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Frege on Formality and the 1906 Independence-Test

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Abstract:
In 1906, Frege proposes a method, one which rests on the "formal" nature of logical laws, for proving mathematical independence claims. There are many curious features of the 1906 proposal, including the fact that Frege seems subsequently to have found it unacceptable. This essay explores Frege's proposal, and rejection, of the 1906 independence-test, with the goal of clarifying Frege's understanding of the nature of logical entailment and of the "formal" nature of logical laws.

§0. Introduction

In 1906, Frege proposes a general procedure for demonstrating the independence of a given (mathematical) claim from others.[1] Three features of this proposal are worth noting. First, the proposal follows hard on the heels of seven years' worth of harsh criticism, on Frege's part, of independence-proofs. Since the appearance of Hilbert's geometrical independence-proofs in 1899, Frege has been uniformly critical of all such proofs. The 1906 discussion is the first instance of any positive proposal on Frege's part for demonstrating independence, and is the first indication he gives that he takes independence to be demonstrable at all. The second striking feature of the 1906 proposal is that it is not just Frege's *first* positive discussion of a potential means of demonstrating independence; it is his only such discussion. Frege never again returns to the procedure proposed here, and indeed, as far as we can tell, never so much as refers to it again. This is not because Frege was satisfied with the 1906 discussion or considered the issue resolved. The proposal made there was, as Frege says, very tentative and without his own usual high standards of care and rigor. Far from being satisfied with this preliminary discussion of the independence test, Frege seems on reflection to have taken the test to be entirely unsatisfactory. Despite later discussions of independence, Frege never returns to the 1906 proposal, and in 1910 claims that the independence of the

[1] Frege, Gottlob [1906]

axiom of parallels - the very kind of thing which was presumably to have been demonstrated by the proposed method - "cannot be proved."[2] The third noteworthy feature of the proposed 1906 independence-test is that it bears a striking similarity both to our own, standard means of proving independence today, and to Hilbert's method, the one relentlessly criticized by Frege from 1900-1906, even in the very essay in which he proposes his own technique. This similarity makes it difficult to understand what Frege had in mind when he proposed the 1906 test. Why, in particular, didn't his criticisms of Hilbert apply immediately and obviously to his own proposal? And why, having presumably thought that his own method did not fail in this immediate way, did Frege later abandon it?

What follows is intended as a contribution to the understanding of Frege's conception of logic by way of coming to understand what lies behind Frege's proposal of, and his apparent subsequent rejection of, the 1906 independence-test.

§1. The Proposal

Frege's proposal occurs in the final section of the 1906 "On the Foundations of Geometry," his second essay-series of that title. The bulk of the essay consists of a criticism of Hilbert's consistency- and independence-results, a criticism continued from the 1899 - 1900 correspondence with Hilbert, and the 1903 "On the Foundations of Geometry."[3] Following the criticism of Hilbert's specific style of independence-proof, Frege at last turns to the question of whether independence can be demonstrated at all.

To begin with, Frege clarifies what he means by "independence." The conception is relatively straightforward: a given thought is independent of a collection of thoughts if that thought can't be obtained by a (presumably finite) series of steps of logical inference from the thoughts in that collection.[4] That it's *thoughts* in question, rather than sentences, is crucial for Frege. As he sees it, the question of logical

[2] [1910] 183n. The claim that Frege's 1910 discussion constitutes a rejection of the proposed 1906 test is not uncontroversial. For a contrary view, see Jamie Tappenden [2000]: for reply, see §2 below.
[3] See the correspondence with Hilbert, English translation in [1980] 31-52, and [1903].
[4] See [1906] 423-4/334. Frege isn't explicit about the finitude, but the context would seem to indicate that he takes the series of inferential steps to be one that we could in principle complete. Nothing in what follows will turn on the assumption that he presumes the number of inferences to be finite. As a reminder: *thoughts*, for Frege, are the nonlinguistic propositions expressed by sentences; they are not mental entities.

entailment, and hence of independence, arises only for thoughts, i.e. for the kinds of things that have a definite truth-value, and are not subject to reinterpretation in the way that sentences are. As Frege puts it,

> When one uses the phrase 'prove a proposition' in mathematics, then by the word 'proposition' one clearly means not a sequence of words or a group of signs, but a thought; something of which one can say that it is true. And similarly, when one is talking about the independence of propositions or axioms, this, too, will be understood as being about the independence of thoughts.[5]

The discussion of how one might *demonstrate* independence begins as follows:

> How can one prove the independence of a thought from a group of thoughts? First of all, it may be noted that with this question we enter into a realm that is otherwise foreign to mathematics. For although like all other disciplines mathematics, too, is carried out in thoughts, still, thoughts are otherwise not the object of its investigations. Even the independence of a thought from a group of thoughts is quite distinct from the relations otherwise investigated in mathematics. Now we may assume that this new realm has its own specific, basic truths which are as essential to the proofs constructed in it as the axioms of geometry are to the proofs of geometry; and that we also need these basic truths especially to prove the independence of a thought from a group of thoughts.[6]

Frege begins his sketch of the new discipline by laying down two straightforward "basic truths." These truths sound strange to a modern ear, since they follow from Frege's unusual view that the premises of an inference are always true, together with the standard view that logical inference is truth-preserving. The two principles are as follows:

(L1) If the thought G follows from the thoughts A, B, C by a logical inference, then G is true.
(L2) If the thought G follows from the thoughts A, B, C by a logical inference, then each of the thoughts A, B, C is true.

