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WHAT IS TARSKI'S *COMMON* CONCEPT OF CONSEQUENCE?

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Abstract. In 1936 Tarski sketched a rigorous definition of the concept of logical consequence which, he claimed, agreed quite well with common usage—or, as he also said, with the *common* concept of consequence. Commentators of Tarski's paper have usually been elusive as to what this common concept is. However, being clear on this issue is important to decide whether Tarski's definition failed (as Etchemendy has contended) or succeeded (as most commentators maintain). I argue that the common concept of consequence that Tarski tried to characterize is not some general, all-purpose notion of consequence, but a rather precise one, namely the concept of consequence at play in axiomatics. I identify this concept and show that Tarski's definition is fully adequate to it.

§1. Introduction. Tarski begins his celebrated paper on the concept of logical consequence [55] by asserting that its "introduction into the field of strict formal investigation was not a matter of arbitrary decision on the part of this or that investigator", but that, "in defining this concept, efforts were made to adhere to *the common usage of the language of everyday life*". Immediately after this, Tarski remarks on the difficulties facing this task, owing to the fact that "[w]ith respect to the clarity of its content *the common concept of consequence* is in no way superior to other concepts of everyday language. Its extension is not sharply bounded and its usage fluctuates". Accordingly, "[a]ny attempt to bring into harmony all possible vague, sometimes contradictory, tendencies which are connected with the use of this concept, is certainly doomed to failure". As a result, "every precise definition of this concept will show arbitrary features to a greater or less degree" (p. 409; emphasis added).[1]

In this first paragraph, Tarski seems to equate the "common concept of consequence" with the "concept of consequence, according to the language of everyday life", and he definitely asserts that such a concept cannot be faithfully rendered by a precise one.

Nevertheless, from the second paragraph onward Tarski speaks of the "common" concept in a less dismissive way. The second paragraph opens with this sentence:

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[1]Page numbers in parenthesis refer to Tarski's paper [55].

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> Even until recently many logicians believed that they had succeeded ... in grasping almost exactly the content of the common concept of consequence, or rather in defining a new concept which coincided in extent with the common one (p. 409).

Although these words do not entail that the common concept has a sharp extension, they do suggest that those logicians Tarski speaks of believed that it did. They suggest this because precise concepts have sharp extensions, and those logicians claimed that a precise concept coincided in extension with the common one. For the new concept defined by logicians, which Tarski calls "the formalized concept of consequence", is a precise one, namely the concept of deducibility by means of definite inference rules.

Taking into account the content of the paragraph which immediately precedes it, this sentence could be understood as a blunt dismissal of the opinions of the many logicians who were unable to see the obvious multifarious aspects of the common concept of consequence. But this is hardly a tenable reading, as it amounts to accusing some of the the ablest logicians of a particularly gross blunder.

There is another, more congenial way to read this sentence, namely as implying that if the logicians in question believed they had defined a rigorous concept coextensional with the common one, then they had in mind a restricted form of the more general common concept that Tarski mentioned in the first paragraph. I submit that this is a sensible reading, and I claim that when Tarski speaks of the "common" concept in the rest of his paper he invariably refers to this restricted form—the restriction intended being to logico-mathematical matters. Actually, since strict deducibility as considered by the pertinent logicians occurs in a mathematical framework, we can make this last reference to the common concept compatible with the one in the first paragraph by allowing that, although the concept as used in everyday language is neither determinate nor uniform, its restriction to mathematical usage is. Such a restriction, on the other hand, is harmonious with Tarski's main concern, as suggested in the first sentence of his article, where he refers to the introduction of the concept of logical consequence in *the field of strict formal investigation*.

Be that as it may, from the second paragraph to the end of the paper, (1) Tarski speaks of the common concept as if it were a definite one (in any event, far more definite that the everyday concept of the first paragraph), and (2) he seems to be concerned mainly (if not exclusively) with consequence in mathematics. Still in the second paragraph, Tarski allows that for their claim that the formalized concept of consequence "really coincided in extent with the common one, the logicians were able to bring forward a weighty argument: the fact that they had actually succeeded in reproducing in the shape of formalized proofs all the exact reasonings which had ever been carried out in mathematics" (p. 410). And the argument he wields against deducibility as a rendering of logical consequence is that, as a matter of fact, there are mathematical cases of logical consequence which are not, and cannot, be accounted for by deducibility. Thus, when he remarks that some sentence A expressing that all natural numbers have a certain property P cannot be derived in a deductive calculus from the infinitely many sentences A_n, expressing of each natural number n that it has the property P, he concludes:

> This fact seems to me to speak for itself. It shows that the formalized concept of consequence, as it is generally used by mathematical logicians, by no means coincides with the common concept. Yet intuitively it seems certain that the universal sentence A follows in the usual sense from the totality of particular sentences $A_0, A_1, \ldots, A_n, \ldots$. Provided all these sentences are true, the sentence A must also be true" (pp. 410–411).

If the common concept of consequence referred to in this excerpt were meant to be as imprecise and fluctuating as the one of the first paragraph, an argument against the formalized concept to the effect that there are instances of common logical consequence that cannot be accounted for by deducibility would be very weak indeed. If no precise concept can match the full extent of common consequence, this kind of incompleteness is something every precise account must exhibit. So, for Tarski to take his ω-argument as a serious counterexample to the deducibility account of consequence, he would have to say much more than "intuitively it seems certain" that A follows from the A_n, or that the inference from the infinitely many A_n to A preserves truth. He should say, for example, that ω-inferences are so central to our idea of consequence that any account that leaves them aside is unacceptable, that they belong to the core of the concept of consequence, and so forth. But he does not say anything of the kind. And he does not, I claim, because he is taking for granted that the common concept he is dealing with is a reasonably precise one (one that, at least, has a definite extension), and that this was accepted by those to whom the paper was directed. They all had a fairly clear idea of what this concept was, they only lacked a rigorous definition of it. This is what Tarski provided, a mathematically acceptable definition of this common concept which could be fruitfully used in mathematical inquiry.

From now on, when speaking of the "common" concept of consequence, I will refer exclusively to Tarski's use of this term in the body of his paper (first paragraph excluded). Moreover, whenever I will speak of the "formalized" concept of consequence I will invariably mean, as Tarski does, deducibility in a calculus. Later uses of "formal" or "formality" are not to be associated with "formalized". In particular, what Tarski calls "formal consequence" is definitely not the formalized concept of consequence.

My main goal in this paper is to substantiate the claim that Tarski's common concept is not some general, all-purpose notion of consequence, but a rather precise one, namely the concept of consequence at play in axiomatics. To this end I will sketch how the axiomatic method was understood since the end of the 19th century, I will discuss early attempts at defining logical consequence which look like informal or casual formulations of Tarski's one, and I will argue that some striking features of Tarski's paper that commentators do not satisfactorily explain receive an obvious explanation under the hypothesis that what Tarski was attempting to define was this version of logical consequence. After that, I will try to discern how Tarski's concept of logical consequence differed from the common one and why Tarski's definition succeeded in capturing the common concept.

Before moving into my main goal, it will be convenient to review some points of Tarski's paper and to examine how Tarski's commentators conceived of his common concept of consequence. All of them had to take a stand on this issue, since to identify what Tarski took this common concept to be is essential to decide whether his definition failed (as John Etchemendy has argued) or succeeded (as many other commentators, including Greg Ray, Mario Gómez-Torrente and Gila Sher, have maintained).

§2. A summary of Tarski's paper. Tarski's paper consists of twenty-two paragraphs which can be divided into an introduction and five parts. The introduction is just the first paragraph, which I have already discussed and quoted almost in full.

In the first part, which extends from the second paragraph to the middle of the sixth (pp. 409–413), Tarski discusses the formalized concept of consequence, i.e., deducibility, and argues that, contrary to the claims of some logicians, it is narrower than the common one. This he does by considering an ω-argument whose conclusion is a logical consequence of the premises but cannot be deduced from them. After discussing the attempts to encompass some form of the ω-rule into ordinary deductive systems, he finally appeals to Gödel's first incompleteness theorem to conclude that, in sufficiently strong deductive theories, "however much we supplement the ordinary rules of inference by new purely structural rules, it is possible to construct sentences which follow, in the usual sense, from the theorems of this theory, but which nevertheless cannot be proved in this theory on the basis of the accepted rules of inference" (pp. 412–413).

In the second part, which includes the second half of the 6th paragraph and paragraphs 7 and 8 (pp. 413–414), Tarski brings into play the "proper" concept of consequence:

> In order to obtain the proper concept of consequence, which is close in essentials to the common concept, we must resort to quite different methods and apply quite different conceptual apparatus in defining it (pp. 413).

He adds that this *new* concept will not supersede the *old* (the formalized) one, which will be important for "the practical construction of deductive theories", but that "in considerations of a general theoretical nature the proper concept of consequence must be placed in the foreground" (p. 413). He also asserts that it was Carnap who first attempted to give a precise definition of this concept and who emphasized its importance.

In the third part, comprising paragraphs 9 to 13 (pp. 414–416), Tarski announces his sketch of a "general method ... to construct an adequate definition of the concept of consequence for a comprehensive class of formalized languages" (p. 414). He lays down two central features of logical consequence: truth preservation and formality. Suppose K is a class of sentences and X is a sentence that follows from K. Truth preservation he formulates thus: "From an intuitive standpoint it can never happen that both the class K consists only of true sentences and the sentence X is false." As to formality, says Tarski:

> [S]ince we are concerned here with the concept of logical, i.e., *formal* consequence, and thus with a relation which is to be uniquely determined by the form of the sentences between it holds, this relation cannot be influenced in any way by empirical knowledge, and in particular by knowledge of the objects to which the sentence X or the sentences of the class K refer (pp. 414–415; emphasis in the original).

Accordingly, replacing the names of the objects mentioned in these sentences by names of any other objects (of the same logical type, that is) does not affect the consequence relation. Tarski then formulates statement (F) to jointly express truth preservation and this replacement condition:

> If, in the sentences of the class K and in the sentence X, the constants—apart from purely logical constants—are replaced by any other constants (like signs being everywhere replaced by like signs), and if we denote the class of sentences thus obtained from K by 'K'', and the sentence obtained from X by 'X'', then the sentence X' must be true provided only that all sentences of the class K' are true (p. 415).

He remarks that this formulation needs some corrections, in particular, that "any defined signs which may possibly occur in the sentences concerned" must be eliminated. Then he adds that (F) is a necessary but by no means a sufficient condition for logical consequence, owing to the fact that it may be fulfilled because of the relative poverty of the language. "The condition (F) could be regarded as sufficient for the sentence X to follow from the class K only if the designations of all possible objects occurred in the language in question. This assumption, however, is fictitious and can never be realized" (p. 416).[2]

In the fourth part (paragraphs 14–18, pp. 416–418), Tarski formulates his semantic definition. First he introduces the concepts of *satisfaction of a sentential function* by a sequence of objects (a sentential function being an open formula, i.e., a formula with free variables), and of *model*. A model or a realization of a class L of sentences is a sequence of objects satisfying all the sentential functions obtained from the sentences in the class by replacing "all extra-logical constants in the sentences belonging to L by corresponding variables, like constants being replaced by like variables, and unlike by unlike" (p. 417). A model of a sentence is a model of the class whose only member is the given sentence. Then he gives his definition of logical consequence as:

> The sentence X *follows logically* from the sentences of the class K if and only if every model of the class K is also a model of the sentence X (p. 417).

Immediately after the definition, Tarski asserts that it is successful with these words:

> It seems to me that everyone who understands the content of the above definition must admit that it agrees quite well with common usage (p. 417).

He does not justify this assertion, but he adds that the adequacy of the definition "becomes still clearer from its various consequences"; in particular, it can be proved that logical consequence thus defined is truth preserving and that it fulfills condition (F). Next, Tarski sketches how to "reconcile the proposed definition with that of Carnap" by laying down suitable "conventions and assumptions" on whose basis "it is easy to prove the *equivalence of the two definitions*" (pp. 417–418).

