Busting a Myth about Leśniewski and Definitions

Rafal Urbaniak*
K. Severi Hämäri†

Preprint: final version forthcoming in *History and Philosophy of Logic*

April 26, 2011

Abstract

A theory of definitions which places the eliminability and conservativeness requirements on definitions is usually called the standard theory. We examine a persistent myth which credits this theory to S. Leśniewski, a Polish logician. After a brief survey of its origins, we show that the myth is highly dubious. First, no place in Leśniewski's published or unpublished work is known where the standard conditions are discussed. Second, Leśniewski's own logical theories allow for creative definitions. Third, Leśniewski's celebrated 'rules of definition' lay merely syntactical restrictions on the form of definitions: they do not provide definitions with such meta-theoretical requirements as eliminability or conservativeness. On the positive side, we point out that among the Polish logicians, in the 1920s and 30s, a study of these meta-theoretical conditions is more readily found in the works of J. Łukasiewicz and K. Ajdukiewicz.

Keywords: definitions, standard theory of definitions, translatability, eliminability, creativity, conservativeness, Leśniewski, Łukasiewicz, Ajdukiewicz, Lvov-Warsaw school.

1 Introduction

Definitions resemble statements of identity: despite their seeming clarity and distinctness, they are a source of confusion and disagreement. Yet there exists a mainstream theory of definitions which we, following Belnap [1993], call the *standard theory*; it treats eliminability and conservativeness as marks of a good definition. It is a folklore that Stanisław Leśniewski[1] created the theory. This common belief has lingered at least since the publication of (Suppes) in 1957

*Department of Philosophy, Sociology and Journalism, Gdańsk University, Poland and Centre for Logic and Philosophy of Science, Ghent University, Belgium, `rfl.urbaniak@gmail.com`.

†Department of Philosophy, History, Culture and Art Studies, University of Helsinki, `severi.hamari@helsinki.fi`.

[1]Leśniewski (1886–1939) was a Polish logician, Tarski's PhD supervisor, and the creator of idiosyncratic logico–mathematical theories called Ontology, Prothotetic, and Mereology.

(but only Nemesszeghy and Nemesszeghy [1977] try to bring up some evidence to corroborate it).

We are not the first to question the folklore. Already in (1973) Dudman indicates the priority of Frege's remarks on creativity. Belnap [1993] complains about the lack of relevant Leśniewski citations. More recently, Gupta [2009: n. 3] hints that the standard theory might be by Kazimierz Ajdukiewicz,[2] although with no reference to a published work. Hodges [2008] and Rickey [1975*b*] present the strongest criticism. Unfortunately, their sections on the myth and Leśniewski's views on definitions are too short to tell the whole story. In addition, it seems that neither Belnap, nor Gupta, nor Hodges are aware of Rickey's critique. We fill the gaps and confirm Rickey's and Hodges's suspicions. We focus on the origins of the folklore in section 3. In section 4 we present how Leśniewski's own writings have indirectly influenced the folklore, and we discuss Rickey's and Hodges's reservations in section 5. The rest of our paper is devoted to a study of Leśniewski's, Ajdukiewicz's and Łukasiewicz's[3] theories.

This issue bears on how we are to estimate Leśniewski's impact on Tarski. Betti [2008] argues that the forerunner of Tarski's semantical investigations was not Leśniewski, but rather Ajdukiewicz. Although Betti does not discuss the history of the desiderata put on definitions, Hodges [2008] asks how Leśniewski's theories affected Tarski's views on definitions. He concludes that '[i]t's impossible to measure how far Leśniewski's other views[4] on definitions influenced Tarski without establishing what those other views were, and this is difficult' (103).

Below (in sections 6 and 7) we try to shed more light on what 'those other views were'. Even though many of these issues are well known among Leśniewski scholars,[5] the persistence of the above-mentioned folklore indicates that the wider audience might not be as familiar with them. This unfamiliarity is not completely surprising, given Leśniewski's unapproachable style (see section 4) and, until recently, the lack of comprehensive and tangible secondary sources on his theory of definitions.

For instance, although Luschei [1962: 36] testifies that 'Leśniewski's rules for definition ... are among his most important scientific contributions, and need to be rescued from comparative oblivion', and further explains that he knows of 'no other rules comparable in adequacy and rigor of formalization to Leśniewski's directives of definition, more comprehensive and exact even than Frege's' [*ibidem*], he leaves the nature of those rules rather unexplained. He brings them up only in his rendering of Terminological Explanations, especially in T.E. XLIV. This terminological explanation starts with 'A is legitimate as propositive definition immediately after thesis B of this system if and only if the following eighteen conditions are fulfilled...' and continues with a rather literal representation of what Leśniewski himself said about the conditions.

[2] Kazimierz Ajdukiewicz (1890–1963) was a Polish logician and philosopher.

[3] Jan Łukasiewicz (1878–1956) was a Polish logician and the father of many-valued logics.

[4] Hodges argues that there is a link between Leśniewski's requirement of 'irresistible intuitive validity' and Tarski's 'wymóg trafności' (i.e. the material adequacy requirement) [102-3,114].

[5] See for instance [Rickey 1975*a*; Simons 2008].

We hope that our treatment here paints an accessible picture of Leśniewski's position on definitions for those not yet acquainted with it. We provide the relevant textual evidence and argue that even though this evidence falsifies the myth about Leśniewski it grants plausibility to the attribution to Łukasiewicz and Ajdukiewicz, among Lvov-Warsaw school.[6]

From the exegetical point of view two separate issues are at play here: on one hand, we may ask who endorsed the theory; on the other, who analyzed the conditions. We argue (in section 7) that Leśniewski's 'rules of definitions' ensured eliminability but *allowed for creativity* without him commenting on why he proceeded this way. Instead, we establish (in section 8) that Łukasiewicz employed the conservativeness requirement in an argument against Leśniewski, and that Ajdukiewicz managed to develop a rather elaborate account of those requirements that forms the grounds of the standard theory. Finally, (in section 9) we dismantle the evidence which Nemesszeghy and Nemesszeghy [1977] use to argue that the theory is Leśniewski's.

2 Basic notions

The standard theory, in contrast to theories about teachers', lawyers', legislators', or lexicographers' definitions, is concerned with definitions in the logicians' sense: a definition is a formula (or a set of such) which, relative to a formal theory and a language, fixes the meaning of new symbols occurring in it. (We can ignore the otherwise important distinction between explicit and implicit definitions here. Also, we are not considering meta-theoretical abbreviations, but object level "acronymic" definitions which introduce new symbols into the object language).

[6] Since we are here concerned only with the Poles of Lvov-Warsaw school, we can only hint at the discussions of e.g. Kant's, Mill's, Frege's, Peano's, and Russell's roles in the early developments of the standard account. (We have a few remarks in sections 3 and 5, though.) Decent introductions to theories of definitions are [Gupta 2009; Abelson 1967] and the monographs [Dubislav 1981; Robinson 1954]. Most original texts prior to Frege are in [Sager 2000] (although it lacks the relevant passages from *Port-Royal Logic* [**?**]). Beck [1956] seems still the authority on Kant's treatment of definitions. Shieh [2008] has extensive Frege references (both primary and secondary) as well as a good introduction to his theory of definitions. On Peano's groups' treatment of definitions and definability in mathematics there appears to be only a few sources in English. Grattan-Guinness [2000] is a notable exception. His book is a valuable source on the other players as well, just search his index for 'definition'. Anyone interested in *the Peanists* should look at [Padoa 1900], which is one of the most important texts in early model theory and the problem of definability. Before E. Beth's breakthrough in the 50s, Tarski was one of the few working on Padoa's method, see [Tarski 1934; Hodges 2008]. Kennedy [1973] sketches the impact Peano's theory of definition had on young Russell.

Dubislav's role in the development of the standard theory might turn out to be important. This German positivist and logician published two editions of his book *Die Definition* [1981] in the late 20s and the third edition in 1931. He discusses creative (*schöpferischen*) definitions in considerable detail. We know that at least Koj [1987] talks about Dubislav's impact on Ajdukiewicz. Grattan-Guinness [2000: 486, 519–20] provides a description of Dubislav's work in general. For anyone interested, most present-day issues are examined in the collection [Fetzer, Shatz and Schlesinger 1991].

As Belnap [1993: 119] reasons, 'a definition of a word should explain *all* the meaning that [the] word has, and ... it should do *only* this and nothing more.' He suggests that a natural connection with these intuitive desiderata might be the reason why the standard theory placed eliminability and conservativeness as the criteria[7] of acceptable definitions. He also argues that the criterion of eliminability ascertains that the intuition about 'all the meaning' is satisfied while the criterion of conservativeness, i.e. non-creativity, ensures the satisfaction of the 'only' part.

Let us look into some technical details. In deductive terms eliminability is defined as follows. Let T be a theory, L its language and Δ a set of formulas in an extended language $L' \supseteq L$. Then, symbols in $L' \setminus L$ are *eliminable* (i.e. "Δ is eliminable") *in* $T \cup \Delta$ *with respect to* L if and only if for any formula ϕ in language L' there exists a formula ψ in L such that $T \cup \Delta \vdash \phi \equiv \psi$. In other words, every expression in the extended language has to be "translatable" into the old language. Conservativeness, on the other hand, is defined as follows. Let T, Δ, L and L' be as above. Δ is *conservative over* T if and only if for all formulas ϕ in the language L, if $T \cup \Delta \vdash \phi$ then $T \vdash \phi$. This means that no new claim that can be formulated in the original language becomes provable once the definition is introduced. As we can see, according to the standard view a set of formulas Δ is a definition only with respect to some theory and some language; there are no general syntactical criteria of definitions here.

To avoid a problem arising from the possibility of two separately conservative (or eliminable) but jointly creative (non-eliminable) definitions, the definitions have to be introduced in stages. Each subsequent stage has to be eliminable and conservative over the previous ones. We cannot go into more details of the standard theory. For the particularities see [Suppes 1957: chapter 8], [Mates 1972: 197–203], and especially [Belnap 1993].[8]

As it turns out, eliminability (even supposing consistency) is not a sufficient criterion for a successful definition. Definitional extensions might differ on a fundamental level from the original theory, e.g. they might lack a model with cardinality in the spectrum of the original theory.

Without the criterion of conservativeness we would also run the risk of Prior's [1960] skepticism or 'tonktitis', as Belnap [1962] called it. Prior argues that the rules of deduction cannot function as definitions that explain all the meanings of the logical constants: there are inconsistent sets of rules (that is, rules which lead to triviality). But there is more here than meets the eye. Belnap replies that there is a difference between rules that can function as definitions and rules that cannot: the rules that define meaning are conservative over the underlying logic. Therefore, the criterion of conservativeness is important to the proof-theoretic meaning theory as well [see also Hacking 1979].

As we will explain in section 6, Leśniewski's definitions (e.g. in his the-

[7] We do not use the word 'criterion' in a technical "Wittgensteinian" sense, but as a synonym of 'condition' and 'requirement'.

[8] Došen and Schroeder-Heister [1985] discuss slightly different notions (which are used in [Belnap 1962]): conservativeness and uniqueness. They show that this pair of conditions has interesting properties (e.g. they are dual to each other in certain contexts).

ory called *Ontology*) satisfy the eliminability but not, in general, the conservativeness requirement (yet we have pretty good reasons to think the system is consistent [Słupecki 1955]). Why, then, was the standard theory ascribed to him?

3 On the origins of the folklore

As far as we can tell, no publication by Leśniewski is the origin of the folklore, and most book passages and articles on definitions between the 30s and the 60s contain nothing about Leśniewski. For instance, even in [Łukasiewicz 1963: 31–33] where Leśniewski's views on definitions are implicitly criticized, none of these views are explicitly attributed to him. (We will come back to Łukasiewicz's lecture notes, originally presented in 1928-9, in section 8.)

A reference to Leśniewski's position, however, can be found at least in [Łukasiewicz 1928*b*], [Tarski 1941], [Mostowski 1948], [Kelley 1955], [Church 1956], [Suppes 1957] and [Ajdukiewicz 1936] (in an introduction written in 1960). We will show in section 8 that Lukasiewicz and Ajdukiewicz have not contributed to the folklore. Let us look a bit closer at the other sources.[9]

In Tarski's *Introduction to Logic* [1941] we find that:

> ... the present day methodology endeavors to replace subjective scrutiny of definitions ... by criteria of an objective nature, in such a way that the decision regarding the correctness of given definitions ... would depend *exclusively upon their structure, that is, upon their exterior form.* For this purpose, special rules of definition ... are introduced. [These rules] tell us what *form the sentences should have* which are used as definitions ... *each definition has to be constructed in accordance with the rules of definition* ... [In footnote:] one of [Leśniewski's] achievements is an exact and exhaustive formulation of the rules of definition. [Tarski 1941: 123, n. 4].[10]

Although Tarski does not explicate what Leśniewski's 'rules of definition' are, we agree with Hodges [2008] that it is clear from the context that Tarski means some syntactical criteria for good definitions. There is no mention of meta-theoretical requirements in the passage. As we will see later, Tarski's remark is adequate. Still, Tarski cites no works and states no explicit rules by Leśniewski and this might have caused some confusion.

[9]For the sources on Leśniewski's views, see also [Hodges 2008: 103-5].

[10]The emphases are changed. There are some small points pertinent to the history of this passage in Tarski's *Introduction*. The German edition [Tarski 1937], which otherwise seems to agree with the translation, does not credit Leśniewski with the formulation of the rules of definition. Hodges [2008] notes that the attribution of 'an exact and exhaustive formulation of the rules of definition' appeared in the English edition of 1941. He suggest that the change might be 'a mark of respect for a teacher who had died just two years earlier.' [103]. Also, in the fourth edition of 1994 'an exact' is replaced by slightly less emphatic 'a precise'. It is not clear whether it was Tarski's decision, Jan Tarski's improvement of his father's style, or John Corcoran's choice of a more appropriate word.

Church when writing about the object language definitions in *Introduction to Mathematical Logic* [1956] states that he...

> ...agrees with Leśniewski that, if such definitions are allowed [in the object language], it must be on the basis of *rules of definition*, included as a part of the primitive basis of the language and as precisely formulated as we have required in the case of the formation and transformation rules ... Unfortunately, authors who use definitions in this sense have not always stated rules of definition with sufficient care ... On the other hand, once the rules of definition have been precisely formulated, they become at least theoretically superfluous, because it would always be possible to oversee in advance everything that could be introduced by definition, and to provide for it instead by primitive notations included in the primitive basis of the language ... Because of the theoretical dispensability of definitions in [this] sense ... we prefer not to use them ... [Church 1956: 76, n. 168].