These laws will of course not get us very far in the investigation of independence claims, particularly when we are interested in the independence of a true thought from a group of true thoughts. As Frege says,

> But our aim is not to be achieved with these basic truths alone. We need yet another law which is not expressed quite so easily. Since a final settlement of the question is not possible here, I shall abstain from a precise formulation of this law and merely attempt to give an approximation of what I have in mind.[7]

The "approximation" Frege gives is as follows: Suppose we have a language L which is fully interpreted, in the sense that its sentences each express determinate thoughts. Suppose also that L is "logically perfect" in the sense that the replacement of a word in a given sentence by a word of the same syntactic category

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[5] Frege [1906] 401. See also Frege [1914] 206: "What we prove is not a sentence, but a thought. And it is neither here nor there which language is used in giving the proof."
[6][1906] 425-6 (336).
[7][1906] 426 (337).

always gives a new well-formed sentence, one which expresses a determinate thought. Consider now a function μ which maps words of L to words of L, and which both preserves syntactic type, and maps "logical" words to themselves. This function will then induce a map from sentences to sentences, and from whole arguments to whole arguments. Given an argument with true premises and a true conclusion, if the function μ maps this argument to a new argument which also has true premises but has a false conclusion, then we can conclude that the original argument's conclusion is independent of its premises. As Frege puts it,

> Let us now consider whether a thought G is dependent upon a group of thoughts Ω. We can give a negative answer to this question if, according to our vocabulary [i.e., according to the mapping μ], to the thoughts of group Ω there corresponds a group of true thoughts Ω', while to the thought G there corresponds a false thought G'. For if G were dependent upon Ω, then, since the thoughts of Ω' are true, G' would also have to be dependent upon Ω' and consequently G' would be true.[8]

We can abbreviate the test for independence as follows:

(L3) Consider a set P of premise-sentences and a conclusion-sentence C of a fully-interpreted, logically-perfect language L. Let μ be a mapping of the primitive vocabulary of the language to itself which meets the following conditions:

(i) μ preserves syntactic type; and

(ii) μ maps logical terms to themselves.

Consider now the set P' and sentence C' obtained from P and C by replacing each term with its image under μ. If each of the thoughts expressed by the members of P' is true, while the thought expressed by C' is false, then the thought expressed by C is independent of the thoughts expressed by the members of P.

Frege closes his discussion of the test on a cautious note, pointing out two difficulties: first, that of giving the proposed test with more precision, and second, that of distinguishing logical from non-logical vocabulary, as is essential in order to precisely specify the second requirement. He does not claim here that these difficulties are insurmountable or that they give rise to serious objections to the proposal. His attitude seems to be rather that there is more to be said, and that further investigation is required before the rule can be clearly formulated and applied.

Prior to suggesting his test for independence, while warning his audience that what he’s presenting is merely an “approximation” of what he has in mind, Frege notes that we can say of the central idea here that:

[8][1906] 428 (339).

"One might call it an emanation of the formal nature of logical laws."[9]

It's going to be important for what follows to have as clear an understanding as possible of what Frege means here by the "formal" nature of logical laws. Note, to begin with, that he does not mean anything like "syntactic." For Frege, logic is not essentially connected with syntax in any way; it's not essentially connected with the "forms" of sentences, or of any linguistic items. Logical inference, and hence the principles that govern that inference, have as their subject-matter, again, Fregean thoughts. Sentences, as merely the contingent means of expressing thoughts, are connected to principles of logic only via the thoughts they express. The kinds of syntactic derivation-rules one finds in a system like that of Frege's *Begriffsschrift* merely tell one how justifiably to derive sentences one from another, where that justification is given by (a) the fundamental logical principles linking thoughts one to another, and (b) the contingent choices we've made concerning how thoughts of various kinds are expressed by sentences of various kinds.

What, then, does Frege mean by the "formal" nature of logical laws? *En route* to explaining the importance of condition (ii) in (L3) above, Frege asks his audience to suppose a mapping μ meeting the conditions other than (ii) listed above, and gives a brief description of how the appeal to such a mapping when applying (the thus-minimized version of) (L3) can give rise to faulty judgments of independence. The demonstration is as follows. Consider a case in which we're presented on the left-hand side of a page a series of sentences expressing true premise-thoughts and a conclusion-thought that's logically entailed by those premise-thoughts, and on the right-hand side the sentences, expressing thoughts, that are induced by a mapping μ meeting condition (i) but not (ii).[10] Assume further that the premise-thoughts on the right are true. We can now ask, says Frege, whether the conclusion-sentence on the right "is the appropriate conclusion-sentence of the inference on the right."[11] That is, we ask whether we'll get, on the right-hand side, an argument whose conclusion-thought is entailed by its premise-thoughts in a way that mirrors the entailment on the left. (In order to see what's coming, note that by disregarding criterion (ii), our mapping

[9] [1906] 426/337
[10] Frege does not explicitly say that the conclusion on the left is entailed by the premises on the left. I take it from the surrounding discussion that this is what he intends.
[11] [1906] 427/338

might e.g. have mapped an "and" on the left-hand side to an "or" on the right, hence mangling, as one might put it, the logical structure of the argument.) Regarding this question, Frege remarks:

> One may now be tempted to answer our question in the affirmative, thereby appealing to the formal nature of the laws of logic according to which, as far as logic itself is concerned, each object is as good as any other, and each concept of the first level as good as any other and can be replaced by it; etc. But this would be excessively hasty, for logic is not as unrestrictedly formal as is here presupposed. If it were, then it would be without content. Just as the concept *point* belongs to geometry, so logic, too, has its own concepts and relations; and it is only in virtue of this that it can have a content. Toward what is thus proper to it, its relation is not at all formal. No science is completely formal; but even gravitational mechanics is formal to a certain degree, in so far as optical and chemical properties are all the same to it. To be sure, as far as it is concerned, bodies with different masses are not mutually replaceable; but in gravitational mechanics the difference of bodies with respect to their chemical properties does not constitute a hindrance to their mutual replacement. To logic, for example, there belong the following: negation, identity, subsumption, subordination of concepts. And here logic brooks no replacement. It is true that in an inference we can replace Charlemagne by Sahara, and the concept *king* by the concept *desert*, in so far as this does not alter the truth of the premises. But one may not thus replace the relation of identity by the lying of a point in a plane. Because for identity there hold certain logical laws which as such need not be numbered among the premises, and to these nothing would correspond on the other side. Consequently a lacuna might arise at that place in the proof. One can express it metaphorically like this: About what is foreign to it, logic knows only what occurs in the premises; about what is proper to it, it knows all. Therefore in order to be sure that in our translation, to a correct inference on the left there again corresponds a correct inference on the right, we must make certain that in the vocabulary to words and expressions that might occur on the left and whose references belong to logic, identical ones are opposed on the right.[12]

That is to say, we must require the mappings in question to meet condition (ii).