The fifth part of the paper, comprising the last four paragraphs (19–22, pp. 418–420), begins with Tarski's assertion that there are still some problems left as regards a "materially adequate definition of the concept of consequence". The one he considers most important, and the only one he mentions, concerns the classification of terms into logical and extra-logical, on which the concept of model, and thus the definition of logical consequence, depends. The division is not arbitrary, since taking the usual connectives and quantifiers as extralogical in Tarski's definition "would lead to results which obviously contradict ordinary usage" (p. 418). Nevertheless, there seem to be different ways of separating the two groups of terms which are consistent with the common concept. As an extreme case (about whose

[2] In [11], Gómez-Torrente conjectures that "F" stands for "form" or for "formality", but as Stroińska and Hitchcock show, it stands for "Folgerung", the German word for "consequence". This can be seen by turning to the Polish version of the paper, where the letter "W" , the initial of "wynikanie" (consequence) is used ([61], 167).

adequacy to common usage he says nothing) he considers the possibility that all terms are counted as logical, in which case, he concludes, *formal* and *material* consequence would coincide, in that X would be a consequence of K whenever X is true or some sentence in K is false.

In the concluding paragraph, Tarski envisages the possibility that further research will offer no objective grounds for a sharp boundary between logical and extra-logical expressions. If this were so, we should be compelled to regard the concept of logical consequence as a relative concept "which must, on each occasion, be related to a definite, although in greater or less degree arbitrary, division of terms into logical and extra-logical." And he closes with a sentence which seems to bring us back to the opening paragraph: "The fluctuation in the common usage of the concept of consequence would—in part at least—be quite naturally reflected in such a compulsory situation" (p. 420).

§3. Three concepts of consequence. Leaving aside the introductory paragraph of his paper, Tarski speaks of three concepts of consequence, namely, the *common* concept, the *formalized* concept, and the *proper* concept. There is no doubt about the identity of the second one, but the nature of the other two and the relation between them needs to be clarified. Although Tarski is not fully explicit about it, he certainly suggests that the common concept of consequence is the one which contemporary logicians and he himself, wanted to define. Otherwise put, the success of a precise definition of consequence is to be gauged by the closeness of the defined concept to the common one. In the first part of the paper, Tarski is able to show that the formalized concept is not faithful to the common one by exhibiting a (relevant) feature of the common concept that the formalized concept lacks. He can do this because both he and his intended audience know what the common concept was. But if we want to argue that Tarski's definition is, or is not, successful, then we have to have a clear idea of what the common concept is. Appeals to intuition at this point are out of place. First, because as the discussions of Tarski's paper prompted by Etchemendy's critique in [6] have made it clear, there is no agreement about what commentators take as intuitive relating to logical consequence. Second, because our intuitions about logical consequence may have been shaped, at least partly, by sustained exposition to Tarski's work. Moreover, how could we establish that our intuitions conform to the common concept that Tarski and his audience had in mind? True, Tarski pondered whether with the formalized concept of consequence "we can finally succeed in grasping the full intuitive concept of consequence" (p. 412). But this can hardly justify us in bringing in alleged intuitions to assess the adequacy of Tarski's definition.

What about the nature of the proper concept of consequence and its relation to the common one? Tarski introduces the proper concept abruptly.

After stating that Gödel's incompleteness theorem shows that no version of the formalized concept of consequence can be faithful to the common one, he says that to obtain the proper concept of consequence new methods must be used. Of the proper concept, he says that "it is close in essentials to the common concept" (p. 413), that Carnap attempted to define it (p. 413), and that truth preservation and formality are two characteristic and essential features of this concept (p. 415). But this much seems to be clear: the proper concept of consequence is the one he wants to define.

After the introduction of the proper concept of consequence at the very beginning of the second part (p. 413), the reader has the feeling that Tarski has the same attitude towards the proper concept as he had before towards the common one, and that whatever he says of the proper concept he could have said of the common one. Thus, when in the third part of the paper he singles out truth preservation and formality as essential characteristics of the proper concept, he draws these two features from certain "considerations of an intuitive nature" (p. 414), which reminds us of the phrase "intuitively it seems certain" (p. 411) with which he alluded earlier to the common concept. Again, when discussing the import of condition (F), he writes that "it may, and it does, happen ... that the sentence *X does not follow in the ordinary sense* from the the sentences of the class *K* although the condition (F) is satisfied" (p. 415, emphasis added). One would say that following in the ordinary sense (im *üblichen* Sinne), is just following according to the common concept of consequence (nach dem *üblichen* Folgerungsbegriff), which induces us to think of the proper and the common concept as one single concept. The inducement seems the strongest when, after laying down his semantic definition of consequence, thus, of the proper concept of consequence, he claims (p. 417) that "it agrees quite well with common usage".

This prompts us to provisionally conjecture that with "the proper concept of consequence" Tarski refers to the common concept as given by a precise definition. In other words, we can tentatively explain the use of the terms "the common concept" and "the proper concept" by saying that there is just one concept, to which Tarski refers as "the common concept" when he thinks of it as used in the absence of a precise definition, and he speaks of it as "the proper concept" when he views it as having been given by a rigorous mathematical definition.

As we saw, Tarski says that the proper concept of consequence is "close in essentials" to the common one. It is hard to discern what this exactly means, but we can understand "closeness in essentials" according to the interpretation just sketched. True, Tarski speaks of *two* concepts, but in the paper under consideration he uses "concept" in a rather general, non-technical way. This much, at least, can be substantiated about Tarski's use of the term "concept": concepts are not extensional. Recall that in the second paragraph of the paper he says that "many logicians believed that they had succeeded ... in defining a new concept which coincided in extent[3] with the common one" (p. 409). Thus, even if deducibility and consequence happened to be coextensional (for a certain language or for a certain class of languages) the concept of deducibility would not be the concept of consequence (even for these languages).

If we look closely at the sixth paragraph, where the proper concept is introduced, we notice that Tarski contrasts it with the formalized concept, rather than with the common one. Indeed, he calls the formalized concept the *old* concept of consequence, and he refers to the proper concept of consequence as the *new* one. This suggests the following setting. Logicians are interested in giving a workable and rigorous definition of the common concept of consequence, that is, of providing a precise concept of consequence which agrees with the common one. The first such definition provides us with the formalized concept of consequence, which differs in extension from the common concept, and, for that reason, is unsuccessful. So, we have to go after the (or after *a*) right definition, one that is faithful to the common concept, that *properly* characterizes it—thus one giving rise to the proper concept of consequence. Actually, Tarski all but says so in the last footnote to the sixth paragraph when he comments on Carnap's *Logische Syntax der Sprache*. In this book, in Tarski's words, "the term (*logical*) *derivation* or *derivability* is applied to the old concept of consequence as commonly used in the construction of deductive theories, in order to distinguish it from the concept of *consequence* as the proper concept" (p. 413, footnote 2, emphasis in the original).

If this is right, then the only purpose served by bringing into play the proper concept of consequence is to highlight that the formalized one is inadequate, so that a proper definition of consequence still has to be found. Accordingly, whenever Tarski speaks of defining the proper concept of consequence, we should understand him speaking of providing a proper definition of the common concept.

I take the first part of the just mentioned footnote to the sixth paragraph as conclusive evidence that the proper concept and the common concept of consequence are one and the same (or, to play safe, they have the same extension). Says Tarski: "An opposition between the two concepts in question [the formalized and the proper one] is clearly pointed out in article IX, pp. 293 ff. Nevertheless, in contrast to my present standpoint, I have there expressed myself in a decidedly negative manner about the possibility of setting up an exact formal definition for the proper concept of consequence"

[3]The German word is *Umfang* ([44], 1; [60], 271). Recall that the *Umfang eines Begriffs* is the *extension of a concept*. As to the *content* (*Inhalt*) of the common concept, Tarski seems to mean the same by "content" as by "extension", as we clearly see later in this paragraph when he explains the sentence: "Logicians thought that these few rules of inference exhausted the content of the concept of consequence" thus: "Whenever a sentence follows from others, it can be obtained from them—so it was thought—in more or less complicated ways by means of the transformations prescribed by the rules" (p. 410).

(p. 413, footnote 2).[4] If we turn to the pages referred of article IX ("Some observations on the concepts of ω-consistency and ω-completeness"), we see no mention at all of the proper concept, but only of the formalized and the common ones. There—as in the article on logical consequence—Tarski appeals to Gödel to conclude that the "formalized concept of consequence will never coincide in extension with the common one"[5] ([58], 295). Clearly, then, what in the paper on logical consequence Tarski calls "the proper concept", in the article on ω-consistency and ω-completeness is simply "the common concept of consequence". Speaking of the proper, rather than the common concept, is a matter of emphasis, not of content.

§**4. Tarski's commentators on the common concept.** Before presenting my proposal of what Tarski's common concept of consequence is, I review the opinions of some commentators of Tarski's paper concerning this issue. As a matter of fact, none of the commentators is very clear about it. This may come as a surprise, because the unclarity about the nature of this common notion may weaken the claims that Tarski's definition is adequate to it or that it is not. On the other hand, one might think that such lack of precision regarding the identity of the common concept should be expected, owing to Tarski's words in the opening paragraph of his paper on which I have already commented.

Thus, in "Logical consequence: A defense of Tarski", Greg Ray takes this introductory paragraph as evidence that Tarski viewed the common usage as so fluctuating and unclear that Tarski's claim that his definition is faithful to the common concept of consequence cannot be taken at face value. Tarski, so Ray contends, could not possibly mean that a sentence is a logical consequence of a set of sentences according to his definition if and only if it is a consequence of the given set according to common usage. It could not, because the former, but not the latter, is a definite relation.

According to Ray, Tarski proceeded in his paper on logical consequence as he had done in his essay on truth [48]. There he had singled out condition T as the criterion of adequacy for a formally correct definition of a truth predicate. Similarly, in the paper on logical consequence Tarski first identified some central characteristics of the informal common notion, and then formulated "a precise adequacy condition which could intuitively be seen to capture the

[4]He explains that the reason for his earlier opinion lies in the fact that there he had worked in the framework of theory of types, and "it can be shown that it is impossible to define the proper concept of consequence adequately" within this theory, "unless we . . . limit our considerations solely to . . . languages of finite order".

[5]"der formalisierte Konsequenzbegriff wird sich seinem Umfange nach nie mit dem üblichen decken" ([43], 111; [59], 635). The English text has "ordinary", where I put "common", but the German word is "üblich", which in the paper on logical consequence is usually rendered as "common": in particular, "der übliche Begriff" is systematically translated as "the common concept".

informally identified characteristics. Once he had a precise condition, the object for him was to define a term which satisfied the adequacy condition" ([36], 623). As to the characteristics in question, "[r]oughly, these were that logical consequence is (a) truth-preserving, and (b) formal-logical, i.e., that sentences stood in this relation due to their logical form" ([36], 624). The precise condition is statement (F).

So far, this account suggests that, as in the case of Condition T and truth, Tarski would consider a formally correct definition of logical consequence to be adequate if it satisfied Condition (F). But this Tarski emphatically denied. What about the adequacy condition, then? Says Ray: "Tarski does *not* tell us that a definition of logical consequence is to be considered *adequate* just in case it provably entails [Condition (F)]. Instead, the adequacy condition that seems to be operative [in the paper on consequence] is much more directly tied to the two characteristics of the logical consequence that Tarski identified" ([36], 624; emphasis in the original). As Ray finally puts it, "we may think of material adequacy, in this context, as co-extensiveness with a carefully circumscribed usage of 'logical consequence' " ([36], 625). And he proposes that what determines this particular usage is its being truth-preserving and formal. This, we may complain, has an air of circularity, the more so as we are not even given a hint about where to look at for such a usage.[6]

For his part, in "On Tarski on models", Timothy Bays is less specific still about Tarski's common notion. He describes Tarski's goal thus:

> On the whole, Tarski's paper is concerned with providing a mathematical analysis of the notion of logical of consequence. In theory, this project should involve two different kinds of investigation: first, a philosophical investigation which clarifies our intuitive conception of logical consequence, and second, a mathematical investigation which develops a formal definition corresponding to our intuitive conception. Tarski's paper, however, tends to proceed as though the first of these investigations has already been completed; Tarski simply runs through a series of candidate definitions, eliminating those which conflict with his intuitions concerning "what counts" as an instance of logical consequence. His final analysis, therefore, has a provisional character: although it does not conflict with the particular intuitions considered in earlier portions of the paper, and although it seems to "agree quite well with common usage," it may have hidden inadequacies which would be revealed if we considered additional intuitions ([3], 1702).