Here it is clear that the mentioned rules of definition are taken to be syntactical and that object language definitions should at least be eliminable. Although it appears that Church is silent about creativity, it is nonetheless plausible that the definitions which he would allow are non-creative, for he states that the correct rules for definitional introduction of symbols would make the system interchangeable with another one that lacks them. Church could well have adhered to the standard theory.

What, then, is Church attributing to Leśniewski? An interpretation which reads Leśniewski as embracing the 'superfluity' of object language definitions is of little credibility. Thus we are left with another more plausible but rather vague interpretation on which Leśniewski wanted definitions to be governed by precise rules. On this reading Church [1956] (in a way, just like Tarski) contributes to the folklore not by what he says, but rather by what he leaves unexplained.

The same is true about Mostowski. The most explicit remark about Leśniewski to be found there is:

> The need of a precise formulation of the rule of definitions has been strongly emphasized by Leśniewski. He gave a precise formulation of this rule with respect to the systems he constructed. [Mostowski 1948: 251][11]

Kelley [1955] and Suppes [1957], on the other hand, subscribe to the folklore. Kelley states in an appendix that he implicitly posits 'an axiom scheme for definition' and that...

> ...the axiom scheme of definition is in the fortunate position of being justifiable in the sense that, if the definitions conform with

[11] For a longer passage, see **text 9** in the appendix.

> the prescribed rules, then no new contradictions and no real enrichment of the theory results. These results are due to S. Leśniewski. [Kelley 1955: 251, n.]

Here Kelley is slightly vague: what would he count as a 'real enrichment'? If, as is natural to assume, he means inferential creativity, then he might be the first to state the folklore. (He is partly right since Leśniewski's rules would ascertain the consistency of the introduced definitions within a consistent theory, even though the relative consistency proof is not due to Leśniewski.) Kelly presents no source for his claim.

Suppes [1957] seems far more explicit than Kelley. He states that:

> it is not intended that a definition shall strengthen the theory in any substantive way. The point of introducing a new symbol is to facilitate ... investigation ... but not to add to ... [the] structure [of a theory]. Two criteria which make more specific these intuitive ideas about the character of definitions are that (i) a defined symbol should always be eliminable from any formula ... and (ii) a new definition does not permit the proof of relationships among the old symbols which were previously unprovable; that is, it does not function as a creative axiom. [In a footnote:] These two criteria were first formulated by the Polish logician S. Leśniewski ... he was also the first person to give rules of definition satisfying the criteria. (153)

As we can see, Suppes states the standard theory; and there is no doubt about what he credits to Leśniewski. Suppes claims that Leśniewski (a) had 'formulated' the conditions and (b) had laid these requirement on some 'rules of definitions'.

The former claim is dubious, bearing in mind Frege's remarks on non-creativity and eliminability of definitions in 1914[12] and earlier (he has remarks about non-creativity already in *Begriffschift* [1879: 55], see also [Shieh 2008: 994]), not to mention the fact that the basic ideas behind eliminability as well as non-creativity were, arguably, known at least by Blaise Pascal. The case of eliminability is clear, since the possibility of substituting mentally the defined term with the *definiens* in all contexts is mentioned many times in *De L'esprit Géométrique* [1814*b*: e.g. 127]. But the case of non-creativity needs an argument. Pascal not only demands that the sentences containing the defined word must be translatable to ones not containing it. He also demands that all proofs containing the defined word must be translatable into proofs not containing the word (otherwise proofs would not be persuasive [Pascal 1814*a*: 161]). Therefore, one might argue (just like in the case of Church) that Pascal, if asked, would have said that definitions are non-creative. But the cases are not identical, and we have here an open question: would Pascal allow for a definition to act as an axiom with existential import?[13] If the answer to this query is positive, then

[12] See **text 1** in the appendix for the relevant passage.

[13] E.g. Hobbes seems to have held such a position, see [Abelson 1967: 318]. Think of a

Pascal would have accepted creative definitions nonetheless.[14] But, without doubt, non-creativity was known to J. S. Mill [1869: 100] who lampooned the idea of using definitions as premises in any other context than when we are dealing with words.

The failure of claim (b) is slightly less obvious, since, as we will see in section 4, Leśniewski's idiosyncratic style is difficult to follow. However, given that Leśniewski's logical systems abide by his rules of definitions and yet definitions in those systems are creative (see section 6), (b) also turns out to be rather implausible. It is remarkable that both Hodges [2008: 104] and Rickey [1975*b*] review the points (a) and (b) of the folklore rather similarly. We discuss their critiques in section 5.

Further, in [Suppes 1957] there is no citation supporting claims (a) and (b). (According to Hodges [2008: 105], Suppes could not name the reference when Hodges enquired about it in 1996 and later.) As far as we know, and contrary to some claims (see section 5), no one has found any discussion of the conditions in Leśniewski's works. It is probable that none of the texts used by Suppes[15] attributed the standard theory to Leśniewski. Most other authors cite either Suppes or Belnap [1993], whose source is also Suppes.[16]

In general, it seems that already before the mid-1950s writers of introductory books wrote about Leśniewski and his so-called rules of definition. Every time this happened, the rules were rather mentioned than explicitly described. This tendency has continued: Leśniewski's rules for definition receive no detailed discussion even in Woleński's classic book [1985] whose English translation serves the role of the standard reference when it comes to the Polish school. This unwillingness to explicate turns out to be nothing surprising given Leśniewski's "Byzantine" writing style.

4 On Leśniewski's idiosyncrasies

One of the reasons why the actual nature of Leśniewski's rules is hardly known is their rather convoluted form. Therefore, the reader might find a quick tour of his approach useful. Once we briefly survey these issues, we move on to the later development of the folklore. Then, we examine the reasons why Leśniewski's systems need creative definitions and what his rules for definitions actually state.

In his papers Leśniewski uses at length a truly idiosyncratic terminology to define the rules of inference and the rules of definitions for his systems by means of what he calls *terminological explanations* (T.E.). Chronologically, the first paper where he employs terminological explanations to talk about definitions is

definition of number zero: zero is the number which has no predecessor. Now, one strategy to eliminate 'zero' would be would be to apply existential generalization: there is a number which has no predecessor. Such an eliminative definition of zero would be creative if used as a premiss.

[14] Compare [Abelson 1967: 319]. See also [**?**: I ch. 12, IV ch. 3-5].

[15] [Tarski 1941] is among Suppes's references, whereas Kelley [1955] and Church [1956] are not.

[16] "I learned most of the theory first from Suppes [1957], who credits Leśniewski ..." [Belnap 1993: 117]. Here he refers to the same quotation in Suppes above.

from 1929 and his [1931*b*] is the second one.[17] In the former he presents rules for his system of Protothetic and in the latter he gives rules for a simpler system of classical propositional logic (here he employs Łukasiewicz's axiomatization).

Few would call Leśniewski's style *user-friendly.* For instance, the first terminological explanation in [Leśniewski 1931*b*], probably the most straightforward one, elaborates on the composition of an expression A from a "collection" a of symbols. The lower-case variables behave like the name variables, which will be described in detail in section 6, and capitalized variables stand for singular terms. (Although it is quite natural to interpret Leśniewski's name variables set theoretically, as collections, it is not in accord with his original ideas, hence the scare quotes. Perhaps, a slightly more plausible reading takes them to be plural variables, but we do not need to get into these details here.)[18] The original formulation of T.E. I goes as follows:

> I say of object A that it is (the) complex of (the) a if and only if the following conditions are fulfilled:
>
> (1) A is an expression;
>
> (2) if any object is a word that belongs to A, then it belongs to a certain a;
>
> (3) if any object B is a, and object C is a, and some words that belongs to B belongs to C, then B is the same object as C;
>
> (4) if any object is a, then it is an expression that belongs to A. [Leśniewski 1931*b*: 631]

The underlying intuition is that for A to be composed of expressions a, (1) A has to be an expression (2) composed of words which occur in an expression which is a only, where (3) expressions a have no words in common, and (4) contain no expression that does not occur in A.[19]

This is only the first terminological explanation in [Leśniewski 1931*b*] and they get more complicated; elsewhere [1929] he is even less reader-friendly: he presents his terminological explanations in his full-fledged, idiosyncratic, formalized metalanguage with little explanation in natural language.[20] For example,

[17] Publication dates are not perfectly representative of when Leśniewski came up with various things, for he often tended to keep his papers in the drawer for a while; so it seems that Mereology dates back to 1916, Ontology dates back to around 1920 [see e.g. Leśniewski 1931*a*: 367], and Protothetic dates back at least to 1923; it is not clear whether the fact that the systems were constructed in those years means that full-blown ready-to-print descriptions of those systems are the same age [for more details, see Urbaniak 2008: 71-74, 105-07, 140-142].

[18] For more details pertaining to the philosophical issues related to Leśniewski's variables and quantifiers see [Urbaniak 2008: ch. 7].

[19] For a slightly elaborate explanation of what Leśniewski means by 'words', he sends the reader to his 1929 paper. We include the relevant passage in the appendix as **text 2**.

[20] Leśniewski, however, does not think of it as a formal system *sensu stricto*, see **text 3** in the appendix.

the 10th terminological explanation in [1929] looks like this:

$$\begin{aligned}\forall A\,[A\,\varepsilon\,qnr1 \equiv\\ \exists B\,(B\,\varepsilon\,qntf \wedge B\,\varepsilon\,ingr(A) \wedge 1ingr(A)\,\varepsilon\,ingr(B)) \wedge\\ \wedge\exists B\,(B\,\varepsilon\,sbqntf \wedge B\,\varepsilon\,ingr(A) \wedge Uingr(A)\,\varepsilon\,ingr(B)) \wedge\\ \wedge\forall B, C\,(B\,\varepsilon\,qntf \wedge Bingr(A) \wedge C\,\varepsilon\,sbqntf \wedge C\,\varepsilon\,ingr(A) \wedge\\ \wedge 1ingr(A)\,\varepsilon\,ingr(B) \wedge Uingr(A)\,\varepsilon\,ingr(C) \rightarrow\\ \rightarrow A\,\varepsilon\,Compl(B \cup C))]\end{aligned}$$

All of this only states the necessary and sufficient conditions for an expression A to be a quantified formula (i.e. that (1) there is a quantifier, which contains the bound variables, which occurs in A, and whose first word is the first word of A; (2) there is a range of a quantifier which occurs in A such that the last word occurring in A occurs in the range of this quantifier; and (3) for any two expressions B and C such that B is a quantifier occurring in A and C is a range of a quantifier and occurs in A, if the first word in A occurs in B and the last word of A occurs in C, A is the result of the composition of B and C).

Leśniewski uses this kind of strategy at length to define the systems under investigation. In [1931*b*] he presents an axiom for the classical propositional calculus, formulates the rules of inference (detachment and substitution), and syntactically defines the shape of correct definitions.

For instance, Ajdukiewicz [1928: 51], who in general agrees that Leśniewski's system 'is the only system with precisely formulated rules for definitions', when comparing Leśniewski's rules for definitions with those of Frege emphasized that the main difference between their views is that Leśniewski explicitly required that the *definiens* should not contain quantifiers and that it should not contain different occurrences of one and the same variable. Nothing beyond that, even if the complicated form of the description may make its content seem more elaborate.

In general, the preceding examples should suffice as an illustration of the challenge that detailed understanding of Leśniewski's rules poses, and as an explanation of the relative unpopularity of his work. An uncharitably minded reader could say that no deep intrinsic logical complexity is involved in Leśniewski's explications; after all, if all the formulation does is provide a description of what the axiom is and what rules of inference are, this can be done in a more accessible manner; and she might conclude that Leśniewski's meticulous emphasis on precision and full formalization is an overkill which, mixed with the unfamiliarity of his language, makes the effort of reading his work seem too strenuous.

This attitude is understandable. Most logicians can lead their lives tackling more intrinsically interesting logical problems without requiring this level of precision. Considering the fact that Leśniewski was publishing in the late 20s and early 30s, when deeper logical problems surfaced, it is no wonder his work received little attention. (For example, Jordan [1945: 44], who knew Leśniewski's works well, says that reading Leśniewski's account of definition is a 'somewhat excruciating experience' for anyone 'anxious to spare themselves the valuable thought'.) But this fact does not imply that Leśniewski's approach is worthless. For example, the development of proof theory in the 60s and 70s required almost the same level of syntactical precision that Leśniewski embraced. Some possible reasons why he adopted such a style and stern standards are discussed in [Simons 2008].

On the other end of the spectrum, some people when faced with Leśniewski's complicated metalanguage get an impression of hidden wisdom and a feeling that more is being said than what they can grasp. They too are somewhat responsible for the long life of the myth.

5 Later developments in the folklore

Probably the most known recent paper about the standard account is [Belnap 1993], where Belnap acknowledges the folklore. Here, however, we can see some caution: he is painfully aware of the lack of an original source. As we discussed earlier, his only reference is to Suppes's *Introduction to Logic*. He writes that '[t]he standard theory of definitions seems to be due to Leśniewski, who modeled his "directives" on the work of Frege, but I cannot tell you where to find a history of its development.' Belnap continues with a guess at which texts might be relevant. He supposes that the theory might be in [Leśniewski 1931*b*], or at least somewhere in Leśniewski's *Collected works*; as far as we know, it is in neither.

Because of Belnap's reservations we see caution, for example, in [Horty 2007]. When Horty discusses how the fruitfulness of definitions relates to the requirements of conservativeness and eliminability in Frege's works,[21] he notes that the 'explicit formulation' of the criteria 'is generally credited to Leśniewski' and that he knows 'of no complete history of the modern theory of definition, but some historical remarks can be found in Belnap [1993] ... ' [34, n. 4].

On the other hand, Gupta [2009] implies that the standard account might be by Ajdukiewicz. He states that one of the present authors, R. Urbaniak, holds this stance. However, at the time, Urbaniak had only remarked on two issues concerning this debate. First, the conditions were to the best of his knowledge not formulated by Leśniewski. Second, as long as the Polish logicians are concerned, Ajdukiewicz studied the conditions. Unlike Gupta, we do not claim unqualified Polish origin of the standard theory; and, as our recent findings suggest, the priority even among the Poles might belong to another logician, Jan Łukasiewicz.

The most exciting period in the development of the folklore is the 1970s. During that time, a discussion over the admissibility of the definition of implication to be found in *Principia Mathematica* raised also a debate over the justification of the ascription of the standard requirements to Leśniewski. This debate appears to be unknown to the later authors on the history of definitions.

