die "Korrektheit" gewisser Schlußfolgerungen fließen, welche in Verbindung mit diesen Voraussetzungen zum Widerspruch führen. Von diesem Gesichtspunkte aus ist die einzige Methode einer wirklichen "Auflösung" der "Antinomien" die Methode intuitive Unterminierung der Schlußfolgerungen oder Voraussetzungen, welche zusammen zum Widerspruch beitragen[3] {[3] [...]}. Eine außerintuitive Mathematik enthält keine wirksamen Remedien für die Übel der Intuition.[37]

However, two years later, in 1929 Warsaw was visited by *Herrn* Ernst Zermelo, who gave a course of nine lectures at the university, May 27–June 8, 1929.[38] In these he presented the cumulative (Mirimanoff-Von Neumann) R-hierarchy

$$R_0 = \emptyset;$$

$$R_{\alpha+1} = \wp(R_\alpha);$$

$$R_\beta = \bigcup_{\alpha<\beta} R_\alpha, \text{ for limit ordinals } \beta,$$

and showed how it could serve as an intuitive, "intended" model for his own set theory, as emended by Skolem, Fraenkel, and Von Neumann.[39] In fact, Zermelo showed how the *second-order* version of this theory characterises its models up to (almost) isomorphism, somewhat along the lines of Dedekind's theorem that the second-order Peano axioms characterise the natural numbers up to isomorphism. If the Zermelo-Fraenkel axioms of the second-order hold in the structure $\mathfrak{A}$, where

$$\mathfrak{A} = \langle A, \in_A \rangle,$$

then, for some ordinal $\alpha_{\mathfrak{A}}$, $\mathfrak{A}$ is isomorphic to the initial segment $< R_{\alpha_{\mathfrak{A}}}, \in\restriction\alpha_{\mathfrak{A}} >$ of the *R*-hierarchy. These results of Zermelo were printed in *Fundamenta Mathematicae* in (1930). With this classic work Zermelo had gone quite some length towards providing, in an almost tangible way, for such *unwiderstehlichen intuitiven Notwendigkeit des Glaubens* that had been somewhat sneeringly demanded by Leśniewski. From *Der Wahrheitsbegriff* we know that Tarski appreciated the importance of categoricity for a second-order system: according to him it provides an objective guarantee that the categorical system in question can serve as a foundation for the corresponding deductive discipline.[40] Accordingly Zermelo could offer Tarski point something much more congenial to a working mathematician than what he had been offered by Leśniewski: an axiom system, namely, that of Zermelo and Fraenkel in its second-order formulation, with only one non-logical primitive notion, to wit the set-theoretic $\in$, that was closely geared to the practical needs of current mathematics, while yet objective, just like well-known second-order systems for the natural and the real numbers, in the familiar sense of (almost) categoricity. It is understandable, if Tarski began to waver in his Leśniewskian orthodoxy. Under the rapidly circumstances in foundational research it seems quite possible that what finally pushed him away from Leśniewski was the unfortunate

(4) Conflict between Sierpiński and Leśniewski

As we learn from Jan Woleński, around this time, Leśniewski's relations with Sierpiński, deteriorated beyond repair owing to their different attitudes to set theory.[41] Leśniewski, certainly, was not the man to mince his words in the face of something with which he disagreed, and he had been engaging in his favourite gambit of non-comprehension, pouring sneering criticism, verging on scorn, on the "happy-go-lucky" set theory, as practised by Wacław Sierpiński and others—Cantor, Hausdorff, and Fraenkel are mentioned by name—in an article from (1927). When Leśniewski's *Grundzüge* were published two years later, Sierpiński retaliated in kind (but apparently not in print) and Polish prickliness ran its course. It appears that matters went completely out of hand: Leśniewski, and with him Łukasiewicz, withdrew from the editorial board of *Fundamenta Mathematicae*, thus upsetting the agreeable delicate balance between mathematicians and logicians that had served Poland so well during the 1920's. Needless to say, he also withdrew the second part of his *Grundzüge* from publication. At the same time, though, as was noted above, the very same journal opened its pages for yet another 'architektonisch raffinierte Konstruktion des Herrn Ernst Zermelo'. Much bitterness speaks from his words:

