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METATHEORY AND MATHEMATICAL PRACTICE IN FREGE[1]

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Orienting remarks

A cluster of recent papers on Frege have urged variations on the theme that Frege's conception of logic is in some crucial way incompatible with 'metatheoretic' investigation. From this observation, significant consequences for our interpretation of Frege's understanding of his enterprise are taken to follow. This chapter aims to critically examine this view, and to isolate what I take to be the core of truth in it. However, I will also argue that once we have isolated the defensible kernel, the sense in which Frege was committed to rejecting 'metatheory' is too narrow and uninteresting to support the conclusions drawn from the thesis by its proponents.

Although the main objective of this chapter is the discussion of this narrowly delineated scholarly point about Frege's texts, there is a more diffuse motivation for the chapter that might best be made explicit. It seems to me to be a crucial observation about everyday mathematical activity – both today and in the late nineteenth century – that such activity, when done productively, incorporates a kind of critical self-scrutiny; not only are problems solved and theorems proven. Attention is also allocated to studying and adjudicating the best (or better and worse) ways to solve problems and prove theorems. To list just a few illustrations, this reflective dimension is displayed when mathematicians make efforts to ascertain the most productive formulations of questions, the most fruitful terms in which to pose problems, the most illuminating general techniques or the theoretical contexts in which the 'essentials' of a problem are 'best laid bare'. Theorem proving, although it is rightly taken to be a characteristic mathematical practice, both arises from and contributes to such critical diagnostic activity. I have

argued elsewhere that Frege's appreciation of this critical reflective dimension of mathematical investigation contributed significantly to the richness of his philosophical work.

Is this sort of work 'metatheoretic'? The obvious answer is, in some senses, yes, and, in others, no. Most loosely, the work is metatheoretic in that it is *about* mathematics. Of course, one might also maintain that 'metatheory' has a more specific, technical meaning, relating specifically to the use of model theory as developed by Tarski. Depending on what is required of an investigation in order for it to be 'metatheoretic', it may well be evident that Frege did not practice 'metatheory', although suggestions of this more narrow kind face a danger of simply collapsing into the trivial observation that Frege was not Tarski. There are sure to be some differences here or there. The objective here is to get a clearer sense of how Frege approached the sorts of questions we might now call 'metatheoretic' and how he might have taken them to be embedded in broader questions of mathematical method.

Mathematical context

In his *Historic Development of Logic*, written early in the twentieth century, Federigo Enriques – titan of Italian algebraic geometry, acquaintance of Peano, historian of science and dabbler in formal logic – suggested that a family of developments in nineteenth-century geometry played a catalytic role in the development of formal logic in the second half of the nineteenth century. The testament of this witness to history was echoed – apparently independently – by Ernest Nagel in his essay 'The Formation of Modern Conceptions of Formal Logic in the Development of Geometry'.[2] One example emphasized by both writers is the principle of projective duality: in the projective geometry of the plane, if one takes a theorem and replaces every occurrence of 'point' with 'line' and every occurrence of 'line' with 'point' and similarly with related expressions ('inscribed' interchanged with 'circum-scribed', etc.) the result is another theorem. This example, which will be revisited later in this essay, is a case in which themes familiar to contemporary logic emerged naturally in the course of nonfoundational investigations.

The gradual emergence of what we would now call 'model – theoretic methods' was driven forward by a range of mathematical problems that were extremely pressing in Frege's mathematical environment, and in the non-foundational research on which Frege spent time. I discuss further details in the work cited in the introduction and I will return to some of these points later in the essay. For present purposes, it will suffice just to mention the key points. There were more general duality principles under investigation, plus a phenomenon recognised and studied since the mid-1860s – then grouped under the label 'transfer principle' (*Übertragungsprinzip*) – that one-one mappings between domains of objects induce correspondences between the truths about those objects. In the first acknowledgement of the phenomenon, Otto Hesse

presented a particular example of what he called an *Übertragungsprinzip* in 1866.[3] Specifically, he showed how facts about points *in space* can be set in one-to-one correspondence with facts about *pairs* of points on a fixed *straight line.* In the right context (complex projective plane) this achieves a full two-way duality between the *theorems* about points in space and the *theorems* about point-pairs on the fixed line. That is, for each statement S in one class, there is a correlated statement S' in the other class, such that S holds only if S' does. In a longer presentation published in the same year, Hesse suggested that the search for such general transfer principles should be seen as a governing methodological objective, more important than the discovery of individual theorems.[4] Hesse's specific principle was given a critical reinterpretation in Klein's Erlangen Program of 1872 as one instance of the *general* phenomenon of transfer principles arising from 1-1 mappings.

Frege was alert to this theme from the start; he notes explicitly in his thesis that 1-1 mappings support transfer principles:

> [Frege has just given as an example of 1-1 mapping the representation of the projective plane through projection onto the sphere] By a geometrical representation of imaginary forms in the plane we understand . . . a kind of correlation in virtue of which every real or imaginary element of the plane has a real, intuitive element corresponding to it. The first advantage to be gained by this is common to all cases where there is a one-one relation between two domains of elements: that we can arrive at new truths by the mere transfer (*Übertragung*) of known propositions.[5]

In addition to the study of duality and transfer principles, other problems of the sort we would describe today as 'metatheoretic' were emerging naturally out of mathematical practice. For instance, tricky definability problems were confronted in generalised geometries – for example, it was a delicate question as to just what was needed to define a surrogate for the concept of distance in a projective space. There were also the first stirrings of independence arguments, as already in the 1870s early, gappy proofs of the independence of the parallels axiom from the other axioms of Euclidean geometry had been worked out. These were recognised as arguments pertaining to (informal) logical consequence. For example, in 1873, Felix Klein produced an interpretation of non-Euclidean geometry and summed its significance up this way:

> The examination of non-Euclidean geometry is in no way intended to decide the validity of the parallel axiom but only to address this question: Is the parallel axiom a mathematical consequence (*eine mathematische Folge*) of the other axioms of Euclid? To this question these investigations provide a definite *no.*[6]

Let us pause here to state explicitly how this relates to the way we currently understand these matters. In model theory, we are today familiar with rigorous analogues of these ideas. One simple and familiar result, that if two models are isomorphic, they are elementarily equivalent. Indeed, far more intricate descendants of the nineteenth-century geometric idea of transfer principle have been studied in model theory. Feferman rightly singles out this work, like the Eklof-Barwise treatment of 'Lefschetz' principle', as a prime example of the way that model theoretic analysis can effect 'conceptual clarification' in the course of what he calls 'working foundations'.[7] Of course, many important details of contemporary model theoretic analyses (like Tarski's definition of satisfaction, or compactness arguments) fell into place long after Frege's work. Were Frege to have sat himself down to work out a logical analysis of (say) Hesse's transfer principle, he would not have produced the same thing that would have been written by Abraham Robinson *circa* 1950. Further pieces had to click into place before such analyses of transfer principles would be automatic. However, the following core semantic principle was in place and formed an integral part of mathematical practice: 1-1 mappings of objects induce systematic associations of truths about those objects. Not only was this idea in the air, its methodological importance was explicitly recognised by writers whose work formed much of the mathematical context for Frege's foundations and, from as early as 1872, Frege acknowledged it too.

A general point of informal methodology is implicit in these examples. For Frege, and for contemporary mathematicians, mathematics includes, as a crucial part of that very activity, the critical scrutiny of primitives and techniques. If Frege aims to capture the relations of ideas informing mathematical activity, he would have to include these studies. It is hard to believe that Frege would knowingly be committed to the view that, in a mature mathematical science, we could not even formulate the fact that metric properties are not definable in projective geometry, or that some metric theorems do not follow from the principles of projective geometry. This critical scrutiny naturally generalises. So, for example, one might be led, in studying the relation between coordinate systems and the geometries they describe, to more general studies of the relation between languages and the structures they describe. The fact that distance is not definable in a projective context invites study in terms of general concepts of definability. And, as we will see later in this chapter, the striking balance displayed in the principle of duality invites a general study of the relations among substitutions of terms and logical deduction. Foundational research of the sort model theory exemplifies thus arises not merely out of idle curiosity or 'pure' philosophical speculation, but as a natural and crucial mathematical development for understanding and solving problems arising in the course of non-foundational mathematics.

