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ON TARSKI'S FOUNDATIONS OF THE GEOMETRY OF SOLIDS

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Abstract. The paper [Tarski: *Les fondements de la géométrie des corps*, ***Annales de la Société Polonaise de Mathématiques***, pp. 29–34, 1929] is in many ways remarkable. We address three historico-philosophical issues that force themselves upon the reader. First we argue that in this paper Tarski did not live up to his own methodological ideals, but displayed instead a much more pragmatic approach. Second we show that Leśniewski's philosophy and systems do not play the significant role that one may be tempted to assign to them at first glance. Especially the role of background logic must be at least partially allocated to Russell's systems of ***Principia mathematica***. This analysis leads us, third, to a threefold distinction of the technical ways in which the domain of discourse comes to be embodied in a theory. Having all of this in place, we discuss why we have to reject the argument in [Gruszczyński and Pietruszczak: *Full development of Tarski's Geometry of Solids*, ***The Bulletin of Symbolic Logic***, vol. 4 (2008), no. 4, pp. 481–540] according to which Tarski has made a certain mistake.

§1. Introduction. Tarski's geometrical work in the late twenties has recently become a subject of general interest for historians of mathematics and axiomatics ([Givant, 1999], p. 50; [Tarski and Givant, 1999], p. 192; [Marchisotto and Smith, 2007], p. 350; [Smith, 2010], p. 483), who have stressed one of its important methodological aspects, namely Tarski's express dislike for formulating axioms with the use of defined notions. This aspect is generally portrayed, both by Tarski and other scholars, as distinctive of his approach to axiomatics with respect to the historical context formed by the work of people such as Hilbert, Pieri and Huntington.

However, an early, rather sketchy paper by Tarski [Tarski, 1929] which offers an axiomatisation of the so-called geometry of solids—and of which [Gruszczyński and Pietruszczak, 2008] offers a "full development"—presents methodological oddities that contrast strikingly with this picture. The original 1929 French paper uses defined notions in the axioms. Besides, the additions made by Tarski to the English translation of the paper in 1956 [Tarski, 1956a], far from removing the methodological oddities, make for even more puzzlement: more axioms are added with defined notions and Tarski proposes a "convenient" variant of the system including a strictly

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speaking superfluous primitive notion which would make the primitive notions of the system interdefinable (i.e., dependent). Letters kept in the Woodger Papers at the Special Collections Library of UCL show no indication that at the time of the translation, done by Joseph Henry Woodger and checked by Tarski, Tarski considered the paper methodologically flawed, which suggests that the oddities in this respect must be taken at face value.

The methodological oddities in [Tarski, 1929, 1956a] are also striking for a reason other than discrepancy with Tarski's own, self-professed methodological convictions, albeit a related one. At first glance, the deductive basis of Tarski's paper seems to be the system of Mereology of Tarski's teacher, Leśniewski, and what Mancosu calls "background logic" ([Mancosu, 2006], p. 217, n. 9) seems also Leśniewskian. So one might be led to think that Tarski's geometry of solids was conceived within a Leśniewskian framework,[1] or even that Tarski's geometry of solids is what Leśniewski's geometry would have looked like had he built one ([Luschei, 1962], p. 318, n. 80).

But Leśniewski was extremely strict in his methodological convictions. He never allowed defined notions in axioms. If Tarski really had wanted to work within Leśniewski's systems, he should and would have proceeded in a different way. Something similar applies also to the background logic and the mereological basis of [Tarski, 1929, 1956a]. If the latter were really Leśniewskian, the paper would have looked considerably different.

In this paper we address both the issues of Tarski's (not) practicing what he preaches in [Tarski, 1929], and his (not) practicing in particular what Leśniewski preaches and practices. Tackling these two issues will lead us to raise a third point: the way in which the notion of domain of discourse of a science was technically embodied in a formal theory in Tarski's times.

As to the first two issues, we argue that [Tarski, 1929] highlights Tarski's pragmatic attitude and his ability to connect various fields and frameworks to arrive at interesting mathematical results, rather than that it witnesses to particular methodological convictions—an analysis that confirms and strengthens the lines of interpretation offered in [Sinaceur, 2001] and [Betti,

[1] This is what [Betti, 2008], p. 42 vaguely suggests. Cf. also [Sundholm, 2003], p. 116, who, speaking of 1929, takes Tarski to be "true to his Leśniewskian calling" and [Mancosu, 2006] p. 217, n. 10 ("The background system here is Lesniewski's ontology"), while [Feferman and Feferman, 2004], p. 102 claim by contrast that after his dissertation in 1923 Tarski "never worked on those [Leśniewski's] systems again". Combining the conclusions of [Betti, 2008] (see e.g., pp. 48–50, 53) with what we say in this paper we can say that although Tarski did not stop abruptly working on Leśniewski's systems after his dissertation, those systems had no privileged status in Tarski's work, but were treated either as just one object of study among other systems, or merely provided isolated notions to be freely used in combination with other notions from other, possibly quite different systems, whenever convenient. As to [Tarski, 1929] in particular, we shall argue that the second case applies and that the role of Leśniewski's systems is marginal.

2008]. In fact, Tarski's choices in the 1929 paper are apparently made to the detriment of such methodological convictions. The use of defined notions in axioms was anathema for the tradition of Polish axiomatics in which Tarski was educated, and in particular for Leśniewski, but having defined notions in axioms is evidently not of much trouble to Tarski here. Besides, the deductive basis of the paper is not Leśniewskian, or at least not any clear-cut way—that is, Tarski does not really base his foundations on Leśniewski's Mereology (which is a non-logical theory) together with the background logic required by Leśniewski's Mereology (the two systems of Protothetics and Ontology). Rather, Tarski is freely lifting notions from Leśniewski's Mereology and combining them with those of the system of ***Principia mathematica***. On top of this he takes notions from topology that are not intrinsic to either of these systems.

This is relevant to our third issue in an important way. The role that the system of Whitehead and Russell's ***Principia mathematica*** plays in [Tarski, 1929, 1956a] is not just that of providing Tarski with some notions: its role seems more prominent. Our conclusion is that only by granting the type theoretical approach of ***Principia mathematica*** a role in the background logic for this paper can one give a meaningful account to some otherwise ununifiable parts of it. This said, one has to keep in mind that the very notion of background logic for a paper as sketchy as [Tarski, 1929, 1956a] is not straightforward. Under the "background logic" or "underlying logic" of a mathematical paper we understand the precise system of (axiomatic) logic and derivation rules that have (tacitly) been assumed as the underlying formal framework for the whole of specific (non-logical) axioms and definitions. The concept of "background" or "underlying logic" (cf. e.g., [Church, 1956], 55., p. 317) may be thus problematic, because it may be the case that the working mathematician has no particular logical system in mind for her mathematical results. Instead she could have the conviction that those results could be formalised in one system or another. And this, we maintain, applies at least to some extent to [Tarski, 1929, 1956a].

Our findings as to the type theoretical approach of this paper are also relevant for the debate on the correct philosophical interpretation of Tarski's notion of logical consequence, a debate famously opened by Etchemendy [Etchemendy, 1988] and centering on the question of whether in 1936 Tarski held a variable or a fixed conception of model, and can be seen as further evidence to [Mancosu, 2006]'s conclusion that Tarski in that period held a *relative fixed-domain* conception of model.[2] The crucial point in this debate is the relation between the *domain of discourse* and the *range of quantifiers*. We propose to refine Mancosu's distinction between these two notions by introducing a difference in levels between the domain of discourse of a science in a non-technical sense and the three technical ways in which in Tarski's

[2]For a reconstruction of the debate, see [Mancosu, 2010].

time the notion of domain of discourse could be codified in a formal theory. The range of quantifiers, we hold, is merely one of these technical ways.

The structure of the paper is as follows. Section 2 is about Tarski's work in geometry during the years 1926–1928 and how his paper on the geometry of solids does not fit in. We point out three differences: first, the use of Mereology in [Tarski, 1929]; second, its focus on the geometry of *solids*; third, and most importantly, its unconcern for methodological principles. The first two are only superficially Leśniewskian because of Tarski's pragmatical attitude of using methods and tools from different areas and philosophical approaches, while the third is strongly non-Leśniewskian.

We discuss the Leśniewskian character of the topic—geometry of solids—in Section 3. We will also point out other influences, like [Whitehead, 1919]'s, and discuss in what respects Tarski's specific approach to the geometry of solids as atomless stands in the way of classifying the topic of [Tarski, 1929] once and for all as Leśniewskian.

