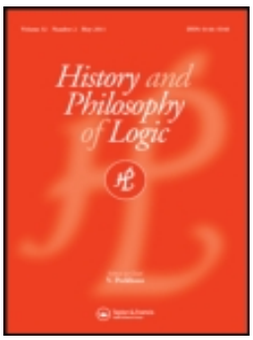

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Was Tarski's Theory of Truth Motivated by Physicalism?

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Many commentators on Alfred Tarski have, following Hartry Field, claimed that Tarski's truth-definition was motivated by physicalism—the doctrine that all facts, including semantic facts, must be reducible to physical facts. I claim, instead, that Tarski did not aim to reduce semantic facts to physical ones. Thus, Field's criticism that Tarski's truth-definition fails to fulfill physicalist ambitions does not reveal Tarski to be inconsistent, since Tarski's goal is not to vindicate physicalism. I argue that Tarski's only published remarks that speak approvingly of physicalism were written in unusual circumstances: Tarski was likely attempting to appease an audience of physicalists that he viewed as hostile to his ideas. In later sections I develop positive accounts of: (1) Tarski's reduction of semantic concepts; (2) Tarski's motivation to develop formal semantics in the particular way he does; and (3) the role physicalism plays in Tarski's thought.

1. Introduction: the received view of Tarski's motivations for his semantic research

Any hagiography of contemporary mathematical logic, however brief, must include Alfred Tarski. Tarski developed his formal semantics[1] in a number of important lectures and writings during the late 1920s and early-to-mid-1930s. The piece that stands above the rest of his *oeuvre* on this topic in terms of both sheer length and subsequent influence is known in English as 'On the Concept of Truth in Formalized Languages'. As Tarski tells us, he discovered the bulk of the paper's results in 1929. He then presented these ideas as lectures to the Philosophical Society in Warsaw on 8 October 1930, and shortly thereafter to the Polish Philosophical Society in Lvov on 5 December of the same year. The paper was submitted for publication to the Warsaw Scientific Society in March of 1931 (*Tarski 1983*, p. 152). However, it was not published until 1933; even then, the piece was only available in Polish, and thus not easily accessible to a larger logical, philosophical and mathematical audience. In 1935, the monograph was translated into German and published as *Der Wahrheitsbegriff in den formalisierten Sprachen*, occupying 135 pages of *Studia Philosophica*. It was in this form that Tarski's work on truth, and on the related semantic concepts of satisfaction and entailment, was studied by the European logical and philosophical community at large. In this *Hauptwerk*, Tarski tackles a 'single problem': '*the definition of truth*' (*Tarski 1983*, p. 152).[2]

What is Tarski's motivation for addressing the problem of truth in the particular way that he does? It is here that the modern commentators on Tarski's project enter the scene, led by Hartry Field *1972* in his influential 'Tarski's Theory of Truth',

1 As Etchemendy *1988* has stressed, the 'formal semantics' of Tarski is essentially different from the 'formal semantics' project pursued by Montague or the Davidsonian program.

2 Skipping to the well-known punchline of Tarski's paper, the definition, which relies essentially upon the previously-defined notion of 'satisfaction', is: 'x is a true sentence—in symbols $x \in \mathrm{Tr}$—if and only if $x \in \mathrm{S}$ [S is the set of meaningful sentences] and every infinite sequence of classes *satisfies x*' (*Tarski 1983*, p. 195).

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published in *The Journal of Philosophy*. Field first notes that, at the beginning of 'The Concept of Truth in Formalized Languages', Tarski imposes a strong constraint on his definition of truth: he writes that in his 'construction' of the definition of truth, 'I shall not make use of any semantical concept if I am not able previously to reduce it to other concepts' (*Tarski 1983*, p. 153). If we take Tarski at his word, he is attempting to define truth in non-semantic terms.

Field then asks a further, more clearly philosophical, question which Tarski does not pose or address in the *Wahrheitsbegriff* essay: 'For what purpose do we want definitions for which the elimination of semantic terms is useful?' (*1972*, p. 356). Why not instead embrace a definition of truth that *did* use irreducibly semantic terms? Field suggests that Tarski is motivated to formulate his definition of truth in the manner he does by the philosophical doctrine of *physicalism* (more on the meaning of this word presently). Field is, to my knowledge, the first commentator on Tarski to notice that Tarski mentions physicalism approvingly in print. Owing to this mention, Field writes: 'Tarski put a heavy stress on the doctrine of physicalism: the doctrine that chemical facts, biological facts, psychological facts, and semantical facts, are all explicable (in principle) in terms of physical facts' (*1972*, p. 357). Field also asserts that Tarski's purported physicalism is 'of the utmost significance in evaluating the philosophical significance of Tarski's work' (*1972*, p. 357). Thus, Field offers us an explanation for why Tarski defines truth in the way he does: Tarski aims to explain the semantic concept of truth, as Field puts it, 'in terms of physical facts', and that is Tarski's motivation for defining truth in terms of non-semantic terms.