[6]Unless this counts as a hint: "I think it is clear that Tarski had in mind the concept of consequence employed by logicians and philosophers. Persons not trained to it do not have a distinguished notion of *logical* (i.e., formal) consequence in Tarski's sense" ([36], 673, footnote 20; emphasis in the original).

Did Tarski really have to go through a philosophical investigation about *our* intuitive conception of logical consequence before attempting his definition? What does this intuitive notion look like? How robust or commonly shared is it? Would it not be more promising to turn to the mathematical practice in order to look for some more articulate notion that Tarski could want to characterize?

It is plain that Bays does not take Tarski's claim of adequacy to common usage seriously, for he even alters Tarski's own words in a slight but significant way. Thus although Tarski says that it seems to him that "everyone who understands the content of the above definition must admit that it agrees quite well with common usage" (p. 417), thus implying that his definition *does agree* quite well with common usage, Bays misquotes him as merely saying that it *seems to agree* with it. Accordingly, it is only natural that Bays does not try to be more specific about what the common concept of consequence could be.

In his paper "Tarski on logical consequence", Mario Gómez-Torrente sets out to "discuss and analyze from a historical perspective some of the aspects of Tarski's work that are of direct relevance for a philosophical understanding of his definition of logical consequence" ([11], 126). Although he tells us that he will "pay special attention to Tarski's description of his reasons for proposing such a definition" ([11], 126), he is almost silent about the nature of Tarski's common concept, except that it is the one implicitly acknowledged by mathematical logicians and by mathematicians generally. Thus, in the last section of the paper, which deals with Tarski's definition in context, we are told that one of the motivations behind Tarski's proposal of his definition was "the need to offer a definition generally applicable to the formalized languages familiar to mathematical logicians, that agreed as much as possible with the 'ordinary usage' among them of the notion of logical consequence" ([11], 145). We are also told that Tarski succeeded in that his definition "accommodated fairly well ordinary mathematical use of informal notions" ([11], 145), but no description is given of this ordinary use at this point, although he had earlier said that for Tarski, logical consequence is the same as formal consequence—thus that formality, properly understood, belongs to the common concept. What reasons Tarski had for identifying logical with formal consequence he does not say.

Neither does Gila Sher attempt to specify what the common notion is, although in *The Bounds of Logic* she views Tarski's two conditions of truth preservation and formality as a description of the intuitive content of the concept of logical consequence, where she understands truth preservation as necessary consequence ([40], 40). In "Did Tarski commit Tarski's fallacy?" she says that "Tarski believed his definition of logical consequence captured the intuitive notion" ([41], 653), which, according to her, "involves two intuitive ideas: the idea that *logical consequence is necessary* and the idea that *logical consequence is formal*". What necessity amounts to she explains thus: "Assume σ is a logical consequence of Γ. Then *it is impossible that* all the sentences of Γ are true and σ is false (where 'it is impossible' is an intuitive modal operator, equivalent to 'necessarily, it is not the case that')" ([41], 654; emphasis in the original).

In "Logical consequence revisited", José M. Sagüillo takes Tarski to deal with "some usages of certain portions of natural languages that logicians and mathematicians use in their ordinary and professional practices" ([37], 218). This, however, leaves us in the dark about what Tarski's common concept is, the more so after Sagüillo's assertion that there are three basic conceptions of logical consequence (the information containment conception, the necessity conception, and the impossibility conception) and "Tarski's 1936 paper does not clarify which of [them] (if any) Tarski had in mind" ([37], 220). Unlike the previous commentators, Sagüillo deals with the question of why formality is a central aspect of the common concept (whatever it may be) of logical consequence. He relates formalization to disinterpretation ("which amounts to 'bracketing' content or subject-matter and, hence, to treating the non-logical symbols as if they were variables") and traces its origin to the need to avoid hidden premises in a deduction: "It is precisely by attending to the formality requirement that premise-smuggling is avoided or detected" ([37], 234).

John Etchemendy's monograph *The Concept of Logical Consequence* "deals with various intuitive or conceptual considerations bearing on the adequacy of Tarski's account" ([6], 9). He wants to show that "the standard, semantic account of logical consequence is mistaken", that when applied to arbitrary languages, "it will regularly and predictably define a relation at variance with the genuine consequence relation of the language in question" ([6], 8). His main "claim is that Tarski's analysis is wrong, that his account of logical consequence does not capture, or even come close to capturing, any pre-theoretic conception of the logical properties" ([6], 6).

This sounds like a very strong thesis, but we should keep in mind that in order to evaluate the adequacy of Tarski's account one has to know what Tarski's actual aim was, for the adequacy of an account is its conformity with the goal pursued. What does Etchemendy say about this aim, or, in other words, how does he understand Tarski's common concept?

According to Etchemendy, "Tarski takes as his goal an account of consequence that remains faithful to the ordinary, intuitive concept from which we borrow the name" ([6], 2). But Etchemendy is never clear enough about the characteristics of this ordinary, intuitive concept—to which he also refers as the *pre-theoretic*, or the *genuine* notion of consequence.[7]

For Etchemendy, "the most important feature of logical consequence, as we ordinarily understand it, is a modal relation that holds between implying

[7] Some critics of Etchemendy, among them Gómez-Torrente, Sher and Ray have followed him in describing Tarski's common concept as pre-theoretic.

sentences and the sentence implied. The premisses of a logically valid argument cannot be true if the conclusion is false; such conclusions can be said to 'follow necessarily' from their premisses" ([6], 81). He claims that Tarski's defined notion does not possess this feature, which renders it inadequate. In order to deal with this criticism, we should have to see whether, as Sher also maintained, Tarski's common notion has this modal component, and, if it does, whether Etchemendy's argument that Tarski's account lacks it is sound. I deal briefly with this issue at the end of the paper.

In contrast to the commentators considered so far, Etchemendy does not take Tarski as dealing with logical consequence as restricted to a logico-mathematical setting. Etchemendy seems to assume that the common concept Tarski wants to characterize with his definition is some notion with a wider use. This can be gathered from the many examples he brings in. As Greg Ray puts it, "Etchemendy seems to construe Tarski's references to the 'ordinary notion of logical consequence' so as to come to something like 'the ordinary person's notion of what follows from what' " ([36], 673, note 40).

Similarly, but more explicitly, this is maintained by Magda Stroińska and David Hitchcock, who call attention to the fact that in the Polish version of Tarski's paper, which they translate into English and compare with the German version,[8] Tarski does not speak of "the common concept", but rather of "the everyday concept" of consequence. They bring this as evidence that "Tarski uses as his touchstone everyday speech, not the inferential habits of mathematicians (as some commentators have assumed)" ([61], 165–166), and they add that "the word 'common' more obviously contrasts with the word 'proper' used both in Polish and German for the concept of following which should be used in the methodology of the deductive sciences"([61], 166). Indeed, these authors suggest that this everyday (or common) concept of consequence may not be a concept of *logical* consequence. As Tarski explicitly asserts, logical consequence is formal consequence, but, according to Stroińska and Hitchcock, Tarski "envisages the possibility that a sentence might follow in the intuitive sense, though not formally, in virtue of our knowledge of the external world" ([61], 169).

§5. Is Tarski's common concept pre-theoretic? In the introduction to his book, Etchemendy compares Tarski's definition of logical consequence to the inductive definition of the set of the natural numbers as the smallest set

[8]As Stroińska and Hitchcock substantiate, Tarski's German paper [44], of which the English version is a translation, was not translated from the Polish, as stated in Tarski's Collected Papers ([60], 270). The German and the Polish versions were written at the same time ([61], 157–158). This being so, I see no reason why, when a discrepancy between the Polish and the German texts arise, we should favor the Polish version, as Stroińska and Hitchcock often do. Actually, as they themselves suggest at one point, some of the differences found should be accounted for by Tarski's adjustments to the philosophical outlook of the intended audiences ([61], 166–167).

that contains 0 and is closed under the successor operation—which we may ascribe to Dedekind. Etchemendy finds that this definition, although it "is not identical to the intuitive notion it supplants", it "obviously captures the essential feature of the intuitive notion, and so its extensional adequacy is apparent from the definition itself" ([6], 9). As to Tarski's definition, "most people react to [it] in the same way they react to the inductive definition of **N**. Neither is given extensive justification since neither seems to need it", but, Etchemendy objects, "this reaction is, in [the consequence] case, mistaken" ([6], 9).

I submit that, contrary to Etchemendy's assertions, the two cases are quite similar. In order to judge whether Dedekind's and Tarski's definitions are adequate, we need first to specify with some detail what their target object is. Otherwise put, we need to reach some agreement as to how one viewed the natural numbers before Dedekind's definition, and what one understood by logical consequence prior to Tarski's. If we only say that the common notion of logical consequence is the intuitive, or the pre-theoretic, or the genuine notion of consequence, then it is likely that we remain unconvinced by Tarski's analysis—but we should also remain unconvinced by Dedekind's analysis if all we said of the concept of natural number that he wanted to characterize were that it is the intuitive, or the pre-theoretic, or the genuine notion of natural number. To begin with, we hardly know what such a notion could be, after so many centuries of being exposed to arithmetic. The concept of natural number that Dedekind meant to characterize in *Was sind und was sollen die Zahlen?* was not in any sensible way pre-theoretic, and the insights one had about their structure were rather elaborate, only articulated after extensive mathematical work. For example, mathematical induction, which is central to Dedekind's account, was first explicitly used as late as the sixteenth century (see [20], 272). What Dedekind's definition captures in a precise set-theoretical way is the idea that all that matters about natural numbers is their being generated from the number zero by means of the successor operation. This is a very abstract concept of number, neither an ordinal nor a cardinal, for example. But since we are familiar enough with it (more to the point, since mathematicians in Dedekind's time were) we can (they could) recognize that Dedekind's analysis or definition is right.

Let's think about Tarski's definition of logical consequence along these lines. The fact that people readily accepted it as correct (more to the point, the fact that mathematical logicians accepted it as such) can be taken as an indication that it was indeed correct—that is to say, that is was adequate to its aim, which need not be, and most likely should not be, anything like a pre-theoretic notion of consequence. As a matter of fact, if (as I claim, and as I will presently substantiate) the common concept of consequence that Tarski had in mind was the one involved in work with axiomatic theories, then the adequacy of Tarski's definition strikes us as right—if we know, that is, how axiomatic theories were understood at the time, and if we are familiar enough with Tarski's semantic methods. (In the same way, to recognize that Dedekind's definition is adequate we have to know how natural numbers were understood at the time, and be familiar enough with the set-theoretical machinery involved.)

