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FREGE AND SEMANTICS

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Summary
In recent work on Frege, one of the most salient issues has been whether he was prepared to make serious use of semantical notions such as reference and truth. I argue here Frege did make very serious use of semantical concepts. I argue, first, that Frege had reason to be interested in the question how the axioms and rules of his formal theory might be justified and, second, that he explicitly commits himself to offering a justification that appeals to the notion of reference. I then discuss the justifications Frege offered, focusing on his discussion of inferences involving free variables, in section 17 of *Grundgesetze*, and his argument, in sections 29–32, that every well-formed expression of his formal language has a unique reference.

1. *Frege and the justification of logical laws*

In recent work on Frege, one of the most salient issues has been whether he was prepared to make serious use of semantical notions such as reference and truth. Those not familiar with this debate are often surprised to hear of it. Surely, they say, Frege's post-1891 writings are replete with uses of 'true' and 'refers'. But no-one wants to deny that Frege makes use of such terms: Rather, what is at issue is how Frege understood them; more precisely, what is at issue is whether Frege employed them for anything like the purposes for which philosophers now employ them. What these purposes are (or should be) is of course itself a matter of philosophical dispute, and, although I shall discuss some aspects of this issue, I will not be addressing it directly. My purpose here, rather, is to argue that Frege did make very serious use of semantical concepts: In particular, he offered informal mathematical arguments, making use of semantical notions, for semantical claims. For example, he argues that all of the axioms of the Begriffsschrift—the formal system[1] in which he proves the basic laws of

1. Frege, like Tarski after him, does not clearly distinguish a formal *language* from a formal

arithmetic—are true, that its rules of inference are truth-preserving, and that every well-formed expression in Begriffsschrift has been assigned a reference by the stipulations he makes about the references of its primitive expressions.

Let me say at the outset that Frege was not Tarski and did not produce, as Tarski (1958) did, a formal semantic theory, a mathematical definition of truth. But that is not of any significance here. One does not have to provide a *formal* semantic theory to make serious use of semantical notions. At most, the question is whether Frege would have been prepared to offer such a theory, or whether he would have accepted the sort of theory Tarski provided (or some alternative), had he known of it. On the other hand, the issue is not whether Frege would have accepted Tarski's theory of truth, or Gödel's proof that first-order logic is complete, as a piece of mathematics;[2] it is whether he would have taken these results to have the kind of significance we (or at least some of us) would ascribe to them. Tarski argues in "The Concept of Truth in Formalized Languages" that all axioms of the calculus of classes are true; the completeness theorem shows that every valid first-order schema is provable in certain formal systems. The question is whether Frege could have accepted Tarski's characterization of truth, or Gödel's characterization of validity, or some alternative, *as* a characterization of truth or validity.

The issue is sometimes framed as concerning whether Frege was interested in justifying the laws of logic. But it is unclear what it would be to 'justify' the laws of logic. On the one hand, the question might be whether Frege would have accepted a proof of the soundness of first-order logic as showing that every formula provable in a certain formal system is valid. Understood in this way, the question is no different from that mentioned in the previous paragraph. Another, more tendentious way to understand the issue is as concerning whether Frege believed the laws of logic could be justified *ex nihilo*: whether an argument in their favor could be produced that would (or should) convince someone antecedently skeptical of their truth or, worse, someone skeptical of the truth of any of the laws of logic.

If this is what is supposed to be at issue,[3] then let me say, as clearly as I can, that neither I nor anyone else, so far as I know, has ever held that

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theory formulated in that language, but we can make the distinction on his behalf. I shall therefore use "the Begriffsschrift" to refer to the theory, and "Begriffsschrift", without the article, to refer to the language.

2. Burton Dreben was fond of making this point.

3. This notion of justification does seem to be the one some commentators have had in mind: See Ricketts (1986a, p. 190) and Weiner (1990, p. 277).

Frege thought logical laws could be justified in this sense. Moreover, so far as I know, no one now does think that the laws of logic can be justified to a logical skeptic—and, to be honest, I doubt that anyone ever has.[4]

So in so far as Frege, or anyone else, thinks the laws of logic can be 'justified',[5] the justification envisaged cannot be an argument designed to convince a logical skeptic. But what then might it be? This is a nice problem, and a very old one, namely, the problem of the Cartesian Circle. I am not going to solve this problem here (and not for lack of space), but there are a few things that should be said about it. The problem is that any justification of a logical law will have to involve some reasoning, which will depend for its correctness on the correctness of the inferences employed in it. Hence, any justification of the laws of logic must, from the point of view of a logical skeptical, be circular. Moreover, even if one were only attempting to justify, say, the law of excluded middle, no argument that appealed to that very law could have any probative force. But, although these considerations do show that no such justification could be used to convince someone of the truth of the law of excluded middle, the circularity is not of the usual sort. One is not assuming, as a premise, that the law of excluded middle is valid: If that were what one were doing, then the 'justification' could establish nothing, since one could not help but reach the conclusion one had assumed as a premise. What one is doing, rather, is appealing *to certain instances* of the law of excluded middle in an argument whose conclusion is that the law is valid. That one is prepared to appeal to (instances of) excluded middle does not imply that one cannot but reach the conclusion that excluded middle is valid: A semantic theory for intuitionistic logic can be developed in a classical meta-language, and that semantic theory does not validate excluded middle. So the mere fact that one uses instances of excluded middle in the course of proving the soundness of classical logic need not imply that the justification of the

4. I have heard it suggested that Michael Dummett believes something like this. But he writes: "… [T]here is no skeptic who denies the validity of all principles of deductive reasoning, and, if there were, there would obviously be no reasoning with him" (Dummett, 1991, p. 204).

5. Note that I am *not* here intending to use this term in whatever sense Frege himself may have used it. I am not concerned, that is, with whether Frege would have said (in translation, of course), "It is (or is not) possible to justify the laws of logic". I am concerned with the question whether Frege thought *that* the laws of logic can be justified and, if so, in what sense, not with whether he would have used (a translation of) these words to make this claim. The point may seem obvious, but some commentators have displayed an extraordinary level of confusion about this simple distinction. But let me not name names.

classical laws so provided is worthless. If one were trying to explain the universal validity of the law of excluded middle, for example, a justification that employed instances of that very law might suffice.[6]

That would be one way of understanding what justifications of logical laws are meant to accomplish: They answer the question why a given logical law is valid. It suggests another. The objection that justifications of logical laws are circular depends upon the assumption that their purpose is to show that the laws are true (or the rules, truth-preserving). It will be circular to appeal to instances of the law of excluded middle in a justification of that very law only if the truth of instances of the law is what is at issue. But justifications of logical laws need not be intended to demonstrate their truth. We might all be agreed that every instance of (say) the law of excluded middle is, as it happens, true but still disagree about whether those instances are logical truths.[7] The purpose of a justification of a law of logic might be, not to show that it is true, but to uncover the source of its truth, to demonstrate that it is indeed a law of logic. It is far from obvious that an argument that assumed that all instances of excluded middle were true could not informatively prove that they were logically true.[8]

There is reason to suppose that Frege should have been interested in giving a justification at least of the validity of the axioms and rules of inference of the Begriffsschrift. Consider, for example, the following remark:[9]

> I became aware of the need for a Begriffsschrift when I was looking for the fundamental principles or axioms upon which the whole of mathematics rests. Only after this question is answered can it be hoped to trace successfully the

6. The discussion in this paragraph is heavily indebted to Dummett's (1991, pp. 200–4). It is also worth emphasizing, with Jamie Tappenden (1997), that an *explanation* of a fact need not amount to a reduction to simpler, or more basic, facts.

7. For example, intuitionists accept all instances of excluded middle for quantifier-free (and, indeed, bounded) formulae of the language of arithmetic, on the ground that any such formulae can, in principle, be proved or refuted. Now imagine a constructivist who was convinced, for whatever reason, that *every* statement could, in principle, either be verified or be refuted. She would accept all instances of excluded middle as true, but not as logical truths.

8. More generally, if one is to accept a proof that a particular sentence is logically true, one will have to agree that the principles from which the proof begins are true and that the means of inference used in it are truth-preserving. But one need not agree that the principles and means of inference are *logical*: The proof does not purport to establish that *it is logically true that* the particular sentence is logically true, only that the sentence is logically true. And in model-theoretic proofs of validity, one routinely employs premises that are obviously *not* logically true, such as axioms of set theory.

9. References to papers reprinted in Frege's *Collected Papers* (1984) are given with the page number in the reprint (p. n) and the page number in the original publication (op. n).

springs of knowledge upon which this science thrives. (Frege 1984c, p. 235, op. 362)

Frege's life's work was devoted to showing that the basic laws of arithmetic are truths of logic, and his strategy for doing this was to prove them in the Begriffsschrift. But no derivation of the basic laws of arithmetic will decide the epistemological status of arithmetic on its own: It will simply leave us with the question of the epistemological status of the axioms and rules used in that derivation. It thus must be at least an intelligible question whether the axioms and rules of the Begriffsschrift are logical in character. What other question could remain?

The discussion that follows the passage just quoted reinforces these points. Frege first argues that epistemological questions about the source of mathematical knowledge are, at least in part, themselves mathematical in character, because the question what the fundamental principles of mathematics are is mathematical in character.

> In order to test whether a list of axioms is complete,[10] we have to try and derive from them all the proofs of the branch of learning to which they relate. And in doing this it is imperative that we draw conclusions only in accordance with purely logical laws. … The reason why verbal languages are ill suited to this purpose lies not just in the occasional ambiguity of expressions, but above all in the absence of fixed forms for inferring. … If we try to list all the laws governing the inferences that occur when arguments are conducted in the usual way, we find an almost unsurveyable multitude which apparently has no precise limits. The reason for this, obviously, is that these inferences are composed of simpler ones. And hence it is easy for something to intrude which is not of a logical nature and which consequently ought to be specified as an axiom. This is where the difficulty of discerning the axioms lies: for this the inferences have to be resolved into their simpler components. By so doing we shall arrive at just a few modes of inference, with which we must then attempt to make do at all times. And if at some point this attempt fails, then we shall have to ask whether we have hit upon a truth issuing from a non-logical source of cognition, whether a new mode of inference has to be acknowledged, or whether perhaps the intended step ought not to have been taken at all. (Frege 1984c, p. 235, opp. 362–3)

Much of this passage will seem familiar, so strong is the echo of remarks Frege had made some years earlier, in the Preface to *Begriffsschrift*, regarding

10 Note that Frege uses this term in a way that is close to, but not identical to, how it is standardly used in contemporary logic.

the need for a formalization of logic (Frege 1967, pp. 5–6). But the most interesting remark is the last one, which addresses the question what we should do if at some point we were to find ourselves unable to formalize the proof of a theorem previously proven informally. The most natural next step would be to try to isolate some principle on which the proof apparently depended, which principle would then be a candidate to be added to our list of fundamental principles of mathematics. Once we had isolated this principle, call it NewAx, there would be three possibilities among which we should have to decide: NewAx may be a "non-logical" truth, one derived from intuition or even from experience; NewAx may be a truth of logic, which is what Frege means when he says that we may have to recognize "a new mode of inference"; or NewAx may not be true at all, which is what Frege means when he says that the "intended step ought not to have been taken".