The claim that Tarski's treatment of truth aims at physicalist ends is only one part of Field's larger project in 'Tarski's Theory of Truth'. Field goes on to assert that Tarski's theory of truth can reduce all semantic concepts (including the concept of truth) to a single semantic concept (which Field calls 'primitive denotation'). However, this concept is not, in turn, explained in terms of physical facts, so that Tarski's apparent reduction of the semantic to the physical is illusory. Using the chemical concept of valence as an example, Field shows that not every elimination of a concept counts as a legitimate physicalist reduction of that concept. (Field asks us to consider the following 'definition' of valence. '$(\forall E)(\forall n)$ (E has valence $n \equiv$ E is potassium and n is $+1$, or ... or E is sulphur and n is -2) where in the blanks go similar clauses, one for each element'. Field then infers: 'though this is an extensionally correct definition of valence, it would not have been an acceptable reduction' [*1972*, p. 363].) Field believes that Tarski's reduction is similarly flawed: Tarski sets up physical reductionism as a goal of his treatment of truth, but fails to achieve that goal. Field's suggestion that physicalism motivated Tarski has been accepted by the vast majority of modern scholars who have written on the subject. John McDowell, for example, writes that 'Field is right about Tarski's view of the relation between physicalism and his work on truth' (*1978*, p. 132). Scott Soames (*1984*, pp. 419–420) and Richard Stalnaker (*1987*, pp. 30–31) both agree that Tarski is a committed physicalist, and they even extend Field's criticism of Tarski. John Fox, who declares Field's analysis 'brilliant', also concurs that Tarski's research was motivated by physicalism (among other things) (*1989*, p. 169). Richard Kirkham *1993*, in 'Tarski's Physicalism', also agrees that Tarski was attempting to achieve physicalist aims, but claims, contra Field, that Tarski succeeds in achieving those aims. Kirkham argues that what Tarski called 'physicalism' in 1935 is in important respects different from Field's more modern definition of the doctrine of physicalism.

(Fundamentally, physicalism in the 1930s does not require that all facts be explicable in terms of physical theory alone; rather, it requires that all claims be expressible in the language used to describe events in the spatiotemporal order. See especially *Neurath 1983*.) He goes on to assert that the conception of truth offered in Tarski's *Wahrheitsbegriff* monograph *does* satisfy this earlier, historically accurate version of physicalism (*Kirkham 1993*).

In the face of such scholarly consensus about Tarski's purposes, dissent is perhaps foolhardy. Nonetheless, my central contention in this essay is that Tarski's work on the concept of truth is *not* motivated by a philosophical desire to reduce the concept of truth to purely physical concepts. However, I do not wish to go so far as to assert *simpliciter* that Tarski is not a physicalist. If we were to ask him directly, I believe Tarski would probably claim that he is a physicalist of some sort; however, I will argue that a desire to reduce semantic concepts to physical concepts did not drive his work on the formal definition of truth. Although Tarski does mention physicalism once in his published writings, it is not central to his ideas in formal semantics, and I will argue that it is probably used as a rhetorical device to ingratiate himself to what he perceived to be a hostile audience.

I am not the first person to make this suggestion. In *Mormann* (*1999*, p. 174), *Rojszczak* (*1999*, pp. 116–117; *2002*, p. 32), the claim that Tarski may not have been a devout physicalist, or that his research may not have been motivated by physicalism, is briefly mentioned. However, these authors have not explored this thesis in a thorough and sustained fashion, as I will attempt here. I will explicitly note the places in which I am rehearsing ideas previously presented.

2. Tarski's apparent endorsement of physicalism

Why does Field's proposal that Tarski's formal characterization of truth was motivated by physicalism enjoy such widespread support? Field bases his claim on a passage in 'The Establishment of Scientific Semantics' (henceforth ESS), which is a brief introduction to Tarski's work on truth. At the point in the essay in which physicalism is mentioned, Tarski is considering difficulties that would face any attempt to study truth by taking 'is true' as an implicitly defined primitive in an axiom-system, instead of offering an explicit definition (as Tarski's own method does):

> [I]t would then be difficult to bring this method [studying truth by introducing a primitive term in an axiomatic system] into harmony with the postulates of the unity of science and of physicalism (since the concepts of semantics would be neither logical nor physical concepts). (*1983*, p. 406, quoted in *Field 1972*, pp. 356–357, ''which glosses 'this method' as 'semantics'''.)

This quotation is what leads Field to suggest that Tarski analyzes truth in the way he does in order to accomplish physicalist aims. That is, Tarski's research does not proceed by characterizing truth axiomatically, because Tarski believes such a procedure would violate the doctrine of physicalism. Following this quotation, Tarski does not further elaborate how such an axiomatic procedure would necessarily violate these two 'postulates', or why such violations are objectionable; rather, he changes the subject. Clearly, this passage is the most serious obstacle to my thesis, and constitutes excellent evidence for Field's position. Why would Tarski mention physicalism approvingly here, if he did not believe his semantic research

achieved physicalist aims? The remainder of this section is devoted to answering that question.

This quotation is the only mention of physicalism in all of Tarski's publications in semantics and metamathematics (I have not systematically examined Tarski's purely mathematical writings). And three recently published letters from Tarski to Otto Neurath, dating from the early-to-mid-1930s, made no mention of physicalism (*Tarski 1992*). This omission is especially telling, given that Neurath and Carnap were quite interested in the topic of physicalism, as their sustained interchange on the topic in the pages of *Erkenntnis* during 1931 and 1932 indicates. Thus, one might question Field's assertion that physicalism was a central motivation for Tarski simply on the grounds that Field only cites one use of the word. How pressing can Tarski's physicalist convictions be, if he mentions the idea in print only once?[3] This is a relatively obvious point—though not thereby insignificant. This 'argument from silence', by itself, is not particularly convincing, but it becomes more interesting and revealing when one realizes that Tarski wrote ESS under fairly peculiar historical circumstances. These external circumstances, I will suggest, provide a possible explanation for Tarski's mention of physicalism, a mention that is anomalous from the point of view of the rest of the Tarskian corpus.