For example, neither Belnap [1993], nor Gupta [2009], nor Hodges [2008]

[21] Here Horty claims that although Frege subscribed to the standard requirements, he gave no logical analysis of these conditions.

refer to these papers from the 70s. In [Nemesszeghy and Nemesszeghy 1971], which started the discussion about implication in *PM* (Nemesszeghys claim that, unexpectedly, the *PM* definition of implication is creative) we find an indubitable affirmation of the folklore.[22]

Dudman [1973], in a reply, points out that the attribution is mistaken because of the priority of Frege's writings. Rickey [1975*b*], on the other hand, brings forth strong arguments against the folklore. Rickey criticizes especially [Nemesszeghy and Nemesszeghy 1971], but notes that the essentially same argument can be put forward against [Kelley 1955], and [Suppes 1957]. On all his points pertaining to Leśniewski Rickey does not just rely on his own expertise, but he expresses gratitude to B. Sobociński who '[verified] all of the comments ... about Leśniewski'. This is interesting because Sobociński (who was a student of Leśniewski and, after his teacher's untimely death, one of the main contributors in the study of Leśniewski's systems alongside with Lejewski) took care of Leśniewski's *Nachlass* from 1939 until it was lost around 1944. [See Simons 2008].

The negative part of Rickey's argument can be reconstructed as follows: he points at (a) the priority of Galileo Galilei on the notion of eliminability of definitions[23] and at (b) the priority of Pascal and Mill on the notion of non-creativity; he also claims that (c) there is no manuscript, paper or book in Leśniewski's oeuvre where these requirements are mentioned as *the criteria*; and that (d) in his theories Leśniewski utilized creative definitions freely.

Hodges [2008: 104] attacks the folklore, as expressed in [Suppes 1957], by independently formulating an argument similar to Rickey's reasoning. Hodges comments in a short passage on (a) Pascal's and Porphyry's prior discussion of eliminability, on (b) the priority of Frege regarding non-creativity, on (c) the absence of textual evidence, and on (d) the fact that Leśniewski endorsed creative definitions. Hodges also claims that 'Leśniewski probably had no general theory of definitions', i.e. that Leśniewski treated definitions in a piecemeal manner, in one deductive system at a time. Nothing in our findings contradicts this statement.

Rickey in addition lays out a summary of Leśniewski's positive achievements pertaining to definitions:[24] (1) Leśniewski showed that definitions can be used on the object language level, and that the symbol '$=_{df}$' is thus superfluous; and (2) '[s]ince definitions are in the object language Leśniewski realized — and this is a valuable contribution — that it is necessary to have rules for introducing

[22] Even though Nemesszeghy and Nemesszeghy do not credit Suppes, they seem to be following him. Here is the quotation from [Nemesszeghy and Nemesszeghy 1971] for comparison with the above citation from [Suppes 1957]: "The idea that definitions should not strengthen the theory in any significant way finds expression in the following two criteria first formulated by the Polish logician S. Leśniewski: (1) a defined symbol should be always eliminable, (2) a definition should not permit the proof of previously unprovable relationships among the old symbols."

[23] Rickey cites [Galilei 2001: 28] where 'mathematical definitions' are described as 'abbreviations'.

[24] He restates these contributions in more detail in [1975*a*]. We treat these issues in section 7.

definitions.' [176]. Rickey notes that the rules Leśniewski placed on definitions in [1931*b*] ascertained eliminability and consistency.

He concludes that when Leśniewski developed his rules '[c]reative definitions were neither defined nor discussed ... However, some of the definitions introducible according to that rule are creative (relative to the particular axiom system chosen)'. [Rickey 1975*b*: 176]. Notwithstanding the clarity of his expression here, Rickey's critique seems to have been mostly unnoticed or forgotten: as far as we know, the only ones who reacted to this criticism were Nemesszeghy and Nemesszeghy [1977]. And they argued against it.

Nemesszeghy and Nemesszeghy presented the basis of their argument already in an earlier reply to Dudman.[25] In their [1973] they write that in the previous paper they 'did *not* attribute to Leśniewski the view that all definitions should satisfy the criteria'.[26] They admit that 'Leśniewski used definitions which satisfied [eliminability] but not [non-creativity]'. On the contrary, they continue to point out that they only meant 'that the idea that definitions should not strengthen the theory in any significant way finds expression in those two criteria of Leśniewski'. In other words, the argument of Nemesszeghy and Nemesszeghy implies the exegetical difference that we mentioned in the introduction, between following the requirements and studying them; and they credit Leśniewski with the latter.[27]

It is just this difference that Nemesszeghy and Nemesszeghy [1977: 111–112] accuse Rickey of "fusing and confusing" and they reiterate that they 'did think, and still think, that one can truly hold' that the '[c]onditions ... can be attributed to Leśniewski.' Nemesszeghy and Nemesszeghy base this conviction on the fact that Leśniewski 'was the first, at least in modern times, to discuss and use definitions that play a creative role' which, according to them, would not have been possible without good knowledge of the eliminability and non-creativity properties as a tool of measure: 'he had a clear idea of the distinction between "creative role" and a "mere abbreviative role" of a definition, which finds expression in [the] conditions'.

Here (unlike in [1971] or [1973]) they provide a citation to justify their claim – page 50 in [Leśniewski 1929] (that is, p. 459 in the English translation).[28] First of all, we don't think this reference supports the claim but because some complexities are involved (and because the citation is of independent interest, e.g. it is closely connected to Tarski's early results in logic) we discuss this quotation separately in section 9.

Secondly, as we argue in section 7, the only meta-theoretical principles that Leśniewski mentions as guiding his position on definitions were consistency and

[25]It appears unlikely that Rickey had read [Nemesszeghy and Nemesszeghy 1973] before writing [Rickey 1975*b*].

[26]They are correct in this, though the opposite reading is natural, too: see n. 22 above.

[27]In comparison with our discussion on [Suppes 1957] in section 3 it seems that Nemesszeghys deny the claim (b) but affirm claim (a) presented there.

[28]The Nemesszeghys give the citation in German and thank Owen Le Blanc for pointing it out to them. We will give it in English. The same reference (without a page number) is in [Jurcic 1987: 198]. He states that 'Leśniewski [1929] first formulated the rules of definition and the requirements of eliminability and noncreativity.'

the avoidance of meta-linguistic treatment of definitions; therefore it seems that he was interested in the correct syntactic form of definitions only. Finally, there is evidence, which we discuss in section 8, that even though it was Leśniewski's work on definitions which prompted the study of the standard requirements in Poland it was Łukasiewicz who noticed the importance of the creativity of definitions, and that (as far as we know) it was Ajdukiewicz who first studied the conditions systematically (at least in Poland).

We think that the Nemesszeghys attribution of the standard requirements to Leśniewski, even only as a measure on definitions, is an overstatement. His role in the development of theory of definitions is important, but to claim on his behalf the invention of the standard conditions just because he may have been aware of them is to stretch the facts. Besides, Frege has technically at least as good a claim for them as Leśniewski, and Frege has priority. We know that Leśniewski was well aware of Frege's published writings because they were well known among the Poles during the early decades of 20th century. According to Woleński [2004], Frege's ideas were frequently discussed by Łukasiewicz and Leśniewski. Woleński even remarks that '[i]n fact, Leśniewski's ... work on definitions ... is a continuation and extension of Frege's work' [45]. On Woleński's view, the main difference between Frege and Leśniewski is that while the former required non-creativity, the latter allowed creative definitions within his systems and thus, to avoid trouble, needed exact rules to govern them.

To whom the standard conditions finally will be attributed remains to be decided since we still know too little of the overall picture. It is quite possible that as potential *desiderata* on good definitions they formed ideas that were "in the air", and that they were, hence, common property.

To sum up, even though the folklore about Leśniewski and the requirements is dying away in the tradition following Belnap [1993], one can still run into people attributing the restrictions to Leśniewski. It seems also that the argument in [Nemesszeghy and Nemesszeghy 1977] has not been contested before. This paper is meant as a *coup de grâce* to all such allegations. As we have indicated above, we will proceed through several steps. First we will explain why definitions in Leśniewski's systems are creative. Then we will look at Leśniewski's rules of definitions, and explain what they actually said: we will show that eliminability and conservativeness as the criteria (or a measure) of definitions are not to be found there. We will argue that, at least on Polish grounds, the study of these criteria should be credited to Łukasiewicz and Ajdukiewicz. Finally, we will reconsider Nemesszeghys' claim.

6 The creativity of Leśniewski's definitions

Now we will turn to Leśniewski's systems, and explain why some definitions in these systems are creative. To start with, definitions for Leśniewski are not meta-linguistic abbreviations; rather, he treats them as axioms (of a specific kind) formulated in the language of the system itself. The introduction of these definitions is to be governed by what he calls 'rules of definitions'.

Since our goal is only to achieve a basic grasp of Leśniewski's ideas, and to explain why definitions in his system are creative, we can put many technical details aside and look at a rather simple example of definitions of name constants in a subsystem of Leśniewski's *Ontology*.[29] Roughly speaking, Ontology is a free version of higher-order logic with arbitrary finite types; the subsystem we will be looking at falls within the ballpark of monadic second-order logic.

Consider a language with only one type of variables $a, b, c, \ldots$ These variables, intuitively speaking, are place-holders for empty names, for individual terms, or for names referring to more than one object; in general, they can be thought of as admissible substituents for countable noun phrases. This language contains also classical Boolean connectives, quantifiers for binding the name variables, and the predication copula ε which forms an atomic formula when flanked with two variables (e.g. '$a\,\varepsilon\,b$').

For heuristic and theoretical reasons it is useful to consider a semantics for this language. There are many ways to give the language of Ontology an interpretation, all of which go beyond Leśniewski. Since Leśniewski developed his systems in the pre-model-theoretic era, he gives his languages no semantics in the modern sense. Most likely, given his strong nominalist inclinations, he would be against using set theoretic semantics. For our purpose, however, we can ignore these issues.

One quite obvious way to proceed is the following: an interpretation is a set of objects D with a function v that maps name variables into the powerset of D (i.e. v assigns subsets of D to name variables). An atomic formula $a\,\varepsilon\,b$, read as 'a is one of the b's' or 'a is b', is satisfied by $\langle D, v\rangle$ iff $v(a)$ is a singleton and a (not necessarily proper) subset of $v(b)$. The satisfaction clauses for complex formulas are straightforward (since the range of variables is just the powerset of D). (Another way would be to provide a Henkin-style semantics for the language. This semantics might sit slightly better with the way the system is set up; and the completeness result for a variant of Ontology with respect to such semantics is available [Stachniak 1981]. Nevertheless, these issues are inessential for our current considerations.)

Originally, Ontology is set up in purely syntactical terms. Inessential details aside, the system contains a specific axiom ruling the behavior of ε, which, in its 1920 version, states:

$$\forall a, b\,[a\,\varepsilon\,b \equiv \exists c\,(c\,\varepsilon\,a) \wedge \forall c, d\,(c\,\varepsilon\,a \wedge d\,\varepsilon\,a \to c\,\varepsilon\,d) \wedge \forall c\,(c\,\varepsilon\,a \to c\,\varepsilon\,b)].$$

The intuitive reading of this axiom would be: a is one of the b's iff (1) a is non-empty, (2) at most one object is a, and (3) the only object which is a is also one of the b's. On top of this axiom, the system contains a set of axioms and rules that, in effect, add up to the same as the rules of natural deduction governing the Boolean connectives, the standard rules for quantifiers, the rules for extensionality, and the rules of definitions for introducing constants (in the full system: constants of arbitrary types; in our toy system: name constants).

[29] See [Leśniewski 1930].

Since we are looking only at a reduced version of the language, the only rules involved are (a) the classical rules for Boolean connectives, (b) standard rules for quantifiers, (c) the extensionality rule which allows to infer $\phi(a) \equiv \phi(b)$ from $\forall c\,(c\,\varepsilon\, a \equiv c\,\varepsilon\, b)$ for any formula ϕ, and (d) the rule of definition for name constants which says that a new constant γ can be introduced by means of a formula:

$$\forall a\,[a\,\varepsilon\,\gamma \equiv a\,\varepsilon\, a \wedge \phi(a)] \tag{1}$$

where $\phi(a)$ is a formula in the language of the system not containing any free variables other than a or other defined constants. (Strictly speaking, Leśniewski allowed defined constants to occur in defining conditions. He did this on the condition that the order in which constants have been introduced is maintained so that the definitions involved contain no circularity. We do not need this level of detail.)

The presence of $a\,\varepsilon\, a$ on the right-hand side might at first seem slightly surprising; the underlying idea here is that since the left-hand side assumes that a "is a singular term", the right-hand side has to do the same, and '$a\,\varepsilon\, a$' expresses exactly this statement (because no distinction between singular terms and other terms is built into the syntax, $a\,\varepsilon\, a$ has to be explicitly stated).

Thus, for instance, a formula that *prima facie* looks like a definition of Russell's class:

$$\forall a\,[a\,\varepsilon\,\lambda \equiv \neg a\,\varepsilon\, a] \tag{2}$$

is inadmissible as a definition; it is not a theorem of Ontology either, and it easily leads to contradiction since it entails $\lambda\,\varepsilon\,\lambda \equiv \neg\lambda\,\varepsilon\,\lambda$. Rather, the correct definition would be:

$$\forall a\,[a\,\varepsilon\,\lambda \equiv a\,\varepsilon\, a \wedge \neg a\,\varepsilon\, a] \tag{3}$$

which, since its right-hand side is a contradiction, entails:

$$\neg\exists a\; a\,\varepsilon\,\lambda, \tag{4}$$

which only says that nothing is λ (or that 'λ' doesn't name anything). In fact, any model[30] which assigns the empty set to λ satisfies (3).

A closer examination reveals that (3) says that a is λ iff, first, a is an object, and second, a is not a. This avoids the contradiction because what we get when we substitute λ for a is:

$$\lambda\,\varepsilon\,\lambda \equiv \lambda\,\varepsilon\,\lambda \wedge \neg\lambda\,\varepsilon\,\lambda \tag{5}$$

Since the right-hand side of (5) is a straightforward contradiction, we can simply derive the negation of the left-hand side:

$$\neg\lambda\,\varepsilon\,\lambda \tag{6}$$

[30] We are assuming that a modification of the notion of a model has been made to accommodate the presence of constants. Basically, what is needed is another function c that, given a definition of a new constant γ, assigns a subset of D to the constant in a way that makes the definition satisfied. It is rather clear that, given a set of constants and their definitions, c does not have to be unique.