Frege is not just describing a hypothetical scenario here: Frege had encountered this sort of problem on at least two occasions. I have discussed these two occasions in more detail elsewhere (Boolos and Heck 1998, and Heck 1998b). Let me summarize those discussions.

In *Grundgesetze*, Frege begins his explanation of the proof of the crucial theorem that every number has a successor by considering a way of attempting to prove it that ultimately does not work, namely, the way outlined in §§ 82–3 of *Die Grundlagen*. As part of that proof, one has to prove a proposition[11] that, Frege remarks in a footnote, "is, as it seems, unprovable ..." (Frege 1964, I § 114). It is notable that Frege does not say that this proposition is *false*, and there is good reason to think he regarded it as true and so true but unprovable in the Begriffsschrift: It follows immediately from the proposition Frege proves in its place, together with Dedekind's result that every infinite set is Dedekind infinite (Dedekind 1963, § 159). Frege knew of Dedekind's proof of this theorem and seems to have accepted it, although he complains in his review of Cantor's *Contributions to the Theory of the Transfinite* that Dedekind's proof "is hardly executed with sufficient rigour" (Frege 1984f, p. 180, op. 271). Frege apparently expended some effort trying to formalize Dedekind's proof. In the course of doing so, he could hardly have avoided discovering the point at which Dedekind relies upon an assumption not obviously available in the Begriffsschrift, namely, the axiom of (countable) choice. One can thus think of the theorem whose proof we have been unable to formalize either

11. The proposition in question is that labeled (1) in § 82 of *Die Grundlagen*.

as Dedekind's result or as the unprovable proposition mentioned in section 114 of *Grundgesetze* and of NewAx as the axiom of choice.

Remarks of Dummett's suggest he would regard the foregoing as anachronistic:

> No doubt Frege would have claimed his axioms, taken together with the additional informal stipulations not embodied in them,[12] as yielding a complete theory: to impute to him an awareness of the incompleteness of higher-order theories would be an anachronism. (Dummett 1981b, p. 423)

But I am suggesting only that Frege was prepared to consider the possibility that his formalization of logic (or arithmetic) was not complete: It is obvious that *particular* formalizations can be incomplete. What Gödel showed was that arithmetic (and therefore higher-order logic) is *essentially* incomplete, that is, that every consistent formal theory extending arithmetic is incomplete. Of that Frege surely had no suspicion, but that is not relevant here.

In any event, the question whether a given (primitive) principle is a truth of logic is clearly one Frege regards as intelligible. And important. The question of the epistemological status of the basic laws of arithmetic is of central significance for Frege's project: His uncovering the fundamental principles of arithmetic will not decide arithmetic's epistemological status on its own. Though he did derive the axioms of arithmetic in the Begriffsschrift, that does not show that the basic laws of arithmetic are logical truths: That will follow only if the axioms of the Begriffsschrift are themselves logical laws and if its rules of inference are logically valid. The question of the epistemological status of arithmetic then reduces to that of the epistemological status of the axioms and rules of the Begriffsschrift—among other things, to the epistemological status of Frege's infamous Basic Law V, which states that functions F and G have the same 'value-range' if, and only if, they are co-extensional.

It is well-known that, even before receiving Russell's letter informing him of the paradox, Frege was uncomfortable about Basic Law V. The passage usually quoted in this connection is this one:[13]

12. These are the stipulations made in section 10 of *Grundgesetze*, which we shall discuss below.

13. Frege also writes, in the appendix to *Grundgesetze* on Russell's paradox: "I have never disguised from myself [Basic Law V's] lack of the self-evidence that belongs to the other axioms and that must properly be demanded of a logical law" (Frege 1964, II, p. 253). The axiom's lacking *self*-evidence is reason to doubt it is a *logical* law: Self-evidence can be demanded only of primitive logical laws, not, say, of the axioms of geometry, which are evident on the basis of intuition.

A dispute can arise, so far as I can see, only with regard to my basic law (V) concerning value-ranges, which logicians perhaps have not yet expressly enunciated, and yet is what people have in mind, for example, where they speak of the extensions of concepts. I hold that it is a law of pure logic. In any event, the place is pointed out where the decision must be made. (Frege 1964, I, p. vii)

Although few commentators have said explicitly that Frege is here expressing doubt that Basic Law V is true, the view would nonetheless appear to be very widely held: It is probably expressed so rarely because it is thought that the point is too obvious to be worth stating.[14] But we must be careful about reading our post-Russellian doubts about Basic Law V back into Frege: He thinks of Basic Law V as codifying something implicit, not only in the way logicians speak of the extensions of concepts, but in the way mathematicians speak of functions (Frege 1964, II § 147).[15] And there is, so far as I can see, no reason to conclude, on the basis of the extant texts, that Frege had any doubts about the Law's truth.

The nature of the dispute Frege expects, and "the decision which must be made", is clarified by what precedes the passage just quoted:

Because there are no gaps in the chains of inference, every 'axiom' … upon which a proof is based is brought to light; and in this way we gain a basis upon which to judge the epistemological nature of the law that is proved. Of course the pronouncement is often made that arithmetic is merely a more highly developed logic; yet that remains disputable [*bestreitbar*] so long as transitions occur in proofs that are not made according to acknowledged laws of logic, but seem rather to be based upon something known by intuition. Only if these transitions are split up into logically simple steps can we be persuaded that the root of the matter is logic alone. I have drawn together everything that can facilitate a judgment as to whether the chains of inference are cohesive and the buttresses solid. If anyone should find anything defective, he must be able to state precisely where the error lies: in the Basic Laws, in

14. An exception is Tyler Burge. Though Burge speaks, at one point, of "Frege's struggle to justify Law (V) as a logical law" (1984, pp. 30ff), what he actually discusses are grounds Frege might have had for doubting its *truth*. Burge (1984, pp. 12ff) claims that Frege's considering alternatives to Basic Law V suggests that he thought it might be false. But given Frege's commitment to logicism, doubts about its epistemological status would also motivate such investigations.

15. Treating concepts as functions then makes Basic Law V sufficient to yield extensions of concepts, too. And there is really nothing puzzling about this treatment of concepts: Technically, it amounts to identifying them with their characteristic functions. For more on this point, see Heck (1997, pp. 282ff).

the Definitions, in the Rules, or in the application of the Rules at a definite point. If we find everything in order, then we have accurate knowledge of the grounds upon which an individual theorem is based. A dispute [*Streit*] can arise, so far as I can see, only with regard to my basic law (V) concerning value-ranges … I hold that it is a law of pure logic. In any event, the place is pointed out where the decision must be made. (Frege 1964, I, p. vii)

The dispute Frege envisions would concern the truth of Basic Law V were the correctness of the proofs all that was at issue here. But as I read this passage, Frege is attempting to explain how the long proofs he gives in *Grundgesetze* support his logicism,[16] how he intends to persuade us "that the root of the matter is logic alone". The three sentences beginning with "I have drawn" constitute a self-contained explanation of how the formal presentation of the proofs gives us "accurate knowledge of the grounds upon which an individual theorem is based", that is, how the proofs provide "a basis upon which to judge the epistemological nature of" arithmetic, by reducing that problem to one about the epistemological status of the axioms and rules. Of course, someone might well object to Frege's proofs on the ground that Basic Law V is not true. But, although Frege must have been aware that this objection might be made, he thought the Law was widely, if implicitly, accepted. Moreover, as we shall see below, Frege took himself to have proven that Basic Law V is true in the intended interpretation of the Begriffsschrift.[17] But, in spite of all of this, Basic Law V was not an acknowledged law of logic. The "dispute" Frege envisages thus concerns what other treatments have left "disputable"—and these words are cognates in Frege's German, too—namely, whether "arithmetic is merely a more highly developed logic". The objection Frege expects, and to which he has no adequate reply, is not that Basic Law V is not true, but that it is not "a law of pure logic". All he can do is to record his own conviction that it is and to remark that, at least, the question of arithmetic's epistemological status has been reduced to the question of Law V's epistemological status.

16. This question is, in fact, taken up again in section 66. It is unfortunate that this wonderful passage is so little known.

17. I thus am not saying that Frege nowhere speaks to the question whether Basic Law V is true, even in *Grundgesetze* itself (compare Burge (1998, p. 337, fn 21)). What I am discussing here is where Frege thought matters stood *after* the arguments of *Grundgesetze* had been given. I am thus claiming that Frege thought he could answer the objection that Basic Law V is not true but would have had to acknowledge that he had no convincing response to the objection that it is not a law of logic. (The foregoing remarks, I believe, answer a criticism made by Burge.)

The general question with which we are concerned here is thus what it is for an axiom of a given formal theory to be a logical truth, a logical axiom.[18] Frege does not say much about this question. One might think that that is because he had no view about the matter, that he had, as Warren Goldfarb has put it, no "overarching view of the logical".[19] Goldfarb is not, of course, merely pointing out that Frege did not have any general account of what distinguishes logical from non-logical truths. Nor do I. His claim is that Frege's philosophical views precluded him from so much as envisaging, attempting, or aspiring to such an account. But I find it hard to see how one can make that claim without committing oneself to the view that, for Frege, it is not even a substantive question whether Basic Law V is a truth of logic. Frege does insist that Basic Law V is a truth of logic, to be sure. But suppose that I were to deny that it is. Does Frege believe that this question is one that can be discussed and, hopefully, resolved rationally? If not, then Frege's logicism is a merely verbal doctrine: It amounts to nothing more than a proposal that we should *call* Basic Law V a 'truth of logic'. I for one cannot believe that Frege's considered views could commit him to this position. But if Frege thinks the epistemological status of Basic Law V is subject to rational discussion, then any principles or claims to which he might be inclined to appeal in attempting to resolve the question of its status will constitute an inchoate (even if incomplete) conception of the logical.