Say we ask: were there types of such critical scrutiny that Frege rejected, or that he was committed to rejecting? In particular, was he committed to rejecting the kinds that we can now see as precursors to contemporary model theory? If Frege's conception of logic had any such consequences, his conception of both mathematics and philosophy was correspondingly impoverished but I do not think that he held, or was committed to, anything of the sort. The purpose of the upcoming sections will be to explain why I do not accept recent claims to the effect that there are deep conflicts between the principles of Frege's philosophy and the principles informing metatheory as it is currently practiced.

The many faces of 'metatheory'

I. The view – a first pass

The topic of this subsection is a composite of several different views: the objective is to lay out a certain core set of claims and argumentative transitions. All of the claims and transitions seem to be endorsed by all of the proponents of interpretations in the tradition under study. To avoid the danger that the views of such a composite figure may not all belong to any actual person, I will concentrate on a specific incarnation – the work of Tom Ricketts. (I am only making the specific attributions to Ricketts. The views of the others mentioned may well differ on this or that detail.) The views in the family examined here emerge from what has been, until recently, largely an oral and 'underground' tradition of seminars, conversations and correspondence, with few detailed published elaborations. The source waters for the interpretation were a classic paper by van Heijenoort[8] and a cluster of seminars, reading groups and general advocacy driven by Burton Dreben at Harvard in the 1970s and onward. Since Dreben himself wrote little on the subject, his views were elaborated and developed by students and junior colleagues who passed through Harvard at the time. Consequently, we can see in the literature on these topics the signs of such incipient traditions: repeated occurrences of distinctive phrases and dialectical manoeuvres suddenly popping up unexplained at crucial turns in articles by many different authors:

> If the system [of logicism] constitutes the universal logical language, then there can be no external standpoint from which one may view and discuss the system. Metasystematic considerations are illegitimate rather than simply undesirable.[9]

> Frege's and Russell's systems are meant to provide a universal language: a framework inside of which all rational discourse proceeds. Thus there can be no position outside the system from which to

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assess it. The laws they derive are general laws with a fixed sense; questions of disinterpretation and reinterpretation cannot arise. . . . All this distinguishes their conception from that more common today . . . which defines logical truth with reference to schemata. . . . [Logic, for Russell/Frege] does not issue metastatements.[10]

[The Begriffsschrift] is *universal* because it is an explicit representation of the (logical) framework within which all rational discourse proceeds . . . questions concerning [a sign's] disinterpretation or reinterpretation do not arise, and logical truth is not defined by way of schemata. For Frege there is no metalogical standpoint from which to interpret or assess the system. (emphasis in the original)[11]

[T]he generality of [standards of correctness for Fregean judgements] does not involve any metaperspective. The general standards for the judgements of a discipline are not provided by statements about the discipline. They are provided by judgements *within* the discipline. (emphasis in the original)[12]

[Frege's] conception of judgement precludes any serious metalogical perspective and hence anything properly labeled a semantic theory.[13]

[A]nything like formal semantics, as it has come to be understood in light of Tarski's work on truth, is utterly foreign to Frege.[14]

For Frege . . . logic was universal: within each explicit formulation of logic all deductive reasoning . . . was to be formalised. Hence . . . metasystematic questions as such . . . 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 as a whole.[15]

Frege's view of the nature of logical laws precludes the existence of a substantive metaperspective for logic . . . he would refuse to regard any metatheoretic reasoning about primitive logical laws as expressing an objective inference.[16]

[on Russell]: The fact that Russell does not see logic as something on which one can take a metatheoretical perspective thus constitutes a crucial difference between his conception of logic and the model theoretic conception. Logic, for Russell, is a systematisation of reasoning in general, of correct reasoning as such. If we have a correct systematisation it will comprehend all correct principles of reasoning. Given such a conception of logic, there can be no external perspective. Any reasoning will, simply in virtue of being reasoning, fall within logic; any proposition we might want to advance is subject to the rules of logic.[17]

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Certainly it is not mere happenstance that such idiosyncratic turns of phrase should appear unexplained in so many different essays. It will be worth some effort to reconstruct the views that prompt them. First, it is worth noting a common suggestion: something in Frege's conception of logic precludes any appeal to a 'metaperspective'. Why is this? In most of the remarks, it seems to be argued that this appeal is precluded *just* by Frege's conception of logic as universal, since metatheory is said to *require* an external perspective. That is, the suggestion seems to be that Frege should not be read as engaging in semantical or other investigations of the sort that we might now call 'metatheoretic' because he did not think there could be a framework more extensive than that given by logic. However, even if we grant the premise that Frege adhered to a 'universalist' conception of logic, the conclusion only follows in a quite uninteresting and restricted sense.[18] In the next few paragraphs I will explain why this is so before moving on.

Note first that there is a hint of anachronism in drawing any conclusions about Frege's attitude to semantics from any commitments that might be incompatible with the existence of a perspective wider than that of logic. Even if Frege thought of the Begriffsschrift as a universal language as per the remarks of Goldfarb and Conant,[19] nothing follows about Frege's views on semantics. It is highly unlikely that Frege would have thought that semantic theory, or other investigations we might now describe as 'metatheoretic', would *require* a separate, 'external' standpoint. In light of Tarski's results on the undefinability of truth and related discoveries, we have come to accept that the semantics for theories of a certain strength might need to be formulated in a metatheory that is in some ways stronger than the theory for which the semantics is being provided. This is a fairly new idea, however, and perhaps it is not an altogether natural one. It is worth bearing in mind how surprised people were by the Gödel-Tarski limitative results. Hilbert, to consider just one example, appears to have thought that the metatheory of mathematical theories of the infinite could be done in proper (finitistic) fragments of those theories. The suggestion that this view requires a ('broader', 'external' . . .) metaperspective did not appear until prompted by the limitative results or the paradoxes that prefigured them.

Furthermore, although to pursue the point would be too much of a digression, it is worth noting that it is not even clearly correct that 'semantics' *in fact* requires an 'external metaperspective'. The adoption of a hierarchy of languages was, of course, Tarski's response to the limitative results he unearthed but, as recent work on the theory of truth has aimed to show, there are theories containing arithmetic that can contain significant fragments of their own semantic theories.[20] Furthermore, even if we conform to all of Tarski's assumptions, a higher-order theory like Frege's will have considerable resources for developing within itself the semantic theory for extensive fragments of itself.

To help bring out how this later twentieth-century presumption is easily read back into Frege, say that we modify the above-cited remarks of Ricketts as follows: 'The sentences in grammars of English do not express statements about the English language. They express judgments *within* the English language.'[21] This should strike us as a rather odd implied dichotomy: that a statement is in English is not incompatible with its being about English. There is no reason arising solely from the universality of logic to which Frege could have had access for thinking of the 'within logic/about logic' contrast as any more exclusive than 'within English/about English'. Quite the opposite. If logic is universal – if its scope includes *absolutely everything* – this should include logic itself.

Furthermore, Frege clearly thinks that, at least in some respects, it is crucial that the scope of logic should extend over logic. Counting is for him regimented as a logical operation, and numbers are logical objects. Yet, as he clearly emphasises, his account of arithmetic will be inadequate unless it supports the possibility of counting numbers. Indeed, Frege abandons an otherwise attractive way of avoiding Russell's paradox for the expressed reason that counting numbers would be impossible.[22] Moreover, it is a repeated theme for Frege that everything can be counted: concepts, thoughts, ideas, events, ...[23] Therefore, it seems unlikely that we could find Frege embracing a principled separation of logical theory and logical scrutiny of a theory and holding that the latter needs a separate metaperspective. If 'semantical metatheory' means 'theoretical' study of a theory using notions of 'language', 'reference', 'interpretation', etc., then it is anachronistic to think that Frege would have thought the universality of logic alone would preclude metatheory. This point is especially worth stressing if the idea that semantics requires a distinct metaperspective is bound up with a view that occasionally seems to be suggested in this line of interpretation; that semantics somehow incorporates a special, distinctively *philosophical* move, different in principle from what might arise naturally in the course of scientific investigation. To the contrary, a need for a study of the structure of the language describing some subject matter arises naturally as an integral part of the study of that very subject matter.