ESS was originally a presentation for the First International Congress for the Unity of Science,[4] held in Paris in 1935. It will be significant that Tarski's audience at this Congress included virtually all of the prominent members of the *Wienerkreis*. This was not Tarski's first direct exposure to the Viennese philosophers. In the fall of 1929, the mathematician *cum* economist *cum* philosopher Karl Menger visited Poland, and met many of the leading Polish philosophers of the time, including the young Alfred Tarski. Menger also met Tarski's philosophical teachers from Tarski's PhD study in Warsaw (which Tarski completed, with a degree in mathematics, in 1924), including Leśniewski and Łukasiewicz. Menger was impressed by much of the logical and philosophical activity in Poland, and invited Tarski to present lectures the following year in Vienna to Menger's mathematical colloquium jointly with the *Wienerkreis*. Tarski gave three lectures, on successive days, in 1930: the first, on 19 February, he gave just to Menger's mathematical colloquium on set theory. For the next two lectures, the members of the Vienna circle were invited. (The source of information for this paragraph and the following one is, unless otherwise noted, *Menger* [*1992*, pp. 146–153].)

The second lecture, whose only philosophical attendees were Rudolf Carnap and the mathematically inclined Hans Hahn, dealt with the formal definition of the concepts of a deductive system and of axiomatizability. These two concepts, Tarski explained, are to be defined in the *metalanguage* of the particular language being studied. Carnap was very interested in the formalized metalanguage Tarski was proposing, and the two continued their discussion privately after Tarski's lecture.[5]

3 Kirkham's article 'Tarski's Physicalism' mentioned above, which criticizes Field's seminal paper for using a modern definition of physicalism instead of a historically appropriate one, actually does not analyze anything *Tarski* himself says about physicalism. Rather, Kirkham presents and explicates what *Neurath and Carnap* take physicalism to be. Kirkham simply asserts that we can identify the version of physicalism articulated by Carnap and Neurath with what Tarski intended by 'physicalism' in ESS.

4 That is (a translation of) the German-language title of the Congress; the French-language title substitutes '*philosophie scientifique*' for '*Einheitswissenschaft*'.

5 In his autobiography, Carnap writes that during Tarski's 1930 visit, 'we also discussed privately many problems in which we were both interested' (*1963*, p. 30).

At Tarski's third and final lecture, on 21 February, all the usual members of the Circle were present (as a result of Menger's direct urging that they attend). On this day, however, Tarski did not cover any topics relating to his notion of a metalanguage.[6] Instead, Tarski discussed truth tables, the three-valued logic of Łukasiewicz, as well as Łukasiewicz's so-called 'frontal' notation, whereby all brackets and parentheses can be removed from the propositional and predicate calculi. Menger reports that the response of the Circle to Tarski's lecture was mixed: Schlick's reaction was 'cool', Neurath appeared bored, and Waismann was antagonistic to Tarski's challenging the Wittgensteinian thesis that parentheses are an indispensable part of logical notion (*Menger 1992*, p. 152). From Menger's account, Tarski had made one substantive intellectual and personal ally in the *Wienerkreis* during this 1930 trip—namely, Carnap. This account is corroborated by Carnap's own recollection of the events surrounding Tarski's 1930 visit to Vienna. Carnap developed the hunch that:

> the formal theory of language was of the greatest importance for the clarification of our philosophical problems. But Schlick and others were skeptical at this point. At the next meeting of our circle, when Tarski was no longer present, I tried to explain it would be a great advantage for our philosophical discussions. (*1963*, p. 30.)

The enthusiasm Carnap showed towards Tarski's original lectures in 1930 continued through the next few years, though there was not extensive contact between the groups in Warsaw and Vienna. For example, upon learning of Tarski's definition of truth, Carnap recalls being very excited: 'the semantical metalanguage. . .evoked my strongest interest' (*1963*, p. 61). Carnap wanted other philosophers to be exposed to these ideas, so he pressured Tarski to introduce his semantic research to a larger audience:

> When I met Tarski again in Vienna in the spring of 1935, I urged him to deliver a paper on semantics and on his definition of truth at the International Congress for Scientific Philosophy to be held in Paris in September. I told him that all those interested in scientific philosophy and the analysis of language would welcome this instrument with enthusiasm, and would be eager to apply it to their own philosophical work. (*1963*, p. 61.)

One might imagine Tarski would seize this opportunity to gain exposure for himself and his work, especially among some of the more influential philosophers and logicians in Western Europe at that time. However, Carnap informs us, Tarski was reluctant to present his formal semantics to this audience:

> but Tarski was very skeptical. He thought that most philosophers, even those working in modern logic, would be not only indifferent, but *hostile* to the explication of the concept of truth. (*1963*, p. 61, additional emphasis.)

6 Incidentally, this fact explains one of the central points of contention between Tarski and Neurath in their correspondence (*Tarski 1992*). Tarski insisted that he was the first to defend the acceptability of 'sentences about sentences' to the *Wienerkreis*, whereas Neurath was convinced he had heard the claim first from Carnap, and later from Tarski. Neurath *did* hear it first from Carnap (see next Carnap quote), since Neurath was not at Tarski's lecture on metalinguistic notions; but Carnap got it from Tarski.

Tarski expected an unsympathetic audience for his ideas in Paris. Nonetheless, he agreed to present his ideas there. Subsequent events at the Congress proved Tarski's initial apprehension was justified. Carnap recalls:

> at the Congress it became clear from the reactions to the papers delivered by Tarski and myself that Tarski's skeptical predictions had been right. To my surprise, there was vehement opposition even on the side of our philosophical friends. . . . Neurath believed that the semantical concept of truth could not be reconciled with a strictly empiricist and anti-metaphysical point of view. Similar objections were raised. . .by Felix Kaufmann and Reichenbach. (*1963*, p. 61.)