One thing that's clear from this passage is that the "formal" nature of logic, as Frege sees it, is of a kind with a certain formality had by every (or almost every?) science. For each science, there's some range of concepts, objects, functions etc. such that the replacement of one concept/object/function by another (preserving type) outside of this range is irrelevant to the science. The uniform replacement of terms referring to the color *red* by terms referring to *blue*, for example, when we're reasoning just about the masses of the objects in question, will have no effect on the scientific viability of that discourse. Logic is just, in Frege's view here, a particular example of this phenomenon: for terms whose referents come from a given range of concepts/objects/functions whose nature is part of the subject-matter of logic, the replacement of one for another can change the expression of good logical reasoning into the expression of fallacious such reasoning, while the substitution of terms outside of this range for one another will not affect the logical validity of such reasoning.

[12] [1906] 427-8; 338-9.

Given this account of formality, it's clear how the formal nature of logic leads naturally to the idea of (L3) as a test for logical independence. In the kind of case at hand, we know that because C′ expresses a false thought while the members of P′ express truths, C′ cannot be reached by steps of logical inference from P′. But then we know that the original C, similarly, cannot be reached by steps of logical inference from P. For each step of logical inference in the path from P to C would, via the mapping μ, correspond to a step of logical inference constituting a path from P′ to C′, which we know there can't be. That the steps of good logical inference in the path from P to C would correspond via μ to steps of good logical inference in the path from P′ to C′ is guaranteed by the fact that μ preserves "logical" objects/concepts/functions, and that all other type-preserving substitutions (i.e. the ones given by μ) are ones with respect to which logic is insensitive. Given Frege's view of independence as the absence of just such an inferential path, it would seem that (L3) is a good test for Fregean independence, and that its success is indeed due, as Frege says, to the "formal" nature of the laws of logic.

Why, then, doesn't Frege just stop here and call it a day? Here there are several questions. Why did Frege consider this account of a method for proving independence to be unfinished? Why, given the importance for mathematics generally and for Frege's work in particular of the notion of independence, does he never return to the topic and take care of whatever remaining doubts he had? And why does he claim four years later, presumably in wholesale rejection of the method given here, that

> The indemonstrability of the axiom of parallels cannot be proved. If we do this apparently, we use the word 'axiom' in a sense quite different from that which is handed down to us. Cf. my essays 'On the Foundations of Geometry'...[13]

The "Foundations of Geometry" essays Frege refers us to here are those in which he engages in a sustained criticism of Hilbert's proofs of consistency and independence. One of his points there, alluded to in this passage and discussed below, is that one can only take Hilbert's proof-technique to be successful in demonstrating the independence of what one calls an "axiom" of Euclidean geometry if one uses this term non-standardly, to stand either for a multiply-interpretable sentence, or for one of the non-Euclidean thoughts expressed under a non-standard interpretation of such a sentence.

[13][1980] 183n. Frege's reference here is to [1903] and [1906].

Before turning to the questions just raised, we look first at a line of reasoning that seeks to show that the 1910 passage just quoted does not, in fact, show Frege rejecting independence proofs in general.

§2. The Import of the 1910 Notes

Jamie Tappenden has argued that the passage from Frege's 1910 notes to Jourdain quoted above does not express the view that independence, of the kind under discussion in the 1906 passage, cannot be demonstrated. As Tappenden sees it, there are two senses of "independence" at play, which we can characterize as follows.

> [Ind-1]: Axiom A is independent of axioms A1…An if it can be assumed without contradiction that A is false while A1…An are true.
>
> [Ind-2]: A thought T is independent of a group of thoughts Ω iff T cannot be proved from Ω via steps of valid logical inference.[14]

Tappenden's further claim is that Frege's rejection of Hilbert's methodology in the *Foundations of Geometry* is a rejection of attempts to prove independence in the first of these senses. Similarly for the rejection expressed in the 1910 Jourdain notes: Frege is here rejecting the idea that we can prove the parallels postulate to be independent in sense [Ind-1] of the other axioms of Euclidean geometry. But, says Tappenden, Frege has no objection to the demonstration of independence in sense [Ind-2].

As Tappenden understands it, Frege's objection to demonstrations of independence in the sense of [Ind-1] is that such demonstrations, and indeed even the statement of independence itself, must involve what is by Frege's lights an incoherent supposition: namely, that an axiom is false. From Frege's point of view, axioms (which, recall, are not sentences but thoughts) are by definition true, and there is no sense to be made of supposing them to be false. If you think you're considering a circumstance in which the parallels axiom is false, as Frege sees it, then you must be considering something other than points, lines, parallelness, etc; and hence it is not really the parallels axiom that you're contemplating.