In Section 4 we discuss briefly the precise mathematical content of Tarski's paper, including the specific way in which the axiom system has been formulated. This joins up with the fact that Tarski used defined notions in axioms.

Section 5 subsequently deals more thoroughly with the role of Russell's system in [Tarski, 1929], which we only touch upon in the forgoing section. This leads to a discussion of the role and extension of the range of the quantifiers and the domain of discourse in [Tarski, 1929]. Mathematical arguments show that the extension of the range of the quantifiers includes that of the domain of discourse ("*solids*") without being further specified, while the extension of the class of (Russellian)"individuals"—objects of the first type—that Tarski assumes should be identified with the latter.

This leads naturally to a discussion of Tarski's use of the term "universe of discourse" in a passage added in [Tarski, 1956a] and of a misconception in [Gruszczyński and Pietruszczak, 2008] as to how to interpret this passage in Section 6. We will argue that Tarski's "universe of discourse" should be interpreted as the domain of discourse in the way we use it in our paper, and restate that the domain of discourse and the range of the quantifiers cannot be equated. Furthermore we will argue that the passage in which this notion appears has been added because in [Tarski, 1929] the domain of discourse does not get represented by a primitive notion in the theory, by contrast with the custom of logicians of that time. This leads to a refinement of the picture sketched in [Mancosu, 2006] as to the way in which the notion of the domain of a theory was conceptualized at that time.

§2. Tarski's 1926–1927 work in geometry. Although [Tarski, 1929] was published in 1929, its content comes from a lecture delivered in 1927 at the first conference of Polish mathematicians, published two years later in the volume of Proceedings of that conference. Tarski gives a specific motivation for the paper:

> Some years ago Mr. Leśniewski suggested the problem of establishing the foundations of a *geometry of solids*, understanding by this term a system of geometry destitute of such geometrical figures as points, lines and surfaces, and admitting as figures only solids—the intuitive correlates of regular open (resp. closed) sets (...) of three-dimensional Euclidean geometry; the specific character of such a geometry of solids—in contrast to each "point" geometry—is shown in particular in the law according to which each figure contains another figure as a proper part. ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 227) (Our translation)[3]

It is likely that the "problem posed by Leśniewski some years ago" refers to a problem that Leśniewski had raised in his course "Foundations of Three-Dimensional Euclidean Geometry in the Light of the New Theory of Classes", 1921–24 ([Surma, Srzednicki, Barnett, and Rickey, 1991]. Vol. 1, p. 12), which Tarski had followed as a student.[4] Notice that "the New Theory of Classes" occurring in the title of Leśniewski's course is his theory of *collective classes*, i.e., Mereology, a deductive, axiomatic, extensional theory of parts and wholes. We take it, then, that the problem concerned how to build geometry on a mereological deductive basis, which is exactly what Tarski sets out to do.

How seriously should we take the Leśniewskian flavour of Tarski's paper, though? It has been suggested that at this time ([Sundholm, 2003], p. 116) or at least specifically in this paper in some sense ([Betti, 2008], p. 42) Tarski was working within Leśniewski's framework, and Luschei ([Luschei, 1962], p. 318, n. 80) even refers to the content of the paper as "the geometries of Lesniewski and Tarski", but this isn't convincing for several reasons. The paper does present two elements that we would qualify as Leśniewskian: first, Tarski's choice to develop Euclidean geometry starting from solids rather than from points; and second, his development of solid geometry from a mereological deductive basis. Both are methodological issues with a strong ontological motivation. In the terms of Leśniewski's systems, saying that the geometry of solids is developed on a deductive mereological basis means that the system of the geometry of solids presupposes Mereology, which is a formal, deductive, non-logical theory, which in turn presupposes Leśniewski's background logic (i.e., the two systems of Ontology and Protothetics). By 1927 Tarski, however, was using tools and approaches which were already quite distant from Leśniewski's approach ([Betti, 2008], p. 49).

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[3]A regular open set is one that is identical to the interior of its closure. Tarski refers here to [Kuratowski, 1922]. We will see the latter's definition later on.

[4]Owen LeBlanc, in a correspondence dated October 8, 2011, reports that this conjecture was confirmed by Leśniewski's student Czesław Lejewski in conversation.

As we shall see better in the following section, the Leśniewskian character of the paper is indeed more superficial than one might think at first, and what is more, decidedly non-Leśniewskian elements are also clearly present. For the time being however, let us note that from the point of view of both these allegedly Leśniewskian elements—Mereology and the geometry of solids—[Tarski, 1929] was an isolated paper in Tarski's production, and this both by comparison with Tarski's published *oeuvre* in the years 1924–1929, and with the work on geometry that Tarski was developing during his courses at Warsaw University in 1926–1927 ([Szczerba, 1986], p. 908) as we know it from his later writings and from contributions by others.

Tarski worked on the geometry of straight lines ([Tarski, 1935], p. 324 [64], fn 53) and gave an "axiomatic development of elementary Euclidean geometry, the part of plane Euclidean geometry that is not based upon set-theoretical notions, or, in other words, the part that can be developed within the framework of first-order logic." ([Tarski and Givant, 1999], p. 175). The axiom system in question, for 2-dimensional Euclidean point geometry with 21 axioms and three primitives (*equality*, *betweenness* and *equidistance*) is given in [Tarski, 1967], p. 329 [see also especially §3, Notes 6, 21 and 34].

According to Szczerba,

> The system that Tarski presented in this course [1926–1927] was designed after Pieri [1908] (rather than Hilbert [1922]) and contained a number of innovations. Only one universe was used, the set of points, with two undefined primitive relations: betweenness and equidistance. ([Szczerba, 1986], p. 908)

So, Tarski's work in geometry in 1926–1927 seems mainly to have been in point geometry, and without the use of mereological notions. However interesting we might find these two differences with respect to [Tarski, 1929], a remaining, third difference is most salient and interesting for our purposes here.

When talking about his work in geometry, both Tarski himself and others, including his students, stress one point again and again, namely that

> Tarski strongly opposed the practice of formulating axioms with the use of defined notions. ([Szczerba, 1986], p. 908)]

The point is made also in a joint, posthumous paper:

> Another distinctive feature of Tarski's system is the formal simplicity of the axioms upon which the development is based. As opposed to Tarski's system, in all the systems of geometry known from the literature, at least some—and sometimes even most—axioms are not formulated directly in terms of primitive notions, but contain also other notions, previously defined.
> ([Tarski and Givant, 1999], p. 192)

And about the system in [Tarski, 1959]:

> For Euclidean geometry he picks two predicates: the ternary predicate **B** to denote the betweeness relation and the quarternary predicate **D** to denote the equidistance relation. (...) In this language Tarski formulates a simple axiom system for plane Euclidean geometry (...) The axiom system consists only of twelve short first order $\Pi\Sigma$ sentences and the continuity axiom, in the case of second order arithmetic, or the continuity axiom schema, in case of the first order (i.e., elementary) geometry; all axioms are being formulated without the use of defined terms, contrary to the prevailing custom. [Footnote: Compare, for instance, The Hilbert axiom system in [***Grundlagen der Geometrie***, Achte Auflage, Teubner Verlagsgesellschaft, Stuttgart, 1956], full of defined terms, even of higher orders.] ([Szmielew, 1974], pp. 123–124)

We find the same point made by Steve Givant:

> Tarski was critical of Hilbert's axiom system from a logical perspective; since defined notions were used in formulating these axioms, their true complexity was not evident. ([Givant, 1999], p. 50)

These passages suggest that there was a "prevailing custom", that of using defined notions in axioms, typical of Hilbert and Pieri, and also that Tarski strongly opposed it, that this opposition was distinctive of Tarski's attitude, and that this attitude had a methodological advantage: namely, that avoiding defined notions in the axioms makes the system more perspicuous, while using them masks the real complexity of the axiomatic structure.

Now, in contrast to the above, Tarski's [Tarski, 1929] *does* have defined notions in the axioms. This is the third difference between this paper in the geometry of solids and the rest of Tarski's coeval work in geometry. If we have to take seriously Tarski's mainly methodological attitude as described by Givant, Szczerba, Szmielew and Tarski himself, this is truly odd. How can we explain this oddity? Before attempting to answer this question, we should note here that the claim that Tarski's mainly methodological attitude was distinctive for his times is not very credible. On the contrary, that attitude was common in the Warsaw milieu where Tarski studied. It was championed by Tarski's teachers Leśniewski and Łukasiewicz, and is especially evident in Leśniewski's practice where it is applied at its strictest. Tarski inherited this attitude, so to speak, and learned to master the technique while working under Leśniewski's supervision to the construction of Leśniewski's systems (see e.g., Tarski's PhD thesis [Tarski, 1923]).