In light of these historical circumstances surrounding the composition and delivery of ESS, Tarski's mention of physicalism and the unity of science in that piece could be given an interpretation other than the one Field suggests. Perhaps Tarski was motivated by a desire to win the sympathy of his listeners, most of whom he believed to be 'hostile' to his ideas. Such a strategy seems fairly reasonable. The postulates of physicalism and of the unity of science were twin darlings of the logical empiricists at that time. Rejecting a certain idea (in this case, studying truth as an undefined primitive) on the grounds that it does not meet physicalist standards might incline Tarski's audience to believe that he thinks as they do, and that he shares their basic conception of the aims and standards of proper philosophy. If they believe that Tarski's basic outlook on intellectual problems is sound, then they might be more receptive to Tarski's work on the definition of truth. This provides one reason, I believe, why the doctrine of physicalism is absent from any other Tarskian pieces; he mentions it in ESS because of the particular audience he imagines he will have in Paris. Seen in this light, perhaps Tarski mentions physicalism *not* because it is a fundamental conviction that drives him to pursue his research, but rather because he is attempting to palliate what he (correctly) considered to be an antagonistic crowd. The quotation upon which Field bases his case, if viewed in its immediate context of Tarski's apprehensiveness, appears to be a rhetorical maneuver, instead of a manifestation of a deeply held and thoroughgoing philosophical orientation. I believe this constitutes strong evidence for my central claim that Tarski's truth definition is not an attempt to realize physicalist goals.

(It should be noted, to avoid historical distortion, that not everyone at the Congress was hostile to Tarski's definition of truth. Carnap, obviously, was on Tarski's side. A. J. Ayer, in retrospect, considered Tarski's paper on truth to be the 'highlight' of the entire meeting [*1977*, p. 116]. And Karl Popper, also in attendance, had already been converted by Tarski earlier that year, 'on a bench (an unforgotten bench) in the *Volksgarten* in Vienna', and Tarski's truth-definition made him 'joyful' [*1974*, p. 398].)

Writing ten years after that Parisian Congress, Tarski still remembers his somewhat chilly reception there: 'various objections. . .have been raised to my investigations; some of these appeared in print, and others were made in public and private discussions of which I was a part'. The footnote appended at the end of this sentence reads: 'this applies, in particular, to public discussions during the I. International Congress for the Unity of Science (Paris, 1935)' (*Tarski 1944*, pp. 69, 88); these 'discussions' are presumably the 'vehement opposition' Carnap refers to above. In sum, Tarski perceived the audience for ESS as hostile to his work in semantics.

There is another interesting historical fact about the circumstances of production for ESS, that has already been pointed out in *Rojszczak* (*1999*, p. 116), which strongly indicates that Tarski tailored his message for this particular audience. ESS was published as 'Grundlegung der wissenschaftlichen Semantik' in the *Actes du Congres International de Philosophie Scientifique* in 1936, and this is the version translated into English in the *Logic, Semantics, and Metamathematics* volume of Tarski's early collected writings. The paper was also, however, published in Polish in 1936 as well. What is fascinating for our purposes is that the section of ESS upon which Field bases his case is importantly different in the Polish publication from the German text that appeared in the above-mentioned *Actes*. In the Polish version, Tarski claims that if one pursues a purely 'axiomatic method' for studying truth, 'it would be difficult to bring ... scientific semantics into harmony with postulates of the unity of science and of physicalism, which are propagated by a great number of philosophers from the so called Vienna Circle' (*Rojszczak 1999*, p. 123). As mentioned above, ESS was presented as a lecture in September 1935, and we have no way to know exactly what words Tarski uttered that day. Nonetheless, it is interesting that in addressing his compatriots, who were not devout disciples of all Vienna circle doctrines,[7] Tarski does not explicitly endorse physicalism and the unity of science himself, choosing instead to place these theses in the voice of another philosophical school. I believe this Polish text constitutes further evidence that physicalism was not a central motivation for Tarski's definition of truth.

Furthermore, in ESS, Tarski mentions the two postulates of physicalism and unity of science as (the last of three) reasons for not studying the concept of truth as an undefined primitive figuring in an axiom system. However, in other places in the Tarskian corpus, he embraces the axiomatic strategy. In 'The Semantic Conception of Truth and the Foundations of Semantics' (hereafter SCT), written in 1944, Tarski claims we are able

> to include the term 'true'... in the list of undefined terms in the metalanguage, and to express fundamental properties of the notion of truth in a series of axioms. *There is nothing essentially wrong in such an axiomatic procedure*, and it may prove useful for various purposes. (*1944*, p. 76, additional emphasis.)

The same idea reappears later in the same piece (*1944*, p. 78). One might then suggest that Tarski came to this position sometime after ESS in 1935 but before SCT in 1944. However, this suggestion is false; for Tarski advocated this position even earlier than 1935. Tarski proposes, and even partially pursues, the axiomatic study in which 'is true' appears as an undefined primitive in the landmark *Wahrheitsbegriff*, submitted in 1931 and published in 1933 . This maneuver, Tarski believes in 1931, is appropriate in the cases of certain types of languages. The rough idea can be stated fairly simply. The concept of truth in languages 'of infinite order' cannot be treated with the definition of truth for which Tarski is famous (*1983*, p. 241). For example, set theory can be of infinite order, because we can form sets, sets of sets, sets of sets of sets, and so on to (denumerable) infinity. In *Wahrheitsbegriff*, Tarski summarizes his most significant results as follows:

7 See *Rojszczak* (*1999*, pp. 126–127) for evidence.

> for formalized languages of infinite order the construction of such a definition [of truth] is impossible . . . . On the other hand, even with respect to formalized languages of infinite order, the consistent and correct use of the concept of truth is rendered possible by including this concept in the system of primitive concepts of the metalanguage and determining its fundamental properties by means of *the axiomatic method*. (*1983*, p. 266, additional emphasis.)

That is, when we are dealing with languages of infinite order, the only way to analyze truth without falling into inconsistency is via the axiomatic method. Tarski retracts this particular claim in the 1935 postscript to *Wahrheitsbegriff*, but he still believes an axiomatic study of truth is legitimate (*1983*, p. 273).