One thing that is clear is that the notion of generality plays a central role in Frege's thought about the nature of logic.[20] According to Frege, logic is the most general science, in the sense that it is universally applicable. There might be special rules one must follow when reasoning about geometry, or physics, or history, which do not apply outside that limited area: But the truths of logic govern reasoning of all sorts. And if this is to be the case, it would seem that there must be another respect in which logic is general:

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18. Similarly, Frege writes in *Die Grundlagen* that the question whether a proposition is analytic is to be decided by "finding the proof of the proposition, and following it all the way back to the primitive truths", those truths "which … neither need nor admit of proof". The proposition is analytic if, and only if, it can be derived, by means of logical inferences, from primitive truths that are "general logical laws and definitions". An analytic truth is thus a truth that follows from primitive logical axioms by means of logical inferences (Frege 1980, § 3). The problem is to say what primitive logical truths and logical means of inference are.

19. Goldfarb expressed the point this way in a lecture based upon his paper "Frege's Conception of Logic" (Goldfarb 2001).

20. Naturally enough, since his discovery of quantification is so central to his conception of logic. See Dummett (1981a, pp. 43ff) for a discussion close in spirit to that to follow.

As Thomas Ricketts puts the point, "… the basic laws of logic [must] generalize over every thing and every property [and] not mention this or that thing …" (Ricketts 1986b, p. 76); there can be nothing topic-specific about their content. Thus, the laws of logic are "[m]aximally general truths … that do not mention any particular thing or any particular property; they are truths whose statement does not require the use of vocabulary belonging to any special science" (Ricketts 1986b, p. 80).[21]

So there is reason to think that Frege thought it necessary, if something is to be a logical law, that it should be maximally general in this sense. Some commentators, however, have flirted with the idea that Frege also held the condition to be sufficient.[22] Let us call this interpretation the 'syntactic' interpretation of Frege's conception of logic. One difficulty with it is that such a characterization of the logical, even if extensionally correct, would not serve Frege's purposes. For consider any truth at all and existentially generalize on all non-logical terms occurring in it. The result will be a truth that is, in the relevant sense, maximally general and so, on the syntactic interpretation, should be a logical truth. Thus, '$\exists x \exists y (x \neq y)$' should be a logical truth, since it is the result of existentially generalizing on all the non-logical terms in 'Caesar is not Brutus'. But the notion of a truth of logic plays a crucial *epistemological* role for Frege. In particular, logical truths are supposed to be analytic, in roughly Kant's sense: Our knowledge of them is not supposed to depend upon intuition or experience. Why should the mere fact that a truth is maximally general imply that it is analytic? Were there no way of knowing the truth of '$\exists x \exists y (x \neq y)$' except by deriving it from a sentence like 'Caesar is not Brutus', it certainly would not be analytic. More worryingly, consider '$\exists x \forall F (x \neq \acute{\varepsilon} F \varepsilon)$', which asserts that some object is not a value-range. This sentence is maximally general—if it is not, that is reason enough to deny that Basic Law V is a truth of logic—and, presumably, Frege regarded it as either true or false. But surely the question whether there are non-logical objects is not one in the province of logic itself.

Still, we need not be attempting to explain what it is for any truth at all

21. For similar views, see van Heijenoort (1967), Goldfarb (1979), and Dreben and van Heijenoort (1986).

22. Ricketts speaks of Frege's "identification of the laws of logic with maximally general truths" (Ricketts 1986b, p. 80), quoting Frege's remark that "logic is the science of the most general laws of truth" (Frege 1979a, p. 128). He glosses the remark as follows: "To say that the laws of logic are the most general laws of truth is to say that they are the most general truths". But whence the identification of the most general laws of truth with the most general truths? Ricketts later (1996, p. 124) disowns this suggestion, however.

to be a truth of logic, only what it is for a *primitive* truth (see Frege (1980, § 3)), an axiom, to be a truth of logic. So perhaps the condition should apply only to primitive truths: The view should be that a primitive truth is logical just in case it is maximally general. And it is eminently plausible that maximally general primitive truths must be analytic, for it is very hard to see how our knowledge of such a truth could depend upon intuition or experience. Intuition and experience deliver, in the first instance, truths that are *not* maximally general but that concern specific matters of fact. Hence, in so far as they support our knowledge of truths that are maximally general, they apparently must do so by means of inference. But then maximally general truths established on the basis of intuition or experience are not primitive.[23]

It might seem, therefore, that semantical concepts will play no role in Frege's conception of a truth of logic, that his conception is essentially syntactic. This, however, would be a hasty conclusion, for there are two respects in which the syntactic interpretation is incomplete, and these matter. First, our earlier statement of what maximally general truths are needs to be refined. Ricketts writes that "[m]aximally general truths … do not mention any particular thing or any particular property". But reference to some specific concepts will be necessary for the expression of any truth at all, logical or otherwise. Frege himself remarks that "logic … has its own concepts and relations; and it is only in virtue of this that it can have a content" (Frege 1984e, p. 338, op. 428): The universal quantifier refers to a specific second-level concept; the negation-sign, a particular first-level concept; the conditional, a first-level relation. And when Frege offers his "emanation of the formal nature of logical laws"—an account not unlike a primitive version of the model-theoretic account of consequence, according to which logical laws are those whose truth does not depend upon what non-logical terms occur in them—the main problem he discusses is precisely that of deciding which notions are logical ones, whose interpretations must remain fixed : "It is true that in an inference we can replace Charlemagne by Sahara, and the concept *king* by the concept *desert* … But one may not thus replace the relation of identity by the lying of a point in a plane" (Frege 1984e, pp. 338-9, op. 428).[24]

23. Something like this line of thought is suggested by Ricketts (1986b, p. 81).

24. The question which concepts are logical is not likely to admit of an answer in non-semantical terms. For some contemporary work, see Sher (1991). Sher's theory relies crucially on model-theoretic notions, such as preservation of truth-value under permutations of the domain. Dummett (1981a, p. 22, fn) considers a similar proposal when discussing Frege's conception of

The problem of the logical constant—the question which concepts belong to logic—is, for this reason, central to Frege's account of logic. His inability to resolve this problem may well have been one of the sources of his doubts about Basic Law V: Unlike the quantifiers and the propositional connectives, the smooth breathing—from which names of value-ranges are formed—is not obviously a logical constant. It is clear enough that what we now regard as logical constants have the generality of application Frege requires them to have: They appear in arguments within all fields of scientific enquiry, arguments that are, at least plausibly, universally governed by the laws of the logical fragment of the Begriffsschrift. It is far less clear that the smooth breathing—and the set-theoretic reasoning in which it would be employed—is similarly ubiquitous. It would therefore hardly have been absurd for one of Frege's contemporaries to insist that the smooth breathing and Basic Law V are peculiar to the 'special science' of *mathematics*. Frege would have disagreed, to be sure. But the syntactic interpretation offers him no ground on which to do so and, worse, seems to preclude him from having any such ground.

The second problem with the syntactic interpretation is that it places a great deal of weight on the notion of primitiveness, and we have not been told how that is to be explained. Our modification of the syntactic interpretation—which consisted in claiming only that maximally general primitive truths are logical—will be vacuous unless there are restrictions upon what can be taken as a primitive truth. Otherwise, we could take '$\exists x \forall F(x \neq \acute{\varepsilon}F\varepsilon)$' as an axiom and its being a logical truth (assuming it is a truth) would follow immediately. One might suppose that Frege's remarks on the nature of analyticity, mentioned above, committed him to the view that certain truths, of their very nature, admit of no proof. But that would be a mistake. Frege is perfectly aware that, although some rules of inference, and some truths, must be taken as primitive, it may be a matter of choice which are taken as primitive. And since it is not obvious that there are any rules or truths that must be taken as primitive in every reasonable formalization, there need be none that are essentially primitive.[25] So, if the notion of primitiveness is to help at all here, we need an account of what

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logic and, in particular, his conception of logic's generality.

25. Thus, Frege writes: "… [I]t is really only relative to a particular system that one can speak of something as an axiom" (Frege 1979b, p. 206). See also Frege (1967, § 13), where Frege says, in effect, that he could have chosen other axioms for the theory and, indeed, that it might be essential to consider other axiomatizations if all relations between laws of thought are to be made clear.

makes a truth a candidate for being a primitive truth in some formalization or other. A natural thought would be that the notion of self-evidence should play some role (see Frege (1964, II, p. 253)), but Frege says almost nothing directly about this question, either.[26]

One way to approach this issue would be via Frege's claim that logical laws are fundamental to thought and reasoning, in the sense that, should we deny them, we would "reduce our thought to confusion" (Frege 1964, I, p. vii; see also Frege (1980, § 14)). I have no interpretation to offer of this claim. But I want to emphasize that it is not enough for Frege simply to assert that his axioms cannot coherently be denied. What Frege would have needed is an account of *why* the particular statements he thought were laws of logic were, in that sense, inalienable.[27] The semantical concepts Frege uses in stating the intended interpretation of Begriffsschrift, which I shall discuss momentarily, also pervade his mature work on the philosophy of logic, and it is a nice question why Frege should have turned to the study of semantical notions at just this time. My hunch, and it is just a hunch, is that he did so because he was struggling with the very questions about the nature of logic we have been discussing: He was developing a conception of logic in which they would play a fundamental role. Frege argues, in the famous papers written around the time he was writing *Grundgesetze*, that semantical concepts are central to any adequate account of our understanding of language, of our capacity to express thoughts by means of sentences, to make judgements and assertions, and so forth.[28] So, if Frege could have shown that negation, the conditional, and the quantifier were explicable in terms of these semantical concepts—and he might well have thought that the semantic theory for Begriffsschrift shows just this—he could then have argued that they are, in principle, available to anyone able to think and reason, that is, that these notions (and the fundamental truths about them) are, in that sense, implicit in our capacity for thought. Unfortunately, such an argument would not apply to Basic Law V: The

26. There has been some recent work on this matter: See Burge (1998) and Jeshion (2001).

27. Vann McGee (1985) at least claims to believe that there are counter-examples to *modus ponens*, and one would suppose that if any law of logic were inalienable, that would be the one. To be sure, it's not clear what the right conception of inalienability is, but that only makes Frege's burden more obvious.

28. Frege claims in "On Sense and Reference" that the truth-values "are recognized, if only implicitly, by everybody who judges something to be true …" (Frege 1984d, p. 163, op. 34). See also Frege's flirtation with a transcendental argument for the laws of logic (Frege 1964, I, p. xvii).

notion of a value-range does not seem to be fundamental to thought in this way, and, as we shall see, Frege's semantic theory does not treat it the same way it treats the other primitives. So that might have provided a second reason for Frege to worry about its epistemological status. But I shall leave the matter here, for we are already well beyond anything Frege ever discussed explicitly.