> Den schon in demselben J. 1929 von mir derselben Zeitschrift eingereichten un von der Redaktion zum Druck akzeptierten Teil der Fortsetzung der erwähnten deutschen Mitteilung habe ich im J. 1930 aus Gründen persönlicher Natur aus

> dem *Fundamenta Mathematicae* zurückgezogen. In dieser Sachlage ist es mir schwer vorauszusehen, ob, wo und wann ich für die genannte Publikation Platz finden könnte.[42]

As a result of this unfortunate clash of ego's much of Leśniewski's work was left unpublished, only in order to perish in Warsaw during World War II.[43]

3. Conclusion

Around 1930 Alfred Tarski, a mathematician by inclination, training, and ability, very much like other contemporary researchers, attempted to apply the techniques of mathematics to problems in logic. Out of necessity this demanded that the formal languages of logic had to be converted into objects of study, from having been major tools for research. For him personally this entailed a conflict between the foundational stance that he had taken over from his teacher Leśniewski and the metamathematical *laisser faire* towards which he, as a mathematician, was inclined. He resolved this dilemma between 1933 and 1935 and his unequivocal choice was in favour of metamathematics. I have suggested that contributing factors in this decision were, possibly among others, (1) the impact of the achievements of of metamathematics; (2) Tarski's own experience of metamathematical work; (3) the availability of an attractive alternative foundation, namely, Zermelo's axiomatic set theory in relation to the cumulative hierarchy; and (4) unfortunate personal conflicts among his teachers and collaborators.

Notes

1. Woleński (1995).

2. Pearce-Woleński (1988) supply the dates in question.

3. Woleński and Simons (1989, p. 425). To the quoted sentence a footnote is added: "Verbal reports by various people who knew either or both confirm this impression."

4. Sundholm (1993).

5. Thus, for instance, De Roulihan (1998), who deals with the Tarski (1935, Nachwort), remarking partly upon the same quotes as myself, apparently missed my paper.

6. 'Polish logic' has the sound of a logic that holds only in Poland. Nevertheless, 'history of the development of logic in Poland' is too cumbersome for ready use.

7. Indeed, the unpleasant 1906–1908 exchange between Frege and Thomae in the *Jahresbericht* of the Society of German Mathematicians dealt with the need for interpretation. Frege argued the side of content, wielding, as was his wont, a polemical bludgeon rather than a rapier, but nevertheless with devastating effect. After this exchange, Thomae apparently, and perhaps not unreasonably so, declined further scientific intercourse with Frege.

8. Frege (1884, §3, p. 4).

9. Scholz (1930) is still an authoritative exposition of the Aristotelian conception of demonstrative science.

10. As was famously noted by Gödel (1944, p. 126).

11. See Lowe (1985, p. 292).

12. See Heyting (1930a), (1931) and Kolmogoroff (1932).

13. For this paradigm, see van Heijenoort (1967), (1976) and its further development by Hintikka (1988), (1996).

14. Gödel (1931).

15. In two authoritative early presentations of Gödel's work, to wit those of Mostowski (1952) and Feferman (1960), the authors give their respective descriptions of syntax directly in terms of various classes of numbers, conveniently leaving out the introduction of a formalized object language. The objectual character of metamathematical expressions is brought out most clearly in the exposition offered by Monk (1976, Ch. 10).

16. Grzegorczyk's congenial passage (1955, 78–79) goes some way towards explaining why Leśniewski has suffered neglect. Nevertheless, some, among whom Eugene Luschei (1962), Jan Woleński (1989, Ch. VIII), and Peter Simons (1992, Chs. 9–12), attempt to keep the flag flying.

17. Leśniewski (1929, p. 78). Jan Woleński is manifestly aware of the importance of this passage: it is excerpted in Pearce and Woleński (1988, pp. 145–146), and quoted (from the English translation Leśniewski (1992, p. 487)) in Woleński (1995, p. 79).

18. See Tarski (1923), (1924), and *many* acknowledgements to Tarski in later writings by Leśniewski, for instance (1929, pp. 8, 11, 13–16, 31–32, 39–44, 46–54, 58–59). I cannot vouch for the claim that these are *all* acknowledgements to Tarski in Leśniewski (1929). However, they are plenty; perhaps, under other circumstances, with other personalities involved, they even ought to justify joint authorship.

19. (1995, p. 68).