The moral I wish to draw from Tarski's willingness to embrace the axiomatic method in 1931 and 1944 is, hopefully, obvious at this point. Given that Tarski only mentions physicalism and the unity of science in ESS as reasons to reject an axiomatic study of truth, and that he later (and earlier!) accepts the possibility and usefulness of such an axiomatic study, I think two interpretive options are available. Either (1) he is not extremely attached to physicalism and the unity of science postulates, and jettisoned or ignored them in other writings or (2) he embraces these postulates, and believes they can be reconciled with an axiomatic study of truth, but he declines to share the reconciliation with us in 1944. I lean towards (1); but neither option places physicalism near the center of Tarski's intellectual universe. For (supposing (2) is correct for the sake of argument) if physicalism is extremely important to Tarski, then presumably he would show how an axiomatic theory of truth, which he had declared incompatible with physicalism in 1935, could be consistent with physicalism after all. However, he does not.

That physicalism is not a central idea of Tarski's work is given further support by another bibliographical feature of SCT in 1944. SCT, as Tarski describes it, is written in two major parts: an exposition, and a 'polemical' section. Tarski informs us in a footnote (*Tarski 1944*, p. 88) that the expository part of the paper is based on ESS, the paper he presented at the Paris Congress, which contains the reference to unity of science and physicalism. However, in this expository section, there is no mention whatsoever of these two postulates. Since Tarski neglected to mention the two postulates in his recapitulation, how important could they be for him? I think this is further evidence that Tarski simply did not view physicalism as a significant motivation for his intellectual enterprise.

3. Tarski's reduction of semantic concepts to non-semantic concepts

As quoted in §1, in *Wahrheitsbegriff*, Tarski claims he does 'not make use of any semantical concept' that he cannot 'reduce to other concepts'. This statement, at first glance, appears to support Field's account. For when we think of 'reduction' in a philosophical context, the examples that spring to mind include the reduction of chemistry to physics, or the reduction of the mind to the brain (these are Field's examples). So when Tarski asserts that he will reduce semantical concepts to other concepts, a natural assumption might be that these other concepts must be *physical* concepts (where e.g. a neuron firing counts as a physical event). Such a line of thought, I take it, makes the received view of Tarski plausible and acceptable.

However, the basic point that I will urge is that for Tarski, 'non-semantic' does not necessarily mean 'physical', or even the less restrictive Logical Empiricist term

'physical*ist'*, as Kirkham *1993* suggests. Immediately preceding Tarski's assertion that he will 'reduce' semantic terms to non-semantic ones, he considers which terms should be allowed in the reducing vocabulary. Tarski makes no mention of physical or material conditions; rather, he says, 'if the definition [of truth] is to fulfill its proper task, the sense of the terms in this list [i.e. the list of available terms in the reducing vocabulary] must admit of no doubt' (*1983*, p. 153). And '*x* admits of no doubt' is very probably not synonymous with '*x* is a physical term'.

Furthermore (and this is the important point for present purposes), the definition of satisfaction, which the definition of truth relies upon crucially, is clearly not physicalist in the least. In general, Tarski places no physicalist constraints on a metalanguage:

> A metalanguage includes only three kinds of undefined terms: (i) terms taken from logic, (ii) terms of the corresponding object language, and (iii) names of expressions in the object language. (*1944*, p. 83.)

The point I wish to draw from this quotation is simply that the three undefined elements of the metalanguage, to which the concept of truth will ultimately be reduced (via satisfaction), are not physical terms in any straightforward sense (the 'object language' of (2) does not have to be about physical objects at all, for it could deal with only mathematical objects—'object language' here is simply the contrast class for 'metalanguage'). The type of 'reduction' operative in this case is, in one way, similar to a logician saying 'The sentential connective ''and'' can be *reduced* to ''not'' and ''or'', for the sentence ''p and q'' is equivalent to ''not (not-p or not-q)''. Furthermore, the usual connectives of the sentential calculus can all be *reduced* to a single connective, the so-called Sheffer stroke'. Here, the reducing vocabulary is *not* physical. These are reductions *within* the realm of classical logical vocabulary (one connective is reduced to others), so there is a difference with the case of truth, since it is a semantic concept, not a logical connective. The point stands nonetheless: a case that many philosophers and logicians are willing to call a 'reduction' does not have to be a reduction to something *physically* or *materially* more basic. And it is clear, given the definition of satisfaction, that Tarski is reducing semantic terms to *set-theoretical* (and other logical) terms.

Of course, this is what Field considers problematic about Tarski's definition of satisfaction—it is an illegitimate reduction, for it does not provide a physical account of the semantic notion of satisfaction. All parties to the dispute agree on the following: Tarski's definition of satisfaction simply does not reduce satisfaction to physical concepts. I part company with Field on what should be inferred about Tarski's project from this fact. Field makes the further assertion that Tarski's project aimed to provide a physical reduction of the concept of truth; whence it follows that Tarski's project fails to meet its aims. I am suggesting we be more charitable in our interpretation of Tarski's motivations for defining truth. If Tarski claims that he can reduce satisfaction to non-semantic concepts, and his definition of satisfaction is manifestly *not* a reduction to a physical vocabulary, then perhaps we should conclude that Tarski did not intend to reduce semantics to physics.

I would like to make the same point in a slightly different way. Even if I am correct, and physicalism is not Tarski's primary motivation for developing formal semantics, he nonetheless could hold views that are akin to physicalism (and in his more philosophical moments, he most likely does: see the final section). That is, let us

assume that Tarski wanted, as Field suggests, to reduce the concepts of semantics to 'logical or physical concepts'. Field and McDowell conclude from this that Tarski wants to reduce truth to *physical* concepts. Why could Tarski not take the *other* half of the disjunction, and believe the reduction to be of a generally 'logical' or logico-mathematical character? (The scare-quotes indicate an expanded view of logic; for Tarski includes in the metalanguage a set-inclusion relation and names of expressions.) Even a completely determined physicalist, I take it, cannot make do without sentential connectives like 'and', 'or', and 'not' *entirely*: these connectives cannot be reduced to physical concepts *only* (i.e. without using *any* logical concepts). Without such connectives, physical theories could not even be stated.