This, however, does not allow us to infer that $\lambda \,\varepsilon\, \lambda$.

By existential generalization, (4) entails that there is an empty name:

$$\exists b \neg \exists a \, a \,\varepsilon\, b. \tag{7}$$

This consequence, however, essentially relies on the definition of λ and is not provable in a system obtained by deleting the rule of definitions.

To see more clearly why definitions are creative in such a Leśniewskian system, the following comparison would be useful. Consider a system which instead of the rule of definition contains what we may call *definitional comprehension*: for any $\phi(a)$ which satisfies the same conditions as those put on defining conditions occurring in definitions the following is an axiom:

$$\exists b \, \forall a \, [a \,\varepsilon\, b \equiv a \,\varepsilon\, a \wedge \phi(a)].^{31} \tag{8}$$

If we were to add to this system with definitional comprehension a definition (or any number of definitions) formulated in accordance with Leśniewski's schema (1), the obtained definitional extension would be non-creative.[32]

Thus the reason why definitions are creative in the original system is that Ontology, as it stands, lacks definitional comprehension: whenever one wants to prove $\exists b \, \phi(b)$ one has to define a γ, prove $\phi(\gamma)$, and then use existential generalization. (Now we can see what makes a Henkin-style semantics slightly more adequate than the power-set-semantics is the fact that in such semantics one can introduce an existentially quantified statement only if one has defined a constant and proved the claim for that constant. Stachniak [1981] gives a Henkin-style completeness proof for a variant of Ontology which contains definitional comprehension for all categories of constants.)

So in a definition-free system with comprehension (henceforth QNL_{com})[33] all constant-free theorems of the version with definitions but without comprehension (henceforth QNL_{df}) are provable. Does the opposite hold? Can QNL_{df} derive all the constant-free formulas that are derivable in QNL_{com}?[34] The answer is positive. For indeed, since every particular definition in QNL_{df} entails its existential generalization, $QNL_{com} \subseteq QNL_{df}$.

7 What did Leśniewski's rules for definitions actually do?

Leśniewski [1931*b*] set out to give precise rules for definitions for Łukasiewicz's axiomatization of classical propositional logic.[35] Here Leśniewski uses his so-called terminological explanations to describe syntactically the axioms and the

[31] Observe that by adding axioms of the form $\exists b \, \forall a \, (a \,\varepsilon\, b \equiv \phi(a))$ we would be able to derive the Russellian contradiction if we take $\phi(a)$ to be $\neg a \,\varepsilon\, a$.

[32] See Stachniak [1981] for a proof of a theorem from which our claim follows.

[33] QNL_{com} is an abbreviation of "**Q**uantified **N**ame **L**ogic with **com**prehension". A certain variant of QNL and the question whether any set of QNL formulas can determine the standard interpretation of the epsilon operator with respect to set-theoretic standard semantics is discussed (and answered negatively) in [Urbaniak 2009].

[34] This question is not addressed in [Stachniak 1981].

[35] [Leśniewski 1931*b*] is a summary of the lectures that Leśniewski gave in Warsaw in 1930-1931. See **text 4** in the appendix for Leśniewski's comment about it.

admissible rules of inference, including the rules of definitions. This syntactical focus makes his line of pursuit essentially different from Ajdukiewicz's strategy (which is discussed in the next section).

Crucial for Leśniewski's approach to definitions is his Terminological Explanation XI where he states what shape a definition is supposed to have. This explanation boils down to the requirement that a definition should be a formula of the form:

$$\neg[(\gamma(\alpha_1,\ldots,\alpha_n)\rightarrow\phi(\alpha_1,\ldots,\alpha_n))\rightarrow\neg(\phi(\alpha_1,\ldots,\alpha_n)\rightarrow\gamma(\alpha_1,\ldots,\alpha_n))],$$

which is just a roundabout way of using negation and implication (which are primitive in the system) to express the equivalence:

$$\gamma(\alpha_1,\ldots,\alpha_n)\equiv\phi(\alpha_1,\ldots,\alpha_n)$$

where γ is a constant symbol being defined, $\alpha_1,\ldots,\alpha_n$ are all different propositional variables, ϕ contains only primitive (or previously defined) symbols, and the formulas contain only the explicitly mentioned variables.[36]

To get a clear picture of what is going on here we need only to look further at the last (twelfth) terminological explanation and the conclusion in [Leśniewski 1931*b*]. Terminological Explanation XII says 'of [an] object A that it is a definition, relative to C if and only if A is a definition of some expression, relative to C, by means of some expression, and with respect to some expression' [647]. This, when translated from *Leśniewskese*, basically states that a formula is a definition at a certain stage of development of a system if there is an expression of which it is a correct definition at that stage.

Leśniewski concludes the paper with a claim that a formula can be added to the system only if it is a consequence by substitution of previously proven theses, or it is a consequence by detachment of previously proven theses, or it is a correctly added definition. In general, Leśniewski's terminological explanations are meant to define the syntactic relation of derivability. They do not *employ* the notion itself and, *a fortiori*, they say nothing about conservativeness. (This point holds for his treatment of definitions for all systems he considers).

At this point, it should be clear what Leśniewski set out to do and what he achieved. Using a rather idiosyncratic semi-formalized metalanguage, without any reference to eliminability or non-creativity, he provided a meticulous description of what syntactic form definitions should have. Definitions that satisfy his rules have consistency and eliminability properties. Yet he presented no proofs to this end: consistency is only mentioned in passing when Leśniewski says that the reason behind his rules is to ensure the consistency of the system and eliminability is just an unmentioned side-effect of the rules. Ajdukiewicz, on the other hand, instead of focusing on the syntactical form of definitions, tries to work out a more general motivation for them, suggesting that the syntactic restrictions result from certain more general meta-theoretical requirements.

[36] In Leśniewskianese this does have its bells and whistles and sounds a bit more complicated, see **text 5** in the appendix.

8 Łukasiewicz and Ajdukiewicz on definitions

On the Polish ground we can find some remarks pertaining to general meta-theoretical constraints on definitions in Jan Łukasiewicz's work, and a rather elaborate discussion in Kazimierz Ajdukiewicz's lecture scripts.

Łukasiewicz [1929],[37] in a rather short passage about definitions in propositional logic (in his logic course materials) remarks that substitution of defined terms should preserve the truth-value of sentences, and that addition of definitions should not make new expressions formulated in the original language provable:[38]

> Sharing the view of the authors of Principia Mathematica I hold that definitions are theoretically superfluous. If we have a theory in which definitions do not appear at all, nothing new should be obtainable in that theory after we introduce definitions. [Łukasiewicz 1929: 52]

Alas, he also remarks that these issues do not belong to a general course in logic, and does not elaborate.

He, however, does not credit himself with the formulation of these criteria. In his introduction to this script he explicitly lists what he thinks the results he can claim are. He mentions (i) his bracket-free notation for propositional logic and Aristotle's syllogistic, (ii) his axiomatization of propositional logic, (iii) his way of writing down proofs in the systems in question and some of the proofs, (iv) his remarks about deduction (which do not pertain to definitions), (v) his systems of many-valued logics, (vi) his completeness proof for propositional logic, (vii) his axiomatization of Aristotle's syllogistic, and (viii) some historical remarks about Aristotle, Stoics, Frege, Origen and Sextus. He notes that he could also claim (ix) his consistency proof for propositional logic and (x) his style of independence proofs, but those were independently invented by Post and Bernays. Most notably, he does not mention the restrictions on definitions, and he explicitly remarks: "Apart from the above-mentioned points, whatever can be found in the lectures, is not my property." [Łukasiewicz 1929: vii]. He also observes that he owes a lot to discussions with his colleagues and their students and that there are many results he simply cannot correctly attribute.[39]

Some new light can be cast on the history of the conditions when we look a few years earlier at the reports from the meetings of the Polish Philosophical Society that can be found in the 1928-1929 volume of *Ruch Filozoficzny* (*Philosophical Movement*). Łukasiewicz [1928*a*] describes a talk he gave at a plenary session of the Society on March 24, 1928, titled *The role of definitions in deductive systems.* There, he opposes to the idea that definitions should be interpreted as theorems of a given system and suggests that they rather should be interpreted meta-linguistically as abbreviations. The reason he presents is that if the former path is chosen, new theorems formulated in the language

[37] As we mentioned in section 3, [Łukasiewicz 1929] was later published in English as [Łukasiewicz 1963].

[38] See **text 6** for a full passage.

[39] See **text 7** in the appendix.

devoid of definitions can become provable.[40] This, however, is not the whole story.

Only a few weeks before this talk, Łukasiewicz presented a more elaborate lecture in the Logic Section of the Society, titled "About definitions in theories of deduction".[41] Although Łukasiewicz does not say this, it is quite possible that he is attacking the views of Leśniewski who having criticized the meta-theoretical treatment of theorems and definitions in *Principia Mathematica*[42] decided to treat definitions intra-theoretically. Łukasiewicz starts off by presenting the opposition between two ways of interpreting definitions — as meta-theoretical abbreviations introduced by means of rules, and as theorems formulated within the system. Then he sketches two examples of creative definitions (in a propositional language) formulated using the latter, intra-theoretical approach (the creativity of one of them has been proven by Wajsberg, and of the other by Łukasiewicz). Łukasiewicz's main point is that we should interpret definitions meta-theoretically since their creativity, which clearly can take place if definitions are interpreted intra-theoretically, is undesirable.

Łukasiewicz mentions then that Professor Leśniewski participated in the discussion insisting that 'in [his] Ontology definitions lead to theses independent of the axioms; this is not a vice; quite the contrary: if one adds definitions, creative is exactly what they should be.' [178]

A few points seem worth bringing up. First, Łukasiewicz's method of finding independence proofs plays a key role here. (Roughly, for a propositional language the strategy is that if one wants to show that given a certain Hilbert-style proof system a formula ϕ is independent of a premise set Γ, one has to find some many-valued characterization of the connectives occurring in the language, on which all assumptions in Γ have one of the chosen values, inference rules preserve chosen value(s), and yet ϕ does not have a chosen value.) Indeed, to prove that a definition is creative in a certain system one not only has to establish that with this definition one can prove a certain formula, but also that this formula is not provable in the system itself, i.e. that it is *independent* of the original axioms.

Second, it is still rather unclear what role creativity plays in arguments against the object-language treatment of definitions. What Łukasiewicz seems to have proven is that if certain definitions are accepted as theorems, they are creative. But even if one values non-creativity, to turn this into an argument against the object-language treatment of definitions, one also has to show that once one switches to the meta-linguistic treatment of definitions, non-creativity vanishes. This however, *prima facie*, seems unlikely: if you can prove a new χ with a theorem $\phi \equiv \psi$, you are also able to prove the same χ with a meta-theoretical rule that captures this equivalence. Thus it appears probable that (at least in some contexts) the distinction between the creative and the conservative cuts across the one between the intra-theoretic and the meta-theoretical.

[40] see **text 8** in the appendix.

[41] A report on this talk [1928*b*] (written by Łukasiewicz himself) appeared also in the same volume as the other report.

[42] See [Urbaniak 2008: 85-92] for details.

Third, the debate emphasizing the importance of non-creativity seems to stem from Leśniewski's view of definitions as theorems, even if it was not Leśniewski who formulated the requirement: this at least partly explains what Leśniewski's impact on these matters was and why his name came to be connected with the standard requirements. Łukasiewicz's report also constitutes evidence for the claim that once Leśniewski was faced with the non-creativity requirement he rejected it.

Now we may turn to Ajdukiewicz's contribution. Ajdukiewicz discusses translatability (which is, *mutatis mutandis*, the same as eliminability) and consistency requirements in a lecture script (in Polish) which dates back to 1928. Ajdukiewicz used these notes when he was teaching in Warsaw. (Those parts of [1928] that pertain to definitions have been published in Polish in 1960.)

Furthermore, translatability, consistency and conservativeness requirements are discussed in a paper Ajdukiewicz [1936] gave later in Paris.[43] As it will turn out by the end of this section, Ajdukiewicz not only mentions these rules, but also provides some proofs concerning them.

In the introduction to a collection of his papers, around thirty years after having written the script, Ajdukiewicz [1960: v-vi] explains his motivation behind the 1928 and 1936 papers by saying that he tried to understand what the ultimate goal of structural rules for definitions are. He contrast his own approach to Leśniewski's rules which were purely syntactical.[44]

Ajdukiewicz observes what we have already learned in the previous sections: Leśniewski was not concerned with general conditions on definitions formulated in terms of derivability, but rather with the project of defining derivability in terms of syntactic relations, which includes a purely syntactic description of what a definition should look like. Ajdukiewicz himself, however, was after more general conditions: those directly related to the purpose a definition should serve in a system.

[43] In the 1956 edition of Tarski's *Logic, Semantics, and Metamathematics*, in the translation of [Tarski 1934], 'consistency and re-translatability' are mentioned as 'the conditions for a correct definition' [307, n. 3]. At the same page of the 1983 edition the criteria are 'non-creativity and eliminability'. This puzzling fact is noticed by Hodges [2008]. Paolo Mancosu pointed out an interesting passage concerning the second edition, written by Tarski in his correspondence with Corcoran:

> Replace "re-translatability" by "non-creativity". [I do not explain the meaning of the term "non-creativity" for the same reason why I did not explain before the meaning of "re-translatability". I have never intended to make LSM a self-contained work. By the way, re-translatability is a stronger property than "non-creativity". In old times it was frequently used in discussing definitions. It seems that now "non-creativity" is more fashionable.]

Tarski might have been alluding to the work done in the theory of definability back in the 1960s and 70s. One of the results was that (supposing consistency) the eliminability of a term is a necessary and sufficient condition for its explicit definability within the given theory, whereas conservativeness is only sufficient: some weaker forms of definability imply the latter property as well [See Rantala 1977: 179-185]. But for some reason the 1983 footnote does not read 'consistency and non-creativity', which would have been the weaker criteria.

[44] See **text 10** in the appendix for Ajdukiewicz's own formulation.

In the 1928 script, which is titled *The main principles of methodology of sciences and formal logic* (*Główne zasady metodologii nauk i logiki formalnej*), Ajdukiewicz first introduces the notion of being a meaningful expression relative to (a background theory consisting of) sentences Z: An expression W is, in this sense, meaningful if it contains only such constants which are equiform with one of the constants occurring in Z, and if its syntax obeys the syntax of Z. [Par. 12, p. 45].