When I finished the original version of this article, I had thought the above points were straightforward enough to preclude misunderstanding but I found more clarification was sometimes necessary. So I will review the point by responding to a recent attempt to address it. In a discussion of how Frege might have approached the question of the completeness of a formulation of logic, Juliet Floyd remarks:

> In response to a 1992 lecture of Dreben's, Quine expressed skepticism about the anti-semantical reading. Does Dreben mean, asked Quine, that if Frege had seen Gödel's proof of the completeness theorem, Frege wouldn't have been able to understand it? Dreben replied:

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the Frege of the *Grundgesetze* would have understood it as a piece of mathematics, as showing that a certain set-theoretically definable class is recursively enumerable; but Frege would have questioned whether this set was a proper specification of his notion of logical truth, of logical validity . . . Dreben's reply to Quine is also his reply to the suggestions of Heck, Stanley, and Tappenden that since logic is universal, by the techniques of Gödel and Tarski, many metasystematic questions, in particular the completeness theorem, can be carried out *within* the system.[24]

Speaking just for myself, this misrepresents my points in two critical ways. First of all, there is a crucial ambiguity in the Dreben/Floyd use of 'specification'. I agree that Frege would have denied that a semantic analysis captured the antecedent meaning of 'logical law' or 'truth of logic' as he understood it. If an *analytic definition* is what is meant by 'proper specification of his notion of logical truth' then I am willing to provisionally grant for the sake of argument – although I think the issue is complicated – that even in his early and middle writings Frege would have rejected a semantic definition. That is irrelevant, however. What is at issue in Quine's question (at least insofar as the question is relevant to anything I have written) is whether or not Frege would have had any objection to an examination of the relations between logical truth, law or validity as he understands it and some other, *stipulatively introduced* semantic notion. (For example, one satisfying Tarski's material adequacy and formal correctness conditions.) Later in this chapter I will speak of 'logical fruth', to avoid the idea that an analytic definition is at issue. There is nothing in Frege to indicate that he would have any objection to exploring the relations between 'logical truth', in his sense, and 'logical fruth' in some stipulatively defined sense.

Again, the distinction between reduction to model theory and study of model theory must be kept in mind. Even if we grant that Frege would not have accepted a definition that *reduces* logical truth or logical validity to facts about structures, there is no reason to think he would not have seen the value (for example, in the study of duality or transfer principles) of a defined expression that is demonstratively *coextensive with* truth (or 'truth in some restricted domain'). So long as this defined expression was not put forward as a reductive definition of truth into more basic terms. It is crucial to see that even though Frege did indeed think that logical truths were to be *defined as* those thoughts that followed from logical laws by means of logical inferences, this indicates no hesitation about studying alternative, less basic definitions and learning from exploring them. A definition of logical consequence of the sort given by Tarski could not be basic for Frege. However, that does not mean that Frege would not regard it as a handy thing to know and use. Certainly, it does not mean the definition must be intrinsically a ladder destined ultimately to be kicked away.

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The second, more serious misunderstanding, is concealed in the phrase 'by the techniques of Gödel and Tarski'. Those techniques come into play if one sees a *prima facie* separation between metatheory and object theory. Faced with this apparent obstacle, it is possible to carry out some surrogate metatheoretic reasoning by *coding* metatheory into arithmetic. But this was not the suggestion of my original article: rather the point is that because logic is universal for Frege (and for other reasons too) it is unlikely in the extreme that Frege would have taken there to be a *prima facie* separation of metatheory and object theory. It is an anachronism to think that Frege would have taken the Gödel-Tarski techniques to be *required* to carry out his analogues of model-theoretic reasoning. The default assumption would have been that the semantic exploration of logic could be developed within the universal logical framework itself. The issue of the unavailability of an 'external standpoint' would not arise, since Frege would have no reason to think that such a standpoint would be required.

Consider in this connection the following remark:

> Frege's logical innovations and notational novelties require extensive elucidation. Some of this rhetoric, including, I believe, much of what we tend to think of as Frege's semantics—is not stateable within the framework of the Begriffsschrift. Frege's universalist conceptions of logic gives it an anomalous status.[25]

I see little reason to believe that 'much of what we tend to call Frege's semantics' cannot be stated within the framework of the Begriffsschrift. Furthermore, as noted, the appeal to 'universalism' in this connection incorporates a confusion. But even setting aside these points, the consequences of this 'anomalous status' for our interpretation of Frege would be quite restricted, unless we take the further step (which seems to me evidently mistaken) of maintaining that Frege was *aware* that the claims in question could not be stated in the Begriffsschrift. Apart from his remarks on a specific, narrow issue – the 'concept horse' problem – and his recognition of the difference between rules of inference and axioms, Frege gives no indication of any potential shortfalls in expressive power. Often he fails to do things the way that we would do them today, but there is no reason to think that this divergence reflects anything more than that certain things had, quite naturally, not occurred to him.

It is important to distinguish three possibilities: I) A currently common practice was unfamiliar to Frege; II) some view or views held by Frege committed him to rejecting a currently common practice and III) Frege was committed to rejecting a currently common practice and, furthermore, he was aware that he was so committed. One recurring leitmotif in the coming pages conforms to the pattern of the next few sentences. Ricketts, or some other writer in this school, will point out an absence in Frege. As a Type I

claim the observation may well be defensible, although it does not support deep conclusions about Frege's methods and attitudes. Deep conclusions will typically require Type II or III claims, which turn out not to be defensible.

Say that, in particular, it is claimed that in the early sections of the *Grundgesetze*, Frege was engaging in a rudimentary kind of proto-semantics. One could consider a common feature of semantic practice today and note that Frege does not employ it. As an example of a Type I observation, one might observe that, although the value of formally and systematically exploring the soundness of inference rules in today as natural as breathing, it was not an objective that seems to have been set by Frege in *Grundgesetze*. That Frege did not work out all the details (identify basic laws, carry out gap free proofs, etc.) of a scientific semantics in *Grundgesetze* is a mildly interesting observation, but it conflicts in no way with the observation that Frege was anticipating contemporary metatheory in these sections. This point is worth stressing since it is often blurred in the work under consideration here. So, for example, in a discussion of whether or not the early sections of *Grundgesetze* contain 'metatheory', Ricketts states anachronistically: 'It is striking how Frege avoids even informal soundness arguments in his exposition of inference rules in *Grundgesetze* §§14–25'.[26] That Frege did certain things differently from us is of course worth pointing out, but to suggest that he did things differently because he anticipated the possibility of proceeding as some textbooks do today, and then consciously avoided that path, is both unlikely and completely without textual support.

In fact, in this particular instance, even a type I suggestion will not work, as in these sections Frege clearly does appeal, in a rudimentary way, to the soundness of *modus ponens*. Frege says the following at *BLA* §14: 'From the propositions [⊢ If Δ then Γ] and [⊢ Δ] we may infer [⊢ Γ] for if Γ were not the True then since Δ is the True [If Δ then Γ] would be the False' (*BLA*, p. 57).[27]

Given that Frege says this, anyone who wants to maintain that the possibility of some kind of informal soundness argument had not occurred to Frege has a tough row to hoe. The only hope is to explain such explicit remarks away with reference to other commitments Frege might have had. Ricketts suggests that these sections cannot be read in the natural way because – he maintains – 'is the True' is the translation of Frege's horizontal and Frege's horizontal has some logical properties that are different from some amongst the things that might be called truth predicates. However, even if we grant that Frege's horizontal is not a truth-predicate there is nothing in these sections to indicate that Frege holds that the expression 'is the True' in *Grundgesetze* is to be translated as the horizontal, or as '( ) = the True', or as the predicate 'is frue' introduced in section VI of this chapter, or anything else. Frege fist introduces 'the True' at §2 and then at §5 specifies the horizontal in a paragraph that appeals to both 'is the True' and 'denotes the True'. Frege does not indicate that the subsequently introduced expression for the horizontal is to be seen as superceding 'is the True'.[28]