Leśniewski writes:

> From a standpoint of correctness, in a given system, definitions should always follow the axioms. ([Leśniewski, 1988a], p. 92)

The strict order in which axioms and definitions should thus appear in a system according to Leśniewski—the axioms first—forces the axioms to use only primitive notions (and defined notions from other systems). Sobociński, who attributes the requirement to Łukasiewicz puts the issue as follows:

> This requirement was first brought into prominence by Łukasiewicz. It was accepted without reservation by Leśniewski. It stipulates that no defined term should be used for the purpose of formulating the axiom system. Definitions can be introduced into the theory only after the axiom system has been stated in full (...) It follows from the requirement under discussion that a well constructed axiom system should be formulated with the aid of primitive terms of the theory (...) Both Łukasiewicz and Leśniewski attached great significance to the present requirement although it is often disregarded by various authors (...) The clarity which we achieve by using defined terms for the purpose of formulating axiom systems, is deceptive. In fact it only conceals the proper structure of the axioms, which may lead to misunderstandings since in such cases we add definitions not to the whole system, but to its part. In Leśniewski's theories the exclusiveness of the primitive terms is secured by the rules of procedure. ([Sobociński, 1956], pp. 57–58)

Also, according to Sobociński the length of an axiom system, for example, should be minimised for the system to be as simple as possible, primitive terms should be mutually independent and their number should be minimal. These ideals play a role also in other work by Tarski. An example is provided in [Tarski and Givant, 1999]:

> If we consider systems of full geometry which are based upon finite axiom sets, we can use as measure of simplicity the most obvious criterion, namely the total length of the axiom set, i.e., the sum of the lengths of all its particular axioms. ([Tarski and Givant, 1999], p. 192)

So Tarski's apparent revulsion against the use of defined notions in axioms did not fall out of thin air. With Tarski's Leśniewskian background in mind, we can understand where his dislike came from. From this perspective it seems much more remarkable that in [Tarski, 1929] Tarski did not pursue these ideals in practice, and, as we will see later, used defined notions in axioms himself. On top of that, in [Tarski, 1956a] Tarski proposes to take an additional notion as primitive, which consequently renders the primitive terms interdefinable.

§3. The foundations of the geometry of solids. We have seen that, by his own admission, Tarski developed the geometry of solids on a mereological basis following a problem raised by Leśniewski. As to Mereology as a deductive basis for the geometry of solids: we know that the development of Mereology had for Leśniewski a philosophical, and in particular an ontological motivation. Mereology is a theory of collective classes which, in opposition to set theory as a theory of distributive classes, does not involve dubious abstract entities such as empty sets. The development of geometry as the geometry of solids had a similar philosophical motivation. The geometry of solids construes geometry as a theory of some relations among *observables* and eschews points as ideal objects.

Jaśkowski is very clear on the aim of developing a geometry of solids. It was to eliminate the abstract, ideal terms from geometry:

> The reasons that contributed to the development of the geometry of solids were philosophical: the main goal was to reduce the abstract, ideal terms of geometry in the language of empirical observations. ([Jaśkowski, 1948], p. 298)

or to avoid the assumption of ideal objects:

> The philosophy of solids possesses a philosophical aspect. Euclidean point geometry was connected to the Greek idealist philosophy. The fact that each object that is accessible to the human senses has a non-zero volume, did not prevent the believe that there exist objects without volume. In the axioms of the geometry of solids one does not postulate this type of ideal objects any more. ([Jaśkowski, 1949], p. 77)

Concretely, this means that

> [a]ll primitive terms of zero-volume, like points, segments, planes etc., were eliminated. Because all primitive terms were solids or relations between solids this method has been called geometry of solids. ([Jaśkowski, 1949], p. 77)

Before Tarski, the geometry of solids had been studied by Huntington [Huntington, 1913] and gained recognition through Whitehead's Theory of Events [Whitehead, 1919, 1920], and—contrary to what is suggested by [Bostock, 2009]: 47)—the geometry of solids is in some form still in use today [Coquand, 1991], [Lubarsky, 2010].

"Theory of Events" seems like a strange name for the geometry of solids, but once we realise that in Whitehead's theory events are spatiotemporal regions the name no longer seems so strange.[5]

Although he did not himself build a proper system of the geometry of solids comparable to his other three deductive systems (Protothetic, Ontology and Mereology), Leśniewski also got involved with the geometry of solids to

[5]Whitehead's work, which was already known to Russell in 1914 but published only later, was immediately picked up in [Russell, 1914] (cf. preface), discussed by de Laguna [de Laguna, 1922] (who has *solids* and prompted Whitehead's successive version of the theory) and by Nicod [Nicod, 1923] (who has *volumes*). The first edition of [Russell, 1914] has only a generic reference to Whitehead, while later, revised editions carry references to [Whitehead, 1919, 1920] (many thanks to Gregory Landini for this information).

some extent. He reports in a long footnote of [Leśniewski, 1928] that in 1926 Tarski had brought to his attention the relationship between Whitehead's theory of events and his Mereology:

> In 1926 Tarski brought to my attention both the conception of 'events' developped in 1919 by Whitehead in his interesting book ***An enquiry concerning the principles of natural knowledge***, and its relation with my 'general class theory' . . .
> ([Leśniewski, 1988a], p. 171)

Leśniewski also mentions that Tarski had conjectured that the axiomatic foundation given by Whitehead of his theory of events was not "sufficient, as an axiomatic foundation for the notion of 'events' which Whitehead develops in his work" ([Surma, Srzednicki, Barnett, and Rickey, 1991], p. 260, n. 84). Leśniewski gives here only "tentative observations [. . .] which could be considered as steps towards a solution of Tarski's problem" (Ibid.)—and which indeed confirm Tarski's conjecture. Leśniewski's cautiousness is due to the fact that Whitehead's is not a formal theory of events ("Whitehead makes no attempt to put his conception of 'events' into the form of a deductive theory [. . .]" (Ibid.)) so that it remained difficult to understand how certain formulations had to be interpreted. A more extended treatment of Whitehead's theory along these lines is contained in [Leśniewski, 1988b]: 171–178. Although this piece is not dated by the editors, it cannot have been written before 1927 as it quotes Russell's "Analysis of Matter" which appeared in that year.

So in 1926 Tarski and Leśniewski were, if not exactly working together on the geometry of solids, at least looking at problems related to systems of the geometry of solids at the same time. In 1927, Tarski gives the talk later published as [Tarski, 1929].

In the light of the above, [Tarski, 1929] has, prima facie, a rather strong Leśniewskian appearance. Could we guess that if Leśniewski had built a system of the geometry of solids, it would have looked more or less like Tarski's? No. As we shall see in the remainder of this paper, except for the use of mereological notions, that wouldn't be a correct guess.[6] Two crucial aspects of difference are the underlying logic used by Tarski and the methodology of the axiomatic construction of the system, both non-Leśniewskian. We shall go deeper into these aspects in Sections 4 and 5 below.

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[6]Therefore, we disagreee with Luschei, who goes so far as to group together Leśniewski's and Tarski's systems of geometry as both building upon Mereology:

> Mereology is too general to be counted a geometrical theory but (if mereological ingredience is interpreted spatiotemporally) may be taken, together with the underlying logic and grammar of protothetic and ontology, as basis for systems of geometry proper . . . such as those constructed by Leśniewski and Tarski; (Luschei 1962: 150).

In the remainder of this section we will discuss a specific aspect of difference between Tarski's conception and construction of the geometry of solids and Mereology. As we have seen, Tarski had a particular take on the geometry solids.

> the specific character of such a geometry of solids—in contrast to each "point" geometry—is shown in particular in the law according to which each figure contains another figure as a proper part. ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 227) (our translation)

In other words: For Tarski a geometry of solids is *atomless*. How would Leśniewski have thought about this?