2. *Formalism and the significance of interpretation*

The discussion in the preceding section began with the question what it might mean to justify the laws of logic. I argued that justifications of logical laws intended to establish their truth must be circular. But the argument for that claim depended upon an assumption that I did not make explicit, namely, that the logical laws whose truth is in question are *the thoughts expressed by* certain sentences. It is quite possible to argue, without circularity, that certain *sentences* that in fact express (or are instances of) laws of logic are true, say, to argue that every instance of '$A \vee \neg A$' is true. I do just that in my introductory logic classes. Of course, the arguments carry conviction only because my students are willing to accept certain claims that I state in English using sentences that are themselves instances of excluded middle. But that discloses no circularity: My purpose is just to convince them of the truth of all sentences of a certain form, and those are not English sentences.

Semantic theories frequently have just this kind of purpose. A formal system is specified: A language is defined, certain sentences are stipulated as axioms, and rules governing the construction of proofs are laid down. The language is then given an interpretation: The references of primitive expressions of the language are specified, and rules are stated that determine the reference of a compound expression from the references of its parts. It is then argued—completely without circularity—that all of the sentences taken as axioms are true and that the rules of inference are truth-preserving. Of course, the argument carries conviction only because we are willing to accept certain claims stated in the meta-language—that is, the language in which the interpretation is given—claims that may well express precisely what the sentences in the formal language express. But that discloses no circularity: The purpose of the argument is to demonstrate the truth of the sentences taken as axioms and the truth-preserving character of the rules. Its purpose is to show not that the *thoughts expressed* *by* certain formal sentences are true but only that those sentences are true.

The semantic theory Frege develops in Part I of *Grundgesetze* has the same purpose. In the case of each of the primitive expressions of Begriffsschrift, he states what its interpretation—that is, its reference—is to be. Thus, for example:[29]

> "$\Gamma = \Delta$" shall denote the True if Γ is the same as Δ; in all other cases it shall denote the False. (Frege 1964, I § 7)

> "$\forall a \Phi(a)$" is to denote the True if, for every argument, the value of the function $\Phi(\xi)$ is the True, and otherwise it is to denote the False. (Frege 1964, I § 8)

Some of Frege's stipulations—which I shall call his *semantical stipulations* regarding the primitive expressions—do not take such an explicitly semantical form. Thus, for example, in connection with the horizontal, Frege writes:

> I regard it as a function-name, as follows:
>
> $—\Delta$
>
> is the True if Δ is the True; on the other hand, it is the False if Δ is not the True. (Frege 1964, I § 5)

Frege wanders back and forth between the explicitly semantical stipulations and ones like this: But the point, in each case, is to say what the reference of the expression is supposed to be, and Frege argues in section 31 of *Grundgesetze* that these stipulations do secure a reference for the primitives. And he argues, in section 30, that the stipulations suffice to assign references to all expressions if they assign references to all the primitive expressions.[30]

Frege goes on to argue that each axiom of the Begriffsschrift is true. Thus, about Axiom I he writes:

> By [the explanation of the conditional given in] § 12,
>
> $\Gamma \rightarrow (\Delta \rightarrow \Gamma)$
>
> could be the False only if both Γ and Δ were the True while Γ was not the True. This is impossible; therefore

29. I am silently converting some of Frege's notation to ours and will continue to do so.
30. For discussion of these arguments, see Heck (1998a and 1999) and Linnebo (2004).

$\vdash \Gamma \rightarrow (\Delta \rightarrow \Gamma)$.

(Frege 1964, I §18)

And, similarly, in the case of each of the rules of inference, he argues that it is truth-preserving. Thus, regarding transitivity for the conditional, he writes:

> From the two propositions
>
> $\vdash \Delta \rightarrow \Gamma$
> $\vdash \Theta \rightarrow \Delta$
>
> we may infer the proposition
>
> $\vdash \Theta \rightarrow \Gamma$
>
> For $\Theta \rightarrow \Gamma$ is the False only if Θ is the True and Γ is not the True. But if Θ is the True, then Δ too must be the True, for otherwise $\Theta \rightarrow \Delta$ would be the False. But if Δ is the True then if Γ were not the True then $\Delta \rightarrow \Gamma$ would be the False. Hence the case in which $\Theta \rightarrow \Gamma$ is not the True cannot arise; and $\Theta \rightarrow \Gamma$ is the True. (Frege 1964, I § 15)

These arguments—which, for the moment, I shall call *elucidatory demonstrations*—tend by and large not to be explicitly semantical: That is, Frege usually speaks not of what the premises and conclusion denote but rather of particular objects' *being* the True or the False. One might suppose that this shows that Frege's arguments should not be taken to be semantical in any sense at all. But, to my mind, the observation is of little significance: What it means is just that Frege is not being as careful about use and mention as he ought to be.

It is sometimes said that Begriffsschrift is not an 'interpreted language': a syntactic object—a language, in the technical sense—that has been given an interpretation. Rather, it is a 'meaningful formalism', something like a language in the ordinary sense, but one that just happens to be written in funny symbols—something in connection with which it would be more appropriate to speak, as Ricketts does, of "foreign language instruction" than of interpretation (Ricketts, 1986a, p. 176). If so, then one might suppose that Frege could not have been interested in 'interpretations' of Begriffsschrift because, in his exchanges with Hilbert, he seems to be opposed to any consideration of varying interpretations of meaningful languages. But, as Jamie Tappenden has pointed out, Frege's own mathematical work involved the provision of just such reinterpretations of, for example, complex number theory. What Frege objected to was Hilbert's claim that content can be bestowed upon a sign *simply* by indicating a range of alternative interpretations (Tappenden 1995).[31] In some sense, it seems to me, Frege thought that the concept of an interpreted language was more basic than that of an uninterpreted one—and it is hard not to be sympathetic. But it simply does not follow that one cannot intelligibly consider other interpretations of the dis-interpreted symbols of a given language.

In any event, Frege was certainly aware that it would be possible to treat Begriffsschrift as an uninterpreted language, with nothing but rules specifying how one sentence may be constructed from others. For the central tenet of Formalism, as Frege understood the position, is precisely that arithmetic ought to be developed as a Formal theory,[32] in the sense that the symbols that occur in it have no meaning (or that any meaning they may have is somehow irrelevant). Such a theory need not be lacking in mathematical interest: It can, in particular, be an object of mathematical investigation. There could, for example, be a mathematical theory that would prove such things as that this 'figure' (formula) can be 'constructed' (derived) from others using certain rules—or that a given figure cannot be so constructed (Frege 1964, II § 93). One can, if one likes, stipulate that certain figures are 'axioms', which specification one might compare to the stipulation of the initial position in chess, and take special interest in the question what figures can be derived from the 'axioms' (Frege 1964, II §§ 90–1). Frege's fundamental objection to Formalism is that it cannot explain the applicability of arithmetic, and this needs to be explained, for "it is applicability alone which elevates arithmetic from a game to the rank of a science" (Frege 1964, II § 91). An examination of Frege's development of this objection will thus reveal what he thought would have been lacking had Begriffsschrift been left uninterpreted—and so what purpose he intended his semantical stipulations to serve.[33]

31. For further consideration of this kind of question, see Tappenden (2000). And even if we were to accept this objection, it still would not follow that Frege was uninterested in semantics (Stanley 1996, p. 64).

32. For a discussion of this notion of a formal theory, see Frege (1984b). I shall capitalize the word "Formal" when I am using it in the sense explained here.

33. Frege's discussion explicitly concerns the rules of arithmetic, not those of logic: But, of course, for Frege, arithmetic *is* logic, and his formal system of arithmetic, the Begriffsschrift, contains no axioms or rules that are (intended to be) non-logical. His discussion of what requirements the rules of arithmetic must meet therefore applies directly to the axioms and rules of inference of the Begriffsschrift itself. Thus, he writes: "Now it is quite true that we could have

Frege distinguishes "Formal" from "Significant"[34] arithmetic. He characterizes Significant arithmetic as the sort of arithmetic that concerns itself with the references of arithmetical signs, as well as with the signs themselves and with rules for their manipulation. Formal arithmetic is interested only in the signs and the rules: It treats Begriffsschrift as an uninterpreted language. On the Formalist view, the references of, say, numerals are of no importance to arithmetic itself, though they may be of significance for the application of arithmetic (Frege 1964, II § 88). And, according to Frege, this refusal to recognize the references of numerical terms is what is behind another of the central tenets of Formalism, that the rules[35] of a system of arithmetic are, from the point of view of arithmetic proper, entirely arbitrary: "In Formal arithmetic we need no basis for the rules of the game—we simply stipulate them" (Frege 1964, II § 89). Though Formalists recognize that the rules of arithmetic cannot really be arbitrary, they take this fact to be of no significance for arithmetic but only for its applications:

> Thomae … contrasts the arbitrary rules of chess with the rules of arithmetic…. But this contrast first arises when the applications of arithmetic are in question. If we stay within its boundaries, its rules appear as arbitrary as those of chess. This applicability cannot be an accident—but in Formal arithmetic we absolve ourselves from accounting for one choice of the rules rather than another. (Frege 1964, II § 89)

It is important to remember that, throughout this discussion, Frege is *contrasting* Formal and Significant arithmetic. When he speaks of "absolv[ing]

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introduced our rules of inference and the other laws of the Begriffsschrift as arbitrary stipulations, without speaking of the reference and the sense of the signs. We would then have been treating the signs as figures" (Frege 1964, II § 90). That is to say, we should then have been adopting a Formalist perspective on the Begriffsschrift.

34. The German term is "inhaltlich", which Geach and Black translate in the first edition of *Translations* as "meaningful". While this was a reasonable translation then, it is now dangerous, since the cognate term "meaning" has become a common translation of Frege's term "Bedeutung". In the third edition, they translate "inhaltliche Arithmetik" as "arithmetic with content"; a literal translation would be "contentful arithmetic". Both of these sound cumbersome to my ear.

35. Frege speaks, throughout these passages, of the "rules" of the Formal game, thereby meaning to include, I think, not just its 'rules of inference', but also its 'axioms'—though he does tend to focus more on the "rules permitting transformations" than on the stipulation of the initial position or "starting points" (Frege 1964, II § 90). The reason is that he tends to think even of the axioms of a Formal theory as rules saying, in effect, that certain things can always be written down. (See here Frege (1964, II § 109).) And, of course, one can think of axioms as a kind of degenerate inference rule.

ourselves from accounting for one choice of the rules rather than another", he is not just saying that the rules of arithmetic are non-arbitrary; he is implying that, if we are to formulate a system of Significant arithmetic, we must ourselves answer the question why we have formulated the rules as we have.