The sole support Ricketts provides for taking §14 to be a case where Frege *avoids* soundness is the following, he says: 'Frege explains his inference rules by arguing in a mixture of German and Begriffsschrift for the truth of conditionals corresponding to representative applications of the rules'.[29] And why is this an accurate description of what is going on in *Grundgesetze* §14? The sole direct support is footnote 38:

> Frege's phrase 'is the True' is not a truth-predicate; it is the translation into German of Frege's horizontal. See footnote 8. The one place where Frege has recourse to the use of the truth-predicate in generalisations is in his very tentative discussion of independence proofs in the third part of 'On the Foundations of Geometry [II]' pp. 426–427. He opines that these generalisations would be the laws of a new science."[30]

The relevant sections of 'Foundations of Geometry' will be considered later in this essay, but it is difficult to see what differences in use underwrite the suggestion that the use of truth talk in the later essay is to be interpreted differently from the truth talk of the *Grundgesetze*. True, Frege says that the laws would be the laws of a new science but why should we not read him as indicating, a little over ten years after writing *Grundgesetze*, that were the truth talk of the *Grundgesetze* to be regimented, the 'new science' sketched in 'Foundations of Geometry' would result? Indeed, as we will see, the 'new science' is the systematic articulation of geometrical practices with which Frege was familiar. The cited footnote 8 just adds reasons for taking the horizontal not to be a truth-predicate, rather that providing any support for interpreting 'is the True' as the horizontal. So what we seem to have is an argument that *Grundgesetze* is not a soundness argument because an interpreter *might* regiment 'is the True' as the horizontal and, if one did, it would not be regimented as a truth-predicate. However, nothing Frege says obviously forces one to so interpret it. To support the claim, the interpretation needs to provide some evidence that, were Frege to regiment §14, he would have to regiment it in a specific way, presumably because of other commitments. In other words, what is needed is some defense in the ballpark of Type II or Type III. Frege simply too often actually says what he is taken not to say in this interpretation. So the remarks Frege makes have to be explained away, by arguing that he was obliged not to treat them with scientific gravity and that they are unserious 'elucidations'. In the next sections I will argue that these efforts are unsuccessful.

II. The basic observation

In *Grundgesetze* §31, Frege certainly seems to be engaging in 'metatheoretic' reasoning. He gives an argument that the singular terms in the system

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denote. Frege seemed to regard this as providing something of a consistency argument when he confronted Russell's paradox. In his response to Russell's fateful letter, he suggested that the existence of the contradiction indicated some flaw in the argument at §31.[31] How could this not be counted as a metatheoretic argument? Apparently, it is not enough that a proof, that all singular terms denote to, be regarded as a demonstration of consistency for it to count as 'metatheoretic'. So what *does* have to be true of a proof for it to be 'metatheoretic' in the relevant sense?

Although we are given little to go on in the writing under discussion, there is hope. When opponents are not explicitly identified, and opposing positions not spelled out in detail, one way to identify the target is – so to speak – by abduction from the arguments given. That is, one can isolate what has to be true of a position in order for the arguments given *against* that position to be cogent. We have such a foothold in Ricketts' case. He repeatedly puts forward a specific regress argument, of the sort familiar from Lewis Carroll's 'What the Tortoise Said to Achilles', from which we can extrapolate fairly confidently to the opposing position.[32] For ease of reference, I will call the argument in question 'the basic argument'.

I will specify exactly what the basic argument is and what position it must be taken to attack with a care that might seem pedantic. The reason for this exactness is that the basic argument strikes a delicate balance. Some philosophical arguments are not only cogent but (as a statistician might say) robust, in that if the position the argument opposes is modified in one way or another, there will be a corresponding modification of the argument, so as to obtain a cogent argument against the modified position. 'The basic argument', however, is not at all robust in this sense. Weakenings of the opposing position yield views that are untouched by modifications of the basic argument. Hence, it becomes crucial, in assessing the force of the basic argument, to establish just what the reconstructed opposing position entails and who, if anyone, embraces it.

We gain a foothold from a larger slice of a passage the last sentence of which was considered above. For a proof to be metatheoretic, it must involve a notion of truth of a certain kind and it must aim at the *deductive reduction* of the correctness of inference rules to facts of the sort that we might now call 'model-theoretic'.

> Even apart from its use of a truth-predicate, Frege would find the attempt to prove his formalism sound to be pointless. Such a proof could achieve scientific status only via formalization inside the framework provided by the formulation of logic it proves sound. The resulting circularity would, in Frege's eyes, vitiate the proof as any sort of justification for the formalism. It is striking how Frege avoids even informal soundness arguments in his exposition of inference rules in *Grundgesetze* §14–25.[33]

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Who, if anyone, holds a conception of logic that is *inconsistent* with the view that Frege is held to endorse (or at least held to be committed to)? There is a tendency, in the writings under discussion, to speak loosely of Frege's conception of logic as fundamentally opposed to 'the modern conception' or 'the contemporary conception' that, oddly, is alleged to regard the correctness of logical rules as *reductively* justified by appeal to their soundness in a way that would run aground on this circularity. Ricketts writes:

> Moreover, were use of an inference rule to be justified by the judgment of a general law, we would encounter the vicious regress in the provision of proofs that Lewis Carroll pointed out. For then, in order to make a proof complete, any use of an inference rule would have to be accompanied by an assertion of a corresponding logical law. Only in this way would all the premises on whose correctness the conclusion depends be explicitly stated. But this added statement creates the need for further inferences, each of which would need to be similarly accompanied by assertion of justifying laws. This regress would make completed proofs impossible.
>
> At this point, appeal to a metaperspective seems inescapable. On the contemporary conception of logic, the acceptance of modus ponens as a correct rule of inference is vouchsafed by our metalogical judgment that if a conditional is true and its antecedent is true, then so is the consequent.[34]

The only way we can arrive at a conception in any tension with these worries about infinite regresses is by interpreting these remarks very strictly. We must understand the force of 'vouchsafed' in 'the acceptance of MP as a correct rule of inference is vouchsafed by our metalogical judgment' to mean that the judgement that *modus ponens* is a correct rule of inference is *justified* by the *reduction* of the correctness of *modus ponens* to model-theoretic facts. (With the model-theoretic facts taken as more fundamental.) Weaker positions have nothing to fear from the regress. In particular, if one can be an adherent of the 'modern conception' merely by accepting that model theoretic investigations are revealing, interesting, important and worth carrying out, the regress argument is completely irrelevant (if we bracket questions arising from Gödel-Tarski type limitative results that Frege would certainly not have anticipated.) To repeat: no vicious regress, and hence no incompatibility with the 'basic observation', will arise unless facts about the correctness of inference are seen as *reduced* to facts about the existence of models and relations amongst them.

This yields a specific, narrow thesis the attribution of which to Frege is defensible by reference to his 'universalism about logic': Frege does not accept that the basic laws of logic can be given a justification whereby the question of their truth is deductively *reduced* to the question of truths of

some other, non-logical sort. In particular, logical laws cannot be justified by reducing them to facts about models. For Frege, the logical 'rightness' of a rule like *modus ponens* is not less fundamental than the fact of its soundness. For ease of reference, I will call this core of the interpretation, the *basic observation*. It occurs repeatedly in the writings under consideration here, as perhaps the crucial support for the other aspects of the interpretations we are considering. Lest my objective here be misunderstood, I should take some care to state emphatically that I take the basic observation to be evidently correct and textually defensible. Surely, Frege would have rejected the idea that laws of logic could be *justified* by a *deductive reduction* to some other, *more basic*, *non-logical* grounds and his 'universalism' would be one reason he would have rejected it. So long as each of the italicised expressions, or some equivalent, are included in the thesis, I have no quarrel with this suggestion at all. Rather my arguments are directed to show that the basic observation by itself is of quite limited interest, both in itself and as a fact about Frege's commitments. The efforts by Ricketts and others to make the basic observation into more than an incidental aside typically involve attempts to draw consequences *not* from the defensible core just considered but from some stronger variation on the thesis, the attribution of which to Frege is typically quite indefensible.