Leśniewski's Mereology is a theory of parts and wholes in general: no presupposition of what exactly these parts and wholes are is built in the system, whether they are concrete, and also in particular whether there are atoms: as Sobociński remarks in [Sobociński, 1971], Leśniewski's Mereology is neither atomistic nor atomless: no thesis can be proved to either effect from the axioms of Mereology (see also [Leśniewski, 1988b], p. 172). Now, how about the system of the geometry of solids that one could base on it? Would a Leśniewskian geometry of solids be atomless or atomistic? Although we have no textual support for Leśniewski's position, neither point of view seems to be a necessary part of the philosophical idea behind the geometry of solids as explained, for example, by Jaśkowski. The fact that Leśniewski in [Surma, Srzednicki, Barnett, and Rickey, 1991], p. 260 n. 84 takes steps towards the formalisation of an atomless geometry of solids does not count as evidence against what we have just said, since here Leśniewski is not putting forward his own system of the geometry of solids, but merely and explicitly trying to shed light on Whitehead's, which happens to be atomless.

By contrast, as we have seen, Tarski's specific characterisation of the geometry of solids clearly corresponds to an atomless point of view. Tarski does not express that this conception may possibly differ from his own views.[7]

Whitehead put the requirement of being atomless as follows:

> Every event extends over other events and is itself part of other events. ([Whitehead, 1919], p. 101)

Note that Whitehead speaks of *events* where Tarski speaks of *figures*.

In Leśniewski's attempt at a formalisation of Whitehead the first part of this statement becomes:

2a. $[a] : a\epsilon\text{event}. \supset .[\exists c].c\epsilon\text{event}.c\epsilon\text{cz}(a).$[8]
([Leśniewski, 1988a], p. 172)

[7]See footnote 14 where we show that the atomless status of Tarski's geometry of solids follows from Tarski's Axiom I.

[8]The term "cz" stands for the relation of being a part of.

So Whitehead's theory was atomless,[9] and so was Leśniewski's formalisation, and Tarski's foundations. This was not, however, the only path that was taken in that period.

Huntington's geometry of solids [Huntington, 1913] is *atomistic*. He defines a point as "any sphere which contains no other sphere within it" ([Huntington, 1913], p. 529), and continues as follows:

> It may be noticed that there is nothing in this definition, or in any of our work, which requires our 'points' to be *small*; for example, a perfectly good geometry is presented by the class of all ordinary spheres whose diameters are not less than one inch; the 'points' of this system are simply the inch-spheres. ([Huntington, 1913], pp. 529–530)

We will see later that Tarski, who also defined points, did so in a different way, and in a way that remains fully within Whitehead-style geometry of solids.

We might conclude that Tarski has added the characterisation of the geometry of solids as atomless to distinguish his Whiteheadian-style foundations from Huntington's. He was aware of Huntington's paper, which Tarski, consistent with his characterisation, regards as *point* geometry ([Tarski, 1929], p. 228) despite the fact that Huntington has spheres as primitives—and needed to clarify the difference in approach. The connection with Whitehead is visible on two other accounts. On the last page of the paper Tarski says that the procedure he uses to define *point* and *equidistance* and which enables him to fix Axiom 1 as we shall see below, is a special case of Whitehead's so-called method of extensive abstraction. But he also names as a new result of the paper

> the precise method of establishing the mathematical foundations of the geometry of solids, with the help of a categorical axiom

[9][Simons, 1987], p. 42 seems to imply that the atomless point of view is natural in light of the problem of continuum since Aristotle (he refers to [Whitehead, 1919], [Whitehead, 1920], [Whitehead, 1929], [Russell, 1914], [de Laguna, 1922] and [Nicod, 1923]). However, one can maintain that the question of whether the world is atomless or atomistic should be decided by the world, not by an a priori theory (see e.g., [Newman, 1928], a critique of Russell's ***Analysis of matter*** (1927), discussed in [Demopoulos and Friedman, 1985]). Leśniewski would have agreed. He built his deductive theories in such a way to avoid presupposing any particular way in which the world has to look like for those theories to hold. This is one reason why we think that Leśniewski's geometry of solids would have been, like Mereology, neither atomless nor atomistic—for the same reason why Mereology is neither. A theorem like 2a. could hold in a Leśniewskian geometry of solids in two ways. (1) we first obtain a system of atomless Mereology, then we build the geometry of solids on this basis by first (i) adding the primitive term "event" (possibly in an axiom) then (ii) stating a theorem equiform to 2a by appropriately substituting 'event' to bound variables in a theorem of Mereology (following the substitution rule). (2) We add an axiom to the geometry of solids from which that theorem follows. Tarski does (2), see p. 14. Neither seems desirable from a Leśniewskian point of view.

> system containing only one additional primitive notion: the notion of *sphere*. ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 231)

Tarski's paper seems indeed to have improved Whitehead's theory of events along the lines of one concern raised by Leśniewski: making the foundations more precise.

§**4. Tarski's axiomatisation.** Tarski wrote [Tarski, 1929] in the style of the working mathematician: informal but precise. This makes it hard to recognise which system should be seen as the background logic for this paper. Although Tarski says that he presupposes Leśniewski's Mereology, even from the notions he uses it becomes clear that matters are not so straightforward. In fact, it becomes apparent that Tarski does not only borrow ideas from Leśniewski's system, but also from the system of Whitehead and Russell's ***Principia mathematica***. Later we will see that this observation has crucial implications. Furthermore Tarski uses topological notions that he takes from Kuratowski ([Kuratowski, 1922]), such as regular open and regular closed sets. Kuratowski's approach, however, does not directly fit either of these systems. Let's now turn to the mathematical content of Tarski's work.

Tarski starts out from the mereological notions *being a part of* and *sum*. *Being a part of* is taken to be a primitive notion (of Leśniewski's Mereology), but *sum* (collection) is not. Only in the 1956 edition does Tarski give the definitions of *sum* and other defined notions of Mereology, giving also its axioms. These were supposed to be known in 1929, so this may be a reason why he does not give them in the earlier edition. It may also be that he omitted them for other reasons. However, the formulation of the definitions and axioms of *Leśniewski's* Mereology shows particularly well—paradoxically enough—the important role that *Russell's* system plays here with the respect to the issue of the background logic of Tarski's paper. We will go deeper into this in Section 5.

Apart from *being a part of*, Tarski takes *sphere* as the only primitive notion specific to the geometry of solids. This suffices to define other geometrical notions. The definitions come in this order: first Tarski gives the definitions of being *externally tangent* (Definition 1), being *internally tangent* (Definition 2), being *externally diametrical* (Definition 3), being *internally diametrical* (Definition 4), being *concentric* (Definition 5), *point* (Definition 6), *equidistant* (Definition 7), *solid* (Definition 8), and being a point *interior* to a solid (Definition 9). The definitions of *point* and *solid* read as follows:

> **Definition 6**. Point *is the class of all spheres that are concentric with an arbitrary sphere.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 229)

Definition 8. Solid *is an arbitrary sum of spheres.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 229)

We shall leave the issue of the competitive formal systems for the next section (which is connected with the use of 'class' in Definition 6 and 'sum' in Definition 8), but at least the following should be noted here. One might think that Tarski's notion of *point* repudiates the idea behind the geometry of solids. However, contrary to Huntington, who took points to be minimal spheres, Tarski defines them as a whole class of spheres. This definition is in line with the idea behind the geometry of solids: a point is not seen as an ideal object with zero volume, as would be the intersection of all spheres that are concentric with an arbitrary sphere would be. Yet it must be said that the extent to which a definition of this sort can be said to be really successful in this respect depends on what classes are taken to be: if they are themselves ideal objects, then the project is still problematic. If classes are instead conceived as concrete objects, as Leśniewski's mereological sums are, then the project can succeed.[10] We come back to the differences between 'class' and 'sum' in the following section.

After the definitions, Tarski merely mentions that because it is well known that all notions of "normal" geometry can be defined by those of *point* and *being equidistant of two points to another*, all other notions of "point geometry" can now also be introduced, like, especially, being *a regular open set*, but, Tarski says, "I dispense here with the explicit statement of the definition in question" ([Tarski, 1929], p. 230). And here he refers to Kuratowski's work [Kuratowski, 1922].

This seems a little too fast. The definition that Kuratowski gives is not in terms of points and equidistance, but in the language of abstract topology.[11] So it still needs to be explained how Kuratowski's definition can be expressed in terms of *point* and *equidistance*, and consequently in terms of *sphere* and *being a part of*.

Subsequently the axioms follow, which are just four in total. Contrary to Tarski's motivation for other research in geometry, and contrary to his own dislike for the use of defined notions in axioms, we will see that this is exactly what he uses here.