> … [A]s long as I understand the words 'straight line', 'parallel', and 'intersect' as I do, I cannot but accept the parallels axiom. If someone else does not accept it, I can only assume that he understands these words differently. Their sense is indissolubly bound up with the axiom of parallels.[15]

[14] Tappenden, [2000] pp 273-4.
[15] [1914] 266/247

In short: considerations that begin with the supposition that an axiom-thought is false are already, from this point of view, incoherent. Hence the methodology naturally suggested by [Ind-1] is, by Frege's lights, fundamentally flawed. And this methodology, i.e. that of supposing the axiom in question to be false and checking for contradiction, is as Tappenden sees it the procedure rejected by Frege both in his controversy with Hilbert and in the 1910 Jourdain notes.

Frege does, to be sure, reject the idea (at least after 1884) that we can make sense of suppositions that involve the falsehood of axiom-thoughts. But it is not accurate to portray this as the reasoning behind Frege's rejection of Hilbert's independence-proofs. As Frege recognizes, Hilbert's method in *FG* does not turn on supposing axiom-thoughts to be false. Instead, Hilbert's proofs proceed by reinterpreting axiom-sentences in such a way that the thoughts newly expressed by those sentences are not the original axiom-thoughts of Euclidean geometry, but are instead new thoughts of an entirely different science altogether. Specifically, in order to show that an axiom-sentence σ is independent of a set Γ of axiom-sentences, Hilbert reinterprets the sentences in such a way that while Γ, as interpreted, expresses a set of theorems of the background theory B (in this case, a theory of constructions out of real numbers), σ on the new interpretation expresses a falsehood, specifically, the negation of a theorem of B. Hence, assuming the consistency of B, it is demonstrated that the sentence σ is not derivable from the set Γ of sentences.

We can refer to the true geometric thoughts originally expressed by the members of Γ and by σ as "$G(\Gamma)$" and "$G(\sigma)$" respectively; the thoughts about real numbers expressed under Hilbert's reinterpretation we'll call "$R(\Gamma)$" and "$R(\sigma)$." Hilbert's procedure isn't that of supposing the members of $G(\Gamma)$ to be true while $G(\sigma)$ is false, which would, as above, be incoherent on Frege's view. His procedure instead involves, to put it in Frege's terms, noting that the sentences can be reinterpreted so as to express (sets of) thoughts $R(\Gamma)$ and $R(\sigma)$ respectively, such that the members of the first are true while the last is false.

Frege criticizes Hilbert's terminological looseness with respect to the term "axiom," and notes that it is confusing to have to deal with Hilbert's use of the term to refer not just to what it should refer to, namely axiom-thoughts, but also to reinterpretable sentences, and to the thoughts expressed by those sentences under various reinterpretations. Those newly-expressed thoughts are typically not the axioms of any science, and indeed this is strikingly clear, from Frege's point of view, with respect to those newly-expressed thoughts that are in fact false. Frege's central complaint is that when Hilbert demonstrates what he, Hilbert, refers to as a situation in which axioms A1…An are true and axiom A is false, what he has done instead is to consider a situation in which newly-expressed thoughts, not the axioms of geometry, have those truth-values.[16] The difficulty, in short, is not that Hilbert supposes, incoherently, of a true axiom-thought G(A) that it is false, but instead that he shifts his focus from such a true axiom-thought to a quite different, false thought R(A) of a different science altogether.

That this is a crucial point for Frege has to do with the fact that the independence of R(A) from R(A1)…R(An), as amply demonstrated by the truth of the latter thoughts and the falsehood of the first, does not imply the independence of G(A) from G(A1)…G(An), despite the use of the same sentences to express these thoughts. This is not the place to enter into all of Frege's reasons for this view, but here we can sum up by noting that for Frege, the independence of one thought from a collection of thoughts turns not just on the syntactic structure of the sentences used to express them, but additionally on logical connections, if any, that obtain between the objects, concepts, and functions referred to by the parts of those sentences.

To return to the central issue of this section: Frege does indeed, considerably earlier than the debate with Hilbert, speak of independence in the kind of modal sense depicted in [Ind-1]. In *Grundlagen*, he says

> For purposes of conceptual thought we can always assume the contrary of some one or other of the geometrical axioms, without involving ourselves in any self-contradictions when we proceed to our deductions, despite the conflict between our assumptions and our intuition. The fact that this is possible shows that the axioms of geometry are independent of one another and of the primitive laws of logic…[17]

This somewhat vague characterization appears several years before Frege had become clear about the nature of thoughts. It is unclear whether this characterization of independence is one that Frege would continue to endorse by the time he had developed his mature position on thoughts as the relata of logical entailment and hence of independence. It is, in any case, not a characterization that appears on either side in the debate with Hilbert.

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[16] For a detailed discussion of Frege's criticism of Hilbert, see my [1996] and [2007], and [2012] Chapter 5.

[17] [1884] §14

What about the comment in the notes to Jourdain that the unprovability of the parallels axiom can't be proven? Here the context is very thin, but it's hard to make a case that Frege here means something other than unprovability from the other axioms of Euclidean geometry. More to the point, he doesn't say or otherwise indicate that the problem has to do with the incoherence of assuming a true axiom-thought to be false. Instead, he refers his reader back to his own criticisms of Hilbert, which as we've seen are of a very different kind.

In short, while there's some indication in *Grundlagen* of an understanding of independence which involves the supposition, later thought by Frege to be incoherent, of conditions under which an axiom-thought is false, this isn't a conception that shows up in his complaints against independence-proofs. His rejection in 1910 of the possibility of proving independence, accompanied as it is by a reference to the complaints against Hilbert, would seem to be a rehearsal of the complaints he has made all along since 1900, complaints against a technique that does not involve the incoherent supposition of the falsehood of an axiom-thought, but that involves instead, as Frege sees it, the mis-application of the technique of reinterpretation. So the claim in 1910 that the unprovability of the parallels postulate can't be demonstrated does, it seems, stand in quite stark contrast with the suggestion in 1906 that there may be a workable method for providing such demonstrations. Finally, Frege's later discussion, in 1914, of independence in the posthumously-published "Logic in Mathematics" contains a rehearsal of the early criticism of Hilbert's independence-proofs, and no mention of the 1906 proposal.[18] It's hard to avoid the conclusion that by 1910 Frege thought there was something seriously wrong with the approach outlined in 1906.