Bearing this in mind, the question naturally arises: who exactly does accept the so-called 'contemporary conception'? The conception of logic at issue is certainly not mine, nor has my informal canvassing of the people I know turned up anyone who *does* hold the 'modern conception', if this conception is to be a view that conflicts with the basic observation. A recent collection of papers entitled *What is a Logical System?*[35] contains fifteen strikingly diverse discussions of the nature of logic, not one of which displays allegiance to the 'contemporary conception'. Of course, one finds semantic investigation in these papers, but not a kind that is incompatible with the basic observation. So, for example, in Hacking's 'What is Logic?', originally published in 1979 but reprinted in that collection, we find a treatment of inference in the style of Gentzen, with a 'do-it-yourself semantics' developed out of that initial presentation.[36] Of course, one need not deny the basic observation to hold that, after an initial presentation of the Gentzen type, semantics can be subsequently developed and studied.

So just who are the 'moderns'? One naturally looks to Dummett, since he is a favoured target in the writings expounding the 'no metatheory' interpretation. Indeed, an incidental remark from Dummett is the *only* contemporary discussion of logic that is cited as a contrast to Frege's conception in Ricketts' 'Logic and Truth in Frege'.[37] However, the 'contemporary conception of logic' is certainly not *his*. Even a cursory acquaintance with *The Logical Basis of Metaphysics* makes it evident that Dummett does not think that logical facts can be simply eliminated by a reduction to semantic ones.

On occasion, specific authors are cited as advancing 'the modern/ contemporary conception': Tarski and Quine.[38] I raise doubts about the attribution to Tarski below, and I think that Quine is also misrepresented but, for the sake of argument, I will not discuss the accuracy of the presentation of Quine. I am in the main content to defer to Quine's students and colleagues on questions of Quine scholarship. But it is parochial to slide effortlessly from attributions to this specific figure to broad claims about 'the modern conception'. Quine's views on logic are in many important respects idiosyncratic; for example, in his emphasis on sentences rather than (say) structured propositions, interpreted logical forms or situations as the object of the theory of meaning, or his deflationism about the truth predicate. Projecting these views on a supposedly monolithic 'contemporary conception' just ignores what has been going on over the past forty years. In particular, since few researchers in semantics today embrace Quine's strong rejection of abstract propositions, leaving them the option of regarding the correctness of *modus ponens* as fundamentally a basic fact about the connections between something like structured propositions (or interpreted logical forms, or situations, . . .) and only derivatively a fact about the connections between the uninterpreted forms of sentences. Some variation of this is, in fact, my view, as well as that of everyone currently working in semantics or philosophical logic with whom I have discussed the idea of the 'schematic conception'.

I will discuss the unrepresentativeness of some more of Quine's positions, and the anachronism that can result from projecting them backwards, in the next section. Here I will pause to consider a paper that illustrates especially vividly the elusiveness of the alleged 'contemporary conception': Warren Goldfarb's 'Frege's Conception of Logic'.[39] This appeared several years after the original of the 'Metatheory and Mathematical Practice . . .' paper. That article (and others) forcefully pointed out that the 'contemporary conception' has few, if any, contemporary adherents, with a possible exception made for seriously dated writings of Quine. In response to such a tossed gauntlet, there is of course a canonical way to uphold the attribution: produce (at least one) example of a contemporary worker in logic or semantics whose considered theoretical utterances (as opposed to casual textbook asides) reveal a commitment to some important aspect of the so-called 'contemporary view' in a way that is incompatible with some central Fregean doctrine. Instead, we read:

> Explicit elaborations of [the contemporary conception] are surprisingly uncommon. (In most writing on issues in philosophical logic, it is implicitly assumed; yet many textbooks gloss over it, for one pedagogical reason or another.) There are various versions; I will lay out the one formulated by Quine in his textbooks as it seems to me the clearest.[40]

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Given the antecedent challenge, it amounts to a grudging acknow-ledgement of defeat that Goldfarb gives no citation for any of the 'various versions' but rather relies on two elementary textbooks from Quine (one of them forty years old and the other fifty). In the subsequent development, the elusiveness of what is called the 'contemporary conception' is further illustrated in a particularly amusing way, although for the sake of space I will leave the discussion to a footnote.[41]

Let us return to the basic observation, however. As a first illustration of how narrow the basic observation is, note that it gives no reason to pre-scind from observing, and even proving, *that modus ponens* is sound, or that the soundness of a rule is a very interesting and important fact indeed. One cannot, it is true, argue that *modus ponens* should be taken to be correct *because the fact of its correctness deductively reduces to the fact that it is sound.* That would indeed be viciously circular. But it is consistent with the basic observation that one might want to formulate soundness theorems and prove them. Such proofs would use *modus ponens* or equivalents but so what? If I may vary a rhetorical flourish from Kreisel: it is by no means viciously circular to use a principle in order *to state the facts about it.* The basic observation only rules out the acceptance of such facts as part of a reductive justification of a logical principle.

A far-fetched example illustrates the distinctions at issue. One fact about Frege that is rightly granted on all sides is that he unequivocally rejects 'psychologism'. So, in particular, he would reject any attempt to justify logical laws by reducing them to descriptive accounts of actual human thought. But say that we have worked out an adequate Begriffsschrift and we discover that corresponding to each basic law there is a specific region of the brain that activates every time we correctly infer one thought from another using that law. Say that it even turns out that corresponding to the normative principles of the logical system there are specific true lawlike neurophysiological statements, with the reinterpreted Begriffsschrift a true descriptive account of the actions of parts of the brain. Of course, such a scenario is unlikely in the extreme but the question here is what attitude Frege's views on logic would commit us to adopting toward a discovery of this sort. Presumably, everyone will agree that Frege's views commit us to rejecting the suggestion that logic is, after all, an empirical science or reducible to empirical science. But what more should we conclude? Should we pretend that this discovery was not made, or refuse to investigate the connections between logic and physiology? I hope it will also be agreed on all sides that Frege's views would not commit us to such willful ignorance. Analogously, nothing in Frege's view precludes the exploration of corres-pondences between logical principles and (broadly) semantic principles that might correspond to them. What is precluded is only the taking of the semantic investigations to be more basic than the logical ones.

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Now of course this example is too fanciful to serve for anything but illustration. However, an analogous case is directly relevant to Frege's interest and research: the geometric interpretation of complex numbers. Of course, Frege was aware that the complex numbers could be interpreted in the Euclidean plane. Hence, claims about complex numbers correspond to synthetic arguments in geometry. What is Frege's attitude toward such research? He *does* feel that a geometric argument leaves more to be done. For example, he remarks in 'Formal Theories of Arithmetic':

> [I]t was with even greater reluctance that complex numbers were finally introduced. The overcoming of this reluctance was facilitated by geometrical interpretations; but with these, something foreign was introduced into arithmetic. Inevitably there arose the desire of once again extruding these geometrical aspects. It appeared contrary to all reason that purely arithmetical theorems should rest on geometrical axioms; and it was inevitable that proofs which apparently established such a dependence should seem to obscure the true state of affairs. The task of deriving what was purely arithmetical by purely arithmetical means, i.e., purely logically, could not be put off.
>
> (*FTA*, pp. 116–117)

Similarly in *Grundlagen*:

> What is commonly called the geometrical representation of complex numbers has at least this advantage over the proposals so far considered . . . : the segment taken to represent *i* stands in a regular relation to the segment which represents 1 . . . . However, even this account seems to make every theorem whose proof has to be based on the existence of a complex number dependent on geometrical intuition and so synthetic.
>
> §104 How are complex numbers to be given to us then . . . ? If we turn for assistance to intuition, we import something foreign into arithmetic.[42]

The diagnostic job is not completed until facts about complex numbers are demonstrated in purely logical terms. However, this does not mean that such representations cannot be coherently worked out, or that they are not worth studying. As early as his PhD thesis, we find Frege appreciating that fine distinctions and principled comparisons can be made amongst different ways of representing the complex numbers geometrically. For example, he closes his thesis with the sketch of a generalisation of Gauss's representation and evaluates both the value of the generalisation and its intuitive relationship to the special case it generalises.