[10]"[A]lthough we perceive solids, we perceive no abstractive sets of solids [. . .] In accepting the abstractive set, we are as veritably going beyond experience as in accepting the solid of zero-length." ([de Laguna, 1922], p. 460). Cf. also the discussion in section 2.2 (Eliminativist theories) of [Varzi, 2008].

[11]We shall say that A is a regular open set, when

$$A = A^{-1-1}.$$

([Kuratowski, 1922], p. 194)
In other words: A is a regular open set when it is identical to the complement of the closure of the complement of its closure.

> **Axiom 1**. *The notions of point and equidistance of two points to a third satisfy all axioms of ordinary Euclidean geometry of three dimensions.* [Footnote: An axiom system of ordinary geometry that contains these notions as the only primitive notions has been put forth by Pieri in his aforementioned work.[12]] ([Tarski, 1929] in [Givant and Mackenzie, 1986a] p. 230)

We want to note two things about this axiom. Firstly, the axiom is somewhat puzzling because to the modern reader the notion of satisfaction gives it a (what we would now call) metamathematical flavour.[13]

Secondly, from this axiom it follows that Tarski's geometry is indeed atomless: It is a result of Pieri's axiomatisation, mentioned in Tarski's Axiom 1, that every sphere contains another sphere as a proper part.[14] And because every solid holds a sphere as a part (by the definition of *solid* and *sum*), and because that sphere has another sphere as a proper part, it also follows that every solid holds another figure as a proper part (by transitivity of the relation of *being a part*). In other words: Tarski's axiomatisation founds indeed a geometry of solids according to his own definition of the field.

The remaining three axioms, the aim of which is to make the system categorical, combine what Tarski calls notions of the geometry of solids (*solid* and *being part of*) and notions of ordinary point geometry, which are in fact notions of topology (*regular open set* and *inclusion*):

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[12]The "aforementioned work" is Pieri's point and sphere memoire [Pieri, 1908].

[13]One should however keep in mind that Tarski's notion of "satisfaction" (and connected to that the notion of "model") differs considerably from the meaning that we attach to it nowadays:

> Let's imagine that in the axioms and theorems of the constructed science the primitive terms have been replaced everywhere by corresponding variables (to avoid making our considerations more complicated we leave aside the theorems containing defined terms). The laws of the science have ceased to be propositions and are now what have been called in modern logic propositional functions: expressions with the grammatical form of propositions, and that become propositions when one replaces the appearing variables by suitable constant terms. Considering such and such other objects, we can examine whether they satisfy the so-obtained axiom system, that is to say whether the names of these objects, put in place of the variables, make these axioms true propositions; if it turns out that this is the case, we say that such objects form a *model of the axiom system* under consideration. ([Tarski, 1937], pp. 331–332, own translation)

This shows how in Tarski's view one axiom system can be a model for another one. A thorough discussion would go beyond the scope of this paper.

[14]Sketch: We say that a sphere through D with centre B is a part of a sphere through C with centre A , if there exists a point on the sphere through B with centre A that lies on $\overline{AC}$; we say that it is a *proper part* when this point does not coincide with C. Let S be a sphere through X with centre Y. Then, by Postulate XII a midpoint M of X and Y exists. Then also a sphere through M with centre Y exists. This sphere is a proper part of the former.

Axiom 2. *If A is a solid, the class a of all points interior of A is a regular open set*.

Axiom 3. *If the class a of points is a regular open set, there exists a solid A such that a is the class of all interior points.*

Contrary to Szczerba's statement ([Szczerba, 1986], p. 911) that Tarski has intended his spheres to correspond to open (Euclidean) balls, or to what we find in [Aiello, Pratt-Hartmann, and Benthem, 2007] (p. 4) that the solids are regular closed sets in $\mathbb{R}^3$ (from which we can deduce that spheres correspond to closed Euclidean balls), it seems that Tarski, as he also explains, left open the possibility of interpreting *solids* as either regular open or regular closed sets throughout, meaning that the spheres can correspond to either open or closed balls respectively.

Then the last axiom reads:

Axiom 4. *For solids A and B: if all interior points of A are interior points of B, then A is a part of B.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 230)

Then he gives two alternatives of Axiom 4 as an example of how he expects that the axiom system can be simplified by using intrinsic properties of the geometry of solids (like *being a part of*)

Axiom $4'$. *If A is a solid and B is a part of A, then B is also a solid.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 230)

Axiom $4''$. *If A is a sphere and B is a part of A, then there exists a sphere C that is a part of B.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a] p. 230)

Finally Tarski mentions that the given axiom system is categorical and that it is equiconsistent with ordinary three dimensional Euclidean geometry. We will not go into these issues here.

§5. The role of Russell's system. We have seen that Tarski's foundations of the geometry of solids, although apparently based on Leśniewski's Mereology, contrary to the original non-committal spirit of the latter, is atomless, and contrary to the general methodological convictions of Leśniewski (and apparently also Tarski's own), admits of defined notions in axioms. However, one might think that, barring these deviations, Tarski is still working inside a Leśniewskian framework insofar as he still works with notions from Mereology; besides, Tarski might still presuppose a Leśniewskian background logic. That the background logic might be Leśniewskian is suggested for instance by the use of subject-predicate form of axioms and definitions "A is b"), as we find in Leśniewski's Ontology (roughly, Leśniewski's predicate logic, which in turn presupposes Protothetics, roughly, Leśniewski's propositional logic).

We will argue in this section that the background logic of Tarski's paper is not Leśniewski's Ontology, but that this role is played at least in part by the logic of the *Principia*. We will consider the role which that idea taken from Russell's system plays in the 1929 edition of Tarski's paper, but note that the significance of the role played by those ideas becomes even more clear when we consider the 1956 edition. It is crucial—as we will see—to assign that system a role in the background logic of Tarski's paper, both to explain the 1956 additions and to clarify the mathematical content of the paper.

Our main idea is that, to make sense of the paper, we have to distinguish three ways in which the *domain of discourse*—i.e.: the "things" that a theory is about—can technically get embodied in a mathematical or formal theory. The first of these three ways is by appealing to the notion of the range of the quantifiers, the second by appealing to a "special" primitive notion in the language of the theory, and the third by specifying a set of individuals akin to objects of the first type in Russell's simple type theory.

In making this distinction we are influenced by Mancosu ([Mancosu, 2006]) who also differentiates between the *range of the quantifiers* and the *domain of discourse*. We will come back to Mancosu's distinction at the end of this section. However, for now it is important to note that by contrast to what we do, when Mancosu distinguishes these notions, he is not speaking about two different levels, namely a formal one embodied in the theory and an informal one that needs to be codified into the theory. The domain of discourse is for him already codified into the theory. Mancosu offers in his papers examples of Tarskian theories in which these formal constructs relate in various ways to each other. We shall just note here that the exact way in which domain of discourse (be it specified by a primitive notion in the language or not), range of the quantifiers and individuals relate in the practice of a working mathematician in Tarski's time depends on the background logical theory. Yet saying anything definite on the background logic of a sketchy paper like this one, in which notions from concurrent type-theoretical deductive frameworks (*Principia* and Leśniewski's systems) are mixed up with topology is no straightforward task. This scenario is of such conceptual complexity that we feel that particular care must be used in approaching the issue.

Let's start with the 1929 edition, or rather with a summary Tarski made of it about a year later (so 1930; our estimation). It contains the following passage:

> By ,,geometry of solids" we mean a geometrical system in which the points, lines, planes do not occur as individuals (things of the 1st type) at all, and in which every spatial figure contains another spatial figure as a proper part. ([Tarski, 1930])[15]

[15]This appears to be a draft for the biography included in [Ajdukiewicz et al., 1935], but differs considerably from it.

Leśniewski's systems use a theory of simple *linguistic* types: if Tarski's background logic had been Leśniewskian, Tarski would have taken the 'points', the 'lines' and the 'planes' in the quote as *expressions*, not as *things*, and those expressions would be typed only according to their linguistic category. Tarski seems instead to draw type-theoretical notions from a simplified version of the logic of *Principia* (this is also confirmed by [Lejewski, 1983], p. 64) to which extralogical notions from Mereology are added and whose (adapted) axioms are here taken for granted. This whole forms the basis of the geometry of solids.