§3. The Anti-Metatheory Explanation

According to one influential understanding of his work, Frege conceives of logic in a way that rules out, as meaningless, a large number of questions (and answers) which form a central part of contemporary logical theory. In particular, Frege's conception of logic rules out, on this understanding, all *metatheoretical* questions. If this interpretation of Frege is correct, then there is - or, at least, it has been argued that there is - a straightforward explanation of Frege's rejection of the 1906 independence-test. The

[18] Frege [1914]

argument, proposed by Tom Ricketts, is that in rejecting the prerequisites for metatheory, Frege rejects exactly what is required for making sense of, and for construing as successful, the 1906 independence-test.[19] Indeed, as Ricketts sees it, Frege's rejection of the 1906 test gives independent support to the claim that Frege's conception of logic entails the incoherence of metatheory.

The general idea that Frege's understanding of logic rules out metatheory rests on the ideas (a) that Frege conceives of logic as "universal," and (b) that this universality makes it impossible to take logic, or a system of logic, as itself a subject-matter about which one can provide rigorous proofs. As van Heijenoort and Dreben put it,

> For Frege, and then for Russell and Whitehead, logic was universal: within each explicit formulation of logic all deductive reasoning, including all of classical analysis and much of Cantorian set theory, was to be formalized. Hence not only was pure quantification theory never at the center of their attention, but metasystematic questions as such, for example the question of completeness, could not be meaningfully raised. ... we have no vantage point from which we can survey a given formalism as a whole, let alone look at logic whole.[20]

Similarly, as Ricketts puts it, it is impossible for Frege to formulate any "overarching conception of the logical."[21]

The view of Frege's conception of logic as anti-metatheoretical has a number of problems, and has been subjected to careful scrutiny and criticism in a number of places.[22] The central difficulty, which in my view is decisive, is that of finding a sense of the "universality" of logic which is both adopted by Frege and incompatible with metatheory. Leaving aside the general difficulties, however, it is of interest to see whether, as Ricketts suggests, the 1906 discussion sheds any light on the "no-metatheory" claim, or vice-versa.

Recall the second "difficulty" Frege notes on proposing the 1906 independence-test: the difficulty of distinguishing logical from non-logical vocabulary. As Ricketts sees it, Frege's understanding of logic entails that this difficulty cannot be met. Because Frege holds that one cannot "step back" and survey logic whole, he must hold that one cannot, in principle, provide a distinction between the logical and the non-logical. And since the proposed test for independence turns crucially on such a distinction, the test is, in

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[19]Ricketts [1997].
[20]van Heijenoort and Dreben, in Gödel [1986] p. 44. See also Warren Goldfarb [1979]
[21]Ricketts [1997] 174.
[22] See Tappenden [1997]; Stanley [1996]; Sullivan [2005]; Blanchette [2012], [*forthcoming*].

principle, unworkable. In short, Frege's universalist conception of logic, on this account, entails both that metatheory makes no sense, and that the distinction between the "logical" and the "nonlogical," a distinction crucial to the 1906 independence-test, cannot, in principle, be drawn.

Before looking directly at this question, it’s interesting to ask to what extent the 1906 discussion is itself a piece of metatheory. If it is, then it would seem that Frege’s very raising of the question of how to demonstrate independence is itself reason against the interpretation on which metatheory is from his point of view incoherent. But in fact, it’s not clear whether one should apply the term “metatheory” here at all. If we take “metatheory” to mean roughly the systematic study of (a certain range of properties of) formal systems, then the independence-claims Frege is talking about in 1906 are not metatheoretical. This distinguishes Frege’s independence question from central independence-issues today. That e.g. CH is independent of ZFC is a claim about the non-existence of a proof in a specific (kind of) formal system. In general, where P is a set of premise-sentences and C a conclusion-sentence, a model of $P \cup \{\sim C\}$ shows that C isn’t derivable from P in any system whose proofs are truth-preserving with respect to each of the members of the class of models under consideration. So such a result is very squarely metatheoretical, demonstrating as it does an important feature of the deductive system, or range of deductive systems, in question. For Frege on the other hand, the question of whether a given thought τ is independent of a collection Π of thoughts is the question of whether τ can be obtained from Π by a finite number of valid steps of logical inference. And while such a series of steps is straightforwardly a proof, it is not a proof in any particular formal system. Frege’s question is not whether a given formula is derivable from a set of formulas in e.g. the system of *Begriffsschrift* or *Grundgesetze*. It’s rather the question of whether a thought follows via steps of logical inference from a collection of thoughts, independently of whether there is available for our use a good codification in a formal system of the inferential steps involved.

A modern, model-theoretic independence-proof reduces the consistency in system F of $P \cup \{\sim C\}$ to the consistency of a background theory B (the theory used to provide a model of this set of formulas), and shows, assuming the consistency of B, that F itself is consistent. (For if F is inconsistent, then no set of formulas is consistent in F.) But because Frege’s independence-question has nothing to do with a particular formal system, an affirmative answer to his type of independence-question will have nothing to do with the consistency of any such system.

Recall Ricketts' point about logical vocabulary. Independently of issues concerning metatheory, it is certainly true that if Frege cannot distinguish logical from non-logical vocabulary, then he can't apply his own suggested law.