And in fact, Tarski does claim his work is of this logical character, perhaps in response to Neurath's above-mentioned view that Tarskian formal semantics was 'metaphysical' and not 'empirical':

> Some people take it to be symptomatic of a metaphysical element in a science when methods of inquiry are employed which are neither deductive nor empirical. However, no trace of this symptom can be found in the development of semantics (unless some metaphysical elements are involved in the object language to which the semantic notions refer). In particular, the semantics of formalized languages is constructed in a *purely deductive* way. (*1944*, p. 83, additional emphasis.)

In other words, Tarski does not see the formal semantics he develops as an empirical enterprise; presumably, if a discipline is not empirical, then it is not physical. Tarski hints that his work is non-empirical elsewhere as well; for example, he contrasts the 'descriptive semantics' of 'natural languages' with 'theoretical semantics' of his formal theory (*Tarski 1944*, pp. 84–85). Furthermore, Tarski's view of the status of semantics is not idiosyncratic or unique among his contemporaries. Carnap likewise considers semantics (as it is practiced by Tarski and other logicians, as opposed to field linguists) to be a fundamentally non-empirical discipline: 'there are no factual assertions in pure semantics. The rules merely lay down conventions' (*Carnap 1942*, p. 25; see also *Carnap 1939*). This view of semantics is obviously far from the so-called causal-historical theory of reference, in which reference is treated as an empirical matter, as much a part of the network of natural causes and effects as photons and *E. Coli*. Field cites the causal theory of reference approvingly, and holds it up as an example of how to supply what is missing from Tarski's formal semantics (*1972*, p. 367). It is perhaps no coincidence that the rise of the causal theory of reference, in the work of Kripke and others, immediately precedes Field's claim that Tarski held that all semantic facts are explicable in terms of physical facts.[8]

My aim here has been to indicate a possible way to avoid the standard criticism of Tarski's alleged 'bogus reductions' (*McDowell 1978*, p. 141): perhaps one can be a physicalist without reducing semantic concepts to *physical* concepts, by reducing semantic concepts to *logico-mathematical* ones instead. However, even if the central thesis of this paper is correct, Field's original *philosophical* criticism of Tarski can perhaps still stand, with a modification. What Field must do, in order to preserve something akin to the original criticism of Tarski, is either assume or argue for the conclusion that *we* want semantic terms to be reduced to physical terms as opposed to logical terms. That is, Field could argue that formal semantics is, in relevant

8 I thank an anonymous reviewer for prompting me to place Field's claims in their historical context.

ways, closer to chemistry than to the propositional calculus; thus truth should be reduced in the way valence is reduced to physical concepts, not in the way propositional connectives are reduced to the Sheffer stroke. Now we are standing on the brink of a question that is much too large and too vexed to address here: the distinction between logical and factual assertions. Specifically, Field's criticism of Tarski would carry weight if one could have to show that (in some sense) it is better to treat truth and reference as empirical concepts instead of logical ones. Thus, a modern-day physicalist might hold that the reduction of semantic terms to non-physical terms is uninformative or otherwise unsatifying, since it does not incorporate truth and reference into the spatiotemporal order decribed in physical theory. If we take that as *our* modern philosophical aim, then we *could* possibly criticize Tarski's truth-definition for failing to meet our standards. However, if we are to respect Tarski's own conception of his project, I have urged, we cannot attribute that aim to Tarski. What Field and his followers *cannot* say, if I am correct, is that Tarski failed to achieve his *own* ends, for reducing the concept of truth to physical concepts simply does not seem to be an end which Tarski sets for himself.

4. What *did* motivate Tarski's particular method of defining truth?

I have spent several pages attempting to support my thesis that Tarski's work in formal semantics was not motivated by a desire to reduce semantic concepts to physical concepts. The question then arises: why *did* Tarski take up the project of reducing semantic concepts to other, non-semantic ones? Although I cannot give an incontestable answer to that question, I can provide a fairly plausible and straightforward alternative account of Tarski's motivations. This interpretation is not particularly sophisticated: my strategy is to take Tarski at his word when he articulates his reasons for defining truth the way he does,[9] instead of (as Field does) using a single intriguing and unusual quotation to interpret the fundamental impulse driving Tarski's project.

One need not view Tarski's work on truth as primarily motivated by a philosophical doctrine; this seems natural in light of the fact that Tarski was not primarily interested in producing works advocating a philosophical school. His doctoral degree, as mentioned above, was in mathematics, and a large chunk of his publications dealt with mathematical topics other than logic (the famous Banach–Tarski paradox is but one example among many). Patrick Suppes, who knew Tarski personally, writes that Tarski 'was extraordinarily cautious and careful in giving any direct philosophical interpretation of his work' in print (*1988*, p. 81). A perusal of Tarski's logical publications bears out Suppes' assertion. For example, in SCT, Tarski explicitly divorces his formal semantics from a cluster of traditional philosophical positions:

> Thus, we may accept the semantic conception of truth without giving up any epistemological attitude we may have had; we may remain naïve realists, critical realists, or idealists, empiricists or metaphysicians—whatever we were before. The semantic conception is completely neutral towards all these issues. (*1944*, p. 82.)