The comparison of Tarski's with Dedekind's definitions is relevant in other respects. For example, Dedekind does not have to explain what he understands by a natural number in order for one to see that his definition is right. This is so because the intended reader of his essay already knows what the natural numbers are. The same, I maintain, happens with Tarski. He does not have to go into details about what notion of consequence he wants to characterize, because those to whom the paper is directed know it well enough. He only emphasizes those points he deems to be crucial, without explaining why the common notion has the properties he asserts it has. But if we want to assess the adequacy of either Dedekind's or Tarski's definition, we have to bring into the open what it is that the definitions are meant to define. In the case of the natural numbers, we could adduce considerations like the ones in sections 6 and 7 of Kleene's *Introduction to Metamathematics* ([19], 19–22). In the case of logical consequence, we should discuss what an axiomatic theory is (or should be). Better, we should see how axiomatic theories were conceived before and at the time Tarski offered his definition—and this with the aim to describe the common concept of consequence. From this description it should be obvious that such a concept is formal in Tarski's sense, something that, curiously, one does not see justified by Tarski's commentators. Tarski did not have to justify it, because everyone involved saw that it was formal. But modern critics who either attack or defend Tarski's analysis should argue why the common notion of consequence is formal, or rather, why Tarski took it to be so. Besides, they should also explain why Tarski took it to be formal without any explicit justification.

It may be also relevant to note that both Dedekind's and Tarski's characterizations required new methods and instruments, and that Dedekind and Tarski were among those who developed them. As Tarski said in the prefacing remarks to his definition of logical consequence, the ideas involved were not at all new, but "only the methods which have been developed in recent years ... and the concepts defined with their aid, allow us to present these ideas in an exact form" (p. 414). These methods were semantics in Tarski's case, and set theory in Dedekind's.[9] They are essential in each case.

§6. Axiomatic theories. In order to identify the consequence relation at play in axiomatic theories I find it convenient to recall Hilbert and Bernays' distinction between *contentual* axiomatics [inhaltliche Axiomatik], on the one hand, and *formal* axiomatics [formale Axiomatik], on the other

[9]See Ferreirós [8] for a detailed account of Dedekind's contribution to early set theory.

([15], 1–2). The respective paradigms are taken to be Euclid's *Elements* and Hilbert's *Grundlagen der Geometrie*. I begin with contentual axiomatics.

When we build an axiomatic theory, either from scratch or as an attempt to systematize the results of a given mathematical discipline, we have to make a decision about the entities (concepts, relations, operations, individuals) in terms of which we want to conceptualize the subject matter of the theory. These we call the *specific entities*, and the terms standing for them the *specific terms* of the theory. The specific terms are among the primitive ones, but not all primitive terms are specific. In particular, those terms of a clear logical nature (as connectives, quantifiers, the identity sign) are among the primitive, non-specific ones.[10]

In contentual axiomatics, the axioms of the theory are meant to express true propositions about the specific entities, and the theorems are meant to follow (logically) from the axioms, hence they are true of the specific entities as well. A warrant of theoremhood is a proof from the axioms, that is, a proof in which nothing is assumed about the specific entities beyond what the axioms state.[11]

In order to draw inferences from the axioms, we certainly have to have recourse to knowledge not explicitly recorded in the axioms. To begin with, knowledge about the logical components of the language, and, more generally, about all non-specific entities, i.e., those to which the primitive non-specific terms of the language refer. How, then, can we make sure that we do not, perhaps implicitly, rely on some unstated assumption or piece of knowledge regarding the specific entities?

Let's consider a well-known example. The first proposition in Euclid's *Elements* concerns the existence (the constructibility) of an equilateral triangle with base on a given segment AB. The proof (the construction) is simple: Describe two circles of radius AB, one centered at A and the other at B. Let C be one point of intersection of the two circles. Since AB, AC, and BC have the same length, namely the radius, the triangle ABC is equilateral. QED. Suppose we ask, how do we know that the two circles intersect? Euclid does not say, he just takes it for granted. Does it follow

[10]The term 'specific' was used on occasion by Tarski. Thus: "In addition to the logical constants, [an axiomatic] theory contains new terms, the so called *extra-logical constants* or the *specific terms* of the theory in question" ([57], 297). Again, in "On definable sets of real numbers", after introducing "the primitive terms of the system of logic" he writes: "It now remains to add the specific primitive terms of the arithmetic of real numbers " ([53], 114). I prefer "specific" to "extra-logical" or "non-logical" because an axiomatic theory may contain primitive non-specific terms which we would hardly call "logical" or of whose logical character we may be in doubt. Actually, in the paper just cited, Tarski lists "the sign '$\in$' of set membership" among the logical terms.

[11]By a theorem I here mean any sentence that follows from the axioms of the theory, but I do not want to commit myself at the start about what this following amounts to. I claim that this following relation is Tarski's common concept of consequence, and we have to see what particular relation it is.

from the axioms that they meet? Nowadays we would have recourse to some axiom of continuity to guarantee an intersection point—and we would argue that such an axiom is needed, for if the plane consisted only of rational points these two circles would not meet. This implies that there is a mistake in Euclid's proof. But the answer might not be that simple. It could be maintained that the continuity of circles need not be explicitly asserted in the axioms because it belongs to the nature of lines to be continuous, so that talk of non-continuous lines is self-contradictory. If this is so, then the existence of point C does follow from the axioms and there is no oversight in Euclid's proof. Inferring the existence of C from the definitions of the circles would be analogous to inferring the consequent of a conditional from the antecedent and the conditional itself: one does it silently, without citing a rule to justify it.

Oversight or not, the fact is that we do not want to assume implicitly that lines are continuous, even if we allow that the notion of a non-continuous line is absurd. Why? Not because it is an implicit assumption (for, as remarked, we assume implicitly quite a bit of knowledge about logical concepts, or about sets, and sometimes about other mathematical entities as real numbers), but because it is a geometrical assumption and we are axiomatizing geometry. The idea is that the specific terms are the geometric terms, and about them no implicit assumptions are to be allowed. As Moritz Pasch put it in his *Vorlesungen über neuere Geometrie*:

> In fact, if geometry is to be really deductive, the deduction process must be everywhere independent of the *meaning* of the geometrical concepts, as it must be independent of the figures; only the *relations* between the geometrical concepts appearing in the theorems or definitions employed must be taken into account. During the deduction, it may be admissible and useful, but *by no means necessary*, to think of the meaning of the geometric concepts. Indeed, if this becomes necessary, then the deficiency of the deduction ... emerges.[12]

How can we make sure that we avoid having recourse to the meaning of the geometric, and more generally, of the specific terms of an axiomatic theory? In this task, formal axiomatics comes to our help. One sure way to avoid making implicit assumptions about the specific entities is to divest

[12] Es muß in der Tat, wenn anders die Geometrie wirklich deduktiv sein soll, die Deduktion überall unabhängig sein soll vom *Sinn* der geometrischen Begriffe, wie er unabhängig sein muß von den Figuren; nur die in den benutzten Sätzen und Definitionen niedergelegten *Beziehungen* zwischen den geometrischen Begriffen dürfen in Betracht kommen. Während der Deduktion ist es zwar statthaft und nützlich, aber *keineswegs nötig*, an die Bedeutung der auftretenden geometrischen Begriffe zu denken; so daß geradezu, wenn dies nötig wird, daraus die Lückenhaftigkeit der Deduktion, unter Umständen sogar die Unzugänglichkeit der als Beweismittel vorausgeschickten Sätze hervorgeht. ([26], 90; emphasis in the original)

the specific terms of all meaning, by viewing them as mere placeholders for indeterminate entities. And this is how Mario Pieri viewed them when in 1895 he presented an axiomatization of projective geometry. In Pieri's own words:

> The undefined entities, that is to say, the *primitive* concepts, which all postulates are about (and which are like the raw material of each proposition), are *three* in number and they are called the *projective point*, the *projective line*, and the *projective segment*. To them any meaning can be assigned as long as it is in harmony with the postulates.[13]

This he expressed with more detail at the International Congress of Philosophy which took place in Paris from 1 to 5 August 1900, immediately before the International Congress of Mathematicians where Hilbert delivered his celebrated lecture on mathematical problems. Commenting on the paragraph by Pasch quoted above, Pieri said:

> This means, in sum, that the *primitive* entity of any deductive system (such as *point* in geometry) can be given *arbitrary* interpretations within some limits imposed by the primitive propositions—in such a way that the content of the words or the signs used to designate any primitive object must be determined *only* by the primitive propositions concerning it, and for the rest, everyone is free to attach a meaning *ad libitum* to these signs, provided it is compatible with the general attributes imposed to this entity by the primitive propositions.[14]

The formal view of axiomatics was generally adopted in the first quarter of the twentieth century. Thus, in 1910, Oswald Veblen and John W. Young described the axiomatic method to be followed in their book on projective geometry thus:

> *The starting point of any strictly logical treatment of geometry (and indeed of any branch of mathematics) must then be a set of undefined elements and relations, and a set of unproved propositions*

[13]"Gli enti qui non definiti, vale a dire, i concetti *primitivi* intorno a cui si aggirano tutti i Postulati (e sono come la materia greggia di ogni proposizione) vengono ad esser *tre* di numero, chiamati il *punto projettivo*, la *retta projettiva*, il *segmento projettivo*. Ad essi può attribuirsi qualsivoglia significato in armonia coi Postulati che saranno man mano introdotti ([30], 607; [35], 13).

[14]Cela revient à dire, en somme, que l'être *primitif* de n'importe quel système déductif (comme serait le *point* en Géométrie) doit être capable d'interprétations *arbitraires* dans certaines limites assignées par les propositions primitives; de telle sorte que le contenu des mots ou des signes qu'on emploie pour désigner un sujet primitif quelconque soit déterminé *uniquement* par les propositions primitives qui portent sur ce sujet, et que du reste chacun soit libre d'attacher à ces mots ou à ces signes un sens *ad libitum*, pourvu qu'il soit compatible avec les attributs généraux imposés à cet être par les propositions primitives ([32], 373; [35], 241); emphasis in the original).

> *involving them; and from these all other propositions* (*theorems*) *are to be derived by the methods of formal logic.* Moreover, since we assumed the point of view of formal (i.e., symbolic) logic, the undefined elements are to be regarded as mere symbols devoid of content, except as implied by the fundamental propositions. Since it is manifestly absurd to speak of a proposition involving these symbols as self-evident, the unproved propositions referred to above must be regarded as mere *assumptions*. It is customary to refer to these propositions as axioms or postulates, but we prefer to retain the term *assumption* as more expressive of their real logical character.
>
> We understand the term *a mathematical science* to mean *any set of propositions arranged according to a sequence of logical deduction*. From the point of view developed above such a science is purely *abstract*. If any concrete system of things may be regarded as satisfying the fundamental assumptions, this system is a concrete *application* or *representation* of the abstract science ([67], 1–2; emphasis in the original).[15]

Strictly speaking, if the primitive terms of an axiomatic theory are mere symbols devoid of content, then there cannot be fundamental propositions involving them, self-evident or not. Accordingly, rather than assumptions, the axioms are more aptly described as conditions that a sequence of chosen entities of suitable types may or may not fulfill. This view we find expounded and defended in 1911 by Edward E. Huntington, who calls them "postulates", proposing to reserve the name "axiom" only in connection with contentual (he says "older") axiomatics:

> We have called them "postulates", from the Latin *postulo*, because they are "demands" or conditions which a given system may or may not happen to satisfy. They are logically analogous to the demands or conditions set up in other fields of activity; for example, just as any man who satisfies the conditions set up for admission to the army is entitled to belong to that particular class of men, so any system $(K, \circ)$ that satisfies the conditions set up in sec. 14 is entitled to belong to a certain class of systems. No one would think of calling the conditions for admissions to the army "axioms"; and there is no more reason for calling the conditions of sec. 14 by that name.
>
> Indeed, if the word "axiom" is preserved in its well-established meaning, the recognition of the distinction between axiom and

[15] Notice that what Veblen and Young describe as a concrete application or representation satisfying the unproved propositions or fundamental assumptions is (or plays the role of) a model of the formal axiomatic theory.