As Mancosu has noted ([Mancosu, 2006], p. 217) the background logic often used by Tarski is a simple theory of types. Since Leśniewski's logic includes a theory of simple types, one might think that Tarski is working within a Leśniewskian logic. But that's not the case: in the paper Tarski uses a Russellian notion of class, and—as we just saw—a Russellian conception of (objectual) types.

The interplay of mereological Leśniewskian notions and Russellian type theory shows also particularly well in Definitions 6 and 8 (which we saw in Section 3, and we now display again, this time with Tarski's footnotes):

> **Definition 6**. Point *is the class of all spheres that are concentric with an arbitrary sphere.* [Footnote: I use here everywhere the term "*class*" in a very different sense than the one adopted by Leśniewski in his aforementioned system and more conform the one of *Principia mathematica* (Vol. I, 2$^{\text{nd}}$ edition, Cambridge 1925) by Whitehead and Russel [sic]. The spheres (resp. the solids) have thus been treated here as individuals, that is as objects of the lowest rank, while the points, as class of these spheres, are objects of a higher rank (the second rank)]. ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 229)
>
> **Definition 8**. Solid *is an arbitrary sum of spheres.* [Footnote: The term ,,sum" coincides here with ,,collection" in Leśniewski's Mereology.] ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 229)

First it can be noted that, in a Russellian way of reading Definition 6, points are not things of the first type. So indeed Tarski stays here within the field of the geometry of solids, according to his own definition of the latter—as we just saw in the 1930 passage quoted above. Apart from the fact that his requirement that every figure contains a figure as a proper part prevented him from doing this, Tarski could not have adopted a definition of point à la Huntington (i.e., points as minimal spheres), because points would then, like spheres, be things of the first type.

In Definition 8 the interplay of the Russellian type-theoretical basis and the mereological notion of sum and topology gets really interesting and complicated, because it necessitates, as we will argue, an acceptance of

Russellian-like individuals as a way for Tarski to restrict the scope of a statement or definition. Here the precise definition of *sum* is important.

This is given as part of the full axiom system of Mereology which is present only in the 1956 edition of the paper ([Tarski, 1956a]), which reads as follows.

> DEFINITION I. *An individual X is called a* proper part *of an individual Y if X is a part of Y and X is not identical with Y.*
>
> DEFINITION II. *An individual X is said to be* disjoint *from an individual Y if no individual Z is part of both X and Y.*
>
> DEFINITION III. *An individual X is called a* sum *of all elements of a class α of individuals if every element of α is a part of X and if no part of X is disjoint from all elements of α.* ([Tarski, 1956a], p. 25)
>
> POSTULATE I. *If X is a part of Y and Y is a part of Z, then X is a part of Z.*
>
> POSTULATE II. *For every non-empty class α of individuals there exists exactly one individual X which is the sum of all elements of α.*[16] ([Tarski, 1956a], p. 25)

In Tarski's formulation of this axiom system of mereology we can again observe several non-Leśniewskian elements. First, just as Tarski had done with the axiom system for the geometry of solids in the 1929 edition, the postulates follow the definitions *and* one of the postulates even uses a defined notion.[17] Second, Tarski speaks of a "non-empty class", whereas for Leśniewski collections are always non-empty. So supposing then that Tarski was working against a Leśniewskian background logic, this remark would have been superfluous. Third and most importantly for our purposes, the term "individual" appears several times. We will argue below that this should be understood in Russellian terms.

[16]Note that there is a change from the use of "Axiom" in [Tarski, 1929] to the use of "Postulate" in article II of [Tarski, 1956b]. From a letter from Montague, representing Tarski, to Woodger in 1954, it is clear that this change was not initiated nor approved by Tarski:

> We cannot understand why, in this article, German 'Axiom' has been translated by 'Postulate' while, in later articles, the term 'Axiom' is everywhere used. This is especially unfortunate since 'Postulates' in article II are real axioms—statements accepted as true without proof—and not postulates characterizing a formal system. (Montague to Woodger, March 9, 1954, Woodger papers: C [Correspondence], Special Collections Library, University College London)

The fact that Montague speaks of the *German* word "Axiom" may be a confusion. We are not aware of a German version of [Tarski, 1929].

[17]Leśniewski himself has formulated axioms of Mereology using defined notions on at least two occasions ([Leśniewski, 1988a], p. 60; [Leśniewski, 1927, 1928, 1929, 1930, 1931], p. 232) in his later career. He has justified this by referring to chronological or historical accuracy, and stated in these cases either that this had troubled him ([Leśniewski, 1927, 1928, 1929, 1930, 1931], p. 315) or that it was not correct ([Leśniewski, 1988a], p. 92).

We will return now to the problem of how to interpret *sum* (and as a consequence of how to interpret "individual"), but within a broader argument and as part of a bigger problem. This consists of first establishing the range of the quantifiers for Tarski's foundations, that we have discussed in Section 4. There are only three possibilities for this: either it consists of less than the class of all solids (like the class of all spheres), or it consists precisely of the class of all solids, or it consists of more than just solids. Before explaining this, let us first remark that we will show that the last option is entangled with the proper interpretation of 'sum', and with the use of the Russellian term 'individuals' in its definition.

So what is the range of the quantifiers for Tarski's foundations? What are the intended values of the variables? Let us first consider the possibility that the range of the quantifiers consists of the class of all spheres, so the objects that are singled out by the primitive notion as well. However, because the class of spheres is not closed under summation, i.e., the class of spheres is strictly smaller than the class of solids in this strong interpretation, it would not make sense to have a predicate for *solids* in this case. This suffices to exclude the possibility that the range of the quantifiers is less than the class of all solids in Tarski's foundations.

Can the range of the quantifiers be the class of solids? In [Gruszczyński and Pietruszczak, 2008] we find the following appealing argument against this. Reconsider Axiom 4′:

> **Axiom** 4′. *If A is a solid and B is a part of A, then B is also a solid.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a], p. 230)

When we take the range of the quantifiers to be the class of all solids, the conclusion that *B* is also a solid would be the case even without stating it in an axiom, and this whole axiom (not just the requirement that *A* be a solid) could be omitted. So having the range of the quantifiers be the class of all solids would make some of the axioms trivial, Gruszczyński and Pietruszczak argue ([Gruszczyński and Pietruszczak, 2008], p. 483). And this is something that Tarski—so one might conjecture—could not have meant to happen. (Note that having superfluous axioms would not be very Leśniewskian either.) We are not going to settle the issue of whether Gruszczyński and Pietruszczak's triviality point is right here. What we cannot exclude is that Tarski was simply non-committal on this issue, and so we conclude the following: the range of the quantifiers contains the class of all solids, but its exact extension is unspecified.

Having narrowed down in this way the possibilities that the range of quantifiers could be taken to be, we now come back to the definition of *sum*:

> Definition III. *An individual X is called a* sum *of all elements of a class α of individuals if every element of α is a part of X and if no part of X is disjoint from all elements of α.* ([Tarski, 1956a], p. 25)

Let us assume for a moment that "individuals" are the objects that the quantifiers range over. We will argue that this will lead to problems.

First note that a range of quantifiers that contains solids, but the precise extent of which remains *unspecified*, will not do here. What object is considered to be the sum depends on what objects exactly we may chose from. If we assume that X may be chosen out of some strictly larger set than just solids, then for *sum* we have to pick a different object than if we had restricted our choice to solids only; see also footnote 18. So, leaving the precise extent of the range of quantifiers unspecified would lead to a non-well-defined notion of *sum*.

Tarski says that the notion of *sum* he introduces is that of Leśniewski's Mereology, but Tarski's use of topological notions complicates things. What notion of *sum* is meant in this paper becomes more clear when we study its relation to Axioms 2 and 3. These axioms establish a correspondence between regular open sets and interior points of solids:

> **Axiom 2**. *If A is a solid, the class a of all points interior of A is a regular open set*.
> **Axiom 3**. *If the class a of points is a regular open set, there exists a solid A such that a is the class of all interior points.* ([Tarski, 1929] in [Givant and Mackenzie, 1986a] p. 230)

If we take seriously Tarski's intention to let solids correspond to regular open or regular closed sets, it is important that the sum of arbitrary solids is a solid. Or, in other words: that the class of solids is closed under summation as introduced in Definition III. Were that not the case, then the correspondence between the interior points of solids and regular open or regular closed sets stated in Axiom 2 and 3 would be faulty, as the following example illustrates.