9 A good summary of Tarski's motives, similar to the one presented here, can be found in *Sher (1999*, pp. 149–151).

In short, Tarski did not undertake his work to execute a particular philosophical agenda. However, this view of Tarski requires certain qualifications and explanations. First, simply because the bulk of Tarski's work is in logic and mathematics, that does not mean that he lacks distinctive philosophical views. For example, Tarski described himself in conversation as 'a mathematician, a logician, and a philosopher of a sort' (*Woleński 1993*, p. 321). As we shall see in the final section, Tarski called himself a 'nominalist' and a 'materialist' (or 'reist'). Second, recent scholarship has illuminated, more generally, the philosophical background and context of Tarski's formal semantics. Both Woleński *2002* and Rojszczak *2002* argue, on different grounds, that Tarski's work in semantics would not have been possible, had Tarski not been exposed to the substantive philosophical traditions present in Poland at the beginning of the twentieth century. Finally, one might object to my claim on the grounds that Tarski claims the 'central problem' addressed in *Wahrheitsbegriff* 'belongs to the theory of knowledge and forms one of the chief problems of this branch of philosophy' (*1983*, p. 267). Of course, I agree that Tarski's theory of truth is philosophical in an important sense, and that Tarski viewed it as such. The narrower question I am concerned to answer, and which Field attempted to answer, is why Tarski chooses the particular *way* of defining truth that he does, i.e. why does Tarski require that semantic concepts be reduced to non-semantic ones? Field's answer is that Tarski wished to reduce semantic concepts to physical ones—I will argue for an alternative hypothesis.

I claim that the motivation for Tarski's chosen way of defining truth is primarily that of a *logician*, not a traditional philosopher—if such a distinction may be drawn. Tarski wanted to resolve the semantic paradoxes such as the paradox of the liar, just as Whitehead and Russell had resolved the set-theoretic paradox inherent in Frege's logic in their *Principia Mathematica* a few years earlier. Tarski's goal is the creation of a consistent formal system that can include and adequately represent the concept of truth; the existence of the semantic paradoxes shows that constructing such a consistent system is not a trivial task. Tarski credits his teacher Leśniewski, at the University of Warsaw in 1919–1920, for the explicit formulation of the semantic paradoxes (*Tarski 1983*, p. 154). The semantical paradoxes that Tarski mentions, other than the paradox of the liar, include the following. The semantic notion of denotation can lead to Grelling's paradox, sometimes referred to as the paradox of heterological adjectives; and the semantic concept of satisfaction can engender contradiction as well. Finally, the concept of definability leads to Richard's antinomy (*1944*, p. 89).

The opening paragraph of the *Wahrheitsbegriff* monograph points out that the word 'true' can lead from 'apparently evident premisses' to 'paradoxes and antinomies' (*1944*, p. 152). The entire first section of this article is devoted to demonstrating that colloquial language must necessarily engender inconsistencies, so that truth cannot be rigorously discussed in everyday speech. Furthermore, Tarski closes the monumental piece with an admonition to philosophers who are wary of highly formal methods of discussing truth, on the grounds that such methods are too 'artificial.' Tarski once again reminds such conceptual Luddites that a less 'artificial' approach 'leads inevitably to confusions and contradictions' (*1944*, p. 267). In short, I am suggesting that Tarski developed his definition of truth in order to avoid the contradictions latent in any language that includes semantic terms about itself in itself; Tarski avoids these contradictions by placing all such semantic terms in the metalanguage.

Tarski conceives his research motivations similarly more than a decade later, in SCT. There, he stresses the importance and centrality of the semantic antinomies for his project:

> In my judgment, it would be quite wrong and dangerous from the standpoint of scientific progress to depreciate the importance of this and other antinomies, and to treat them as jokes or sophistries . . . . If we take our work seriously, we cannot be reconciled with this fact . . . [w]e must analyze the premises upon which the antinomy is based; we must then reject at least one . . . . It should be emphasized that antinomies have played a preeminent role in establishing the foundations of the modern deductive sciences. (*1944*, p. 74.)

Thus we can see that Tarski considered the semantic antinomies to be a central motivation for his research, and that he believes antinomies, generally speaking, are the driving force behind much innovative logical and mathematical research in general. So why does Tarski define truth in the particular way that he does, i.e. without using any terms in the definiens that cannot be reduced to non-semantic terms? Tarski goes on to explain that if semantic terms are *only* introduced into the metalanguage by definition, then 'we have a kind of guarantee that the use of semantic concepts will not involve us in any contradictions' (*1944*, p. 75). This is the reason Tarski defines the semantical terms in the way he does: he begins with a consistent language (the object-language, plus names of expressions in the object language), so merely adding definitions, using only the resources of this language, cannot engender inconsistency.

Solomon Feferman *1999* has emphasized that Tarski conceives his aims similarly in his treatment of the notion of *definability*, found in "On Definable Sets of Real Numbers" (*Tarski 1983*). Feferman goes one step further in *Feferman forthcoming* and stresses that Tarski not only sought to deliver perennially-suspect terms such as 'true' and 'definable' from inconsistency, but that he also sought to give them a purely *mathematical* explication.[10] In sum, my hypothesis is that what motivates Tarski's definition of truth to take the form it does is, in a sense, a logician's motivation: the elimination of inconsistency.[11] That said, I have only made this a plausible alternative to Field's suggestion; I do not claim to have demonstrated that the motivation suggested in this section is correct. The central point of my essay, that Field's interpretation of Tarski's motives is incorrect, remains intact, even if the hypothesis of this section is rejected. The strategic aim of this section is to prevent someone from accepting Field's account on the grounds that there is no viable alternative available.

5. Conclusion: philosophical proclivities and scientific motivations

I have argued that Tarski's *Wahrheitsbegriff* is not motivated by a desire to reduce semantic concepts to physical concepts. In conclusion, I offer one final reason for this

10 Tarski claims his definition of 'definability' is 'formulated exclusively in mathematical terms. Under this new definition, the notion of definability . . . can be discussed within the domain of normal mathematical reasoning' (*Tarski 1983*, p. 111).