Frege says very little throughout his corpus about the distinction between logical and non-logical vocabulary. This is as one should expect. Unlike his logicist successors, Frege does not understand logicism to involve any claims about logical vocabulary. His thesis does not involve the claim, arguably crucial to Russell and Carnap, that mathematical concepts are definable in terms of "purely logical" ones. Frege's understanding of the "purely logical" as it pertains to logicism is that purely-logical truths – a class which, he would argue, includes all truths of arithmetic – are those truths *provable* in the appropriate way from a canonical and independently-recognizable collection of fundamental logical principles. The mark of the purely logical is not a matter of a sentence's containing just the right kind of vocabulary: it's a matter of that sentence expressing a thought that's provable via clearly logical inferences from plainly logical premises. So the Fregean logicist project provides Frege with no reason to distinguish logical from non-logical vocabulary, objects, or concepts.

Frege also lacks the roughly Tarskian motivation for distinguishing logical from non-logical vocabulary, i.e. the motivation supplied by the use of the apparatus of formal semantics to provide accounts of such relations as that of logical entailment. Frege has no involvement with relations defined in terms of the reinterpretation of vocabulary, and hence the most important reason for us post-Fregeans to distinguish logical from non-logical vocabulary – namely to mark off the former as those to be exempted from reinterpretation – is a reason that, aside from the considerations found in the 1906 passage, Frege doesn't have. The 1906 passage is novel in Frege's work not just in its discussion of a potential means for demonstrating independence, but also in its involvement with any project that provides a reason to distinguish logical from non-logical vocabulary.

Does the universality of logic as Frege conceives of it make it impossible, as Ricketts suggests, to draw such a distinction? Here, the answer would seem to be a clear "no." Frege's conception of logic as "universal" is a combination of the idea that the fundamental logical principles apply in all domains whatsoever, and that his own systems of formal logic are to be applicable, once we expand their vocabulary appropriately, to the formalization of discourse in any area whatsoever. But neither of these claims has any bearing on whether there is a helpful way to distinguish the range of concepts/objects/functions that count as "logical" in the sense required for (L3) from all other concepts, objects, and functions. Similarly, the question of whether Frege can make sense of metatheoretical reasoning would seem far removed from the question of whether he thinks he can provide a clear such distinction. For, again, the distinction in question has nothing to do with properties of particular formal systems, i.e. with metatheory.

In sum: as I'll argue below, the evidence suggests that Ricketts is right to focus on the difficulty of distinguishing logical terms and their references from others as a key to understanding Frege's rejection not just of his own 1906 independence-test, but also of the general idea of proving independence. But the difficulties here have nothing to do with metatheory or universalism.

§4. The Similarity with Hilbert

Recall that the central difficulty with Hilbert's independence-proofs, as Frege sees it, is that they can establish the independence of the geometric axiom-thought G(A) from the collection of geometric axiom-thoughts G(A1)…G(An) only on the assumption that this independence-result follows from the superficially-similar result regarding wholly different thoughts, namely the independence of the thought R(A) from the collection R(A1)…R(An) of thoughts. And, for Frege, this assumption is generally unreliable, given the possibility of logical connections obtaining amongst the G-thoughts that don't obtain amongst the superficially-similar R-thoughts.

But now we face the striking fact that Frege's own proposed method of demonstrating independence would seem to suffer from exactly the same difficulty. Assuming no expressive limitations on the language in question, it would seem that a geometric axiom-sentence A will be independent of a collection of geometric axiom-sentences A1…An in Hilbert's sense if and only if the axiom-thought G(A) is independent of the axiom-thoughts G(A1)…G(An) in the sense of Frege's 1906 test. For there will be an interpretation (in Hilbert's sense) on which each member of A1...An is true while A is false, if and only if there is a function μ of the kind Frege mentions mapping A to A′, each An to An′, and such that each member of A1′...An′ expresses a true thought while A′ expresses a false one. The only difference between the Hilbert-interpretation *I* and Frege's function μ is that while *I* maps each term *t* to a set or object *o*, μ maps *t* to a term *t′* which refers to *o*. So as long as the Fregean language in question contains names for all of the objects and relations (or their extensions) to which Hilbert has recourse in constructing interpretations, Hilbert's independence-test and the proposed 1906 independence-test will have exactly the same results.

If this is right, then we can easily see that the cases in which Hilbert's test gives, from Frege's point of view, the wrong results will also be cases in which Frege's proposed test gives the same incorrect results. The difficulty is that if the mapping μ takes us from vocabulary whose contents bear important logical relations to one another – e.g. the terms of geometry or of analysis – to ones that don't, then the independence-declaration will be unreliable. Given such conceptual relations, it can straightforwardly be the case that the thought expressed by A is logically entailed by those expressed by A1…An despite the existence of a mapping μ delivering false A′ and true A1′…An′. Consider for example the kind of entailment central to Frege's logicist project: the thought expressed by

(α) "0 has a successor"

follows logically from that expressed by

(β) "0 is a cardinal number"

despite the fact the 1906 test would say otherwise via a mapping of the arithmetical terms to non-arithmetical ones.[23] In general, if the language in question includes such unanalyzed arithmetical terms as "0," "successor," and so on, then the 1906 test will, it seems, give results that flatly contradict Frege's logicist thesis.

And if this is correct, i.e. that Frege's proposed test and Hilbert's own test for independence are equivalent (given the satisfaction of some unexceptional criteria by the language in question), then it would

[23] Indeed, it's stronger than this: for Frege, the thoughts expressed by (α) and by (β) each follow logically from the empty set, i.e. are truths of logic. For us, the important point is the weaker one that the first thought is not independent of the second.

seem that the important question is not that of why Frege rejected the test, but of why he proposed it in the first place.

One point in the above line of reasoning, however, deserves scrutiny. The essential equivalence between Hilbert's method and the method proposed by Frege in 1906 requires not just the linguistic richness noted above (i.e. that Frege's language contains terms for Hilbert's objects and functions), but also an agreement between the two methods with respect to the terms that are to be held fixed by the mapping (Frege) or the reinterpretation (Hilbert) in question.