Frege does not think of this account as a mere appendage to Significant arithmetic, but as a crucial part of the work of the arithmetician:

> It is likely that the problem of the usefulness of arithmetic is to be solved—in part, at least—independently of those sciences to which it is to be applied. Therefore it is reasonable to ask the arithmetician to undertake the task. …
>
> This much, it appears to me, can be demanded of arithmetic. Otherwise it might happen that, while [arithmetic] handled its formulas simply as groups of figures without sense, a physicist wishing to apply them might assume quite without justification that they expressed thoughts whose truth had been demonstrated. This would be—at best—to create the illusion of knowledge. The gulf between arithmetical formulas and their applications would not be bridged. In order to bridge it, it is necessary that the formulas express a sense and that the rules be grounded in the reference of the signs. (Frege 1964, II § 92)

The rules must be so grounded because arithmetic is expected to deliver truths—not just truths, in fact, but knowledge. As Frege concludes the passage: "The end must be knowledge, and it must determine everything that happens" (Frege 1964, II § 92).

On the Formalist view, the numerals and other signs of a system of arithmetic can have no reference, as far as arithmetic itself is concerned: "If their reference were considered, the ground for the rules would be found in these same references …" (Frege 1964, II § 90). What is most important, for present purposes, is Frege's conception of how the references of the expressions ground the rules:

> The question, 'What is to be demanded of numbers in arithmetic?' is, says Thomae, to be answered as follows: In arithmetic we require of numbers only their signs, which, however, are not treated as being signs of numbers, but solely as figures; and rules are needed to manipulate these figures. We do not take these rules from the reference of the signs, but lay them down on our own authority, retaining full freedom and acknowledging no necessity to justify the rules. (Frege 1964, II § 94)

Thus, not only do the references of the signs ground the rules that govern them, but, unless we are Formalists, we must recognize an obligation to justify these rules, presumably by showing that they are grounded in the references of the signs. Frege elsewhere specifies what condition rules of inference, in particular, must be shown to satisfy:

> Whereas in Significant arithmetic equations and inequations are sentences expressing thoughts, in Formal arithmetic they are comparable with the positions of chess pieces, transformed in accordance with certain rules without consideration for any sense. For if they were viewed as having a sense, the rules could not be arbitrarily stipulated; they would have to be so chosen that, from formulas expressing true thoughts, only formulas likewise expressing true thoughts could be derived. (Frege 1964, II § 94)

Thus, the rules of inference in a system of Significant arithmetic must be truth-preserving. And this condition—that the rules should be truth-preserving—is not arbitrarily stipulated, either. It follows from arithmetic's ambition to contribute to the growth of knowledge:

> If in a sentence of Significant arithmetic the group '3 + 5' occurs, we may substitute the sign '8' without changing the truth-value, since both signs designate the same object, the same actual number, and therefore everything which is true of the object designated by '3 + 5' must be true of the object designated by '8'. … It is therefore the goal of knowledge that determines the rule that the group '3 + 5' may be replaced by the sign '8'. This goal requires the character of the rules to be such that, if in accordance with them a sentence is derived from true sentences, the new sentence will also be true. (Frege 1964, II § 104)

Derivation must preserve truth, for only if it does, and only if the axioms are themselves true, will the theorems of the system be guaranteed to express true thoughts; it is only because the thoughts expressed by these formulas are true—and, indeed, are known to be true—that their application contributes to the growth of knowledge, rather than producing a mere "illusion of knowledge" (see Frege (1964, II §§ 92, 140)).[36]

Since Frege is interested in developing a system of Significant arithmetic, he in particular owes some account of why the rules of the Begriffsschrift are non-arbitrary, that is, a demonstration that they are truth-preserv-

36. Note that Frege is arguing here not only that the rules are required to be truth-preserving if arithmetic is to deliver knowledge but, conversely, that the substitution of terms having the same reference is permissible because the goal of arithmetic is knowledge. Substitution of co-referential terms—indeed, even of terms with the same sense—is not permitted everywhere: It is not permitted in poetry or in comedy, for example.

ing (and a similar demonstration that its axioms are true). Unless Frege flagrantly failed to do just what he is criticizing the Formalists for failing to do, he must somewhere have provided such an account. There is no option but to suppose that he does so in Part I of *Grundgesetze* and that the elucidatory demonstrations in particular are intended to show that the rules of the system are truth-preserving and that the axioms are true. Indeed, since Frege himself speaks of a need to justify the rules and of their being grounded in the references of the signs, we may dispense with our euphemism and speak, not of elucidatory demonstrations, but of Frege's *semantical justifications* of the axioms and rules.

3. *Frege's semantical justifications*

I have argued that Frege's semantical justifications of the axioms and rules of his system are intended to establish that, under the intended interpretation of the Begriffsschrift—this being given by the semantical stipulations governing the primitive expressions—its axioms are true and its rules are truth-preserving. But, according to Ricketts, they cannot have been intended to serve this purpose, because Frege's "conception of judgment precludes any serious metalogical perspective" from which he could attempt to justify his axioms and rules (Ricketts 1986b, p. 76).[37] His philosophical views "preclude ineliminable uses of a truth-predicate, including uses in bona fide generalizations", such as would be necessary were one even to be able to *say* that a rule of inference is valid. Ricketts is

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37. Van Heijenoort goes so far as to claim that Frege's rules of inference "are void of any intuitive logic" (van Heijenoort 1967, p. 326). But Frege simply spends too much time explaining the intuitive basis for his rules for this claim to be plausible; and, if that weren't enough, if the point were correct, it would make Frege a formalist.

The following passage is often cited as expressing Frege's opposition to meta-perspectives:

> We have already introduced a number of fundamental principles of thought in the first chapter in order to transform them into rules for the use of our signs. These rules and the laws whose transforms they are cannot be expressed in the Begriffsschrift, because they form its basis. (Frege 1967, § 13)

But it would be absurd for Frege to suggest that the *axioms* cannot be expressed in Begriffsschrift. He is speaking here simply of *rules*, in particular, of rules of inference, and noting that they cannot be so expressed: In fact, in the first chapter, Frege does not introduce any of his system's axioms but only its rules. He goes on to explain that he is out, in the second chapter, to find axioms from which all "judgements of pure thought" will follow by means of those rules. In the passage quoted, then, Frege is simply making the distinction between rules and axioms, not expressing his alleged opposition to meta-perspectives.

not, of course, unaware of what goes on in Part I of *Grundgesetze*, but he claims that Frege's sole purpose in Part I is to[38]

> teach his audience Begriffsschrift. Frege's stipulations, examples, and commentary function like foreign language instruction to put his readers in a position to know what would be affirmed by the assertion of any Begriffsschrift formula. The understanding produced by Frege's elucidatory remarks should have two immediate upshots. First, it should lead to the affirmation of the formulas Frege propounds as axioms; second, it should prompt the appreciation of the validity of the inference rules Frege sets forth. (Ricketts 1986a, pp. 176–7)

Frege's elucidations thus enable his reader to know what is expressed by any Begriffsschrift formula; so knowing, the reader can determine whether the formulae expressing the axioms are true by asking herself whether she is prepared to assert what they express. She may be aided by Frege's examples, commentary, and so forth, but this heuristic purpose is the only purpose the elucidations serve: The semantical justifications are not demonstrations of the truth of the axioms, nor of the validity of the rules, but are meant to *persuade*.

But it is unclear why, if Frege's only purpose were to teach his audience Begriffsschrift, he should make use of such notions as that of an object, or of a truth-value, or of reference, and why his 'explanations' should be, in the usual sense, compositional. It would do as well (and be far simpler) just to explain how to *translate* a proposition of Begriffsschrift into English (or German).[39] But Frege does not say simply that '$\Gamma = \Delta$' expresses the thought that Γ is the same as Δ: He says that it "shall denote the True if Γ is the same as Δ [and] in all other cases … shall denote the False" (Frege 1964, I § 7). One might reply that natural languages do not perspicuously express what Frege wishes to express in the Begriffsschrift. But while this is fine so far as it goes, it suggests merely that some technical vocabulary might be needed to 'teach Begriffsschrift'. It does not explain why that vocabulary should be semantical.

Moreover, Frege's semantical justifications become a great deal more

38. I have capitalized "Begriffsschrift" in both occurrences. I do not have the space to consider Ricketts's reasons for ascribing this view to Frege, but see Stanley (1996), Tappenden (1997), and Burge (2005b) for extended discussion.

39. The contrast between a semantic theory and a translation manual is, of course, emphasized in Davidson (1984, pp. 129–30). And surely the contrast is obvious to anyone who has taught introductory logic. It is one thing to teach students how to 'read' logical notation and another to show them a semantics for it.

complicated than those cited so far, particularly in cases in which free variables—which he calls Roman letters—occur in the premises and conclusion of an inference.[40] But this has been obscured by an almost universal misunderstanding of Frege's use of Roman letters. I just said that they are free variables, but it is widely held that there really aren't any free variables in Begriffsschrift: that Roman letters are tacitly bound by invisible initial universal quantifiers. Frege does say that the scope of a Roman letter "shall comprise everything that occurs in the proposition" (Frege 1964, I § 17), which amounts to his stipulating that a formula containing free variables is true just in case its universal closure is true. But he rejects the interpretation of Roman letters as tacitly bound almost immediately thereafter:

> Our stipulation regarding the *scope* of a *Roman letter* is to set only a lower bound upon the scope, not an upper bound. Thus it remains permissible to extend such a scope over several propositions, and this renders the Roman letters suitable to do duty in inferences, which the Gothic letters, with the strict closure of their scopes, cannot. If we have the premises '$\vdash x^2 = 1 \rightarrow x^4 = 1$' and '$\vdash x^4 = 1 \rightarrow x^8 = 1$' and infer the proposition '$\vdash x^2 = 1 \rightarrow x^8 = 1$', in making the transition we extend the scope of the '*x*' over both of the premises and the conclusion, in order to perform the inference, although each of these propositions still holds good apart from this extension. (Frege 1964, I § 17)

There is, for Frege, an important difference between a proposition of the form '$\Phi(x)$' and its universal closure '$\forall x \Phi(x)$'.[41] The nature of this difference, however, is puzzling: What could Frege mean by saying that, in making certain inferences, we must "extend the scope of the '*x*' over

40. The interpretive claims made in the remainder of this section and the next are developed in more detail, and defended, in Heck (1998a). That paper limits itself to discussion of the technical details of Frege's arguments in §§ 29–32 and does not, as the present paper does, discuss the bearing of my interpretation on questions about Frege's conception of logic. This paper and that one are therefore companion pieces, to some extent, although the discussion here is independent of the messy details encountered there. A more unified discussion will appear in a book on *Grundgesetze* now in preparation.