11 The picture of Tarski sketched here is basically similar to Etchemendy's: Etchemendy claims that Tarski's 'overriding motivation' was to show not just that truth is definable, but also that it is 'coherent' (*1988*, p. 53). Thus, Tarski insists that the concept of truth must be reducible into non-semantic terms 'in order to vouchsafe the consistency of truth' (*Etchemendy 1988*, p. 54).

claim. Several people, including Tarski himself, have noted that there is a certain tension between his announced philosophical tastes on the one hand, and his published research on the other. When Tarski was asked about his philosophical predilections during a symposium at the Association of Symbolic Logic in 1965, Tarski replied that he is an 'extreme anti-Platonist' about mathematics. In this taped conversation, he went on to say:

> however, I represent this very crude, naïve kind of anti-Platonism, one thing which I could describe as materialism, or nominalism with some materialistic taint, and it is very difficult for a man to live his whole life with this philosophical attitude, especially if he is a mathematician, especially if for some reason he has a hobby which is called set theory. (Quoted in *Feferman 1999*, p. 61.)

As party lines are standardly drawn in the philosophy of mathematics in the last 100 years, people who declare themselves nominalists usually express serious reservations about the infinitary concepts employed in set theory; this almost defines modern mathematical nominalism. Yet Tarski, in this conversation, shows that he holds both set theory and nominalism dear.

A more detailed picture of what Tarski calls his 'materialism' or 'nominalism' emerges from (among other places[12]) unpublished discussion notes in the Rudolf Carnap Collection. During 1940–1941, several prominent philosophers spent a substantial portion of the academic year at Harvard University: Bertrand Russell, Carnap, Tarski, W. V. Quine, C. G. Hempel and Nelson Goodman were all present. Carnap, Tarski and Quine met privately several times to discuss a language-construction project that Tarski proposed; Carnap took detailed notes of these conversations. On 10 January 1941, Carnap records the following:

> Tarski: At bottom, I only understand a language that fulfills the following conditions:
>
> 1. Finite number of individuals.[13]
> 2. Reistic (Kotarbiński): The individuals are physical things.
> 3. Non-Platonic: Only variables for individuals (things) occur, not for universals (classes etc.) (ASP RC 090-16-28.)

Any language not fulfilling these conditions, says Tarski, is not fully 'understandable'; rather, he views it as a mere 'calculus'. Carnap, Tarski and Quine attempt to reconstruct as much logic, mathematics, and science as they can upon this slender basis; even classical arithmetic of natural numbers (to say nothing of higher mathematics) is endangered by these restrictions. (See *Carnap* [*1963*, p. 79] for Carnap's published recollection of these conversations.) Clearly, Tarski's philosophical stance expressed in 1.- 3. is incompatible with modern set theory, as standardly understood.

12 See *Woleński 1993* for other expressions of Tarski's philosophical inclinations.

13 Later, Tarski relaxes this requirement: the number of individuals is allowed to be infinite or finite; neither is assumed (ASP RC 090-16-25).

Based on Tarskian assertions similar to the one above, Jan Woleński states that there is 'an explicit cognitive dissonance in Tarski's views, dissonance consisting in a tension between his logical and mathematical practice and some of his philosophical sympathies' (*1993*, p. 322). Mostowski also noticed this 'unresolved conflict' between Tarski's professed nominalism and his published work (*1967*, p. 81). And as we have just seen, Tarski himself sensed a type of 'dissonance' between his mathematical and logical research, which leans heavily on the methods of infinitary set theory (even when set theory is not the topic at hand), and his 'extreme anti-Platonist' views in the philosophy of mathematics. Feferman *1999* also recognizes Tarski's separation between philosophical attitudes and mathematical methods, and then notes that, interestingly enough, Kurt Gödel's philosophy and practice were exactly the converse of Tarski's. Gödel used set-theoretical methods only once in his mathematical and logical publications, and thus 'followed the practice of the Hilbert school' in relying on purely finitistic methods whenever possible. However, from a philosophical point of view, Gödel 'was a staunch Platonist about set theory,' exactly unlike Tarski (*Feferman 1999*, p. 61).

The moral is clear: for these two mathematicians, philosophical assertions and working assumptions can dissociate, and Gödel's case indicates that the 'dissonance' in Tarski's views is not completely anomalous. However, this moral should not be overstated; such dissociation is certainly neither necessary nor universal. Artur Rojszczak has argued that certain aspects of Tarski's projects *were* prompted by certain philosophical issues: 'the logician and the mathematician Alfred Tarski was also a philosopher *sensu stricto'* (*2002*, p. 31). And Jan Woleński has recently suggested that certain philosophical currents in the Polish community during the late 1920s explain why Tarski would have the fundamental presuppositions about the nature of language and truth needed to create modern formal semantics (*2002*, p. 18).

Alfred Tarski's work on truth simply was not aiming to execute a program of reducing semantic concepts to physicalist ones, whether that program is conceived in modern terms, *à la* Field and McDowell, or in the terms of Vienna in the early 1930s, as Kirkham would have it. Tarski's single printed mention of physicalism does not, I have argued, allow us to infer that his entire semantic *oeuvre* is aimed at achieving physicalist ends. I have offered several reasons for this contention: Tarski's perceived antagonism to his ideas, Tarski's approval of an axiomatic study of truth elsewhere, the fact that 'reduction' is not (for Tarski) an ellipsis for 'reduction to the physical', and Tarski's insistence that his treatment of truth is 'epistemologically neutral.' Finally, Tarski himself admits that his philosophical proclivities do not mesh with his logical and mathematical research program.

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