20. A decade later also the work of Leśniewski became partly accessible in the useful pendant volume *Polish Logic* of translations that was brought out by Storrs McCall (1967), while at least one of his chief articles (1929) was readily available in German in a major journal.

21. In Michael p. O'Neil's English translation of Leśniewski (1929), however, this list, for reasons unclear to me, is removed to the end of the final §11; furthermore, the layout is drastically altered and the characteristic use of wide spacing and subscript numbering is jettisoned. The Pearce-Wolński (1988, pp. 136–137) excerpt, on the other hand, is—almost—faithful to Leśniewski: the references are set out at the beginning, using quotation-marks and all, and only the wide spacing of names is lost.

22. Also the Polish original opens similarly. In the English translation in Tarski (1956), in its expanded French translation Tarski (1972–74), and also in the Polish collected edition (1995), a standardized, uniform bibliography is used, whence the information contained in Tarski's own typographical choice is lost. The invaluable Berka-Kreiser (1983) volume, on the other hand, reprints the text properly, as given in *Der Wahrheitsbegriff*.

23. Tarski (1930, p. 363), in English translation (1956, p. 62). Tarski also added a footnote in the English translation to the effect that this passage 'expresses the views of the author at the time when this article was originally published and does not adequately reflect his present attitude'.

24. Tarski (1930, p. 363, fn. 2), in English translation (1956, p. 62, fn. 3).

25. (1935, p. 7, fn. 3).

26. (1935, p. 7, fn. 2). To all respects, then, the original text of the main body of *Der Wahreheitsbegriff* was ready in early 1931. The only changes—see p. 110, footnote 88 and p. 145—in the two intervening years before publication pertain to *Satz I*, p. 110, which makes use of the ideas of Gödel (1931).

27. Woleński (1995, p. 68–69). The full text of the letter, in English translation, is available electronically at the *Polish Philosophy Page* website: http://www.fmag.unict.it/PolPhil/lesnie/LesnieDoc.html.

28. Tarski (1935, p. 75).

29. Tarski (1935, Nachwort, p. 133).

30. Tarski (1935, p. 20).

31. Tarski (1935, p. 68, fn. 56). The remarks on Leśniewski are not included in the corresponding footnote in the English translation (1956, p. 210, fn. 2). The motives for this deletion are unclear to me.

32. Tarski (1935, Nachwort, p. 134) here refers back to his previous Leśniewskian position on semantic categories, as stated in the passage cited at footnote 26.

33. As is readily brought out by even a cursory inspection of the writings collected in his (1970).

34. As Woleński (1995, p. 78) observes, Łukasiewicz seems, at first, to have held a position close to that of Leśniewski, namely that only one of the many systems of sentential logic that he studied was valid in the world. However, it is fair to say, that, even then, in the thirties, his research was not at all focused upon determining that system. When Łukasiewicz had left Poland, and after the death of Leśniewski, his view seem to have progressed towards what must be regarded as the natural stand for a metamathematician, namely something like Carnap's (1934) instrumentalism.

35. For instance, the articles III, IV, V, XII, XIV, and XVII in Tarski (1956).

36. For instance, in the passage from (1930, p. 363) that was quoted above—cf. footnote 23—Tarski explicitly states that his results do not presuppose a particular foundational standpoint. The two remarks concerning "intuitionistic formalism", and especially the one in the footnote 2, read as if they had been added mainly out of "filial" duty for the *Doktorvater*.

37. Leśniewski (1929, p. 6), also in Pearce-Woleński (1988, p. 140). English translation Leśniewski (1992, p. 416).

38. Moore (1980, Appendix 10.2, pp. 134–136) offers valuable information on Zermelo's Polish visit. The Skolem-Fraenkel and Von Neumann emendations of Zermelo's set theory concern the Axioms of Replacement and Foundation, respectively.

39. Zermelo also allowed for so called *Urelements* in his formulation; this need not detain us here.

40. Tarski (1935, p. 30, fn. 22).

41. Woleński (1995, pp. 66–67).

42. Leśniewski (1930, p. 112).

43. See McCall (1967, p. 88, fn. 5), where Sobociński's account of the fate of the *Collectanea Logica* is reprinted, and Luschei (1962, p. 26) for further information about the demise of Leśniewski's *Nachlaß*.

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