41. Compare this remark: "Now when the scope of the generality is to extend over the whole of a sentence closed off by the judgement stroke, then as a rule I employ Latin letters. … But if generality is to extend over only part of the sentence, then I adopt German letters. … Instead of the German letters, I could have chosen Latin ones here, just as Mr. Peano does. But from the point of view of inference, generality which extends over the content of the entire sentence is of vitally different significance from that whose scope constitutes only a part of the sentence. Hence it contributes substantially to perspicuity that the eye discerns these different roles in the different sorts of letters, Latin and German" (Frege 1984c, p. 378; I have altered the translation slightly).

both of the premises and the conclusion"? Surely he cannot mean that something like

$$\forall x\{(\vdash x^2 = 1 \rightarrow x^4 = 1) \wedge (\vdash x^4 = 1 \rightarrow x^8 = 1) \rightarrow (\vdash x^2 = 1 \rightarrow x^8 = 1)\}$$

is supposed to be well-formed!

Frege is concerned here with what licenses us to make the inference under discussion. There is a rule in his system, rule (7), that permits it.[42] That rule—transitivity for the conditional—allows the inference from '$\Delta \rightarrow \Gamma$ ' and '$\Theta \rightarrow \Delta$' to '$\Theta \rightarrow \Gamma$ '. But if Roman letters were treated as tacitly bound, rule (7) would not apply: Rule (7) does not allow an inference from '$\forall x(x^2 = 1 \rightarrow x^4 = 1)$' and '$\forall x(x^4 = 1 \rightarrow x^8 = 1)$' to '$\forall x(x^2 = 1 \rightarrow x^8 = 1)$'. The point is not that this formal rule could not be made to apply: It can, if we introduce a notation in which initial universal quantifiers can be suppressed; some formal systems treat free variables in just that way. Nor is there any substantive worry about whether the inference is in fact valid. Rather, the problem is that we are at present without any argument that inferences of this form *are* valid when the premises and conclusion contain free variables:[43] The semantical justification of rule (7) given in section 15 of *Grundgesetze* (and quoted earlier) did not allow for the possibility that 'Γ', 'Δ', and 'Θ' might contain free variables. That justification, which is essentially a justification in terms of truth-tables, presupposes that 'Γ', 'Δ', and 'Θ' *have truth-values* and, moreover, that the truth-values they have, when they occur in one premise, are the same as those they have when they occur in the other or in the conclusion. Only if we may speak of the truth-value of the occurrence of '$x^2 = 1$' in the first premise, and only if it has the same truth-value in all of its occurrences, will the justification apply. And we cannot so speak.

Nowadays, what we would say is that the inference is valid because, whenever we make a simultaneous assignment of objects to free variables in the premises and the conclusion, the usual argument on behalf of transitivity—the argument in terms of truth-tables—still goes through, if we replace occurrences of 'true' with occurrences of 'true under that assignment': That is to say, that argument can be adapted to show that, if

42. The rules of the system are listed in Frege (1964, I § 48).

43. It is worth emphasizing that free variable reasoning is distinctive of Frege's new logic (polyadic quantification theory). There is no need for such reasoning in syllogistic logic (which is not to deny that monadic quantification theory can be formulated as a sub-theory of polyadic).

the premises are both true under a given assignment, the conclusion must also be true under that same assignment. When Frege says that the scope of 'x' is to be "extend[ed] … over several propositions", he is attempting to express the relevant notion of simultaneous assignment: The idea is that, as we perform the inference, we treat the variable as 'indicating' (as Frege puts it) the same object in every one of its occurrences, whether in one of the premises or in the conclusion.

What Frege has said to this point speaks only to the notion of simultaneity and not to the notion of an assignment itself. But what follows the passage just discussed are further remarks on the nature of free variables and inferences involving them, including rule (5) of the Begriffsschrift, the rule of universal generalization:

> A Roman letter may be replaced at all of its occurrences in a proposition by one and the same Gothic letter. … The Gothic letter must then at the same time be inserted over a concavity in front of a main component outside which the Gothic letter does not occur. (Frege 1964, I § 48)

Decoding Frege's terminology, what the rule says is that one can infer '$A \to \forall x B(x)$' from '$A \to B(x)$' if 'x' is not free in A.[44] Frege's semantical justification of this rule is contained in section 17 of *Grundgesetze* and is in three stages. First, he notes that '$\Gamma \to \Phi(x)$' is equivalent to '$\forall x[\Gamma \to \Phi(x)]$', since a formula containing a Roman letter is true just in case its universal closure is true. Secondly, he argues that, if 'x' is not free in Γ *and no other variables are free in either* Γ *or* $\Phi(x)$, then '$\forall x[\Gamma \to \Phi(x)]$' is equivalent to '$\Gamma \to \forall x \Phi(x)$': That is, he shows, by means of what is now a familiar argument, that '$\forall x(p \to Fx)$' is equivalent to '$p \to \forall x(Fx)$'. The final stage of the argument is contained in the following passage:

> If for 'Γ' and '$\Phi(x)$', combinations of signs are substituted that do not refer to an object and a function respectively, but only indicate, because they contain Roman letters, then the foregoing still holds generally if for each Roman letter a name is substituted, whatever this may be. (Frege 1964, I § 17)

It is important to see how odd this final stage of the argument is. What Frege wants to show is that, if 'x' is not free in A, then '$\forall x(A \to B(x))$' is equivalent to '$A \to \forall x(B(x))$'. But what he says is that, if we substitute

44. A Roman letter is a free variable; a Gothic letter, a bound one; and the concavity, the universal quantifier. To say that the quantifier must appear "in front of a main component outside which the Gothic letter does not occur" is to say that it need not contain the antecedent of the conditional in its scope if the Roman letter in question does not occur in the antecedent.

names for all free variables, other than '*x*', in *A* and *B*(*x*), the argument that establishes that '$\forall x(p \rightarrow Fx)$' is equivalent to '$p \rightarrow \forall x(Fx)$' will go through.

It is not immediately obvious why that should suffice. What we would say nowadays is that, if we make a simultaneous assignment to the free variables other than '*x*' in *A* and *B*(*x*), that same argument will go through, 'true' again being replaced by 'true under the assignment'. The only difference between this argument and Frege's is that, where we speak of assignments, he speaks of substitutions. Frege does not, however, mean to speak here of substitutions of *actual terms* of Begriffsschrift for the variable,[45] but of *auxiliary names* assumed only to denote some object in the domain. What Frege is assuming, in the argument at which we have just looked, is that the inference from '$\varphi(x)$' to '$\psi(x)$' will be valid just in case $\psi(\Delta)$ is true whenever $\phi(\Delta)$ is true, Δ being a name new to the language and subject only to the condition that it must denote a member of the domain. This idea can be made precise: Applied to quantification, it constitutes a coherent alternative to Tarski's treatment in terms of satisfaction.[46]

It is a mark of the depth of Frege's understanding of logic that he realized that the presence of free variables in the language implies that the validity of rules of inference belonging to its *propositional* fragment—rules like *modus ponens* and transitivity for the conditional—cannot be justified simply in terms of the truth-tables. It is all the more remarkable that, in thinking about this problem, Frege was led to produce this alternative to Tarski's treatment of the quantifiers. And I, for one, find it hard to believe that the arguments at which we have just looked are but part of an attempt to 'teach Begriffsschrift'. The argument Frege gives in favor of the validity of universal generalization is surely not intended merely to encourage the reader not to object to the applications he makes of it. If that were all he wanted, he could have had it far more easily.

45. If he were so to speak, the argument would show only (to put the point in Tarskian language) that the conclusion is true under any assignment that makes the premise true and that assigns objects *denoted by terms in the language* to the free variables. Compare Dummett (1981a, p. 17). For discussion of how Frege's argument leads to the conclusion that the inference is valid, see Heck (1998a, p. 446).

46. See Heck (1998a, appendix) for a sketch of such a theory and for references. A similar treatment of quantification is given in Benson Mates's textbook *Elementary Logic* (1972).

Matters become yet more complicated with Basic Law V.[47] The semantical stipulation governing the smooth breathing is not like the stipulations Frege gives for the other primitives: He does not directly stipulate what its reference is to be. Of course, it would not have been difficult for him to do so: He need only have said that a term of the form $\acute{\varepsilon}\Phi(\varepsilon)$ denotes the value-range of $\Phi(\xi)$;[48] he could then have argued that, since the value-range of $\Phi(\xi)$ is the same as the value-range of $\Psi(\xi)$ just in case the same objects fall under $\Phi(\xi)$ and $\Psi(\xi)$, Basic Law V holds. Now, in fact, Frege does consider such a stipulation at one point (Frege 1964, I § 9), but all we are told about value-ranges is that the value-range of the function $\Phi(\xi)$ is the same as that of $\Psi(\xi)$ just in case they have the same values for the same arguments (Frege 1964, I § 3). In effect, then, the only stipulation Frege makes about the smooth breathing—and, more importantly, the only one he uses in his arguments—is that $\acute{\varepsilon}\Phi(\varepsilon) = \acute{\varepsilon}\Psi(\varepsilon)$ has the same truth-value as $\forall x(\Phi(x) = \Psi(x))$. Frege (1964, I § 20) notes that the truth of Basic Law V follows immediately from this stipulation (or from the combined effect of those made in sections 3 and 9). But it will do so only if the stipulation is in good order, only if it suffices to assign a reference to the smooth breathing.

The problem, however, is that this stipulation does *not* directly assign a reference to the smooth breathing. And unless it somehow succeeds in doing so indirectly, Basic Law V cannot be justified in terms of it: Officially, the axiom ought then to be declared neither true nor false, on the ground that it contains an expression that has no reference. Frege therefore needs to argue that his stipulation, augmented by others to be mentioned shortly, does indeed secure a reference for the smooth breathing: That argument comprises most of section 31 of *Grundgesetze*. Had it been successful, Frege would have *proven* that Basic Law V is true in the intended interpretation of the system. That is why I said earlier that Frege could have had no real doubts about the *truth* of Basic Law V.[49]

47. The discussion in this section summarizes some of the results of earlier papers (Heck 1998a and 1999), which should be consulted for defenses of claims that are not defended here.