> postulate, if properly understood, may well serve to mark the transition from the older to the more modern point of view in regard to the nature of abstract mathematical reasoning. (Huntington [17], 172).[16]

I said that in the step from contentual to formal axiomatics the specific terms became mere placeholders for arbitrary entities. One way to make this more precise is to view them as variables, and this is how they were often considered. Thus, Huntington, after his defence of the name "postulate" for the newly understood axioms, says that "[s]trictly speaking, these postulates, and all the theorems deducible from them, are not propositions at all, but rather what Bertrand Russell has called 'propositional functions', which become propositions (true or false) only after particular values are assigned to the variable symbols" ([17], 172). Notice that this formal treatment of axiomatic theories makes it almost inevitable to conceive of the theorems as those propositional functions which are true of the entities which satisfy the axioms—thus as the consequences of the axioms in Tarski's sense.[17] Our task now is to see whether the consequence relation involved was in fact explicitly described in this way.

§7. Theoremhood and logical consequence. The theorems of an axiomatic theory were customarily described as the logical consequences of the axioms, or as following from the axioms, or as being deducible from the axioms. Thus, in the excerpt from Pasch, he spoke about deducing theorems from the axioms. We may also recall the already quoted fragment of Veblen and Young, where they say that the theorems of an axiomatic theory are to be derived from the axioms by the methods of formal logic. As one last example, I bring in Huntington's assertion that "any set of *consistent* postulates would give rise to a corresponding algebra,—namely the totality of propositions which follow from these postulates by logical deduction" ([16], 290).

[16]Ironically, in 1900 Alessandro Padoa had objected to calling the formal axioms "postulates", as we see at the end of the following excerpt, where he sketches how axiomatic theories are built and how they are related to interpretations: "Indeed, during the period of *elaboration* of any deductive theory we choose the *ideas* to be represented by the undefined symbols and the *facts* to be stated by the unproved propositions; but, when we begin to *formulate* the theory, we can imagine that the undefined symbols are *completely devoid of meaning* and that the unproved propositions (instead of stating *facts*, that is, *relations* between the *ideas* represented by the undefined symbols) are simply *conditions* imposed upon the undefined symbols. Then, the *system* of *ideas* that we have initially chosen is simply *one interpretation* of the *system of undefined symbols*; but from the deductive point of view this interpretation can be ignored by the reader, who is free to replace it in his mind by *another interpretation* that satisfies the conditions stated by the *unproved propositions*. And since these propositions, from the deductive point of view, do not state *facts*, but *conditions*, we cannot consider them genuine *postulates*" ([25], 120–1; emphasis in the original).

[17]One difference, however, meets the eye, namely that Tarski's consequence relation involves sentences, and not open formulas (propositional functions).

This explicit appeal to logical deduction should not be taken as evidence that the consequence relation intended in axiomatics was that of provability in a calculus, in particular, it should not be taken as showing that the concept of consequence involved was what Tarski called "the concept of formalized consequence". To begin with, both the concept of formalized consequence and the one Tarski characterized with his semantic definition were meant to be rigorous versions of one and the same concept, namely the one involved in axiomatic considerations. Besides, it is not clear that the opposition between provability and some early version of model-theoretic consequence was a meaningful one at that time. A cogent piece of set-theoretical reasoning to the effect that a proposition holds in all models of the axioms would have counted as a proof.

Consider, for example, Hilbert's axioms for the real numbers. These include (i) the *axioms for ordered fields*, (ii) the *Archimedean axiom* (if $0 < a < b$, there is a positive integer n such that $b < na$) and (iii) the *axiom of completeness*, in the form that the ordered field of real numbers is maximal among Archimedean ordered fields, i.e., that no proper extension of the ordered real field satisfies all the preceding axioms. In Hilbert's words:

> *Axiom of Completeness*: It is not possible to add to the system of numbers another system of things so that the [preceding] axioms are also satisfied in the combined system; in short, the numbers form a system of things which is incapable of being extended while continuing to satisfy all the axioms ([13], 1094.)[18]

It is clear that something can be inferred from the completeness axiom only by means of set-theoretic considerations involving relations among Archimedean fields. Otherwise, how could one prove from the axioms that every non-empty set of real numbers bounded above has a least upper bound? Certainly not with considerations that an ordinary deductive calculus can support.

Nevertheless, there may be some early evidence of the distinction between provability and some form of model-theoretic consequence. After he defined what it is for a system of axioms to be categorical, and after he announced that his axiom system for geometry (with only *point* and *order* as specific terms) is categorical, Oswald Veblen wrote in 1904:

> Consequently any proposition which can be made in terms of points and order either is in contradiction with our axioms or is equally true of all classes that verify our axioms. The validity of any possible statement in these terms is therefore completely determined by the axioms; and so any further axiom would have to be considered redundant ([66], 346).

[18]This is analogous to Hilbert's axiom of completeness for Euclidean geometry, which says that the set of points, lines and planes cannot be extended without violating some of the preceding axioms.

And he appended in a footnote: "Even if not deducible from the axioms by a finite number of syllogisms".

Commenting on this passage, Awodey and Reck say that "what Veblen suggests here is that a potential new axiom might be a semantic consequence of the old axioms without being a deductive consequence of them, i.e., without being 'deducible in a finite number of syllogisms'. What that implies, of course, is that the notion of semantic consequence might not coincide with that of deductive consequence. This is a radically new suggestion." And they bring in a further excerpt by Veblen from 1906: "But if [a proposition] is a consequence of the axioms, can it be derived by a syllogistic process? Perhaps not"([2], 18–19). It is not clear, however, that such a conclusion is warranted, since Veblen's words could mean that "syllogistic processes" are merely a part of all available methods of proof. On the other hand, these two remarks by Veblen do seem to suggest that what matters for theoremhood is holding in all models of the axioms rather than being derivable from them by means of a definite list of chosen methods.

It is not easy to find a clear statement of what membership in an axiomatic theory meant beyond saying that theorems are provable form the axioms. The most frequent context in which models or interpretations were introduced was in showing that a set of axioms is independent. Independence of a set of axioms means that no axiom follows from the rest, and this is proven by exhibiting, for each particular axiom, an interpretation of the specific terms in which all axioms but the selected one are true. However, this method for proving independence can be used without requiring that consequence be understood as truth in all models of the axioms. In order to legitimize its use it is enough to accept that proofs from the axioms preserve truth in all interpretations of the specific terms, which is something weaker and reasonably obvious. This is precisely how Fraenkel, who understood theoremhood in terms of deducibility, saw the matter in the third 1928 edition of *Einleitung in die Mengenlehre*, where a long section is devoted to the axiomatic method:

> Such an interpretation ... shows the independence of the violated axiom from the rest. Because if this axiom could be deductively derived from the remaining ones, so would it be *automatically* satisfied by every interpretation that satisfied the others.[19]

In order to be sure that an author understands consequence in a model-theoretic sense we should look for some explicit statement to this effect. We find an early and apparently clear case of understanding logical consequence as truth in all interpretations that satisfy the axioms in Alessandro Padoa:[20]

[19]Eine solche Deutung ... zeigt die Unabhängigkeit des verletzten Axioms von den übrigen; denn ließe sich jenes aus den übrigen Axiomen deduktiv herleiten, so müßte bei jeder Deutung zugleich mit den übrigen Axiomen auch dieses letzte *von selbst* befriedigt werden ([9], 342; emphasis in the original).

[20]In this excerpt by Padoa, the *unproved propositions* are the axioms.

> We say that *the system of unproved propositions is irreducible* (or that these propositions are *absolutely independent*) when *it is not possible to deduce any unproved proposition from other such propositions* (and the logical propositions).
>
> Let us assume that we have established an *interpretation* of the system of undefined symbols that verifies the system of unproved propositions, except for *one* of these propositions. Then this proposition is not a logical consequence of the other propositions; that is, it is not possible to deduce the proposition in question from the other unproved propositions.
>
> Conversely, in order to deny the possibility of deducing the proposition in question from the other unproved propositions, we must show that it could be false even if all the others were true; and this we do by establishing an interpretation of the system of undefined symbols that verifies all the other unproved propositions.
>
> Consequently, *to prove that the system of unproved propositions is irreducible it is necessary and sufficient to find, for each of these propositions, an interpretation of the system of undefined symbols that verifies all the other unproved propositions but not that one* ([25], 122–3; emphasis in the original).[21]

From these assertions I infer that, for Padoa, given propositions A_1, A_2, $\dots$, A_n and B, involving a definite set of undefined symbols:

1. B is not a logical consequence of $A_1, A_2, \dots, A_n$ if and only if there is an interpretation of the undefined symbols that verifies $A_1, A_2, \dots, A_n$ but not B. Hence:
2. B is a logical consequence of $A_1, A_2, \dots, A_n$ if and only if every interpretation of the undefined symbols that verifies $A_1, A_2, \dots, A_n$ also verifies B.

Padoa read his paper at the 1900 Paris International Congress of Philosophy. At the same congress, Pieri defined logical consequence in the same setting as Padoa, that is to say, in stating the independence of postulates in formal axiomatics. Pieri demands that, as far as possible, the postulates should be "mutually independent, so that none of them be deducible from the rest", and he adds, in a footnote:

> Given several conditional propositions[22] $P(x, y, z, \dots)$, $Q(x, y, z, \dots)$, $R(x, y, z, \dots)$, etc., between the variable entities $x, y, z, \dots$,

[21] Padoa takes "is a logical consequence of" as synonymous with "is deducible from" (with the help of the logical propositions). This synonymy occurs in Peano (see, e.g., [29], 87). But not distinguishing deducibility from logical consequence cannot be taken as evidence that the members of the Peano school reduced one of them to the other. It is more likely that they conflated them.

[22] In the terminology of the Peano school, a conditional proposition is an open formula, i.e., a formula containing free variables, thus a propositional or, for Tarski, a sentential function.

> no doubt can arise as to the meaning of the assertions: "R is not deducible from P and Q", "R is not a consequence of P and Q". These two sentences express no more than the following particular proposition: "there exist some $x, y, z, \ldots$ for which P and Q are true, but R is not true". Hence, $P, Q, R, \ldots$ will be mutually *independent* if for each of them one can find some $x, y, z, \ldots$ which do not verify it, while they verify the rest.[23]

When this definition of independent propositions is applied to the postulates of an axiomatic theory, the "variable entities" $x, y, z, \ldots$ are simply the values that the specific terms can take in different interpretations. Thus, when applied to this particular case, Pieri—as Padoa, to whom he refers—is saying that a postulate is independent of the rest just in case there is an interpretation of the specific terms under which the postulate in question is false, while the others are true. That the "variable entities" can be so understood is implied by what Pieri says immediately after the last quote:

> Hence, in order to establish the absolute mutual independence of a given system of n postulates it will be necessary to produce n interpretations of the primitive concepts, each one on them verifying all but one of these n principles.[24]

Accordingly, Pieri, as Padoa, understands logical consequence of the axioms as truth in all interpretations of the specific terms that satisfy the axioms.[25]

If a fully explicit piece of evidence for this is needed, we can find it in Pieri's [31], written in 1898, two years before the preceding essay, which also contains in the appendix the Italian original of the French excerpt just quoted. Says Pieri (and keep in mind that the entities he refers to can be those meant by the specific terms of an axiomatic theory):

[23]Étant données plusieurs propositions conditionnelles $P(x, y, z, \ldots)$, $Q(x, y, z, \ldots)$, $R(x, y, z, \ldots)$, etc., entre les êtres variables $x, y, z, \ldots$, on ne peut avoir aucun doute sur la valeur des assertions: "de P et Q, on ne peut déduire R", "R n'est pas conséquence de P et Q". Ces deux phrases n'expriment pas autre chose que cette proposition particulière: "il éxiste des $x, y, z, \ldots$ pour lesquels P et Q sont vraies, mais R n'est pas vraie". Donc, P, Q, R, ... seront *indépendantes* les unes des autres, si pour chacune on peut trouver des $x, y, z, \ldots$ qui ne la vérifient pas, tandis qu'ils vérifient les autres. ([32], 380; [35], 248.)