The notion of a meaningful expression is used in paragraph 19 titled *Rules of Definitions* (*Dyrektywy definiowania*) to introduce the requirements of translatability and consistency, pretty much as we know them.[45] Say we extend the language of Z, J_Z, i.e. 'the set of sentences meaningful relative to Z', to a new language J_{Z+D} by using a definition D to introduce an expression δ, which is new relative to Z. The first condition Ajdukiewicz introduces is translatability which requires that any sentence in J_{Z+D} should (*modulo* accepted inference rules and theory $Z + D$) be inferentially equivalent to a sentence in J_Z. The second condition he introduces is consistency: if Z was consistent (relative to given inference rules which are kept fixed) then $Z + D$ also has to be consistent. [Ajdukiewicz 1928: 46-47].

Non-creativity, even though not mentioned in 1928, receives attention only a few years later. Ajdukiewicz [1936: 244], after pretty much repeating his previous formulation of translatability and consistency requirements, remarks that...

> ... Often, but not always, one also wishes the rules of definitions to exclude creative definitions. A definition is called creative on the grounds of a certain language, if from the theses of that language according to the rules of deduction by means of that definition it is possible to derive a sentence of that language (and so, not containing the defined term), which cannot be derived without that definition.[46]

In other words he claims that sometimes one requires also that no formulas of the old language which were not theorems of the original theory become derivable in the theory obtained by adding a definition. This is exactly the non-creativity mentioned e.g. by Suppes [1957: 153],[47] by Łukasiewicz (see above), and already by Frege:

> In fact it is not possible to prove something new from a definition alone that would be unprovable without it. When something that looks like a definition really makes it possible to prove something which could not be proved before, then it is no mere definition but must conceal something which would have either to be proved as a theorem or accepted as an axiom. [Frege 1914: 208][48]

[45] Since these passages are not well known, we will cite them *in extenso* as **text 11** in the appendix.
[46] Polish original is **text 12** in the appendix.
[47] See the quotation in section 3.
[48] See **text 1** in the appendix for the longer passage.

Although it is unlikely that Ajdukiewicz independently reinvented the conditions of eliminability and conservativeness (as they are called nowadays), what sets him apart from his predecessors and contemporaries is his genuinely meta-meta-theoretical treatment of these criteria.[49] He, for instance, not only mentions non-creativity in [1936: 245-246], but also attempts to find formal conditions whose satisfaction by a system guarantees the non-creativity of definitions within the system. He assumes the consistency requirement and argues that if the rules of inference cannot distinguish between constants, definitions are non-creative on a rather straightforward condition of irrelevancy of the defined terms for the language's inference rules (i.e. that derivability is preserved under uniform substitution).[50]

Given this assumption of "irrelevancy", the argument that the consistency requirement is (in such a setting) sufficient for non-creativity becomes rather straightforward. Suppose a definition D of a word W leads to inconsistency with premises Z, where formulas in Z do not contain W. Then we can with a substitution of D, which instead of W contains an expression of the same category but occurring already in Z, derive a contradiction just from Z as well. So for D to satisfy the consistency requirement it is sufficient that Z derives an instance of the definition which is constructed within the language of Z. Of course, if the consistency requirement is not satisfied then definitions will be creative, allowing for the derivation of $\perp$ which was underivable in the system devoid of definitions.

Ajdukiewicz observes that a similar reasoning applies to creativity. If a W-free formula ϕ is to be derivable from D with Z, then if Z already proves a W-free instance of D then Z also proves ϕ.

It should be clear how this applies to the creativity of Leśniewski's definitions. The condition sufficient for non-creativity discussed by Ajdukiewicz requires that no constants should be distinguished in the system. In QNL_{df} it is clearly violated: it is possible to derive expression (3) from section 6, that is:

$$\forall a\, [a \,\varepsilon\, \lambda \equiv a \,\varepsilon\, a \wedge \neg a \,\varepsilon\, a]$$

but (without the definition itself) it is neither possible to derive an instance of it:

$$\forall a\, [a \,\varepsilon\, b \equiv a \,\varepsilon\, a \wedge \neg a \,\varepsilon\, a]$$

nor its existential generalization:

$$\exists b\, \forall a\, [a \,\varepsilon\, b \equiv a \,\varepsilon\, a \wedge \neg a \,\varepsilon\, a].$$

In a sense, the whole point of introducing a definition in QNL_{df} is to distinguish one constant and to be able to prove about it something not provable about any

[49]For example, we do not find such approach in [Frege 1914], [Łukasiewicz 1929] or [Suppes 1957]. More recently, however, Ajdukiewicz's interest in the criteria reappear independently in e.g. [Belnap 1993] and [Došen and Schroeder-Heister 1985]. Yet, since the history of the standard theory remains still unwritten, the study of little-known sources can, as our discussion here demonstrates, reveal some surprises.

[50]See Ajdukiewicz's own formulation in **text 13** in the appendix.

other constant. Also, in a way, it is the idea that all we need is instances of definitions that stands behind the move to QNL_{com} and the conservativeness of definitions in that system.

Later Ajdukiewicz [1958] presented a sketch of what he called 'a general theory of definitions' which would treat about all types of definitions: real, nominal, and conventional, and which would reveal the logical relations between these classes of definitions. On both of these tasks the criteria of translatability and non-creativity (latter in a disguise of a need to proof existence) are utilized.[51]

To sum up, Łukasiewicz and Ajdukiewicz in the 20s and 30s both mention and use the consistency, translatability, and conservativeness requirements for definitions. Ajdukiewicz studied them in more detail and indicated one source of the creativity of definitions in some systems: the fact that certain constants are in such systems, so to speak, distinguished.

9 A look at the Nemesszeghys's argument

Recall that Nemesszeghy and Nemesszeghy [1977: 111,112] insisted that a citation from [Leśniewski 1929] supports their claim that Leśniewski was the first person in Lvov-Warsaw school who had a clear concept of non-creativeness of definitions. Let us take a look at the quote itself:

> A system of Protothetic can be constructed on the basis of a single axiom if, besides directives (α_1), (β_1), (γ_1) and ($\eta_1^\star$) of SS4, two definition directives ($\delta_1^\star$) and and ($\epsilon_1^\star$) are adopted which introduce definitions not constructed as would be expected from directives (δ_1) and (ϵ_1) of SS4, but instead two mutually reciprocal conditional propositions representing, therefore, the one corresponding equivalence (directive ($\delta_1^\star$)), and two such conditional propositions with their preceding universal quantifier (directive ($\epsilon_1^\star$)) - Axiom 2 of the system (just given in A) can serve as an example of an axiom of this kind.[52] [Leśniewski 1929: 459]

Notice first that Leśniewski mentions no meta-theoretical requirements in this passage. It gives no *explicit* reason to think that Leśniewski formulated the requirement. Why would anyone think that this citation supports the Leśniewski attribution? *Prima facie*, one can reason as follows:

[51] Ajdukiewicz's general theory could be more fruitful theoretical basis for a practical account of definitions used in computer science and network technologies than, for instance, that of Robinson [1954], whose system is developed further in [Cregan 2005].

[52] The same quotation on p. 50 of the German original goes: 'Man kann ein System der Protothetik auf Grund eines einzigen Axioms aufbauen wenn man neben den Direktiven $\alpha_1, \beta_1, \gamma_1, \eta_1^\star$ des Systems Σ_4, die zwei Definitions direktive – $\delta_1^\star$, und $\epsilon_1^\star$ – annimmt, die statt der Definitionen, welche auf eine in den Direktiven δ_1, und ϵ_1, des Systems Σ_4 vorhergesehene Wiese konstruiert sind, Definitionen in Gestalt zweier einander reziproker und deshalb die entsprechende eine Äquivalenz vertretender Konditionalsätze mit den ihnen vorangehended universalen Quantifikatoren (Direktive $\epsilon_1^\star$) einführen. Als ein Axiom dieser Art kann z.B. das Axiom 2 das sub A erwähnten Systems gelten.'

> Leśniewski observes (although, strictly speaking, it was Tarski who proved this in 1922) that if we take one formulation of Protothetic based on two axioms,[53] throw out its rules of definitions and replace them with other rules of definition, then only one axiom will suffice to generate the same set of theorems. Surely this means that Leśniewski almost explicitly says that definitions can be creative.

As we have seen in section 8, Leśniewski (at least after hearing Łukasiewicz's talk) was aware of the creativity of his definitions. A separate question is, however, whether the passage quoted above suggests that he was aware of it when describing Tarski's result from 1922. The first problem in the reading of Nemesszeghy and Nemesszeghy of [Leśniewski 1929: 459] is that the passage does not even *implicitly* contain a statement of creativity: whereas the citation indicates that one axiom can be dropped when the rules of definitions are changed, no proof is given that this axiom was independent of the others in the original system to start with (indeed, this would require Łukasiewicz-style, or some other, underivability proofs which were not around in 1922).

But even if such proofs were provided, it would still take a real distortion of the passage to make it count as the statement of creativity of definitions. To see why, we have to fit together a few pieces of the puzzle and figure out what exactly is being said in the text.

First, we have to know what rules are involved. SS4 in a sense is constructed in contrast with SS3. The main difference between SS3 and SS4 is that whereas SS3 is a system whose only primitive symbols are quantifiers and material equivalence, the primitive symbols of SS4 are quantifiers and material implication. The original system SS4 can be based on two axioms:

Axiom 1	$\forall p, q\,[p \to (q \to p)]$
Axiom 2	$\forall p, q, r, f\,[f(r,p) \to (f(r,(p \to \forall s\, s)) \to f(r,q))]$

The first axiom is fairly straightforward. The intuition behind the second one is this: Suppose you do not want to have negation among your primitive symbols, but you have propositional quantification (in extensional context) and material implication available. How do you define negation?[54] Observe that $\forall s\, s$ is just a different way to express $\perp$ and consider writing $p \to \forall s\, s$ instead of $\neg p$. (The reasoning goes like this: If p is false, then any implication $p \to \phi$ will be true, so $p \to \forall s\, s$ will be true. So if $\neg p$ is true, $p \to \forall s\, s$ is also true. If $\neg p$ is false, p is true, and then $p \to \forall s\, s$ is false, for it says $p \to \perp$. So $p \to \forall s\, s$ expresses the negation of p.) Thus Axiom 2 says:

$$\forall p, q, r, f\,[f(r,p) \to (f(r,\neg p) \to f(r,q))],$$

[53] Protothetic is a system that results from generalizing propositional calculus by introducing quantifiers binding propositional variables, variables representing various connectives, quantifiers binding such variables, etc. See chapter 3 of [Urbaniak 2008] for more details.

[54] This question, for a language with equivalence as the only primitive symbol was answered by Tarski in his dissertation [Tarski 1923].

which, basically, means: take any 2-place connective f and any r, if both $f(r,p)$ and $f(r,\neg p)$ then for any q, $f(r,q)$.

Apart from these axioms, SS4 involved a few rules of inference: α_1, detachment for implication, to be contrasted with rule α of SS3 – detachment for equivalence; β_1, the rule of substitution and quantifier elimination, pretty much the same as in SS3; γ_1, distribution of universal quantifier over a conditional, to be contrasted with γ which distributed the quantifier over equivalence in SS3; and $\eta_1^\star$, the rule of higher-order extensionality for implication, corresponding to $\eta^\star$ – extensionality for equivalence in SS3.[55]

Now, the original SS3-rules of definitions, δ and ϵ, stated the proper form of definitions for propositional constants (δ) and other expressions (ϵ) using material equivalence. So, for instance, rule δ of SS3 says that $\tau \equiv \chi$ is a definition of a new propositional constant τ if χ is a wff with no free variables. When we move to SS4 we have to replace these rules with corresponding rules for implication. In the original system SS4 each definition has to be a single formula. How do you express equivalence in a single formula using implication and quantification only? Well, if we had negation and wanted to express the equivalence of ϕ and ψ by means of implication we could say:

$$\neg((\phi \to \psi) \to \neg(\psi \to \phi)).$$

Now, we already know how to eliminate negation using universal quantification. First we get:

$$\neg((\phi \to \psi) \to ((\psi \to \phi) \to \forall\sigma\, \sigma)),$$

and then:

$$((\phi \to \psi) \to ((\psi \to \phi) \to \forall\sigma\, \sigma)) \to \forall\sigma\, \sigma.$$

Formulas of the last sort (with slight simplifications resulting from moving quantifiers around) were used in SS4 instead of equivalences described by rules δ and ϵ of SS3. So, for instance, δ_1 of SS4 said that a definition of a new propositional constant τ has the form of:

$$[\text{Def } \delta_1] \quad ((\tau \to \chi) \to ((\chi \to \tau) \to \forall\sigma\, \sigma)) \to \forall\sigma\, \sigma$$

where χ is a closed formula (minor issues related to the positioning of quantifiers aside).

Such formulas seem a tad complicated when compared with usual equivalences and it is not really obvious how to deduce both $\phi \to \psi$ and $\psi \to \phi$ from them. Thus, in a sense, it is not clear how application of definition can be performed by mere use of a detachment rule. Indeed, as Leśniewski's lecture notes in logic (taken by Choynowski) confirm,[56] this forms a special case of a more general problem that Tarski run into:

[55] The rule of extensionality is quite interesting in itself, but its full formulation is inessential for our purposes.

[56] By the way, there is no mention of creativity of definitions in these lecture notes dating back to 1932-1935.

> The question of the simplification of na axiom-system of protothetic by joining axioms into the 'logical product' was posed by Dr. Tarski. His first attempt to join four implicational axioms into one was unsuccessful. The problem was how to deduce P and Q from $\forall r\,((P \to (Q \to r) \to r))$ the 'logical product' of P and Q. [Srzednicki and Stachniak 1988: 25]

Tarski was looking at a formulation of SS4 which employed four axioms and asked a simple question: *why don't we use one axiom, a big conjunction of those?* The problem was that the language had no conjunction and once you use implication and universal quantification to express conjunction the resulting formula[57] gets a bit convoluted and you have hard time deriving the conjuncts. First, he solved the problem by using Axiom 1 (to assist him in deriving the conjuncts) and the conjunction of all other axioms.