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We should, however, hardly succeed in making our general way of representing complex numbers as fruitful as Gauss's.

The relationship between the two methods of representation corresponds to the relationship between Euclidean geometry and a geometry in which the line at infinity with the two circular points is replaced by a non-degenerate conic.[43]

What we find in this case is a perfect example of an interpretation of a theory providing illumination and diagnosis, with more and less fruitful versions. Furthermore, the relationships by virtue of which some are more, and some less, fruitful can be studied. Of course, this would not end the job: it would still be necessary to prove the theorem logically to know its truth with the proper 'extent of validity'. Thus, if one reserves the word 'justification' for such ultimate proofs, there is no justification of theorems of complex number theory in this system. But that is irrelevant here. The point is that nothing in Frege's views forbids him from exploring semantics in this vein. Certainly, such investigations are fully compatible with the 'basic observation'.[44]

III. 'Substantive' and 'schematic'

I will return to these points but first I will highlight another distinctive turn of phrase that is highly charged in this line of interpretation: the suggestion that, for Frege, 'logical truth is not defined by schemata' and 'Logical laws are substantive, not schematic'. These locutions are first elaborated in any detail in, I believe, Ricketts' 'Objectivity and Objecthood'. There the 'substantive/schematic' division is put forward as marking one of the basic differences between Frege's conception and 'the modern conception' of logic. It requires some care to delineate just what Frege is taken to be unwilling to accept in this characterisation of his conception of logical truth 'not being defined by schemata' but the core observation, from which significant consequences are supposed to follow, is that when Frege's schematic talk is completely regimented, it will be represented by quantified sentences.

Whatever the schematic/nonschematic contrast is to amount to, it must account for the distinctions and moves that Frege makes in his dispute with Hilbert over the foundations of geometry. There, Frege directly confronts a 'schematic' presentation of geometries in terms of axioms with uninterpreted expressions. Frege makes it evident that he is opposed to the idea that one can determine a subject matter by writing down a set of such uninterpreted sentences and indirectly fixing a family of interpretations for them. However, Frege also indicates a means of approaching these questions that he takes to be acceptable.[45] He emphasises that one can (so long as various articles of logical hygiene are observed) acceptably develop second-order concepts of 'a geometry', a 'point of a geometry', and so on. Euclidean

geometry would then become, from this point of view, one of a family of geometries. Frege does indicate some logical complications that might ensue but he has no objection to the basic approach. That is, although he objects to the idea of resting with a set of only partially interpreted schemata and a class of models for them, he has no objection in principle to the exploration of families of models using second-order quantification.

Later, in the 'Foundations of Geometry' essays, Frege considers how one might mathematically address dependence and independence amongst thoughts. His treatment is careful but he suggests that the science can be developed rigorously:

> 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 need these basic truths especially to prove the independence of a thought from a group of thoughts.
>
> To lay down such laws, let us recall that our definition reduced the dependence of thoughts to the following of a thought from other thoughts by means of an inference. This is to be understood in such a way that all these other thoughts are used as premises of the inference and that apart from the laws of logic no other thought is used. The basic truths of our new discipline which we need here will be expressed in sentences of the form:
>
> If such and such is the case, then the thought G does not follow by a logical inference from the thoughts A, B, C.
>
> Instead of this, we may also employ the form:
> If the thought G follows from the thoughts A, B, C by a logical inference, then such and such is the case.
>
> In fact, laws like the following may be laid down:
> If the thought G follows from the thoughts A, B, C by a logical inference, then G is true.
>
> (*FG II*, p. 336)

Bearing in mind that, as Frege understands the expression 'inference', only true thoughts can be premises of inferences, the last of these 'laws' certainly *looks like* the inductive step of an inductive proof of the soundness of logical rules. Why should we not understand this to be just what it seems to be? On face value, the 'law' seems like a schematic statement of the soundness of single inferences. So what is specifically 'substantive' about Frege's view?

To be sure, when fully regimented, there will be appeals to quantification in places where, it is suggested, contemporary writers would rely on unquantified schemata. As I will explain in a moment, little hangs on this. Another point

raised in this connection concerns Frege's use of the truth-predicate. I will set that point aside for consideration later in the chapter. Right now, I will consider a third point that is put forward: although Frege appears to be laying the groundwork for proving something like soundness, he is not *defining* 'A follows from B, C, D' as 'The inference from B, C, D to A is sound' or 'If B, C, D are true then A will also be true'. The *definition* of 'A follows from B, C, D' is that A can be obtained from B, C, D using logical laws and inferences.

True enough: that is how Frege defines these ideas. However, that does not mean that Frege has any objection to the study of other notions of consequence that might be, in the 'new science' he sketches, provably equivalent to the one he defines. Frege is of course aware that there will typically be many logically equivalent definitions of concepts. So he might accept that there could be an equivalent semantic definition of consequence. Once again, the only reservation he would have would be that the equivalent definition would not be the basic one.

True, once all this reasoning is fully regimented, all general statements will be quantified but unless we are to attach a special, unexplained significance to quantification, it is unclear why this matters. Neither Frege nor the interpreters under consideration here give any indication of what that significance might be. This point is worth lingering over. Today, thanks largely to Quine, the question of whether or not a claim involves a quantifier is seen by many to be a matter of potentially great philosophical importance. Quantifiers, objectually interpreted, rather than singular terms, are seen by many as the bearers of 'ontological commitment', for example. But it is anachronistic to project this Quinean obsession back onto Frege. The formal structure of quantifiers alone does not force us to an objectual interpretation of the quantifiers: someone could accept the *Grundgesetze* and read the quantifiers as substitutional. As is well-known, the distinction between quantified sentences and schemata essentially disappears when the quantifiers are substitutional; the quantifiers serve just as indicators of substitutional order. Indeed, there are scholars who hold – not implausibly – that, in *Grundgesetze*, Frege's interpretation of the quantifiers was *in fact* what we would today describe as a substitutional one.[46] For our purposes, it will suffice to make a weaker point: nothing in Frege's writings should lead us to conclude that he *does not* interpret quantifiers substitutionally. Without any indication on Frege's part that quantification is to be treated as it is widely treated today, it is hard to see how the mere use of quantification indicates a deep division between Frege and his contemporary successors.

IV. A 'new basic law'

It will be helpful to consider these issues in connection with an extended passage from the controversy with Hilbert, following on the heels of the

discussion of soundness just considered. Frege first sets the stage for a new basic law by envisioning some sentences expressing thoughts the vocabulary of which can be correlated one-to-one:

> But our aim is not to be achieved with just these basic truths alone. We need 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. One might call it an emanation of the formal nature of logical laws.
>
> Imagine a vocabulary: not, however, one in which words of one language are opposed to corresponding ones of another, but where on both sides there stand words of the same language but having different senses. Let this occur in such a way that proper names are once again opposed to proper names [. . . and more generally:] words with the same grammatical function are to stand opposite one another. Each word occurring on the left has its determinate sense – at least we assume this – and likewise for each occurring on the right. . . . We can now translate; not, however from one language to another, whereby the same sense is retained; but into the very same language whereby the sense is changed. . . . Now let the premises of an inference be expressed on the left. We then ask whether the thoughts corresponding to them on the right are the premises of an inference of the same kind; and whether the proposition corresponding to the conclusion-proposition on the left is the appropriate conclusion-proposition of the inference on the right.
>
> (*FG II*, pp. 337–338)

Frege answers: yes, if the translation leaves (what we would now call) logical constants untouched. He does not give a criterion for logical constants. Since there does not seem to be much agreement on the characteristics of logical constants even today, however, this does not set him apart from us. To secure the desired invariants in the translation, Frege places additional constraints on which mappings from expression to expression can be acceptable:

> 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, insofar as optical and chemical properties are all the same to it. To be sure, so far as it is concerned, bodies with different masses are not mutually replaceable; but in gravitational mechanics the difference of

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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, sub-
ordination 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*, insofar 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. . . . 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. Let us assume the
vocabulary meets this condition. Then not only will a conclusion
again correspond to a conclusion, but also a whole inference-chain
to an inference-chain. I.e., to a proof on the left there will correspond
a proof on the right. . . .