[24]Par suite, pour établir l'indépendance mutuelle absolue d'un système donnée de n postulats, il faudra produire n interprétations des concepts primitifs, dont chacune laisse non vérifié un seul de ces n principes ([32], 380; [35], 248.)

[25]Here is another excerpt from Pieri written in 1906: "What could the judgment 'B and C do not imply A' mean but: 'there are some entities that verify B and C without verifying A'? Thus, the existence of such entities ... is not only a sufficient condition, but also a necessary one, for asserting the independence of A from B and C." (De même, que pourra signifier le jugement "B et C n'impliquent pas A", sinon: "il y a des entités qui vérifient B et C sans vérifier A"? De sorte que l'existence de telles entités (si individuelles ou spéciales qu'elles soient) est une condition non seulement suffisante, mais encore nécessaire, pour affirmer l'indépendance de A par rapport à B et C.) ([33], 199; [35], 380).

> It will not be superfluous to mention, once and for all, that for the judgment "$Q(a, b, c, \dots)$ is deducible from $P(a, b, c, \dots)$"—where P and Q are propositions involving the entities represented by the letters $a, b, c, \dots,$ which vary arbitrarily, that is, which have no more meaning than what P assigns them—one must understand the same as: "whatever $a, b, c, \dots$ are, if $P(a, b, c, \dots)$ is true of them, so will also be $Q(a, b, c, \dots)$; whoever asserts P of these entities, cannot deny Q of them".[26]

Twenty-eight years later, we find Fraenkel contrasting deducibility with model-theoretic consequence. Mathematical logic had undergone a deep and extensive development since 1900. In particular, the distinction between informal proof and deducibility in a calculus was much more clear, and one could no longer speak of deducibility from the axioms and of truth in all interpretations of the axioms as if they meant the same thing. As a matter of fact, deducibility was a well-understood concept, but truth in all interpretations was not.

As I have implied above, in his 1928 discussion of the axiomatic method, Fraenkel takes an axiomatic system to be given by the axioms and a definite list of inference rules, so that the members of the theory (its theorems) are the sentences derivable from the axioms by means of the chosen rules. Only by fixing the inference rules will the theory be sufficiently determinate ([9], 339). Nevertheless, he seems to allow that deducibility may fail to secure a stronger form of consequence. This becomes apparent when he compares the three senses in which an axiom system may be said to be complete. The first and the third senses are clear enough: An axiom system is complete in the first sense if every sentence in the language of the theory is derivable from the axioms whenever its negation is not ([9], 347); it is complete in the third sense if it is categorical, i.e., if all its models are pairwise isomorphic ([9], 349).

But there is another sense in which an axiom system can be complete, namely if for every sentence in the language of the theory, either it or its negation, but not both, is compatible [vereinbar] with the axioms ([9], 348).[27]

[26]Non sarà superfluo avvertire, una volta per sempre, che il giudizio "da $P(a, b, c, \dots)$ si deduce $Q(a, b, c, \dots)$" — dove P e Q siano proposizioni negli enti rappresentati dalle lettere $a, b, c, \dots$ mutabili ad arbitrio, cioè non aventi significato indipendente da quello che è loro assegnato in P — è da ritenersi il medesimo che: "qualunque siano $a, b, c, \dots$ se per essi è vera $P(a, b, c, \dots)$ sarà altresì vera $Q(a, b, c, \dots)$; chi di questi enti asserisce P, non può negar Q pei medesimi" ([31], 9; [35], 109).

[27]"Let $\mathfrak{A}$ be a statement relevant to the axiom system; regardless of whether one may succeed in deducing from the system the correctness or the falsity of $\mathfrak{A}$, or at least to establish this deducibility only theoretically, it must happen that *either* the correctness *or* the falsity of $\mathfrak{A}$—but not both of these possibilities—is compatible with the axiom system, if this is to be regarded as complete." ($\mathfrak{A}$ sei eine in das Axiomensystem einschlägige Aussage; gleichviel ob es gelingen mag, die Richtigkeit oder Falschheit von $\mathfrak{A}$ aus dem System zu deduzieren oder eine solche Deduzierbarkeit auch nur theoretisch sicherzustellen, so soll jedenfalls nur *entweder*

A system incomplete in this second sense, says Fraenkel,

> will leave it open—not simply in the sense of deducibility with the present or future tools of mathematics, but in an absolute sense (describable by way of independence proofs)—the question whether certain relevant questions must be answered thus or thus.[28]

This reference to independence proofs, which he had described before in terms of models ([9], 340–343), and the term "absolute" applied to this sense of completeness, strongly suggest that Fraenkel has in mind a sharper notion of consequence, namely the model-theoretic one. Why then, does he not spell out this notion of consequence and use it to define theoremhood in an axiomatic theory—an "absolute" form of theoremhood, not one relative to "the present or future tools of mathematics"? One reason may have to do with the difficulty of accounting for the totality of all models, which is invoked in the definition of model-theoretic consequence. Another reason might be Fraenkel's inability to express in a rigorous and general way that a statement holds ("is correct") in a model, and hence his inability to give a mathematically acceptable definition of model-theoretic consequence.

§**8. The semantic obstacle.** As I have already remarked, stepping into formal axiomatics made it almost inevitable to understand theoremhood in an axiomatic theory as truth in all interpretations of the specific terms that satisfy the axioms. As the early excerpts from Padoa and Pieri show, this is how theoremhood was actually understood since the beginning. This is not to say that we find in these authors an early version of Tarski's semantic definition of logical consequence. The informal wording of their definitions and explanations was semantic in intent, but the mathematical formulation was not. It could not be semantic because it was not even metalinguistic. Let a, b, c be the specific terms (thus variables) of an axiomatic theory and suppose that $P(a,b,c)$ is the conjunction of its axioms. Pieri tells us that a consequence of $P(a,b,c)$, thus a member of the theory, is any formula $Q(a,b,c)$ such that whatever objects a, b and c stand for, if $P(a,b,c)$ is true of them, so is $Q(a,b,c)$. This looks like a semantic assertion, involving

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die Richtigkeit *oder* die Falschheit von $\mathfrak{A}$—nicht aber jede dieser beiden Möglichkeiten—mit dem Axiomensystem vereinbar sein, wenn dieses als "vollständig" gelten soll ([9], 348).) As an example of an incomplete axiom system in this second sense, he mentions the axioms of Euclidean geometry with the parallel axiom removed. Since both it and its negation are compatible with this system, the system is incomplete. As an example of a complete system, he offers Peano Arithmetic. It is complete in this third sense, even allowing that it is not in the first sense, because, as he explains in a footnote, it is categorical.

[28] Ein solches Axiomensystem läßt nicht bloß im Sinn der Deduzierbarkeit mit den gegenwärtigen oder künftigen Hilfsmitteln der Mathematik, sondern in einem absoluten Sinn (darstellbar durch Unabhängigkeitsbeweise) die Frage offen, ob gewisse einschlägige Fragen so oder so zu beantworten sind ([9], 348).

formulas of a language and entities of which these formulas are true or false. Nevertheless, we come to doubt the justice of this attribution when we see him formalizing that $Q(a, b, c)$ is a consequence of $P(a, b, c)$ as:

$$P(a, b, c) \supset Q(a, b, c)$$

—that is, $\forall abc\ (P(a, b, c) \rightarrow Q(a, b, c))$—which is a formula of the language in which the theory is framed and has no metalinguistic component at all.[29,30]

In spite of lacking an adequate definition—mainly because what they lacked was the means to define it—, the relation intended between the axioms of a theory and the members of the theory (i.e., its theorems) is clearly enough conveyed by Padoa and Pieri, and it is the one that Tarski at last succeeded in defining. That is why Tarski says that his "treatment of the concept of consequence makes no very high claim to complete originality [and that the] ideas involved in this treatment will certainly seem to be something well known, or even something of its own, to many a logician who has given close attention to the concept of consequence"(p. 414). But one thing is to recognize that Tarski's treatment, once communicated to us, corresponds to our own ideas, and quite another thing is to have the means to develop these ideas into a rigorous definition. This Tarski justly emphasized when he added that "only the methods which have been developed in recent years for the establishment of scientific semantics, and the concepts defined with their aid, allow us to present these ideas in an exact form" (p. 414).

In Pieri and in Padoa, as in Peano, to whose school they belong, there seems to have been no tension in identifying provability from the axioms with truth in all interpretations satisfying them—the reason being that these two notions were not really differentiated. And there was no tension either in speaking in the vernacular about interpretations of a formal language and believing that they succeed in expressing what they mean with a formula of that very language. Later, with the progressive development of deductive systems and the advent of metamathematics, the distinction between assertions (or formulas) of a language and assertions about properties of and relations between formulas of the language became apparent. The consequence relation being, like deducibility, metalinguistic, and deducibility admitting of a

[29]Se $P(x, y, z, \ldots)$, $Q(x, y, z, \ldots)$ sono proposizioni negli enti variabili $x, y, z, \ldots$, la scrittura "$P(x, y, z, \ldots) \supset_{x,y} Q(x, y, z, \ldots)$" significa: "qualunque siano x, y, purchè soddisfacenti a $P(x, y, z, \ldots)$, dovranno altresì verificare la $Q(x, y, z, \ldots)$"; e può leggersi: da P si deduce, rispetto ad x, y, Q". Mancando ogn'indice al segno $\supset$, la deduzione s'intende estesa a tutte quante le lettere $z, y, z, \ldots$ ([31], 44–45; [35], 144–145, note.)

[30]As pointed out above when discussing Padoa's definition of consequence, this way, internal to the language, of formalizing consequence is due to Peano, and was adopted by the members of his school and by Russell (whose converse he called "formal implication"). Incidently, Peano allowed the use of the symbol $\supset$ only between formulas containing free variables. As he explicitly said in 1897, in this he disagrees with Frege, who admits (while Peano rejects) a formula such as "$2^2 = 4 \supset 3 + 7 = 10$"([27], 571; [28], 207).

sufficiently rigorous mathematical definition, it is not surprising that whenever one wanted to be precise about logical consequence, one described it as deducibility. These two concepts came to be identified, but not as Peano had conflated them before, but mainly because the one (deducibility) was seen as the rigorous formulation of the other (logical consequence).

Strictly speaking, they were not identified. Rather, deducibility in a calculus, i.e., what Tarski called the "formalized" concept of consequence, was viewed as the truly mathematical form of logical consequence, because the semantic aspect of the intended relation could not be given a rigorous treatment. Of course, one spoke of models and of a formula holding, or being true, in a model, but the way to express this mathematically was to say that *substituting* the entities of the model for the variables of the formula yielded a true (or a "correct") sentence. In the first, 1928 edition of their *Principles of Mathematical Logic*, Hilbert and Ackermann pose the question of the completeness of their calculus of first-order logic with these words: "Whether the axiom system is complete at least in the sense that all logical formulas that are correct in every domain of individuals can actually be derived is still an unsolved question".[31] How they would express that a formula is correct in a domain of individuals can be inferred from the following excerpt:

> The problem of *universal validity* is the following question: *How can we decide for any arbitrary logical expression containing no individual signs whether the expression states a correct assertion for arbitrary substitutions for the variables?*
>
> The problem of satisfiability is the question, *whether there is any substitution for the variables such that a correct assertion is expressed by the resulting expression.*[32]

Even Gödel, in his doctoral dissertation, describes satisfaction of a formula in terms of substitution, when he explains in passing the meaning of "a system of relations *satisfies* a logical expression" as "the sentence obtained through substitution is true" ([10], 65). Two pages later he is more explicit. After describing the formal system whose completeness he will prove, he writes:

> What the notions thus far introduced have in common is that, in the relation they bear to signs, these enter purely as figures in space. In contrast, there are those notions that depend upon the *meaning*

[31] Ob das Axiomensystem wenigstens in dem Sinne vollständig ist, daß wirklich alle logischen Formeln, die für jeden Individuenbereich richtig sind, daraus abgeleitet werden können, ist eine noch ungelöste Frage ([14], 68).