Tarski's next idea (discussed in the Leśniewski quote) was to simplify further. The essential kind of conjunction elimination you need is the one where you have a formula of the kind of [Def δ_1] and want to derive $\chi \to \tau$ and $\tau \to \chi$ (for instance, when you wish to exchange a definiens with its definiendum in a proof). Tarski decided to circumvent this difficulty by replacing formulas of the kind of [Def δ_1] with two separate implications: $\chi \to \tau$ and $\tau \to \chi$, dropping Axiom 1, and saying that each definition is composed of two formulas:

> Next, Dr. Tarski established that the above axiom of verification [our Axiom 2] can serve as the only axiom of a system of implicational protothetic. It was done by replacing the directives for the writing of definitions by new directives that permit the writing of definitions in the form of two implications corresponding to one equivalence ... Prof. Leśniewski, however, did not consider these forms of directives desirable. [Srzednicki and Stachniak 1988: 25]

The rules of definitions thus modified were called $\delta_1^\star$ and $\epsilon_1^\star$: instead of equivalence of SS3 they used material implication (just like δ_1 and ϵ_1 of SS3), but instead of using one complex formula to express the equivalence (like SS4), they used two separate relatively simple implications to perform this task. It turned out that once this move is made, Axiom 1 became redundant in practice (as noted above, it is unclear, though, whether this move is what *makes* it actually superfluous).

Now, considering all the above, what does [Leśniewski 1929: 459] say about the creativity of definitions? Not much. It only indicates that, if you don't take your system to contain full propositional logic automatically, what you can derive from definitions is sensitive to what form those definitions have. It does not imply creativity of definitions, i.e. it is not a case where adding definitions to a system devoid of them allows you to prove new formulas in the old language. It is just a case where having the same definitions in a different format allows you to dispose of one of the axioms (even though you didn't give a proof that

[57] You have to use $\forall\sigma\,((\phi \to (\psi \to \sigma) \to \sigma))$ instead of $\phi \wedge \sigma$.

that axiom was initially really necessary). In any strength is gained here, it is rather by using the same definitions, simplifying their syntactic form and handling material implication in a slightly different manner.

To restate, the only thing Leśniewski mentions is Tarski's proof that in system SS4 with definition rules $\delta_1^\star$ and $\epsilon_1^\star$ Axiom 1 is redundant. He expresses no opinion whether this fact should be read as a case of creativeness of definitions; and he does not show that Axiom 1 is independent of other axioms in SS4 given previous rules of definitions. He does not even show that the corresponding axiom in SS3 is independent. In general, if this quote is the best indicator of Leśniewski's interest in the meta-theoretical property of (non-)creativity of definitions, this is by no means a compelling evidence for Nemesszeghys's claim (especially since Leśniewski was essentially describing a work by Tarski who made moves which Leśniewski considered undesirable).

10 Final remarks

We have tried to provide answers to three sets of problems: (i) where did the idea that it was Leśniewski who introduced the restrictions of eliminability and conservativeness originate and why has this impression been around for so long? (ii) is the conviction true at all, i.e. was it really Leśniewski who established these requirements and, if not, what was his actual stance on definitions? (iii) if not Leśniewski then who in fact presented and studied the meta-theoretical criteria of good definitions in Poland?

Let us briefly summarize our findings. The myth about Leśniewski's role has died hard because:

1. It is general knowledge that Leśniewski's works on definitions are of importance,
2. Leśniewski is often praised for having introduced precise rules for definitions by authors who leave the nature of those rules obscure [e.g. Luschei 1962], or even misrepresent them [e.g. Suppes 1957], and the influence of some of these texts has been tremendous,
3. the accessibility of Leśniewski's works has been low: the English translation of his collected works dates back only to 1991[58] and given his idiosyncratic style and the complexity of his formulations the translations are not much easier to read (even for non-Polish and non-German speakers),
4. further, the availability of the relevant works by Łukasiewicz and by Ajdukiewicz is even lower, especially for non-Polish readers.[59]

With high plausibility we can state that it was *not* Leśniewski who introduced translatability, consistency and conservativeness requirements for definitions; and, for certain, he did not study them:

[58] Some of Leśniewski's texts have been translated before, most notably [1931*b*] in the important collection of papers by Polish logicians: [McCall 1967].

[59] The best source for translations of their seminal works in English is [McCall 1967].

5. Leśniewski's use of creative definitions appears essential to his systems,

6. Leśniewski's rules of definitions are concerned only with the syntactic form of definitions as admitted in particular systems,

7. Leśniewski does not bring up eliminability and non-creativity requirements even as potential desiderata of good definitions anywhere in his writings (not even in those bits which are devoted to definitions),

8. Ajdukiewicz, who was familiar with Leśniewski's work and knew him personally, also states that Leśniewski has formulated no meta-theoretical requirements for definitions (and this fact was, according to Rickey [1975*b*], confirmed by B. Sobociński as well).

Most importantly, we have found good reasons to believe that it was Łukasiewicz who brought the issue up, and Ajdukiewicz who was first to provide a meta-theoretical study of how the criteria of conservativeness affect the definitions, at least on the Polish ground:

9. Łukasiewicz in 1928 uses the conservativeness requirement to argue against the intra-theoretic treatment of definitions and presents the criterion in his lecture script (although he does not credit himself with its formulation),

10. Ajdukiewicz describes the translatability and consistency requirements explicitly in 1928,

11. Ajdukiewicz mentions and studies, with respect to some other meta-theoretical conditions, conservative definitions in 1936,

12. Ajdukiewicz (**text 10**) seems to be crediting *not* Leśniewski but himself with the claim that the goal of Leśniewski's syntactic restrictions is to satisfy the meta-theoretical requirements Ajdukiewicz mentioned in 1928.

There still are many open questions pertaining to the history of the standard account of definitions (as well as to the history of the non-standard account of creative and/or circular definitions). For instance, what happened during the 20 year gap between the works of Frege, Peano, Russell and Whitehead, and the works of Łukasiewicz, Leśniewski and Ajdukiewicz? How does Dubislav fit into the picture? There seems to be room for a more extensive study of Peano's group as well.[60]

As is clear from our discussion, the study of the Polish golden age of philosophy and logic, i.e. the time between Kazimierz Twardowski's (1866–1938) appointment as a professor at Lvov in the end of 19th century and the Second World War, might be sometimes surprising.

Even if some pieces of the puzzle are still missing, it seems we can now at least conclude: The myth about Leśniewski and definitions is (almost) definitely busted.

[60]See the references in n. 6.

Acknowledgements

Many thanks for their comments to John MacFarlane, Nuel Belnap, Wilfrid Hodges, Paolo Mancosu, Øystein Linnebo and Jan von Plato. Work on this paper was supported by the Special Research Fund of Ghent University through project [BOF07/GOA/019].

A Texts

Text 1. Frege on eliminability and non-creativity of definitions in his lecture notes from the year 1914. Emphases are ours.

> Now when a simple sign is thus introduced to replace a group of signs, such a stipulation is a definition. The simple sign thereby acquires a sense which is the same as that of the group of signs. *Definitions are not absolutely essential to a system. We could make do with the original group of signs.* The introduction of a simple sign adds nothing to the content; it only makes for ease and simplicity of expression.
>
> ...
>
> A sign has a meaning once one has been bestowed upon it by definition, and the definition goes over into a sentence asserting an identity. Of course the sentence is really only a tautology and does not add to our knowledge. It contains a truth which is so self-evident that it appears devoid of content, and yet in setting up a system it is apparently used as a premise. I say apparently, for what is thus presented in the form of conclusion makes no addition to our knowledge; all it does in fact is to effect an alteration of expression, and we might dispense with this if the resultant simplification of expression did not strike us as desirable. *In fact it is not possible to prove something new from a definition alone that would be unprovable without it. When something that looks like a definition really makes it possible to prove something which could not be proved before, then it is no mere definition but must conceal something which would have either to be proved as a theorem or accepted as an axiom.*
>
> [Frege 1914: 208].

For Leśniewski's texts we are using the English translation and pagination from [Leśniewski 1991]. Sometimes, slight changes have been made after comparing the translation with the original. At some points we have updated the symbolism.

Text 2. Leśniewski's notion of a word.

> The expressions 'man', 'word', 'p', $\equiv$, [,][61], '(', ')', '}' are examples of words. The expressions 'the man', '(p)', 'f[]word' are examples of objects which are collections of words, but not themselves words. The expression 'the man' consists of two words, the expression '(p)' of three words, the expression 'f[]word' of four words. Axiom A3 consists of 80 words. Individual letters of words consisting of at least two letters are not words.
>
> [...] Every word is an expression. The collection of any number of successive words of any expression is an expression. The collection of words consisting of the first, third, and fourth words of any expression is not an expression. Every expression consists of words. I would not call a collection consisting of infinitely many words an expression. [Leśniewski 1929: 469-470]

Text 3. Leśniewski on his formalized meta-language.

[61] The last two brackets were originally Leśniewski's corner quotes marking the scope of quantifiers.

The symbolic formulation of the terminological explanations and directives should be regarded as typographical abbreviations which would be replaced by expressions of ordinary speech had I more space at my disposal. [Leśniewski 1929: 468]

Text 4. Leśniewski about his paper on definitions.

This paper is a résumé of the course of lectures (in Polish) 'On foundations of the 'theory of deduction' ' that I delivered in Warsaw University in the academic year 1930-31. My main task here is to formulate a directive permitting addition, to the system of the 'theory of deduction', of theses of the special kind that I call *definitions*, as distinguished from *axioms* and *theorems*, and codifying as precisely as possible conditions to be satisfied by such definitions.

The problem of definition in the theory of deduction lies quite outside my system of foundations of mathematics, which I have begun publishing in the last few years. What interested me in this problem, if I may so express myself, was its own constructive appeal – in view of the still rather stepmotherly treatment of it even in the current scientific trend in theory of deduction and theory of theory of deduction. [Leśniewski 1931*b*: 629]

Text 5. Leśniewski's key rule of definitions.

I say of object A that it is a definition of B, relative to C, by means of D, and with respect to R if and only if the following conditions are fulfilled:

(1) D is propositional relative to C;

(2) the first of the words that belong to B is not a variable;

(3) if any object F is the same object as C or is a thesis of this system which thesis precedes C, and any object G is a word that belongs to F, then the first of the words that belong to B is not an expression equiform to G;

(4) if any object F is a word that belongs to B, any object G is a word that belongs to B, and F is an expression equiform to G, then F is the same object as G;

(5) if any object is a variable that belongs to D, then it is an expression equiform to some word that belongs to B,

(6) if any object is a word that belongs to B and follows the first of the words that belong to B, then it is an expression equiform to some variable that belongs to D;

(7) the implicant of B in the negate of E is an expression equiform to D,

(8) the implicant of D in the implicant of E in the negate of A is an expression equiform to B. [Leśniewski 1931*b*: 645]

Text 6. Łukasiewicz on conservativeness in his 1929 lecture script.

Apart from properties already noted, definitions have to possess one more very important feature. If the definiendum occurs in a true sentence, a sentence obtained from it by replacing this definiendum by an appropriate definiens should be also true. [...] The reverse also holds, if the definiens occurs in a true sentence, the sentence obtained from it by replacing the definiens with an appropriate definiendum should also be true.

What has been said above about definitions, was already known in traditional logic. Yet research in mathematical logic has raised another issue pertaining to definitions. Are definitions to be mere abbreviations, or can they also perform a creative role and play an important part in reasoning? It has turned out that definitions may be treated so that a definition D makes it possible to prove a theorem T in which the definiendum of the definition D does not occur but which nevertheless cannot be proved without the said definition D. This way, definition D would play an essential role in the proof and by the same token, just like the premises of the proof, it would contribute a new element.

Without getting into any more detailed analysis not belonging to a general course in logic, I will only say that in my opinion definitions cannot play any creative role. Sharing the view of the authors of Principia Mathematica I hold that definitions are theoretically superfluous. If we have a theory in which definitions do not appear at all, nothing new should be obtainable in that theory after we introduce definitions. Only the superficial form of certain theorems may be changed as a result of replacing the definiens by the definiendum. We think that the only advantage to be obtained from definitions is two-fold: (1) definitions help us abbreviate certain expressions of a given theory, and (2) by introducing a new term definitions may, together with that new term, contribute some new intuitions to the theory and enrich the terms of a theory with some terms that have meaning beyond it.

[Polish version] Poza przytoczonemi własnościami definicje muszą posiadać jeszcze jedną bardzo istotną cechę. Jeżeli definiendum występuje w jakiemś zdaniu prawdziwem, to zdanie otrzymane po zastąpieniu tego definiendum przez odpowiednie definiens powinno być nadal prawdziwe. [...] I na odwrót, jeżeli definiens występuje w jakiemś zdaniu prawdziwem, to zdanie otrzymane przez zastąpienie tego definiens przez odpowiednie definiendum winno być nadal prawdziwe.

To, co powiedzieliśmy wyżej o definicjach, znane już było w logice tradycyjnej. Jednakże badania logiki matematycznej wysunęły do rozstrzygnięcia pewną inną kwestję, dotyczącą definicyj. Czy definicje mają być jedynie skrótami, a co za tem idzie być teoretycznie zbędne, czy też mogą one spełniać pozatem jeszcze jakąś rolę twórczą i brać [52] jakiś istotny udział w rozumowaniach? Okazało się, że można w ten sposób traktować definicje, że przy pomocy jakiejś definicji D daje się udowodnić pewne twierdzenie T, w którem nie występuje definiendum definicji D, a mimo to twierdzenie T nie daje się udowodnić bez rozważanej definicji D. W ten sposób definicja D odgrywałyby [sic] jakąś istotną role w dowodzie i wnosiłaby tak jak przesłanki dowodu, jakiś element nowy.

Nie wdając się w bliższe rozważania, nie należące do ogólnego kursu logiki, zaznaczamy jedynie, że zgodnie z naszem stanowiskiem definicje nie mogą odgrywać żadnej roli twórczej. Podzielając stanowisko autorów "Principia mathematica", uważamy, że definicje są teoretycznie zbędne. Jeśli mamy jakąś teorję, w której wogóle definicje nie występują, to nic nowego nie powinno się dać w tej teorji otrzymać po wprowadzeniu definicyj; jedynie pewne twierdzenia mogą zewnętrznie przybrać inną postać na skutek zastąpienia definiens przez definiendum. Według nas korzyść definicyj może być tylko dwojaka: 1) definicje służą do skracania pewnych wyrażeń teorji, 2) wprowadzając nowy jakiś wyraz, definicje mogą wnieść z tym wyrazem jakieś nowe intuicje do teorji i wzbogacić w ten sposób wyrazy teorji o takie, które mają sens poza tą teorją.