We will follow Tarski's suggestion ([Tarski, 1956a], p. 29) to let spheres correspond to the interiors of Euclidean spheres, and—deviating from Tarski, who clearly avoids the problem we present below—the relation of parts and whole as the inclusion relation between sets (and not, as Tarski does, as the inclusion relation *restricted to non-empty regular open sets* ([Tarski, 1956a], p. 29; a significant pick.) Additionally, as not unlikely in the context of Euclidean geometry, we take the range of the quantifiers to be the collection of sets, which is strictly more than *solids*, or, to be more precise: more than the interpretation of solids in Euclidean geometry.

As a simplification—but not an *over*simplification—we explain the problem in the 1-dimensional case, instead of the 3-dimensional case. A 1-dimensional "sphere" then corresponds to an open interval; a 1-dimensional "solid" to an arbitrary sum of those. A similar argument to the argument below can be given for the 3-dimensional case.

Note that the interval $(0, 1)$ does not only correspond to a *sphere*, it is also the class of all interior points of some 1-dimensional solid by Axiom 3 and the observation that $(0, 1)$ is a regular open set—the interior of the closure of $(0, 1)$ is again $(0, 1)$—and hence corresponds to a *solid* in our setting.

Also the interval $(1, 2)$ corresponds then, of course, to a solid. So what would be the sum of solids $(0, 1)$ and $(1, 2)$? Well, according to Definition III in our current interpretation of individuals, we will have to look for some object that contains as parts the solids corresponding to the intervals $(0, 1)$ and $(1, 2)$, and of which no part is disjoint from these solids. In other words: the smallest object that includes both the solids corresponding to $(0, 1)$ and $(1, 2)$. The only candidate for this is the object corresponding to the *union* of the intervals $(0, 1)$ and $(1, 2)$, i.e., $(0, 1) \cup (1, 2)$.

There is more that we can say about this object: by Definition 8 we know it is a solid, and by Axiom 2 we find that it is thus a regular open set. But is it a regular open set? No.

Let's verify that. The closure of $(0, 1) \cup (1, 2)$ is the closed interval $[0, 2]$. The interior of that is the open interval $(0, 2)$ which is not equal to $(0, 1) \cup (1, 2)$.[18] So we see that the sum of the two solids we started out with is *not* a regular open set. This is not only a formal problem of the axiom system (since what Tarski calls "regular open sets" would not be what is normally called so), but moreover clashes with Tarski's intention to let solids correspond to regular open or closed sets. So if we assume—as we are currently doing—that the class of individuals contains more than just solids (all sets, in this case), the class of solids is not closed under summation.

From the above we conclude, to come back to Definition III, that the class of individuals should be the class of *solids*: One cannot be non-committal on its precise extension. Because, taking the class of individuals to be unspecified to some extent—as we take the range of the quantifiers to be—leads to the problem explained above, namely that the regular open (or, respectively: closed) sets in Tarski's theory do not correspond to what one would normally call regular open (resp. closed) sets.

Tarski's use of individuals is in line with Russell's use of individuals in simple type theory, because in that theory the individuals are the objects of first type—and this squares both with Tarski's characterisation of the geometry of solids in the 1930 passage and the footnote of Definition 6. Note however that this use is not in line with Leśniewski's systems, where 'individuals' in this sense have no place. Moreover, Leśniewski's idea of formal systems obeys a hierarchy ruled by generality constraints. If the foundations of the geometry of solids were to be founded upon a system of Mereology, then that system should be of a general, multiple-purpose sort, and not one already geared towards the geometry of solids (i.e., with sum defined only on certain objects, i.e., on solids). By mentioning individuals that are intended to correspond to solids already in the axiomatisation of

[18] So the smallest regular open set that includes $(0, 1)$ and $(1, 2)$ is $(0, 2)$. So we see that the precise object that is the sum of two solids depends on the set that we may chose X from.

Mereology, the idea of that hierarchy—an axiomatic layer of the geometry of solids upon a distinct axiomatic layer of Mereology as a more general theory—is abandoned. Note, also, that like types, quantifiers in Leśniewski's systems are not objectual, and nominal variables cannot be said to 'range' over anything, let alone over a set of objects identical or bigger than a collection of objects said 'of the first type' called 'individuals'. This said, however, it remains difficult to claim something definite as to the general formal setup of Tarski's paper, i.e., the 'background logic', for the paper shows a mixture of elements in this sense.[19]

The findings on the range of the quantifiers that we have presented in this section are inspired by Mancosu's analysis in [Mancosu, 2006], but differ in the sense explained at the beginning of this section. To see where exactly the difference lies, let us have a closer look Mancosu's ideas. In 2006 Mancosu emphasises, in an argument against Gomez-Torrente, that Tarski often works with theories in which the domain of discourse—i.e., what a theory is about—is singled out by a primitive predicate and which allow for the possibility of individuals falling outside the domain of discourse:

> (...) I will show that Tarski would reject that every mathematical theory he is considering must be inconsistent with the statement that there are individuals that fall outside the 'domain of discourse'. (...) I will provide evidence that Tarski and other logicians of that time made a distinction between 'range of the quantifiers' (or range of the significance of the individual variables) and 'domain of discourse', that is they entertained theories that, while presenting a predicate S for the 'domain of discourse', either prove $\exists x.\neg S(x)$ or simply do not decide the issue either way. ([Mancosu, 2006], p. 220)[20]

[19] An anonymous referee has pointed out that our choice of keeping open the option of the range of the quantifiers as being bigger than the individuals involved in the theory contrasts with the Russellian setup of Tarski's paper. For in the case of a Russellian setup, the range of the first-order variables is always identical to the individuals (and is either strictly larger than the domain of discourse or identical to it; the latter case appearing only when the domain of discourse is given by a single primitive predicate). In non-Russellian setups the range of the quantifiers is strictly larger than the domain of discourse. If we were to take [Tarski, 1929] to be undoubtedly Russellian, we could thus immediately conclude that in [Tarski, 1929] the range of the quantifiers is identical to the class of individuals. As we have mentioned, however, the situation in this paper is complicated by the fact that Tarski is drawing from a variety of theories, in particular the Russellian nature of [Tarski, 1929] is not immediately clear in the presence of Leśniewski's systems (see also [Lejewski, 1983]). Moreover, arguments have been offered that the range of the quantifiers cannot be identical to the class of individuals; see the argument from triviality from [Gruszczyński and Pietruszczak, 2008] we mentioned earlier in this section. By keeping open the option that the range of quantifiers contains more than individuals allows us to keep the most neutral interpretive stance with respect to [Tarski, 1929].

[20] Mancosu makes this distinction in the context of the discussion on Tarski's conception of model in 1936. He argues in favour of what he calls "the relative fixed domain conception".

In these theories the domain of discourse consists of the objects a theory is about and these are represented by a certain primitive notion in the theory at hand. Note that this is not to say that there can be no other primitive notions, just that there is one special one.

It is somewhat problematic to apply to Tarski's foundations of the geometry of solids the idea that the domain of discourse is represented in a theory by a primitive notion.[21] For whereas Tarski's theory is clearly about *solids*, these do not get represented by a primitive notion. The objects that do get represented by a primitive notion are *spheres*, which, when we close under summation the objects falling under it, yield the domain of discourse (*solids*). We will argue in the next section that exactly this discrepancy between the domain of discourse and the primitive notion of the theory may explain a passage that Tarski added to the 1956 edition.

Before we turn to that, let us explain the precise relation between Mancosu's distinction on the one hand, and ours on the other. We follow Mancosu in the distinction between *range of the quantifiers* and *domain of discourse*. We wish to propose a distinction of levels between the pretheoretical, non-technical *idea* of a domain of discourse of a science, and the specific, technical ways in which that idea gets embodied in a (formal) *theory*. The pre-theoretical idea of a domain of discourse is to be found at many junctures in history, starting from antiquity, whenever the problem of singling out the proper field or *genus* of a science is addressed.[22] In modern times, this idea has been codified in various technical ways. The range of the quantifiers is just one of these specific, technical ways in which the idea of the domain of discourse gets codified in a theory. The range of quantifiers is a notion that has a direct, unique relation with a formal theory: it is the intended value of the variables in a formal language. But there is not just one way in which this specification of the domain of the discourse takes place, or works in a theory, nor even two. Nowadays the domain of discourse is often taken to correspond to the range of the quantifiers. Mancosu stresses a second way in which the domain of discourse, to put it in our terms, got codified in a theory in Tarski's times: it often got reflected in the theory by a primitive notion (and not by the range of the quantifiers). We take singling out a primitive notion in the language and considering the collection of objects picked out by that notion to be a second way to codify technically the domain of discourse in a theory. We have seen that in the case of [Tarski, 1929], the pre-theoretical idea of the domain of discourse is introduced into the formal theory in a

Exactly how the findings that we present in our current paper can be seen as additional arguments for this conception will be part of future research.