[32] Bei dem Problem der *Allgemeingültigkeit* handelt es sich um die folgende Frage: *Wie kann man bei einem beliebigen vorgelegten logischen Ausdruck, der keine individuellen Zeichen enthält, feststellen, ob der Ausdruck bei beliebigen Einsetzungen für die vorkommenden Variabeln eine richtige Behauptung darstellt oder nicht?* Bei dem Problem der *Erfüllbarkeit* handelt es sich um die Frage, *ob es überhaupt eine Einsetzung für die Variabeln gibt, so daß durch den betreffenden Ausdruck eine richtige Behauptung dargestellt wird* ([14], 72–73; emphasis in the original).

> of the formulas. Let A be any logical expression that contains the functional variables $F_1, F_2, \ldots, F_k$, the free individual variables $x_1, x_2, \ldots, x_l$, the propositional variables $X_1, X_2, \ldots, X_m$, and, otherwise, only bound variables. Let S be the system of functions $f_1, f_2, \ldots, f_k$ (all defined in the same universal domain), and of individuals (belonging to the same domain), $a_1, a_2, \ldots, a_l$, as well as propositional constants, $A_1, A_2, \ldots, A_m$. We say that the system, namely
>
> $$(f_1, f_2, \ldots, f_k; a_1, a_2, \ldots, a_l; A_1, A_2, \ldots, A_m),$$
>
> *satisfies* the logical expression if it yields a proposition that is true (in the domain in question) when it is substituted in the expression ([10], 67, 69; emphasis in the original).

§9. On the adequacy of Tarski's definition. From the data brought in so far we can conclude that (1) the intended notion of consequence at play in axiomatics since the end of the 19th century is precisely the one that Tarski's definition succeeded in characterizing, and (2) a crucial obstacle in the path to its characterization was the lack of a rigorous mathematical treatment of semantics (perhaps because it was thought that such a task was impossible). In the next section I will argue that the axiomatic concept of consequence is indeed one with which Tarski was thoroughly familiar, which lends support to the idea that he thought of it as the common one. But now I want to show how the assumption that Tarski's aim was in fact to characterize this notion of consequence (i.e., that this particular notion was his *common* concept of consequence) allows us to understand some important details of Tarski's paper about which commentators have been silent.

In the first place, the fact that Tarski does not try to be explicit about what the common concept is becomes fully understandable, since what he calls the "common" concept of consequence was indeed the common one, that is, the one that was common where the consequence relation is most apparent, namely in axiomatics.[33]

One important point that becomes clear under the assumption that Tarski's common concept of consequence is the one at play in axiomatic theories is Tarski's taking logical consequence to be formal consequence. More than that, under this assumption we understand as well why he identifies logical consequence with formal consequence as a matter of course, without attempting to justify it at all. This absence of any attempt at justification of such a fundamental point in Tarski's account should be explained, and

[33]To see how essential logical consequence is to axiomatics, we should keep in mind that a set of axioms determines a theory only in so far as the consequence relation involved is itself determinate. This affords a ground for preferring deducibility in a calculus to model-theoretic consequence whenever (as happened before Tarski—and still happens in higher-order logic) the determinacy of the latter is in doubt or depends on strong hypotheses.

Tarski's commentators do not explain it. Some of them, as we have seen, attribute to Tarski a philosophical analysis of some unidentified concept of consequence according to which logical consequence is formal consequence. But there is no hint in Tarski's paper, and no other evidence is brought out, of such a philosophical analysis (either performed by Tarski himself, or by some other author to whom Tarski refers).

As Tarski understands it, the formality of the consequence relation amounts to its being impervious to the particular identity of the entities to which the extra-logical constants refer ("this relation cannot be influenced in any way ... by knowledge of the objects to which the sentence X or the sentences of the class K refer" (pp. 414–415)), and he makes sure that his defined concept is formal in this sense by allowing the extra-logical constants (via its being replaced with variables) to be arbitrarily reinterpreted. Now, as we have seen, this sort of formality is the central aspect of formal axiomatics, where the specific terms of the theory are taken to be mere placeholders for any entities whatever that satisfy the axioms. Thus, if we assume that Tarski's common concept is the concept of consequence involved in formal axiomatics, then we not only see why Tarski takes it to be formal, but we also understand that justifying that formality belongs to logical consequence is not something Tarski should be expected to do.

Another point on which our claim regarding Tarski's common concept sheds light is Tarski's way of justifying the adequacy of his semantic definition. He says: "It seems to me that everyone who understands the content of the above definition must admit that it agrees quite well with common usage" (p. 417). Under our assumption, these words can be taken at face value, for whoever understands the notions involved in Tarski's definition (and is versed in axiomatics, as contemporary logicians certainly were) recognizes that the definition yields a precise, rigorous version of the consequence relation at play in axiomatics. As a matter of fact, if we contented ourselves with the wording of Tarski's definition ("X follows logically from the sentences of the class K if and only if every model of the class K is also a model of the sentence X" (p. 417)), we could say that his definition was already given more than thirty-five years before by Padoa and Pieri. Of course, as we have already seen, it was not, because they did not have the semantic apparatus that is required to carry it out. This notwithstanding, what they meant to define is what Tarski succeeded in defining. (It may not be idle to recall at this juncture that after giving his definition of model Tarski adds in parentheses: "in just this sense one speaks of models of an axiom system of a deductive theory" (p. 417).)

There is one more aspect of Tarski's paper which commentators do not touch on and which is seen in a clear light under the assumption that the common concept of consequence is the one involved in axiomatics. As Tarski explains in the last fourth paragraphs of his paper (pp. 418–420), his definition is incomplete, since it rests on a distinction between logical and extra-logical terms which he has not given. And not only Tarski has not given it, but he even doubts that there is a sharp distinction to be found. If we keep in mind that (1) what Tarski aims to be defining is *the* relation of logical consequence, and (2) where the boundary is drawn between logical and extra-logical terms affects the extension of the relation defined (as he explicitly observes in his remark on material consequence (p. 419)), then we should find it surprising that Tarski stands by his incomplete characterization—and that, in spite of its incompleteness, it strikes many commentators as right. For recall that Tarski objected to the formalized concept of consequence because it did not coincide in extent with the common one. Why does he not acknowledge that his definition, given its incomplete character, does not coincide with the common concept either? His choice of an instance of consequence that is not accounted for by the formalized concept was a particular ω-argument. How does he know that this particular argument will turn out to be correct under his definition of consequence, if its correctness should depend on a distinction between logical and extra-logical terms that he has not given. Moreover, the terms that have to be taken as logical in order for that argument to be correct under his definition of logical consequence are not among the uncontroversial ones that belong to propositional or to first-order logic. So, we must admit that he has not shown us that his definition fares better, as regards this particular argument (which he took as a touchstone of consequence), than the formalized concept that he dismissed.

I find it surprising that commentators, especially those that defend Tarski's account, do not discuss this sort of question. For if Tarski's aim had been to define *the* true relation of logical consequence, then we should declare that he did not succeed, that at most he gave us something like a schema with some slots to be filled in order to have the right definition.

All these considerations are out of place if Tarski's aim was, as I claim, to define the concept of consequence at work in axiomatic considerations. For the crucial distinction relating to the primitive terms of an axiomatic theory is not between logical and extra-logical in some absolute sense, but between specific and non-specific. And where does the difference between them lie according to pre-Tarskian axiomatics? In contentual axiomatics, the specific terms are those standing for the entities specific to the discipline the theory is about. This may not induce a sharp distinction in some cases, as there may be some doubt as to whether a particular entity belongs to the discipline being axiomatized or not. But in formal axiomatics there is no doubt as to what the distinction is. Specific terms are those which are left uninterpreted, which are mere placeholders, which function as variables—while the remaining primitive terms are taken as fully understood. The only implicit requirement about the boundary of the distinction is that some particular terms—as sentential connectives and first-order quantifiers—be non-specific (at least in so far as we are not axiomatizing some fragment of logic). But it is worth insisting that among the non-specific terms one can find terms for concepts which one would not want to regard as logical in an absolute sense. In fact, some terms which occur in one axiomatic theory as non-specific can be among the specific terms of another axiomatic theory. It is not hard to find examples of this phenomenon. Thus, we may axiomatize Euclidean geometry by making use of the order of the real numbers, in which case the set of the real numbers and its order relation will be referred by non-specific, fully meaningful terms. However, if we want to axiomatize the order relation of the real numbers, these terms will be taken as specific, thus left uninterpreted. Another example is afforded by the standard axiomatization of metric spaces, where the set of the real numbers, its order relation, and the operation of addition are taken as non-specific entities, but the terms standing for them are taken as specific when axiomatizing the real field.

Tarski's definition of consequence applies smoothly to these and other situations, and in each case it yields the correct results by treating as logical the primitive non-specific terms and as non-logical the specific ones, without inquiring about transcendent marks of logicality. It is because it deals with the logical/extra-logical distinction in this immanent, non-committal way that Tarski can ensure without checking each particular case that his definition agrees with common (that is to say, axiomatic) usage. In particular, he can ensure that his definition will certify as correct those ω-arguments that are deemed correct by ordinary mathematical considerations. This only requires that the logical (i.e., non-specific) ingredients of the language be sufficient to mimic Dedekind's definition of the set of natural numbers in terms of zero and the successor operation. Second-order quantification or some weak applied set theory is enough for this.[34]

I have argued that Tarski's definition is faithful to the concept of consequence as found in formal axiomatics. There is, however, an apparent mismatch that we should not ignore, namely that in formal axiomatics the specific terms are uninterpreted, while Tarski deals with fully interpreted languages.

[34]If we have an individual constant c (for the number zero) and a unary function symbol f (for the successor operation), we can define in second-order logic the predicate N (for the natural numbers) as:

$$\forall x\,\left[Nx \leftrightarrow \forall X\,(Xc \wedge \forall y\,(Xy \to Xfy) \to Xx)\right].$$

Then for any unary predicate P, the formula $\forall x\,(Nx \to Px)$ (expressing that every natural number has property P) is a second-order consequence of the set consisting of the infinitely many sentences

$$Pc, Pfc, Pffc, Pfffc, \ldots$$

(expressing of each natural number that it has property P). To see that this is so, we have, as Tarski requires (p. 415), to eliminate the defined sign N from $\forall x\,(Nx \to Px)$.

In an axiomatic setting, using a fully interpreted language means that we are dealing with contentual, not formal, axiomatics. Nonetheless, the consequence relation at work in contentual axiomatics agrees with the one in formal axiomatics. If we are willing to view the specific terms of an axiomatic theory ambiguously (as fully meaningful or as mere placeholders, according to the context), we can state the relation of contentual to formal axiomatics by saying that a sentence belongs to the contentual version of an axiomatic theory if and only if it belongs to its formal version.

This intimate relation between the two conceptions of an axiomatic theory is the rationale for formal axiomatics. As we saw, passing to the formal version of a contentual axiomatic theory is the precise way to implement the decision to make the consequences of a set of axioms (in the contentual sense) independent of any features of the specific entities that are not explicitly stated in the axioms—it is a rigorous way to get rid of the imprecision of requiring that something be "explicitly stated".

Obvious as this may be, some reference prior to Tarski's paper would be helpful. In his discussion of the axiomatic method in [9], Fraenkel introduces the two versions (contentual and formal) of an axiomatic theory by saying that an axiom system "can systematically be conceived in two different ways that in practice are seldom kept strictly apart (and need not be)".[35] After describing the two versions, he says:

> The two described conceptions have in common the formal character that belongs to every axiomatic theory in the following sense: Since all the proven sentences of the theory are deductive consequences of the axioms, the theorems of the theory hold unchanged *for whatever conceivable interpretation of the basic concepts which is compatible with the axioms*—independently of whether originally, i.e., when laying down the axioms, these were thought to be provided with meaningful content or only formally defined by the axioms.[36]

Thus, even if Tarski deals with fully interpreted languages, his semantical definition is faithful to the consequence relation as found in formal axiomatics. Indeed, the substitution in Tarski's definition of suitable variables for the extra-logical terms corresponds to the transition from the contentual to the

[35] daß eine Axiomatik in der Regel auf zwei verschiedene Arten aufgefaßt werden kann, die in praxi freilich selten scharf auseinandergehalten werden (und es auch nicht brauchen) ([9], 337.)