[Łukasiewicz 1929: 52]

Text 7. Łukasiewicz on the influence of his colleagues:

I owe the most to the scientific atmosphere in mathematical logic at Warsaw University. In discussions with my colleagues, especially prof. St. Leśniewski and doc. dr. A. Tarski, and often in discussions with my and their students, I have clarified many notions, acquired many ways of expressing myself, and learned about many new results, whose authors today I no longer can identify.

[Polish version] Najwięcej atoli zawdzięczam atmosferze naukowej, która w dziedzinie logiki matematycznej wytworzyła się w Uniwersytecie Warszawskim. W dyskusjach z kolegami moimi, głównie z p. prof. St. Leśniewskim i z p. doc. dr. A. Tarskim, a często także w dyskusjach z moimi i ich uczniami, wyjaśniłem sobie niejedno pojęcie, przyswoiłem sobie niejeden sposób wyrażania się i dowiedziałem się o niejednym nowym wyniku, o których niekiedy dziś już powiedzie nie umiem, do kogo należy ich autorstwo. [Łukasiewicz 1929: vii]

Text 8. Łukasiewicz on his March 24, 1928 talk:

At the 282-nd plenary session on March 24 1928, Prof. Dr. Jan Łukasiewicz gave a talk titled "The role of definitions in deductive systems' [...] the speaker defends the first way of introducing definitions to deductive systems, mostly because definitions of the latter kind allow us in certain cases to deduce from axioms by means of substitution and modus ponens theorems, which contain only primitive terms and are independent of the axioms. Such definitions thus play some creative role, and are, in a way, axioms in disguise. The speaker's stand is that definitions should not be creative, and that their role in deductive systems reduces to the following two practical points: making expressions which are too long shorter, and introducing intuitive virtues.

[Polish version] Na 282 plenarnem posiedzeniu naukowem 24 marca 1928 Prof. Dr. Jan Łukasiewicz wygłosił odczyt p. t. "Rola definicyj w systemach dedukcyjnych". [...] Prelegent oświadcza się za pierwszym sposobem wprowadzania definicyj do systemów dedukcyjnych, przedewszystkiem z tego względu, że definicye drugiego rodzaju pozwalają w pewnych przypadkach na wyprowadzenie z aksjyomatów drogą podstawiania i odrywania takich tez, które zawierają same wyrazy pierwotne a są niezależne od aksyomatów. Definicye takie odgrywają tedy jakąś rolę twórczą, są jakby zamaskowanemi aksyomatami. Prelegent zaś stoi na stanowisku, że definicye nie powinny być twórcze, lecz że rola ich w systemach dedukcyjnych sprowadza się wyącznie do stagpujących dwu punktów natury praktycznej: skracać wyrażenia zbyt długie i wprowadzać nowe walory intuicyjne. [Łukasiewicz 1928*a*: 164]

Text 9. Mostowski [1948: 251] on definitions and Leśniewski

Works devoted to methodology of mathematics rarely discuss the rule of definition in more detail, and are usually limited to a short mention that — to make formulas shorter — a long *definiens* is replaced by a short *definiendum* and that this could be avoided if we decided to write long formulas. This view of definitions is not justified, especially with respect to formalized theories, in which no doubts should be had about which expressions can be used and what principle allows one to introduce expressions as theorems. The rule of definition doesn't really differ from other rules for proofs [...] just like those rules it allows one to accept certain sentences as true (and just like those rules it is obvious). There is no reason to treat the rule of definition differently. [here, a footnote starts, its content being as follows:] Like in many other branches of logic, Frege was the first to characterize the role of definitions correctly. Here is what he says in *Grundlagen der Arithmetic* on p. 78: "A definition of an object as such doesn't say anything about it, it only establishes the meaning of a sign. When this has been done, however, a definition changes into a proposition pertaining to an object; it no longer circumscribes it, but rather is on a par with other statements about it".

The need of a precise formulation of the rule of definitions has been strongly emphasized by Leśniewski. He gave a precise formulation of this rule with respect to the systems he constructed.

[Polish version:] Prace poświęcone metodologii natematyki rzadko kiedy zajmują się dokładnie regułą definiowania, ograniczając się zazwyczaj do krótkiej wzmianki, że – dla skrócenia wzorów – długi *definiens* zastępujemy krótkim *definiendum* i że można by tego uniknąć, gdybyśmy się zdecydowali na pisanie długich wzorów. Taki pogląd na rolę definicji nie jest jednak uzasadniony, zwłaszcza w odniesieniu do teorii sformalizowanych, w których nie powinno być żadnych wątpliwości co do tego, jakimi wyrażeniami wolno w nich operować i na jakiej zasadzie uznaje się w nich pewne wyrażenia za twierdzenia. Reguła definiowania nie różni się zasadniczo od innych reguł dowodzenia [...] tak jak i tamte reguły pozwala ona uznawać pewne zdania za prawdziwe (i jest równie jak one oczywista). Nie ma więc powodu, by traktować regułę definiowania inaczej niż pozostałe reguły. [przypis:] Jak w wielu innych działach logiki, tak i tu Frege pierwszy scharakteryzował należycie rolę, jaką grają definicje. Oto co pisze on w *Grundlagen der Arithmetik* na str. 78: ,,Definicja przedmiotu jako taka nie mówi o nim łaściwie nic, tylko ustala znaczenie znaku. Gdy jednak zostało to już dokonane, definicja zamienia się w sąd dotyczący przedmiotu; nie określa go już ona, lecz stoi w równym rzędzie z innymi o nim wypowiedziami". Potrzebę dokładnego sformułowania reguły definiowana podkreślał dobitnie Leśniewski; podał on ścisłe sformułowanie tej reguły w odniesieniu do stworzonych przez siebie systemów logiki.

Text 10. Ajdukiewicz on the purpose of the eliminability and conservativeness requirements.

> My papers are an attempt to understand the way definitions are used in deductive sciences. I wrote them after familiarizing myself with the so-called rules of definitions formulated by Stanisław Leśniewski for his logical systems. Those directives [i.e. Leśniewski's] were of structural character, that is, they described what form an expression has to have to be accepted as a definition. *I, on the other hand, tried to understand the purpose of putting those structural requirements on definitions.* [the emphasis is ours] Answering the question I claimed that they're put forward to ensure that definitions satisfy the translatability and consistency requirements.
>
> [Polish version] Otóż moje rozprawy są próbą takiego zrozumienia praktyki definiowania w naukach dedukcyjnych. Napisane zostały po zaznajomieniu się z tzw. dyrektywami definicji, jakie sformułował Stanisław Leśniewski dla swoich systemów logicznych. Dyrektywy te miały charakter strukturalny, mianowicie opisywały, jaką postać musi posiadać wyrażenie, aby je można było przyjąć jako definicję. *Mnie zaś chodziło o to, aby zrozumieć, w jakim celu nakłada się na definicję te warunki strukturalne.* Odpowiadając na to, twierdziłem, że nakłada się je po to, aby definicjom tym zapewnić spełnianie tzw. warunku przekładalności i warunku niesprzeczności. [Ajdukiewicz 1960: v-vi]

Text 11. Ajdukiewicz's formulation of the requirements.

> Now, we will wonder what conditions are usually put on an expression D containing an expression δ new relative to accepted sentences Z, if it is to be added to Z and accepted. Let "D" denote an expression, which 1) has the syntactic form of a sentence, 2) contains a new (relative to Z) expression δ. Let "J_Z" ("language J_Z") denote the set of sentences meaningful relative to sentences Z [...] Expression D is not a meaningful sentence relative to Z, because it contains a new (relative to Z) word δ. Let the symbol "$Z + D$" denote each of the sentences Z, and the expression D. Expression D, of course, is a meaningful sentence relative to expressions $Z + D$. Let J_{Z+D} denote the set of sentences meaningful relative to the expressions belonging to $Z + D$. Expression D belongs to language J_{Z+D}, but not to language J_Z.

When we add to Z expression D we enrich our language J_Z with an assembly of new sentences and move to language J_{Z+D}. While doing so, we want two conditions to be satisfied [...] we suppose that someone who has mastered J_Z should be able to use J_{Z+D}. This will be achieved if we provide them with means to translate each sentence of J_{Z+D} into a sentence of J_Z, in a sense reducing each decision about a sentence of J_{Z+D} to a decision about a sentence of J_Z. This translatability is seen as inferential equivalence [of the corresponding sentences]. So, when we add to already accepted sentences Z an expression D, we require, first of all, that each sentence of the new language J_{Z+D} were on the ground of $Z + D$ and accepted inference rules R inferentially equivalent to a certain sentence of J_Z.

[...]

The second requirement on such expressions D – is the consistency postulate. When we add to a set of sentences Z with the inference rules R a word δ new relative to Z by means of an expression D which has the syntactic form of a sentence, we require that D shouldn't become a source of contradiction. Namely, we require that if it was impossible to derive a pair of contradictory sentences from Z using R, such a pair shouldn't also be derivable from sentences $Z + D$.[62]

[Polish version] Zastanowimy się obecnie nad tym, jakie warunki nakłada się zwykle na wyrażenie D, które zawiera taki wyraz δ, nowy ze względu na uznane zdania Z, aby je wolno było do zdań Z dołączyć i uznać za zdanie. Niechaj "D" oznacza takie właśnie wyrażenie, które 1) posiada budowę syntaktyczną zdania, 2) zawiera jakiś nowy ze względu na zdania Z wyraz δ. Niechaj "J_Z" ("język J_Z") oznacza zbiór zdań sensownych ze względu na zdania Z. Oczywista, że zbiór J_Z jest na ogół obszerniejszy od zbioru Z [...] Wyrażenie D nie jest zdaniem sensownym ze względu na zbiór zdań Z, gdyż zawiera nowy ze względu na zdania Z wyraz δ. Niechaj symbol "$Z + D$" oznacza każde ze zdań Z oraz wyrażenie D. Wyrażenie D jest już oczywiście zdaniem sensownym ze względu na wyrażenia $Z + D$. Niechaj J_{Z+D} oznacza zbiór zdań sensownych ze względy na wyrażenia należące do zbioru $Z + D$. Wyrażenie D należy do języka J_{Z+D}, ale nie należy do języka J_Z.

Otóż dołączając do zdań Z wyrażenie D wzbogacamy nasz dotychchczasowy język J_Z o szereg nowych zdań i przechodzimy do języka J_{Z+D}. Przy tym kroku pragniemy, by były zachowane dwa warunki. Wprowadzając do języka J_Z nowe

[62] Literally, the translatability requirement as formulated by Ajdukiewicz may be interpreted as

$$\forall \phi \in J_{Z+D}\, \exists \psi \in J_Z\, (Z + D \vdash_R \phi \equiv Z + D \vdash_R \psi)$$

But this is rather clearly not what Ajdukiewicz had in mind. For consider any Z such that $Z + D \vdash_R \top$ and $Z + D \nvdash_R \bot$ where it is safe to assume that $\bot$ and $\top$ are in J_Z. Then take $\top$ to be the witness for any J_{Z+D}-formula derivable from $Z + D$, and $\bot$ to be the witness for any J_{Z+D}-formula not derivable from $Z + D$. Then the condition is satisfied, but it doesn't seem to have much to do with translation.

Indeed, Ajdukiewicz [1936: 243] slightly changed his definition saying explicitly that a translation requires translation rules that can go both ways: for ϕ to be translatable into ψ given assumptions C and rules U, ψ should be deducible from C and ϕ by means of U, and ϕ should be deducible from C and ψ by means of U. This at least excludes degenerate translations of the sort mentioned above.

ze względu na zdania Z wyraz δ przy pomocy wyrażenia D suponujemy, że kogoś, kto włada językiem J_Z mamy uzdolnić do operowania językiem J_{Z+D}. Uczynimy to, gdy dostarczymy mu środka, pozwalającego każde zdanie języka J_{Z+D} przełożyć niejako na jakieś zdanie języka J_Z, rozstrzygnięcie każdego zdania w języku J_{Z+D} sprowadzić w pewnym sensie do rozstrzygnięcia pewnego zdania w języku J_Z. Tę przekładalność zdań upatrujemy w ich inferencyjnej równoważności. Zatem, dołączając do uznanych zdań Z opisane wyżej wyrażenie D, pragniemy, po pierwsze, by każde zdanie nowego języka J_{Z+D} było na gruncie zdań $Z + D$ i przyjętych przez nas dyrektyw rozumowania R inferencyjnie równoważne pewnemu zdaniu języka J_Z.

[...]

Drugi postulat stawiany takim wyrażeniom D — to postulat niesprzeczności. Wprowadzając do zbioru zdań Z, w którym obowiązują dyrektywy rozumowania R, nowy ze względu na zdania Z wyraz δ przy pomocy wyrażenia D o budowie syntaktycznej zdania, wymagamy, by wyrażenie D nie stało się źródłem sprzeczności. Żądamy mianowicie, by – jeśli ze zdań Z nie można było wywieść wedle dyrektyw R pary zdań sprzecznych — nie dała się też wywieść para takich zdań ze zdań $Z + D$. [Ajdukiewicz 1928: 46-47]

Text 12. Ajdukiewicz on non-creativity. For the translation, see section 8, page 21.

[Polish version] Często, chociaż nie zawsze, dąży się ponadto do tego, by reguły definiowania wykluczały definicje twórcze. Definicję nazywamy twórczą na gruncie pewnego języka, gdy daje się z tez tego języka zgodnie z regułami dedukcyjnymi i przy pomocy tej definicji wywieść takie zdanie tego języka (a więc wolne od wyrazu definiowanego), którego nie można by wywieść bez pomocy tej definicji. [Ajdukiewicz 1936: 244]

Text 13. Ajdukiewicz on structural rules of inference.

...we'll assume that the defined term is irrelevant for the inference rules of a given language. That is, whenever from a sentence Z_W containing a word W it is possible to deduce according to the directives of that language a sentence P_W, then from any sentence Z_X obtained from Z_W by replacing W by any expression X it is possible to derive the sentence P_X using the same inference rules, where P_X is identical with P_W if P_W doesn't contain the word W, and if it isn't it is obtained from P_W by replacing W with X. This requirement is almost always satisfied, for the defined word is almost always irrelevant for the inference rules.