48. I'll write quotation marks and corner quotes with invisible ink in this section, to avoid cluttering the exposition.

49. Frege also speaks of the "legitimacy" of the semantical stipulation as having been "established once for all" and makes reference to his intention to "develop the whole wealth of objects and functions treated of in mathematics out of the germ of the eight functions whose

The question whether the smooth breathing has been assigned a reference is made pressing by the peculiar nature of the semantical stipulation governing it. But Frege still argues that a reference has been assigned to the other primitives of Begriffsschrift.[50] The complete argument of sections 30–31 has a more general conclusion: That the stipulations provide every well-formed expression with a reference—and not just *a* reference (that is, at least one reference) but a *unique* reference. Since Frege argues in section 31 that a reference has been assigned to each of the primitive expressions, he need only show that, if every primitive expression of the language has a reference, then every expression that can be formed from these primitives also has a reference. That argument, which is probably the first proof by induction on the complexity of expressions ever given, is contained in section 30. In fact, that section contains two things, which are not separated in Frege's exposition: A reasonably precise account of the syntax of Begriffsschrift and a demonstration that every expression correctly formed from referring expressions refers. Frege explains that complex names are formed by applying certain combinatorial operations to the primitive expressions of the language and that every name is formed by successive applications of these operations. This 'closure clause' serves to define the class of well-formed expressions by means of the ancestral and so implies the validity of proof by induction on the complexity of expressions:[51] It is this that allows Frege to argue that, if all primitive expressions of Begriffsschrift refer, then every well-formed expression refers, by arguing that the two ways of forming complex expressions from simpler ones preserve referentiality. The proof is not trivial: The argument that complex predicates such as '$\xi = \xi$' denote is both subtle and elegant.[52]

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names are enumerated in vol. I, § 31" (Frege 1964, II § 147). The primitive expressions of the Begriffsschrift are indeed listed in section 31, but it is hard to believe that Frege refers to it at this point simply for that reason: Rather, the argument given in section 31 is what shows that all of these expressions refer, and that is what *makes* them legitimate.

50. That Frege should argue for this claim contradicts Weiner's view that, for Frege, "no work is required to show that primitive terms have Bedeutung" (Weiner 1990, p. 129). To be sure, not much work is required to show that most of them refer, but a *lot* of work is required to show that the smooth breathing does.

51. Thus, Weiner's objection that the induction principle employed in this proof is never stated (Weiner 1990, p. 240) is met, since no special induction principle needs to be stated here.

52. A complex predicate is one formed by omitting occurrences of one term from another, leaving argument-places in its wake. Thus, one can form the complex predicate '$\xi = \xi$' by omitting both occurrences of 't' from '$t = t$'. See, again, Heck (1998a, pp. 439–40) for discussion of the argument.

Frege's argument that the smooth breathing denotes is complex and difficult to interpret. For present purposes, we do not need to discuss its details, but there is a feature of the argument that is worth mentioning. Frege takes it to be enough to prove:[53]

(I) If $\Phi(\xi)$ and $\Psi(\xi)$ denote, then $\acute{\varepsilon}\Phi(\varepsilon) = \acute{\varepsilon}\Psi(\varepsilon)$ denotes;
(II) If $\Phi(\xi)$ denotes, and if p denotes a truth-value, then $p = \acute{\varepsilon}\Phi(\varepsilon)$ denotes.

Claim (I) is supposed to follow from the semantical stipulation governing the smooth breathing, that $\acute{\varepsilon}\Phi(\varepsilon) = \acute{\varepsilon}\Psi(\varepsilon)$ has the same reference as $\forall x[\Phi(x) = \Psi(x)]$, the latter formula itself having a reference because the expressions from which it is constructed do. To establish (II), Frege needs to specify whether the truth-values are value-ranges and, if so, which ones they are: If they are not value-ranges, $p = \acute{\varepsilon}\Phi(\varepsilon)$ will always be false (and so will denote); if they are, then p will have the same reference as some expression of the form $\acute{\varepsilon}\Psi(\varepsilon)$, whence $p = \acute{\varepsilon}\Phi(\varepsilon)$ will have the same reference as some sentence of the form $\acute{\varepsilon}\Psi(\varepsilon) = \acute{\varepsilon}\Phi(\varepsilon)$, and case (II) will reduce to case (I). In section 10, Frege argues that it is consistent with the other semantical stipulations that the truth-values are their own unit classes and then stipulates that they are.

It is often said that Frege needs to make this stipulation because he requires every predicate to denote a total function, one that has a value for every argument. This is right, but we are now in a position to appreciate the reason for this requirement: It is imposed by the purpose of the proof being given in section 31 and, more generally, by the fact that Begriffsschrift is supposed to have a classical semantics. The truth-values of complex sentences are specified in terms of the references of their simpler components, by means of the truth-tables and the usual sorts of (objectual) stipulations for the quantifiers. If $\Delta = \acute{\varepsilon}\Phi(\varepsilon)$ did not have a reference when Δ denotes a truth-value, $\forall x(x = \acute{\varepsilon}\Phi(\varepsilon))$ would not have a reference, and the argument would collapse.[54] The stipulation that the truth-values are their own unit

53. What Frege needs to show is that $\Delta = \acute{\varepsilon}\Phi(\varepsilon)$ denotes, so long as Δ and $\Phi(\xi)$ do. His assumption that these two cases are the only ones that need to be considered involves a tacit restriction of the domain to truth-values and value-ranges. (Thus, the oft-heard claim that, for Frege, the quantifiers always have an unrestricted range is false: see Heck (1999, pp. 271–4)). If the domain contains only such objects, then each of them is either the value of p, for some assignment of a truth-value to p, or the reference of $\acute{\varepsilon}\Phi(\varepsilon)$, for some assignment of a function to $\Phi(\xi)$, since every value-range is the value-range of some function.

54. And its reason for collapsing would be quite independent of whether any *term* of the

classes thus plays an essential role in Frege's proof that every well-formed expression denotes, and it is not mentioned outside section 10, where it is made, and section 31, where it is applied, except in a handful of sections that themselves refer to one of these. In particular, the stipulation *is not embodied in the axioms and rules of the Begriffsschrift*. The sentence stating that the truth-values are their unit classes is neither provable nor refutable in the Begriffsschrift, as Frege essentially shows in section 10. Of course, Frege could have adopted additional axioms embodying the stipulation. But he doesn't bother, because the reason he needs to make the stipulation has nothing to do with the syntax of the formal theory but rather concerns its semantics.[55]

The purpose of sections 30–31 is thus to prove[56] that every well-formed expression in Begriffsschrift refers (and, in particular, that the smooth breathing does). It follows (or would follow, were the argument not fatally flawed) that Basic Law V is true and, moreover, that the system is consistent, since all axioms of the theory are true, the rules are truth-preserving—whence every theorem has the value True—and there is a sentence, e.g., '$\forall x(x \neq x)$', that is assigned the value False by the stipulations and so is not a theorem. As we have seen, the argument makes heavy use of semantical notions, in particular, of the notion of reference. Moreover, although the argument that the smooth breathing refers is flawed, there is nothing wrong with the remainder of the proof: The remainder of sections 30–31 constitutes a *correct* proof that the semantical stipulations governing the primitive logical expressions suffice to assign each of them a unique reference and so to assign a unique reference to every expression properly formed from them. Since the semantical justifications really do show that the axioms and rules of the Begriffsschrift, other than Basic Laws V and VI, are true and truth-preserving, respectively, Part I of *Grundgesetze* con-

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language—let alone any primitive term—denotes a truth-value (Heck 1999, p. 272).

55. Parallel remarks could be made about Frege's stipulation, in section 11, concerning the references of improper descriptions: It too is embodied in no axiom or rule of the system.

56. Weiner has argued that "there are serious obstacles to reading sections 28–31 as the presentation of a proof" (Weiner 1990, p. 240). She notes that the conclusion of the proof is not used in Frege's proofs of the axioms of arithmetic (Weiner 1990, p. 242). But the proof is meta-theoretic: Its conclusion is a claim *about* Begriffsschrift; there is no reason that appeal need be made to it in later proofs. She also says that the argument does not meet the standards for "a metatheoretic proof in an introductory logic course" (Weiner 1990, p. 240). But it should not be surprising if Frege is unclear about the conceptual underpinnings of the argument, since it is likely the first meta-theoretic argument ever given. And, with the exception of the failed proof that the smooth breathing denotes, I'd have to disagree: It's a *very* sophisticated proof, especially the part concerning complex predicates.

tains a *correct* proof that the logical fragment of the Begriffsschrift—that is, Frege's formulation of second-order logic—is sound, that is, that all of its theorems are true.[57]

5. *Closing*

We have thus seen that, in *Grundgesetze*, Frege gives a number of arguments whose purpose is to show that the axioms and rules of the Begriffsschrift are, respectively, true and truth-preserving. There are the semantical justifications of the axioms and rules, found scattered throughout Part I; and there is the argument of sections 30–31, which is supposed to show that every well-formed expression has a reference. These arguments have explicitly semantical conclusions, and they make heavy use of semantical notions. Their character makes it extremely unlikely that they are intended merely as a peculiar sort of foreign language instruction. Such oft-heard claims as that "Frege never raises any metasystematic question" (van Heijenoort 1967, p. 326) or, more strongly, that "metasystematic questions as such … could not meaningfully be raised" by him (Dreben and van Heijenoort 1986, p. 44) seem simply to be wrong.

One might yet question how seriously these apparently semantical arguments are to be taken, on the ground that, if they are to be understood as 'properly scientific', rather than as merely 'elucidatory', they would have to be formalizable in the Begriffsschrift itself. And perhaps, for some reason or other, Frege would have denied that semantical arguments *could* be formalized in the Begriffsschrift. But why?[58] Of course, it follows from Tarski's theorem on the indefinability of truth that, since the Begriffsschrift formalizes a classical theory sufficient for arithmetic, it is inconsistent if its own truth-predicate is definable in it. But *Frege* had no reason to think this and so no reason to think that the semantical arguments he gives in

57. And note again that this proof does not depend upon any assumptions about what is in the domain. Granted, then, that all of Frege's 'logical' primitives really are *logical* primitives, whose interpretation may remain fixed, the proof shows that all of the system's theorems are *logically valid*, not just true, where the notion of 'logical validity' here is the one we have inherited (more or less) from Tarski. But see below for some warnings about how far this point can be extended.

58. One might have thought that the concept horse problem would pose technical difficulties: But that problem does not arise when the argument is carried out in a higher-order formal theory, but only when one is attempting to talk about the semantics of Begriffsschrift in natural language.

Grundgesetze could not be formalized in the Begriffsschrift. Indeed, the natural view would surely have been that such reasoning can be reproduced within the Begriffsschrift—as, indeed, it can.[59] So the Begriffsschrift is inconsistent. Again.