[36] Den beiden oben geschilderten Auffassungen gemeinsam ist aber der formale Character einer jeden axiomatischen Theorie in folgendem Sinne: Da alle in der Theorie bewiesenen Aussagen deduktive Folgerungen aus den Axiomen sind, so gelten die Sätze der Theorie unverändert *für jede wie immer denkbare, mit den Axiomen verträgliche Deutung der Grundbegriffe*—gleichviel, ob diese ursprünglich, nämlich bei Aufstellung der Axiome, mit einer inhaltlichen Bedeutung umkleidet oder nur formal durch die Axiome definiert gedacht wurden ([9], 338; emphasis in the original).

formal version of an axiomatic theory. It is, as in Tarski's procedure, with the help of this transition that the consequence relation of the contentual theory was actually explained.

§**10. Tarski's involvement in axiomatics.** One last issue remains to be considered. I have argued that the concept of consequence involved in axiomatics was the common one at the time Tarski gave his definition, and this I did by quoting and discussing the views of authors belonging to various traditions (members of the Peano school, American postulate theorists[37], Hilbert, and Fraenkel). I have argued as well that Tarski's semantic concept of logical consequence agrees quite well with the axiomatic one. However, I have not explicitly justified that this view of consequence is one which Tarski was familiar with and could think of as common. This I do now by briefly substantiating that by 1936 Tarski was well acquainted with the axiomatic traditions under consideration, and that he had made many contributions to axiomatics.

One piece of evidence has to do with Tarski's deep interest in the foundations of geometry, on which he lectured in Warsaw in 1926–1927. A concise account of the contents of Tarski's course and of his ensuing work in the area is given in the first paragraph of Tarski and Givant's [62]. From it we see how focused it was on axiomatic issues:

> In his 1926–1927 lectures at the University of Warsaw, Alfred Tarski 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 than can be developed within the framework of first-order logic. He proved, around 1930, that his system of geometry admits elimination of quantifiers: every formula is provably equivalent (on the basis of the axioms) to a Boolean combination of basic formulas. From this theorem he drew several fundamental corollaries. First, the theory is complete: every assertion is either provable or refutable. Second, the theory is decidable—there is a mathematical procedure for determining whether or not any given assertion is provable. Third, there is a constructive consistency proof for the theory ([62], 175).

The system of geometry that Tarski presented in this course, and which he kept perfecting for many years, was an improvement of the one in Pieri's [34]. All variables range over points, and there are only two primitive specific terms: the ternary relation of *betweennes* and the quaternary relation of *equidistance* ([42], 908). A very elegant system of plane geometry originating from this is Tarski's axiom set in [47].

[37]For a treatment of the American postulate theorists see Scanlan [38].

As explicit evidence that Tarski was well acquainted with the European and American axiomatic traditions, I can mention the various references and comments in Tarski's papers to authors belonging to these traditions, in particular, to Pieri, Padoa, Huntington, and Veblen, as well as to section 18 of Fraenkel's [9], where axiomatization of theories in a general setting is discussed. In [50], Tarski presents an axiomatization of three-dimensional Euclidean geometry, based on "the deductive theory founded by S. Leśniewski, and called by him *mereology*", and having only the "notion of a *sphere* ... as the only specific primitive notion" ([50], 24, 25–26), and compares his axiomatization to Huntington's in [18] and to Pieri's in [34]. Padoa's method for establishing the undefinability of a term by means of other terms in an axiomatic setting is discussed and justified in Tarski's [57]. In this paper, Tarski briefly refers to Veblen, alluding to this author's purported proof in [66] that equidistance is definable in terms of betweenness, which in fact it is not ([57], 307).[38]

Veblen is also mentioned in this and in other papers where categoricity of an axiom system is discussed, and Tarski credits him for the introduction of this concept ([58], 282). Categoricity plays an important role in [57], as well as in [63], coauthored with Adolf Lindenbaum.[39] As to Huntington, Tarski refers to him in [56] as the author of various axiom systems for boolean algebras ([56], 324), while in Tarski's essay on the concept of truth, the axiom system chosen for the calculus of classes (which is the theory for whose language he defines truth) is a simplification of one given by Huntington in [16] ([48], 179). Finally, Fraenkel's section 18 of [9], where axiomatization in a general setting is carefully discussed, is repeatedly referred to and commented on in various papers by Tarski, namely in [51], [48], [63].

For further evidence of Tarski's intimate knowledge of the American axiomatic tradition I refer to Michael Scanlan's [39]. Besides relating Huntington's and Veblen's work to Tarski, Scanlan argues at length that postulate theorist C. H. Langford "had an influence *of a certain sort* at a critical juncture" on Tarski ([39], 315). According to Scanlan, Langford influenced Tarski at least in that discussion of Langford's papers [21] and [22] in Tarski's Warsaw seminar in 1927–1928 contributed to develop Tarski's urgency of the

[38] See Scanlan ([39], 311) for an explanation of Tarski's reasons.

[39] It is worth remarking that here they distinguish between two forms of categoricity, which they call "intrinsic" and "absolute". A set of axioms is intrinsically categorical if any two of its models are isomorphic, while it is absolutely categorical if for any two models M and N there is a one-to-one map of "the class of all individuals into itself" whose restriction to the universe of M is an isomorphism between M and N ([63], 310 note 2, and 314 note 1). This distinction may be relevant to the ongoing discussion (which I have deliberately avoided in this paper) whether the universe of Tarski's models in his definition of logical consequence may be allowed to vary or not. For the discussion in question see Bays [3], Gómez-Torrente [11], and Mancosu [24].

need to give strictly mathematical definitions of semantic notions.[40] Other American postulate theorists, as H. M. Sheffer and C. H. Moore, are also referred to by Tarski in relation to axiomatic matters, in particular regarding the independence of sets of axioms ([54], 36; [49], 362).

I should also point out that Tarski was well acquainted with Carnap's work,[41] and Carnap had been deeply involved in the characterization and properties of axiomatic theories. As Awodey and Carus explain, in 1930 Carnap had abandoned his then current approach to general axiomatics (a sketch of which can be found in his [4]) mainly under the influence of Tarski.[42] Moreover, not only was Tarski fully aware of the trends of research in axiomatics, but he had made important contributions to it himself (one of them being, as I hope to have made clear, his definition of logical consequence). Tarski's research in metamathematics had been mainly about axiomatic theories. On the one had, as in the already mentioned case of Euclidean geometry, he had been concerned with the axiomatization of particular theories with a view to studying their metamathematical properties, witness [50], [58] and [56]. On the other hand, he had dealt with general questions related to axiomatization, as in [54], [51], [57], [49] and [63].[43] Tarski's "deductive sciences" are axiomatic theories, and about them he also wrote in the wake of his definition of logical consequence. I am referring to the paper "Sur la méthode déductive" [46], and to his *Introduction to Logic and to the Methodology of Deductive Sciences*, whose first Polish edition was published in 1936. As Saunders MacLane wrote in his review of the first 1937 German translation [45] of the Polish original, this book is an "introduction to mathematical logic and to *axiomatic methods*" ([23], 51; emphasis added).

§11. On the modality of logical consequence. We saw that, among Tarski's commentators, Sher and Etchemendy insist that an essential trait of any relation worth the name of "logical consequence" is its modal character: if a sentence α is a logical consequence of a set of sentences Σ, it is *impossible* that all the sentences in Σ be true and α false. According to Sher, Tarski's

[40]According to Vaught, "Langford established the decidability of the theory of dense orders and the decidability and completeness of the theory of discrete orders having a first element and no last ([65], 870). However, as Scanlan remarks, since "Langford does a poor job of organising and expressing his results and proofs, ... [r]eading Langford's paper reveals some ambiguity about what it is exactly that Langford is proving." Moreover, it "is unclear whether Langford actually thought of his paper as providing a decision procedure for the theories treated"([39], 315, 316).

[41]Carnap is the only author whose views on logical consequence Tarski in [55] explicitly discusses and compares to his own.

[42]See the fifth section, "Historical Coda", of [1].

[43]In the English edition, published in 1937, of Carnap's *The Logical Syntax of Language*, Carnap includes Tarski's [51] and [58] in his references of new works "on the subject of axiomatics" ([5], 275).

relation of consequence has this feature, while Etchemendy protests that it lacks it, thereby rendering it inadequate.

The view that necessity belongs to logical consequence has been widely maintained, although its import varies according to one's demands on necessity. My goal regarding this issue is very limited: first to discern what kind of necessity, if any, is involved in the common concept of consequence, and then to check whether Tarski's definition accounts for it.

The question about the modality of the concept of consequence at work in axiomatic theories is only sensible for contentual axiomatics. This is so because, in all discussions on the matter, the items involved in the consequence relation are taken to be fully meaningful—thus sentences (or perhaps propositions), but not mere open formulas (or propositional functions). These items have to be true or false, not merely true for some values of the variables they contain.

Consider a set Σ of true axioms about certain specific entities. A true sentence α about these entities is a consequence of Σ not because it is true, but because its truth depends only on the way the entities are mutually related according to the axioms. It is in order to guarantee this that (1) we replace the specific terms with variables, and (2) we require that the open formula α' that α becomes after this replacement be a consequence (in the formal axiomatic sense) of the set of open formulas Σ' arising from the sentences in Σ. This happens whenever every model of Σ' is also a model of α'.

Is any kind of necessity involved here? Not, apparently, in any strong sense. At most, we find that weak form of necessity which befits generality, and which we can convey roughly thus: α follows form the axioms because (1) the axioms say of the specific entities that they stand in some complex relation R, (2) α says of these same entities that they stand in some relation S, and (3) whenever any entities stand in the relation R they also stand in the relation S. Thus, since the axioms are true (i.e., since the specific entities stand in the relation R), α *must* be true as well (i.e., the specific entities *must* stand in the relation S). This is the same weak sort of necessity which is meant when we say that $\sqrt{2}$ *must* be irrational because only those integers have a rational square root that are perfect squares, and 2 is not a perfect square.

It is obvious that Tarski's semantic concept of logical consequence shares this weak form of necessity, which is a by-product of formality. Moreover, there is no clear hint in Tarski's paper that he aims at a stronger form. Sher, who thinks that he actually does, feels compelled to attribute to Tarski a doubtful assumption. Tarski, says Sher, mentions the existence of a proof that logical consequence is necessary, but he does not disclose it. That's why Sher supplies an argument that, she maintains, "is in the spirit of Tarski": If the sentence X is not a necessary consequence of the set of sentences K, then "it is possible that all the members of K are true and X is false. But in that case there is a model in which all the members of K come out true and X comes out false." Thus, X is not a logical consequence of K according to Tarski's definition ([40], 41). This argument, as she observes, depends on the "crucial assumption" that "every possible state of affairs relative to the expressive power of [the formal language in question] be represented by some model" ([40], 42).

We may wonder if Tarski actually asserted that logical consequence is necessary in this obscure sense, and if he claimed that he had a proof that it is. The evidence Sher adduces is this excerpt from Tarski's paper:

> [i]t can be proved, on the basis of this definition, that every consequence of true sentences must be true (p. 417).

We should allow, however, that it is far from clear that "must" in this sentence expresses the sort of necessity Sher ascribes to Tarski. A careful (and, to my mind, compelling) defence that this and other occurrences of "must" in Tarski's paper only signal generality can be found in Gómez-Torrente's "On a fallacy attributed to Tarski" [12].

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