[Polish version] ...założymy, że wyraz definiowany jest irrelewantny dla reguł wnioskowania odnośnego języka. Znaczy to, że ilekroć z jakiegoś zdania Z_W, zawierającego wyraz W, da się wyprowadzić według dyrektyw języka zdanie P_W, to z każdego zdania Z_X, powstającego z Z_W przez zastąpienie wyrażenia W przez dowolne wyrażenie X, da się wyprowadzić zdanie P_X według tych samych dyrektyw wnioskowania, przy czym P_X jest identyczne z P_W, jeśli P_W nie zawiera wyrazu W, poza tym zaś powstaje z P_W przez podstawienie X za W. To założenie jest prawie zawsze spełnione gdyż definiowany wyraz jest prawie zawsze irrelewantny dla dyrektyw wnioskowania języka. [Ajdukiewicz 1936: 245-246]

References

Abelson, Raziel. 1967. Definition. In *The Encyclopedia of Philosophy*, ed. Paul Edwards. Vol. 2 New York: Macmillan & The Free Press pp. 314–324.

Ajdukiewicz, Kazimierz. 1928. "Definicja." (Parts published in *Język i Poznanie. Tom I. Wybór Pism z Lat 1920-1939*, Warszawa 1960 and 1985, pp. 44-61).

Ajdukiewicz, Kazimierz. 1936. Die Definitionen. In *Actes du I Congrés International de Philosophie Scientifique.* Paris: Hermann pp. 1–6. (The Polish version translated by Franciszek Zeidler published in *Język i Poznanie. Tom I. Wybór Pism z Lat 1920-1939*, Warszawa 1960 and 1985, pp. 243-248).

Ajdukiewicz, Kazimierz. 1960. Przedmowa. In *Język i Poznanie. Tom I. Wybór Pism z Lat 1920-1939.* Warszawa: PWN pp. v–viii. (Introduction to the collection).

Ajdukiewicz, Kazimirz. 1958. "Three Concepts of Definition." *Logique et Analyse* 1(3-4):115–126.

Beck, Lewis White. 1956. "Kant's Theory of Definition." *The Philosophical Review* 65(2):179–191.

Belnap, Nuel. 1993. "On Rigorous Definitions." *Philosophical Studies* 72(2-3):115–146.

Belnap, Nuel D. 1962. "Tonk, Plonk and Plink." *Analysis* 22(6):130–134.

Betti, Arianna. 2008. Polish Axiomatics and its Truth — On Tarski's Leśniewskian Background and the Ajdukiewicz Connection. *in* Patterson [2008] pp. 44–71.

Church, Alonzo. 1956. *Introduction to Mathematical Logic. Vol. 1.* Princeton: Princeton University Press.

Cregan, Anne M. 2005. Towards a Science of Definition. In *AOW '05: Proceedings of the 2005 Australasian Ontology Workshop.* Darlinghurst, Australia: Australian Computer Society pp. 25–32.

Došen, Kosta and Peter Schroeder-Heister. 1985. "Conservativeness and Uniqueness." *Theoria* 51:159–173.

Dubislav, Walter. 1981. *Die Definition.* 4th ed. Hamburg: Felix Meiner Verlag. (Unchanged from the 3rd edition of 1931).

Dudman, V. H. 1973. "Frege on Definition." *Mind* 82(328):609–610.

Fetzer, James H., David Shatz and George N. Schlesinger, eds. 1991. *Definitions and Definability: Philosophical Perspectives.* Vol. 216 of *Synthese Library* Dordrecht: Kluwer.

Frege, Gotlob. 1914. "Logic in Mathematics." (A lecture note, German original in Hermes, Kambartel and Kaulbach [1969], page numbers refer to the translation in [Hermes, Kambartel and Kaulbach 1979: 203–250]).

Frege, Gottlob. 1879. *Begriffsschrift: eine der arithmetischen nachgebildete Formelsprache des reinen Denkens.* Halle: L. Nebert. (Page numbers refer to the translation in Heijenoort [1967], pp. 1-82).

Galilei, Galileo. 2001. *Dialogues Concerning Two New Sciences.* Norwich, NY: William Andrew Publishing. (Translation by Henry Crew and Alfonso de Salvio, Electronic publication, retrieved from `www.knovel.com`).

Grattan-Guinness, I. 2000. *The Search for Mathematical Roots, 1870-1940: Logics, Set Theories and the Foundations of Mathematics from Cantor through Russell to Gödel.* Princeton, N.J.: Princeton University Press.

Gupta, Anil. 2009. Definitions. In *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta. Spring 2009 ed.

Hacking, Ian. 1979. "What is Logic?" *The Journal of Philosophy* 76(6):285–319.

Heijenoort, Jean van, ed. 1967. *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931.* Source Books in the History of the Sciences Cambridge, Massachusetts: Harvard University Press.

Hermes, Hans, Friedrich Kambartel and Friedrich Kaulbach, eds. 1969. *Gotlob Frege: Nachgelassene Schriften.* Vol. 1 Hamburg: Meiner. (English translations in Hermes, Kambartel and Kaulbach [1979]).

Hermes, Hans, Friedrich Kambartel and Friedrich Kaulbach, eds. 1979. *Gottlob Frege: Posthumous Writings.* Oxford: Basil Blackwell. (Translation of Hermes, Kambartel and Kaulbach [1969]).

Hodges, Wilfrid. 2008. Tarski's Theory of Definition. *in* Patterson [2008] pp. 94–132.

Horty, John. 2007. *Frege on Definitions: A Case Study of Semantic Content.* Oxford: Oxford University Press.

Jordan, Z. 1945. *The Development of Mathematical Logic and of Logical Positivism in Poland between the Two Wars.* Polish Science and Learning London: Oxford University Press. (Partly republished in [McCall 1967: 346–397]).

Jurcic, Joseph. 1987. "On Defining Sentential Connectives." *Notre Dame Journal of Formal Logic* 28(2):189–199.

Kelley, John L. 1955. *General Topology.* The University Series in Higher Mathematics Princeton, NJ: D. van Nostrand Company.

Kennedy, H. C. 1973. "What Russell Learned from Peano." *Notre Dame J. Formal Logic* 14(3):367–372.

Koj, Leon. 1987. "Walter Dubislav und Kazimierz Ajdukiewicz über die Definition." *Studies in Logic, Grammar and Rhetoric* VI:93–116.

Leśniewski, Stanisław. 1929. "Grundzüge eines neuen Systems der Grundlagen der Mathematik § 1–11." *Fundamenta Mathematicae* 14:1–81. (Fundamentals of a new system of the foundation of mathematics, §1-11, [Leśniewski 1991: 410-605]).

Leśniewski, Stanisław. 1930. "Über die Grundlagen der Ontologie." *Sprawozdania z posiedzeń Towarzystwa Naukowego Warszawskiego, Wydział Nauk Matematyczno-Fizycznych* 23:111–132. (On the foundations of Ontology, [Leśniewski 1991: 606-628]).

Leśniewski, Stanisław. 1931*a*. "O Podstawach Matematyki, Rozdział X: Aksjomatyka 'Ogólnej Teorji Mnogości Pochodząca z r. 1921. Rozdział XI: O Zdaniach 'Jednostkowych' Typu '$A\varepsilon b$'." *Przegląd Filozoficzny* 34:142–170. (On the foundations of mathematics. Ch. X. The axiomatization of the 'general theory of sets' from the year 1921. Ch. XI. On 'singular' propositions of the tyle '$A\,\varepsilon\,b$', [Leśniewski 1991: 350-382]).

Leśniewski, Stanisław. 1931*b*. "Über Definitionen in der sogenannten Theorie der Deduction." *Sprawozdania z posiedzeń Towarzystwa Naukowego Warszawskiego, Wydział Nauk Matematyczno-Fizycznych* 24:289309. (On definitions in the so-called theory of deduction, [Leśniewski 1991: 629-648]).

Leśniewski, Stanisław. 1991. *Stanisław Leśniewski. Collected Works (two vols.).* Dordrecht: Kluwer Academic Publishers. (Edited and translated by S. Surma, J. Srzednicki, and D. I. Barnett, continuous pagination).

Łukasiewicz, Jan. 1928*a*. "Plenarne posiedzenie naukowe z dnia 24 marca 1928, autoreferat." *Ruch Filozoficzny* 11:164.

Łukasiewicz, Jan. 1928*b*. "Posiedzenie Naukowe Sekcji Logiki Polskiego Towarzystwa Filozoficznego z dnia 18 lutego 1928, autoreferat." *Ruch Filozoficzny* 11:177–178.

Łukasiewicz, Jan. 1929. "Elementy Logiki Matematycznej." (Reprinted in 1958 in Warsaw by Państwowe Wydawnictwo Naukowe and in 2008 in Poznań, Wydawnictwo Naukowe UAM).

Łukasiewicz, Jan. 1963. *Elements of Mathematical Logic.* International Series of Monographs in Pure and Applied Mathematics Oxford: Pergamon Press. (Translation of *Elementy Logiki Matematycznej*, PWN, Warszawa, 1958. Originally appeared as mineographed notes, [Łukasiewicz 1929]).

Luschei, Eugene. 1962. *The Logical Systems of Leśniewski.* Amsterdam: North-Holland.

Mates, Benson. 1972. *Elementary Logic.* Second ed. New York: Oxford University Press.

McCall, Storrs, ed. 1967. *Polish Logic 1920-1939.* Oxford: Clarendon press.

Mill, John Stuart. 1869. *A System of Logic, Ratiocinative and Inductive: Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation.* New York: Harper.

Mostowski, Andrzej. 1948. *Logika Matematyczna. Kurs Uniwersytecki.* Seminarium Matematyczne Uniwersytetu Wrocławskiego.

Nemesszeghy, E. Z. and E. A. Nemesszeghy. 1971. "Is $(p \supset q) = (\sim p \vee q)$ Df. A Proper Definition in the System of Principia Mathematica?" *Mind* 80(318):282–283.

Nemesszeghy, E. Z. and E. A. Nemesszeghy. 1973. "On the Creative Role of the Definition $(p \supset q) = (\sim p \vee q)$ Df. In the System of Principia: Reply to V. H. Dudman (I) and R. Black (II)." *Mind* 82(328):613–616.

Nemesszeghy, E. Z. and E. A. Nemesszeghy. 1977. "On Strongly Creative Definitions: A reply to V.F. Rickey." *Logique et Analyse* 20(77-78):111–115.

Padoa, Alessandro. 1900. Logical Introduction to any Deductive Theory. *in* Heijenoort [1967] pp. 119–123. (Translation of part of 'Essai d'une théorie algébrique des nombres entiers, précédé d'une introduction logique à une théorie déductive quelconque', originally published in Bibliothèque du Congrès International de Philosophie, Vol 3., Armand Colin, Paris).

Pascal, Blaise. 1814*a*. De l'art de persuader. *in* Pascal [1814*c*] pp. 153–173.

Pascal, Blaise. 1814*b*. De l'esprit géométrique. *in* Pascal [1814*c*] pp. 123–152.

Pascal, Blaise. 1814*c*. *Pensées, fragments et lettres.* Vol. 1 Paris: Andrieux.

Patterson, Douglas, ed. 2008. *New Essays on Tarski and Philosophy.* Oxford University Press.

Prior, A. N. 1960. "The Runabout Inference-Ticket." *Analysis* 21(2):38–39.

Rantala, Veikko. 1977. *Aspects of Definability.* Vol. 29 of *Acta Philosphica Fennica* Amsterdam: North-Holland Publishing Company.

Rickey, V. Frederick. 1975*a*. "Creative Definitions in Propositional Calculi." *Notre Dame Journal of Formal Logic* 16(2):273–294.

Rickey, V. Frederick. 1975*b*. "On Creative Definitions in the *Principia Mathematica*." *Logique et Analyse* 18(69-70):175–182.

Robinson, Richard. 1954. *Definition.* Oxford: Oxford University Press.

Sager, Juan C., ed. 2000. *Essays on Definition.* Terminology and Lexiography Research and Practice Amsterdam: John Benjamins Publishing Co.

Shieh, Sanford. 2008. "Frege on Definitions." *Philosophy Compass* 3(5):992–1012.

Simons, Peter. 2008. Stanisław Leśniewski. In *Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta. Fall 2008 ed.

Słupecki, Jerzy. 1955. "St. Leśniewski's Calculus of Names." *Studia Logica* 3:7–72.

Srzednicki, Jan and Zbigniew Stachniak, eds. 1988. *S. Leśniewski's Lecture Notes in Logic*. Dordrecht: Kluwer Academic Publishers.

Stachniak, Zbigniew. 1981. *Introduction to Model Theory for Lesniewski's Ontology*. Wrocław: Wydawnictwo Uniwersytetu Wrocławskiego.

Suppes, Patrick. 1957. *Introduction to Logic*. New York: Van Nostrand Reinhold. (Republished unabridged, 1999, Dover Publications, Mineola, New York).

Tarski, A. 1934. "Z badań metodologicznych nad definiowalnością terminów." *Przegląd Filozoficzny* 37:438–460. (Translated as "Some methodological investigations on the definability of concepts" in [Tarski 1956: 298–319]).

Tarski, Alfred. 1923. "O Wyrazie Pierwotnym Logistyki." *Przegląd Filozoficzny [Philosophical Movement]* 26:68–69. (Translated as "On the primitive term of logistic" in *Logic, semantics, metamathematics*, Clarendon Press, Oxford 1956, 1-24).

Tarski, Alfred. 1937. *Einführung in die Mathematische Logik und in die Methodologie der Mathematik*. Wien: Julius Springer.

Tarski, Alfred. 1941. *Introduction to Logic and to the Methodology of Deductive Sciences*. Oxford: Oxford University Press. (Original Polish edition 1936, translated by Olaf Helmer from the German edition, Tarski [1937]).

Tarski, Alfred. 1956. *Logic, Semantics, Metamathematics*. Oxford: Claredon Press. (Translation by J. H. Woodger.).

Tarski, Alfred. 1983. *Logic, Semantics, Metamathematics*. Second ed. Indianapolis: Hackett Publishing Company. (Edited by John Corcoran, translation by J. H. Woodger).

Urbaniak, Rafal. 2008. Leśniewski's Systems of Logic and Mereology. History and Re-evaluation PhD thesis University of Calgary. (Available at `https://dspace.ucalgary.ca/handle/1880/46697`).
URL: `https://dspace.ucalgary.ca/handle/1880/46697`

Urbaniak, Rafał. 2009. "A Note on Identity and Higher-Order Quantification." *The Australasian Journal of Logic* 7:48–55.
URL: `http://www.philosophy.unimelb.edu.au/ajl/2009/`

Woleński, Jan. 1985. *Filozoficzna Szkoła Lwowsko-Warszawska.* Warszawa: PWN. (Translated as *Logic and philosophy in the Lvov-Warsaw school*, Reidel, Dordrecht, 1989).

Woleński, Jan. 2004. "The Reception of Frege in Poland." *History and Philosophy of Logic* 25(1):37–51.