Recall again Frege's requirement on the mapping μ that it map terms with "logical" contents to themselves. It will be important here not to leap to the conclusion that by "logical" objects/concepts/functions Frege means what *we* would mean by "logical" objects/concepts/functions. All we know so far is that Frege recognizes a distinction between terms (or their references) whose substitution one for another can make a difference to the logical validity of a step of inference, and all others; and that he uses the term "logical" for the former. While Hilbert is not explicit in *Foundations of Geometry* about the distinction between those words open for re-translation and those whose interpretation must remain fixed, his practice is the straightforward and familiar one of re-interpreting all geometric terms, and of holding fixed just a small core of paradigmatically-logical terms like "all," "not," "and," etc.[24] An important question to ask is that of whether Frege's category of terms that must be held fixed in the mapping μ is broader than is Hilbert's category of terms whose interpretation must be held fixed. If Frege's category is significantly broader than Hilbert's, then the equivalence suggested above disappears: independence in Hilbert's sense will not, under this condition, imply independence in Frege's 1906 sense.

With respect to that question, there are reasons that pull in both directions. First of all, Frege's 1906 test is clearly inspired by the kinds of duality principles well known from projective geometry.[25] That the interchange e.g. of "point" with "line," together with corresponding exchanges amongst related vocabulary, is guaranteed to map a theorem to a theorem, and a proof to a proof in projective geometry, provides a straightforward means of demonstrating independence: under such circumstances, the

[24] There are no unrestricted quantifiers in *FG*. The sense in which "all" is fixed is that the only allowed re-interpretation of 'all F's…' is that given by the reinterpretation of F.
[25] For helpful discussion of this point see Tappenden [2000].

demonstration that such a map delivers true $A1'\ldots An'$ and false A' gives a guarantee that A is not provable in that science from $A1\ldots An$. Even when we re-phrase this in terms of the Fregean thoughts involved, the guarantee stands: giving the terms in these sentences their ordinary references, where "point" means *point* and so on, the independence result is clear. That is to say, it's clear as long as the mapping is of the kind just described, in which "point" is mapped to "line" and vice-versa, "lies on" to "passes through," etc. In this paradigm case, there are two things worth noting. First, every geometric term would seem to be available for mapping to another; the only terms held fixed are the narrowly-circumscribed, now-canonical terms of logic. This feature of the paradigm setting would seem to inspire a narrow understanding of the class of fixed terms. But secondly, the geometric terms so mapped in this model case are generally all simple: their references are not definable or analyzable in terms of one another. And this simplicity is essential to the success of the paradigmatic independence-tests inspired by duality principles. Taking this crucial feature into account might be taken to inspire the view that the class of non-fixed terms can only include logically-simple ones. In a suitably rich language, this principle will give rise to a broad understanding of the class of fixed terms.

Frege does not seem to have thought that all of the terms of Euclidean geometry are simple in this sense. He reports to Hilbert, for example, that in his own unfinished investigations into the foundations of geometry, he has been able to make do with fewer primitive terms than has Hilbert, this presumably because he takes it that it is possible to define some in terms of others.[26] Along these lines we find, for example, Frege's 1879 definition of the lying of a point A on a line BC in terms of the congruence of pairs of points.[27]

Let's return to the case of arithmetic. Frege's view is that when we have fully analyzed the contents of (α) and (β), we will be left with considerably more-complex sentences (α*) and (β*), sentences which cash out those thoughts in terms of considerably simpler functions and objects, as is done in Frege's *Grundlagen* and *Grundgesetze*. The new sentence (α*) is straightforwardly derivable from (β*), which shows, as far as Frege is concerned, that the original thought expressed by (α) is provable from the original thought expressed by (β). If we were to treat the terms "0," "successor," and "cardinal number" as open for

[26] See Frege's letter to Hilbert of 27 December 1899; translation on p. 34 of [1980].
[27] See Frege [1879b].

reinterpretation (in the Hilbert test) or mapping to different terms (in the 1906 test), then we will achieve what is by Frege's lights exactly the wrong result via both methods: i.e. the result that (α) (or the thought it expresses) is independent of (β) (or the thought it expresses). The difficulty here is that we've treated logically-complex terms as outside the range of the "fixed" terms, hence undermining (from Frege's point of view) the essential role of the mapping, i.e. that it not disturb logical structure or "form."

The same point will presumably arise in geometry once our vocabulary includes terms with logically-complex content. Here we have very little to go on by way of concrete texts, since Frege's work on analyzing geometrical concepts has mostly not survived. But let's consider a hypothetical example, along the lines of the analysis Frege gives of "point A lies on line BC" in terms of the congruence of pairs of points.[28] If for Frege the content of the term "between" can be analyzed in terms of the contents of simpler terms, and in such a way that, when fully cashed out, the content of

(γ) "Point B lies between points A and C"

is provable from that of

(δ) "Point B lies between points C and A,"

then the thought expressed by (δ) is not, in Frege's considered judgment, independent of the thought expressed by (γ). The sentence (δ) is of course, in Hilbert's sense, independent of the sentence (γ).[29] One way to put our question above about the breadth of Frege's understanding of "logical" terms is this: Is the thought expressed by (δ) independent of the thought expressed by (γ) in the sense of the 1906 test? The answer to this will depend entirely on whether the mapping μ is required to map "between" to itself or not. If by "logical term" Frege means something like what we post-Tarskians tend to mean, so as to include just the kinds of terms that Hilbert himself holds fixed in *FG*, then "between" is not one of those terms that must be mapped to itself by μ, and hence the 1906 test gives, by Frege's lights, the wrong result – i.e., essentially, Hilbert's result. If on the other hand the "logical" terms are for Frege those terms such that the replacement of one of them by some other term can disrupt the logical structure of an argument, then

[28] That analysis is as follows. "Point A lies on line BC" is analyzed as: *For all points D: If CA is congruent with CD, and BA is congruent with BD, then A=D*. Given this analysis, it is immediate that *Point A lies on BC* is provable, just using principles of logic, from *Point A lies on CB*. See [1879b] p. 204

[29] "If A, B, C are points on a line and B lies between A and C, then B lies between C and A" is an axiom of order for Hilbert, and demonstrably independent of other axioms of order. Any interpretation falsifying that axiom is one that demonstrates the independence of (δ) from (γ).