Such terms as 'metalogical perspective', 'semantical metaperspective', and 'metasystematic standpoint'—these being the buzzwords of a now familiar tradition in Frege scholarship—are deeply misleading:[60] There is an almost subliminal suggestion that semantical reasoning requires a perspective beyond the Begriffsschrift, that such reasoning *cannot* be carried out within it. But the mere fact that the conclusion of an argument concerns the semantical properties of a particular theory does not show that it cannot be formalized within it: Though not all arguments for semantical claims concerning Peano Arithmetic can be formalized within Peano Arithmetic, many can be.[61] Nor are semantical claims about PA the only ones that cannot be proven in PA: That PA is consistent is a *syntactic* claim, purely syntactic proofs of which (for example, Gentzen's) cannot be carried out in PA.

But we do need to be careful here. Ricketts claims, at one point, that "anything like formal semantics, as it has come to be understood in the light of Tarski's work on truth, is utterly foreign to Frege" (Ricketts 1986b, p. 67). This claim I think I have shown to be untenable. But I have not argued that formal semantics, as it has come to be understood in the light of Tarski's work on *logical consequence*, is not foreign to Frege. The mathematical work of Frege's at which we have looked is concerned with such questions as whether the axioms are *true*, or whether the rules are *truth-preserving*, or whether the primitive expressions of Begriffsschrift *refer*. None of the work at which we have looked was intended to address such questions as whether the axioms are *logically* true or the rules are *logically* valid. And although I have argued that Frege ought to have been,

59. Tarski shows us how to formulate a definition of truth for a second-order language in a third-order language. But Basic Law V can be used to reduce quantification over third-level concepts to quantification over second-level concepts—or, indeed, over objects.

60. Jamie Tappenden (1997) has well documented the extent to which certain forms of argument have become something akin to secret handshakes among the members of this tradition. The terms I've just mentioned are among those that signal the occurrences of such arguments.

61. For example, a materially adequate definition of truth for Σ_n sentences, for any *n* you like, can be formulated within PA, and using these definitions one can then give a semantical proof in PA of the consistency of Σ_n arithmetic, for every *n*. (Inconsistency is averted because PA gives us no way to paste all these definitions together into a definition of truth for the whole of the language of arithmetic.) Or again: PA proves that Q (and therefore PA) proves every true Σ_1 sentence.

and was, interested in these questions, it is unclear whether he thought mathematical work might bear upon them, let alone whether he would have accepted Tarski's characterization of the notion of logical consequence (or some alternative).[62] Though there are indications that, a few years after the publication of *Grundgesetze*, Frege was beginning to think about logical consequence in mathematical terms,[63] we do not, in my opinion, yet know enough to decide this interpretive issue.[64]

REFERENCES

Boolos, George and Heck, Richard G., 1998. *Die Grundlagen der Arithmetik* §§ 82–83. In: *Logic, Logic, and Logic*. Cambridge MA: Harvard University Press, 315–338.

Burge, Tyler, 1984. Frege on extensions of concepts from 1884 to 1903. *Philosophical Review* 93, 3–34. Reprinted in: Burge (2005a), 273–98.

— 1998. Frege on knowing the foundation. *Mind* 107, 305–347. Reprinted in: Burge (2005a), 317–55.

62. It is worth remembering that some contemporary philosophers have also rejected Tarski's characterization of consequence, notably, John Etchemendy (1990).

63. The relevant discussion is in Frege (1984e, Part III). For discussion of these passages, see Ricketts (1997) and Tappenden (1997).

64. I am fortunate to have had very helpful comments on various drafts of this paper: Thanks are due to George Boolos, Tyler Burge, Warren Goldfarb, Michael Kremer, Ian Proops, Thomas Ricketts, Alison Simmons, and Joan Weiner. And thanks, too, to Dirk Greimann for suggesting the paper be included in the present volume.

I have two *very* large debts, which I decided not to acknowledge at every point at which they were felt, as that would have cluttered the paper. The first is owed to Jamie Tappenden. While he was visiting at Harvard, during the 1994–95 academic year, we had an extraordinarily fruitful, year-long discussion about Frege and, in particular, about the issues with which this paper is concerned. Those conversations were crucial to the development of my views on these matters. The second such debt is to Jason Stanley. Much of the first half of this paper was born in conversation with him. It is difficult to remember which ideas originated with whom, and so he deserves some of the credit for what may be of value here and all of the blame for anything that may not be.

The first draft of this paper was written in the summer of 1995; it reached essentially its current form in the summer of 1997. That the paper has remained unpublished for nearly a decade is due to circumstances over which I had no control. So, if the paper seems a little out of date, that is why. I did once consider trying to take more serious account of papers published or written since, but it quickly became clear that I would essentially have to re-write the entire paper. I have therefore added references to some more recent work but otherwise have chosen to be silent. That silence should not itself be interpreted.

— 2005a. *Truth, Thought, Reason: Essays on Frege*. Oxford: Clarendon Press.
— 2005b. Postscript to 'Frege on Truth'. In: Burge (2005a), 133–52.
Davidson, Donald, 1984. Radical interpretation. In: *Inquiries Into Truth and Interpretation*. Oxford: Clarendon Press, 125–139.
Dedekind, Richard, 1963. The nature and meaning of numbers. In: *Essays on the Theory of Numbers*. Dover: New York. Tr. by W.W. Beman.
Dreben, Burton, and van Heijenoort, Jean, 1986. Introductory note to 1929, 1930, and 1930a. In: S. Feferman, J. Dawson, and St. Kleene, eds. *Collected Works*, volume 1. Oxford: Oxford University Press, 44–59.
Dummett, Michael, 1981a. *Frege: Philosophy of Language*. 2d edition. Cambridge MA: Harvard University Press.
— 1981b. *The Interpretation of Frege's Philosophy*. Cambridge MA: Harvard University Press.
— 1991. *The Logical Basis of Metaphysics*. Cambridge MA: Harvard University Press.
Etchemendy, John, 1990. *The Concept of Logical Consequence*. Cambridge MA: Harvard University Press.
Frege, Gottlob, 1962. *Grundgesetze der Arithmetik*. Hildesheim: Georg Olms Verlagsbuchhandlung. Translations of Part I are based upon Frege (1964); of the prose parts of Part III, upon Frege (1970).
— 1964. *The Basic Laws of Arithmetic: Exposition of the System*. M. Furth, ed. and tr. Berkeley CA: University of California Press.
— 1967. Begriffsschrift: A formula language modeled upon that of arithmetic, for pure thought. J. van Heijenoort, tr. In: J. van Heijenoort, ed. *From Frege to Gödel: A Sourcebook in Mathematical Logic*. Cambridge MA: Harvard University Press, 5–82.
— 1970. *Translations from the Philosophical Writings of Gottlob Frege.* P. Geach and M. Black, eds. Oxford: Blackwell.
— 1979a. Logic (1897). In: Frege (1979c), 126–151.
— 1979b. Logic in mathematics. In: Frege (1979c), 203–250.
— 1979c. *Posthumous Writings.* H. Hermes, F. Kambartel, and F. Kaulbach, eds. P. Long and R. White, trs. Chicago: University of Chicago Press.
— 1980. *The Foundations of Arithmetic*. 2d edition. J. Austin, tr. Evanston IL: Northwestern University Press.
— 1984a. *Collected Papers on Mathematics, Logic, and Philosophy*. B. McGuiness, ed. Oxford: Basil Blackwell.
— 1984b. Formal theories of arithmetic. In: Frege (1984a), 112–21.
— 1984c. On Mr. Peano's conceptual notation and my own. In: Frege (1984a), 234–248.
— 1984d. On sense and meaning. In: Frege (1984a), 157–177.

— 1984e. On the foundations of geometry: Second series. In: Frege (1984a), 293–340.
— 1984f. Review of Georg Cantor, *Zur Lehre vom Transfiniten*. In: Frege (1984a), 178–81.
Goldfarb, Warren, 1979. Logic in the twenties. *Journal of Symbolic Logic* 44, 351–68.
— 2001. Frege's conception of logic. In: J. Floyd and S. Shieh, eds. *Future Pasts: The Analytic Tradition in Twentieth-Century Philosophy*. New York: Oxford University Press, 25–41.
Heck, Richard G., 1997. The Julius Caesar objection. In: R.G. Heck, ed. *Language, Thought, and Logic: Essays in Honour of Michael Dummett.* Oxford: Clarendon Press, Oxford, 273–308.
— 1998a. *Grundgesetze der Arithmetik* I §§ 29–32. *Notre Dame Journal of Formal Logic* 38, 437–74.
— 1998b. The finite and the infinite in Frege's *Grundgesetze der Arithmetik.* In: M. Schirn, ed. *Philosophy of Mathematics Today.* Oxford: Oxford University Press, 429–466.
— 1999. *Grundgesetze der Arithmetik* I § 10. *Philosophia Mathematica* 7, 258–92.
Jeshion, Robin, 2001. Frege's notion of self-evidence. *Mind* 110, 937–76.
Linnebo, Øystein, 2004. Frege's proof of referentiality. *Notre Dame Journal of Formal Logic* 45, 73–98.
Mates, Benson, 1972. *Elementary Logic*. 2nd edition. Oxford: Oxford University Press.
McGee, Vann, 1985. A counter-example to modus ponens. *Journal of Philosophy* 82, 462-71.
Ricketts, Thomas, 1986a. Generality, sense, and meaning in Frege. *Pacific Philosophical Quarterly* 67, 172–95.
— 1986b. Objectivity and objecthood: Frege's metaphysics of judgement. In: L. Haaparanta and J. Hintikka, eds. *Frege Synthesized: Essays on the Philosophical and Foundational Work of Gottlob Frege*. Dordrecht: Reidel, 65–95.
— 1996. Logic and truth in Frege. *Proceedings of the Aristotelian Society* 70, 121-40.
— 1997. Frege's 1906 foray into meta-logic. *Philosophical Topics* 25, 169–88.
Sher, Gila, 1991. *The Bounds of Logic: A Generalized Viewpoint*. Cambridge MA: MIT Press.
Stanley, Jason, 1996. Truth and metatheory in Frege. *Pacific Philosophical Quarterly* 77, 45–70.
Tappenden, Jamie, 1995. Geometry and generality in Frege. *Synthese* 102, 319–61.

— 1997. Metatheory and mathematical practice in Frege. *Philosophical Topics* 25, 213–64.
— 2000. Frege on axioms, indirect proof, and independence arguments in geometry: Did Frege reject independence arguments? *Notre Dame Journal of Formal Logic* 41, 271–315.
Tarski, Alfred, 1958. The concept of truth in formalized languages. In: J. Corcoran, ed. *Logic, Semantics, and Metamathematics*. Indianapolis: Hackett, 152–278.
Van Heijenoort, Jean, 1967. Logic as calculus and logic as language. *Synthese* 17, 324–30.
Weiner, Joan, 1990. *Frege in Perspective*. Ithaca NY: Cornell University Press.