Let us now consider whether a thought G is dependent on
a group of thoughts Ω. We can give a negative answer to this
question if . . . to the thoughts of group Ω there corresponds a
group of true thoughts Ω' while to the thought G there corresponds
a false thought Ω'.

<div align="right">(FG II, p. 338)</div>

Frege is taking a long time to arrive at a familiar conclusion: a proposition/
thought C is independent of a group of propositions/thoughts Ω if one can
obtain a collection of true thoughts Ω' and a false thought C′ by replacing
the non-logical vocabulary of the sentences expressing Ω and C with different
non-logical vocabulary. He takes time, not because he believes there to
be anything illegitimate about what he is doing, but rather because he is
attempting to correct what he takes to be Hilbert's unacceptably loose writ-
ing. He does not use uninterpreted symbols but rather speaks of replacing
interpreted symbols with other interpreted symbols. However, this does
not set him outside the spectrum of views in the contemporary mainstream.
His point is that one can arrive at general statements about consequence
and logical dependence by looking to the possibilities of interchanging
non-logical vocabulary while holding the logical vocabulary fixed.

If we define consequence in terms of following logical laws, what are the
'logical law'? Ricketts suggests that it is an important feature of Frege's
view that no criterion is given:

More than this, Frege lacks any general conception of logical con-
sequence, any overarching conception of logic [Ricketts' footnote
here reads: The closest he comes, in a very tentative discussion

in part 3 of 'On the Foundations of Geometry' (1906), p. 423, is a characterisation of a notion of logical dependence: one truth is logically dependent on another, if the first can be obtained from the second and logical laws by logical inferences. Neither in this paper nor elsewhere does Frege give a general characterisation of logical laws and inferences.] Frege has only a retail conception of logic, not a wholesale one. He tells us what logic is by identifying specific laws and inferences as logical.[47]

It is true that nowhere does Frege give a criterion of the logical, although this could simply reflect that he had not arrived at one. We cannot conclude much from the fact that Frege stops where he does. Since he was at this time attempting to patch up the system of *Grundgesetze*, it is not as if he did not have enough work to do. But whether or not a general criterion of logicality is possible is beside the point: all Frege's account of independence arguments need is a complete list of logical principles, whether or not that list is subsumed under a unifying criterion. Frege seems to be committed – at least early on – to the possibility of listing all logical laws, since in *Grundlagen* he says that one of the 'first requirements of Reason' is that '[Reason] must be able to embrace all first principles in a survey'.[48]

Following his definition of dependence, Frege notes the absence of a delineation of the logical, in words that suggest he regards this hurdle as difficult but not insuperable. It would be odd to write of a question he regards, in principle, as insoluble that it 'cannot be settled briefly':

> With this we have an indication of the way in which it may be possible to prove independence of a real axiom from other real axioms. Of course, we are far from having a precise execution of this. In particular, we will find that this *final basic law* which I have attempted to elucidate by means of the above-mentioned vocabulary still needs more precise formulation, and that to give this will not be easy. Furthermore, it will have to be determined what counts as a logical inference and what is proper to logic. . . . One can easily see that these questions cannot be settled briefly; and therefore I shall not attempt to carry this investigation any further here.
>
> (*FG II*, p. 339, my emphasis)[49]

This passage reveals several interesting thing: note in particular that Frege describes the translation principle he has just sketched as underwritten by a 'basic law'. That is, Frege is acknowledging the potential for a 'new science' underwritten by at least one evidently metatheoretic (presumably heretofore unformulated) 'basic law'.[50] A further observation pertains to the stance we can conjecture that Frege had about the importance and role of his inchoate new 'basic law'. To put oneself in Frege's position, it is important to know

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that the procedure he is describing – in which two sequences of sentences are lined up on the left and right, and the vocabulary is matched up one-to-one with certain canonical vocabulary held fixed, so that if the right hand side is a proof, the left-hand side is as well – was thoroughly familiar to geometers of the nineteenth century. As it happens, Frege is describing precisely the format that projective geometry texts used to illustrate the overarching character of projective plane duality. In most textbooks of projective geometry of the time, a standard format was adopted: plane projective theorems are written in two columns down the page, with each sentence in the right-hand column matched with its plane dual on the left.[51] Statements correlated perfectly so that paired expressions ('point'–'line', 'inscribed'–'circumscribed', 'conic'–'conic' (this last is self-dual), . . .) are lined up: the arguments are laid out so that each proof corresponds line by line and expression by expression with the dual proof of the dual theorem. It is, in fact, precisely the layout described by Frege when sketching his 'new basic law'.

There is no passage in the 'Foundations of Geometry II' essay where Frege explicitly states the connection with projective geometry and the principle of duality. However, Frege could not have failed to be aware that projective duality was an evident realisation of the 'new basic law' he was describing. Projective geometry was at the time seen as the core of all geometry. It apparently formed the very first topic covered in the graduate lectures Frege attended on geometry, for example.[52] Duality was seen as such a core fact that the 'dual columns' format was standard in both elementary textbooks and advanced research monographs. It is inconceivable that Frege would not have expected the readers of the mathematics journal in which this essay appeared to see the connection automatically.

Ricketts' discussion of the mathematical character of the 'new basic law' is an especially striking illustration of the way that neglect of the historical background can create astonishing blind spots.[53] Ricketts sets out to squeeze argumentative juice out of the claim that Frege did not regard this new basic law – or any other reasoning about thoughts – as *mathematical.* The expectations that arise from neglect of the context, plus apparently a reliance on an unreasonably sharp mathematics/philosophy distinction lead Ricketts to interpret Frege's words as meaning exactly the opposite of what they state on face value.

Ricketts cites these sentence:

> . . . we find ourselves with this question stepping into an area otherwise foreign to mathematics. For although mathematics is carried out in thoughts, thoughts themselves are otherwise not the objects of its consideration. [Even the independence of a thought from a group of thoughts is quite distinct from the relations otherwise investigated in mathematics.] We may conjecture that this new realm has its proprietary basic truths that are as necessary to proofs in

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that field as the geometrical axioms are for proofs in geometry, and that, in particular, we require these basic truths in order to prove the independence of a thought from a group of thoughts.[54]

Ricketts reads these lines as arguing that relations amongst thoughts are *not* part of the subject matter of mathematics, and uses that reading to support his overall interpretation. Just after citing the passage cited on the preceding page, Ricketts glosses them as follows:

> We have here a criticism of Hilbert. Hilbert presents his independence results as a piece of mathematics. As Frege interprets Hilbert's achievement . . . it is a straightforward piece of mathematics. The question now before us is the relevance of this achievement to proving the independence of genuine axioms. Independence proofs directed at axioms in the traditional sense, Frege urges, must invoke extra-mathematical laws, laws about thoughts themselves. Hilbert, citing no such laws, fails to give his mathematical results the application he claims for them . . . Frege clearly takes the new science whose possibility he is exploring to be distinct not only from mathematics but also from logic itself. Logic, as Frege conceives it, is no more about thoughts than mathematics is.[55]

Ricketts says that it is 'clear' that Frege takes the new science to be distinct from mathematics and logic. I will consider first the point about mathematics. Drawing on the first-quoted passage, which is all that Ricketts gives as evidence, not only is it not *clear* that Frege thought the new science to be distinct from mathematics, but the plain text – specifically Frege's repeated use of 'sonst/otherwise' – shows it is clearly *false*.[56] The threefold repetition of 'otherwise' ('*sonst*') points unequivocally to this reading of the passage: deductive and dependence relations amongst thoughts *are* part of the subject matter of mathematics, although they differ from the kinds of things normally studied in mathematics.