“between” is, under our assumption, a logical term. And in this case, the 1906 test does not declare (δ) independent of (γ), which is to say that it gives, by Frege’s lights, the right result. In the passage quoted in §1 above, Frege claims that “subsumption … [and] subordination of concepts” form part of the subject-matter of logic, and hence part of what the mapping μ must maintain. One might take it that he has in mind here the kind of preservation of logical form obtained by holding fixed those terms whose contents bear such conceptual relations to one another.

The question raised at the outset of this section was that of how to make sense of Frege’s proposal of the 1906 test, given its apparent very close similarity to the methodology of Hilbert’s *FG*, a methodology which Frege had emphatically, and with good reason, rejected. We’ve now recognized that the methodology may not, after all, be as similar to Hilbert’s as it first appeared. Whether it is or not will depend on how broadly we understand the class of terms which the mapping μ is required to map to themselves. We have seen that there are some reasons, though not conclusive, to take Frege to have included amongst these terms not just the narrow range of terms typically treated as “logical” today, but also more broadly any whose content is relevant to the logical connections between those thoughts expressed by their use. Read in this broader way, Frege’s proposed 1906 test is not equivalent to Hilbert’s, and is not susceptible to the same failings. Nevertheless, read in this way, the test is entirely unwieldy, given Frege’s views about the pervasiveness and the difficulty of recognizing such entailment-relevant content. The import of this unwieldiness is taken up below.

§ Conclusion

To recap the situation thus far: with “logical term” read narrowly so as to include just the usual post-Tarskian array of connectives and perhaps identity, the 1906 test gives, in the context of a sufficiently-rich language, what are by Frege’s lights the wrong answers. Here, by “sufficiently rich” is meant a language in which some terms outside of that narrow range have contents with logical connections to one another, e.g. ones definable in terms of others. The languages for which the 1906 test will give what are by Frege’s lights the right answers are of two kinds. First are those languages all of whose terms are logically independent, so that none expresses a content definable or analyzable in terms of others. Here for example are, arguably, languages whose only geometric terms are such simple ones as “point,” “lies on,” “line,” etc.

Alternatively, there are those languages that are richer, but with respect to which "logical term" is read broadly so as to include any terms whose replacement by other grammatically-appropriate terms can turn a step of good logical inference into a fallacious such step. If the above hypothetical case accurately reflects Frege's views, then the word "between" would be held fixed in such a language. Certainly "successor" and "cardinal number" would be.

From Frege's point of view, in order to systematically and effectively apply the 1906 test, then, one would need a way to distinguish terms whose contents bear logical connections to one another from those that don't. One will need, that is, to be able to distinguish the "logical" from the "non-logical" in the broad sense of that term. And here it's clear that Frege does not think there is a systematic or straightforward way to do this. For as far as Frege is concerned, the question of whether a given term has a content that will yield on conceptual analysis to as-yet-unnoticed complexity is a question which is often very difficulty to answer, a question whose resolution can take generations of mathematical or other analytic work to answer. As Frege puts it in 1892,

> Now something logically simple is no more given us at the outset than most of the chemical elements are; it is reached only by means of scientific work.[30]

and with respect to the conceptual complexity responsible for logical incompatibilities amongst thoughts:

> "That a concept contains a contradiction is not always obvious without investigation"[31]

Frege's worries about distinguishing "logical" from other terms can now be made sense of. While as Hilbert understands them, independence and formality in geometry have to do largely with the kind of structure explicitly reflected in syntax, both independence and formality for Frege have to do additionally with conceptual connections not reflected in bare syntactic structure. Hence the choice of which terms to hold fixed in a mapping (or re-interpretation) whose goal is to preserve form and provide information about independence, while easy and straightforward from the Hilbertian point of view, is difficult and philosophically contentions from Frege's point of view.

My suggestion regarding the proposal and later rejection of the 1906 test, then, is this. Focusing on geometric cases in which the paradigmatic mappings so useful in projective settings really do preserve

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[30] 1892 193/182

[31] [1884] §74. See also [1903b] §145: "[N]ot every contradiction lies quite open to view." Translation p. 159 of Geach & Black.

“form” in Frege’s broad sense (because of the simplicity of the vocabulary involved), Frege’s goal in the 1906 essay is to sketch out the general case of which these trustworthy examples are instances. This general case is the method we see tentatively laid out there, with a good deal of hesitation, and a warning that further clarification is needed with respect to the class of terms to be held fixed by the mapping. We can now see why this worry about the fixed terms can only have grown deeper as soon as Frege tried to circumscribe them in any general way. For given Frege’s views about logical entailment, the only way to be sure, in a given case, that one is holding fixed the right collection of terms is to be sure that one is including all of those terms whose contents bear logical relations to others. And while one can sometimes tell in particular cases that a term is or is not logically complex in this way, Frege’s view about the highly non-trivial nature of the kinds of conceptual analysis necessary to ferret out such connections means that there can be no general recipe for distinguishing fixed from non-fixed terms. And without such a general recipe, the 1906 sketch can never be completed. This, I would like to suggest, explains his rejection of the method tentatively proposed there.

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