[21]Note that Mancosu ([Mancosu, 2006]) discusses and is aware of the variety of systems used by Tarski in his logical practice. Especially, he emphasises that there are theories in which no single predicate is available to characterise the domain of discourse.

[22]On this see the 'Domain Postulate' of [Jong and Betti, 2010] and the discussion in [Cantù, 2010]

third way: Russellian-like individuals. What is the domain of discourse of the geometry of solids? Solids. These are, in [Tarski, 1929], the objects of the first Russellian type of the theory, the individuals. Acknowledging this fact is fundamental to make sense of this paper.

§**6. Universe of discourse.** Mancosu's "domain of discourse" comes very close to a term Tarski himself uses in the passage that he added in the 1956 edition [Tarski, 1956a]: "universe of discourse". We will see how this passage connects to the fact that in [Tarski, 1929] the domain of discourse (solids) does not get represented by a primitive notion (because the primitive notion is here *sphere*), although that would have been more common in that period.

Tarski wrote:

> It should be noticed that the class of all solids (and not the class of all spheres) constitutes what is called the 'universe of discourse' for the geometry of solids. For this reason it may be convenient to adopt the notion of solid as an additional primitive notion. In this case Def. 8 should, of course, be omitted; in its place the fact that the class of solids coincides with that of arbitrary sums should be stated in a new postulate. ([Tarski, 1956a], p. 28)

Two parts of this passage call for explanation: first, that solids are indicated to be the universe of discourse; second, that spheres are denied to be the universe of discourse. Both should be part of an explanation of what Tarski had in mind here; i.e., of what "universe of discourse" means.

An explanation is offered by [Gruszczyński and Pietruszczak, 2008], but we will argue that it goes wrong in failing to clarify why one should want to deny that *spheres* are the universe of discourse. Moreover, their argument does not take into account the distinction between range of the quantifiers and domain of discourse that we have made in the previous section. This leads them eventually to the conclusion that Tarski has made a mistake.

We will see later that by taking the distinction into account, we will be able to put forward a more satisfying explanation of the passage above.

Let's have a look at the argument of [Gruszczyński and Pietruszczak, 2008]. First the authors try to establish the range of the quantifiers in [Tarski, 1929], which they take as what Tarski means by *universe of discourse* ([Gruszczyński and Pietruszczak, 2008], p. 482) and settle for (*spatial*) *region*.

Their strategy for arguing that the class of regions forms indeed the range of the quantifiers is to show that the class of solids does *not* make up the (whole) range of the quantifiers. We have seen a similar argument in Section 5 (their triviality argument). From this, however, they conclude that: "The above examples seem to prove that the notion of *region* is present in Tarski's theory, although it is absent from Tarski's paper" ([Gruszczyński and Pietruszczak, 2008], p. 483). Now, we have agreed that the range of the quantifiers has an (unspecified) extension; to call this *regions* is innocent, but to claim, as [Gruszczyński and Pietruszczak, 2008] do, that there is some preconceived notion of region that gets reflected in Tarski's paper would require some stronger argumentation.

Having taken "universe of discourse" to mean the range of the quantifiers, they interpret Tarski's later addition as follows:

> Thus it seems that for Tarski the theory with Postulates 1–4 and Definition 8 is equivalent to the theory in which: (a) all regions are *solids*, (b) we take Postulates 1–4 together with:
> ($\star$) *any region is the mereological sum of some set of balls.*
> ([Gruszczyński and Pietruszczak, 2008], p. 484)

Then the authors show that the equivalence between these two theories does not hold. So it seems that Tarski must have made a mistake.

However, first of all, it is not at all obvious that Tarski's "universe of discourse" should be interpreted as meaning the range of the quantifiers. For one, this interpretation does not explain why Tarski points out that the class of all *spheres* does not constitute the universe of discourse. No one would have the false idea that *spheres* make up the range of the quantifiers (as we have also pointed out in Section 5), so it would be very strange for Tarski explicitly to deny this. Second, [Gruszczyński and Pietruszczak, 2008] does not consider any other possibility, like interpreting "universe of discourse" as domain of discourse, or as the class of individuals, or perhaps even as something else entirely.

We say that Tarski's "universe of discourse" should be interpreted as our domain of discourse, namely the objects that a theory is about. We have argued (with Mancosu) that logicians of Tarski's time often represent these objects by a primitive notion of the theory. The motivation for Tarski's remark above can then be clarified by the discrepancy that we have here between the domain of discourse (solids), and the lack of a primitive notion for it (since the primitive here is *sphere*).

[Mancosu, 2006] argues that the domain of discourse in Tarski's time was often represented by a predicate symbol in the theory. Except by reference to Tarski, Mancosu shows this by means of, for example, a text by Langford [Langford, 1926] (see [Mancosu, 2006], p. 220). Assuming that this was indeed common practice, a reader of [Tarski, 1929] may thus look for a primitive notion and, finding *sphere*, conclude wrongly that the class of all spheres makes up the domain of discourse, or at least may get somewhat confused by the discrepancy here. This means that it would make sense to deny spheres the role of domain of discourse, and at the same time, according to common practice, to adopt solids as a primitive notion. The methodological worry that the theory then contains *two* primitive notions whereas one would suffice, was apparently of lesser concern to Tarski. This confirms that

Tarski is not concerned with any principal question of methodology in this paper.

Moreover, this interpretation lifts the pivotal problem of [Gruszczyński and Pietruszczak, 2008]. Changing the range of the quantifiers would indeed be a major step, but to add another primitive notion is formally of so little consequence that Tarski indeed could not have spent many more words on it than he has actually done. What is left is only to spell out the changes. Instead of Definition 8 the following axiom should be adopted, call it Axiom 5:

Axiom 5. *A solid is an arbitrary sum of spheres.*

in which *solid* appears as a primitive notion. That is all.[23]

§**7. Conclusion.** From the above we conclude that although Tarski refers to Leśniewski's system in [Tarski, 1929], it can be argued that he was not working in the spirit of the latter's tradition. This is shown, for example, by the fact that Tarski's foundation of the geometry of solids is simply atomless (instead of being neither atomless nor atomistic). More importantly, Tarski uses defined notions in axioms, a move which not only breaches his own principles, but also conflicts with the mores in the Polish tradition. Finally, Tarski's use of Russellian notions is also clearly non-Leśniewskian. The paper [Tarski, 1929] does not seem to obey any particular methodological conviction. It shows instead a generally pragmatic attitude admitting a variety of methods and tools, more typical of the practice of a working mathematician than that of a philosopher.

The 1956 edition of this paper ([Tarski, 1956a]) reinforced this conclusion not only by its introduction of even more axioms containing defined notions, but also by its proposed addition of *solid* as a primitive notion of the theory, which would make the set of primitive notions dependent, i.e., interdefinable. Scrutiny of the passage in which Tarski proposes this, together with the newly added axioms of Mereology and the axioms of the geometry of solids, necessitates a distinction between the range of the quantifiers, the (objects falling under the) primitive terms, and the collection of individuals of a theory. Only then can we make sense of the paper. The explicit recourse to a collection of individuals making up the first type in a Russellian type theoretical framework in the 1956 edition shows even more clearly that Tarski was not truly working in Leśniewski's system.

The 1956 proposal itself, finally, to add *solid* as a primitive notion, can be explained when one recalls that it was common in the beginning of the 20th century to represent the domain of discourse by a primitive notion of the theory, and that using *that* was the way to restrict the scope of a

[23] Another motivation for Tarski's addition of the passage above may be related to the interconnectivity between [Tarski, 1956a] and [Tarski, 1956c] (on Boolean algebra). See [Loeb, 2011].

statement or definition, and not (as Tarski) by reference to the individuals of a Russellian type theoretical framework. The change that he suggests would have the effect of identifying the extension of the primitive notion *solid* and the collection of individuals, and thus of solving the problem.

Tarski's suggestion does not need to be seen as motivated by methodological worries (which even seems unlikely, given the other methodological issues with this paper), but may simply stem from the desire not to be misunderstood, or may be intended to simplify further use or interpretation of this theory.

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