As to whether or not the science of independence arguments can be part of logic, the situation is not *clear*, as Ricketts states, but at least this part of Ricketts' claim is not clearly false. The situation is complicated, so I will leave most of the discussion for other work. However, it should be noted that what textual evidence there is suggests that Frege does hold independence arguments in themselves to be, at least in principle, derived from logical laws. The key texts here are in the second volume of *Grundgesetze*. In the body of the text, Frege makes remarks that might be taken to suggest principled doubts about independence arguments. After laying out a definition of 'Positivalklasse' containing a long list of conditions, his words *seem* to suggest that the independence of the conditions can only admit of the most simpleminded inductive support:

With the installation of this definition, I have taken the trouble to fix only the necessary conditions, and only those that are independent from each other. That this has succeeded can not admittedly be proven, but it becomes likely however, if attempts to derive one of these conditions from others fail many times. It appears impossible to achieve the objectives without [formula; omitted here].[57]

In 1903, Frege *recognised* that these words might be wrongly taken to indicate a principled stance and he added a remark – in press – to disavow the impression. His point, he says, is not that this independence argument is impossible in principle but rather that he doubts that the system of the *Grundgesetze* had been, at that point, developed far enough to support such an argument. The added remark suggests that Frege sees no reason to doubt that at a further 'stage of the investigation' such examples could be provided if the conditions are in fact independent.

> **Remark** on §175 P. 172 First Column
> It should not necessarily have been stated that the independence of the stated conditions from one another could not be proven. It is of course conceivable that one could find classes of relations, to which every condition would apply but one, and that every condition would fail in one of the examples. But it should be questioned whether at this stage of the investigation it is possible to give such examples without presupposing geometry, or fractional, negative and irrational numbers, or facts of experience.[58]
>
> ([*Gz II*], p. 243)

Here, Frege indicates that the independence (in the sense of 'no derivation possible') can be demonstrated by producing a counter-interpretation. In listing what cannot be used, Frege mentions 'fractional, negative and irrational numbers'; conspicuously omitted is 'integers'. He lists as unavailable only things he does not take himself – at this stage – to have logically derived. In addition, by excluding geometry and facts of experience, the only sources of knowledge besides the logical source that he considers in his previous writings – Frege reinforces the impression that he takes the prospective independence argument to be logical, flowing from 'the logical source of knowledge'.

Let us briefly revisit the point about the mathematical naturalness of Frege's rudimentary semantics in light of a place where Frege does explicitly discuss the principle of duality. His words reveal his embrace of the idea that one can illuminate this sort of fact by considering it from two sides: in terms of the language used and in terms of the structures described.

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The authors show an insufficient insight into the respective positions of projective and metrical geometry. The correct relationship may be intuited by means of the following picture. Projective geometry may be likened to a symmetrical figure where every proposition has a proposition corresponding to it according to the principle of duality. If we cut out some arbitrary portion, the figure is in general no longer symmetrical. Metrical geometry may be likened to such a cut-out. . . . To put it in non-pictorial terms, metrical geometry arises from projective geometry by specialisation, and this is precisely why the principle of duality loses its validity.[59]

Frege shows a healthy respect for the value of studying in tandem both the logical structure of duality principles and the corresponding symmetries of the underlying geometric realizations. This sort of work was already being done outside of logic. For example, even at the time, duality principles in geometry were of crucial importance in studying the geometric structures of symmetric crystals.[60] The relations between the dualities of the theorems about crystals and the corresponding symmetries in the crystals described were recognised as important and as the basis for further study. Frege need not have been familiar with that work, but he may well have been. It was the sort of thing that, at that time, was done. One could only assume that if Frege thought such work unrepresentable in his system, he would see that as a basis for adding yet another new basic law, and perhaps more primitives, rather than as consigning such studies to the realm of inexpressible propaedeutic.[61]

V. Truth and semantics

A further point marshalled in Ricketts' writing in support of the thesis that Frege could not have endorsed 'semantic metatheory', revolves around Frege's attitude toward truth. So, for example, Ricketts states that Frege is precluded from taking up the 'contemporary view' because that view requires a use of the truth-predicate that Frege was committed to rejecting:

Nor is it possible, through reasonable emendations, to read the contemporary view back into Frege. For the contemporary view requires the ineliminable use of a truth predicate. Such a use is antithetical to Frege's conception of judgement. This conception of judgement precludes any serious metalogical perspective and hence anything properly labeled a semantic theory.[62]

Later on, Ricketts reiterates the argument of his 'Objectivity and Objecthood' as follows:

From a contemporary perspective, we would say that the basis for the permission that [*modus ponens*] grants is the soundness of the rule under the intended interpretation of Frege's formalism. Formulation of this basis requires, however, the use of a truth predicate. I have argued elsewhere[63] that Frege's view of truth bars the serious, scientific use of a truth-predicate, for truth is not a property of the thoughts that sentences express. In this sense, then, there is no stateable basis for the permissions that Frege's inference rules grant, and thus no scientific theorizing about provability. There is, in the end, just the rigorous, explicit construction of proofs in the Begriffsschrift.[64]

So far, we have seen two arguments for the conclusion that Frege would have rejected 'semantic metatheory': the 'basic observation' and the implicit appeal to limitative results that Frege surely did not anticipate. The discussion so far indicates that these give no reason to think that Frege would have held such soundness statements incapable of regimentation so as to participate in 'rigorous, explicit construction of proofs in the Begriffsschrift', so long as the soundness of a rule is not treated as a more basic fact than the correctness of inferences according to the rule. In these remarks, we see a third consideration, turning on Frege's attitude to the truth-predicate. What does this additional consideration add?

Two questions must be distinguished: I) is Ricketts' account of Frege's treatment of the truth-predicate correct? II) if we accept, for the sake of argument, that Ricketts gets Frege's views on the truth-predicate right, is there anything in this view of the truth-predicate that would be incompatible with a view of semantics like Tarski's? (For polemical reasons, it will be useful to ask this question about the historical Tarski, although the point is independent of the attribution.)

I do, in fact, regard Ricketts' claim as textually indefensible as far as Frege's writings throughout the nineteenth century are concerned, which is what matters for our interpretation of his technical writings. (After 1906, the issue became cloudier, but that shift is not relevant to *Grundgesetze*.) However, on this point I have little to add to the discussion in Stanley 1996 so I will refer the reader there.[65] What I will address is II): as far as Tarski's conception of logical consequence is concerned, Frege's views of the truth-predicate can only be a red herring.

To sharpen the discussion, let us (sketchily) imagine how one might develop 'semantics in the manner of Tarski' for the first-order fragment of the Begriffsschrift within the framework of the Begriffsschrift as a whole. There should be no objection to functions from names to what those things name. Frege did not hesitate to argue (in *Grundgesetze*) that all the well-formed singular terms of his system denote. The treatment of functional expressions, of course, would be more delicate because of the 'concept

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horse' problem. However, Frege does accept that one can approach concepts by talking about signs, if certain niceties are adhered to:

> If we want to express ourselves precisely [about function and object], our only option is to talk about words or signs. We can analyse the proposition "3 is a prime number" into '3' and 'is a prime number'. These are essentially different: the former complete in itself, the latter in need of completion. . . . This difference in the signs must correspond to a difference in the realm of meanings; though it is not possible to speak of this without turning what is in need of completion into something complete and thus falsifying the real situation.
>
> (*Corr*, pp. 141–142)

So let us note: if we assign objects, like sets, extensions of concepts or 'courses of values' to functional expressions, we are 'falsifying the real situation'. In other words, we are not specifying a function but rather 'letting an object go proxy' for a function (*CO*, p. 186): fair enough. However, say that our concern is not to capture 'the real situation' but only to work out a definition of 'is true' that will a) satisfy Tarski's formal correctness and material-adequacy conditions and b) will allow a characterisation of semantic consequence such that all and only those sentences expressing thoughts that are logical consequences in Frege's sense will turn out to be semantic consequences in our defined sense. In this case, the 'concept horse' problem is just beside the point. The resulting definition would be a definition of truth for sentences rather than for thoughts. But Frege has no objection to studying the structure of thoughts in a 'mirror', through systematic reflections on the structure of the sentences that express thoughts.

Clearly, some such development could be worked out, and the definitions of first-order consequence and truth could be laid out. So long as the definition of consequence is not put forward as a *reduction* of the notion of consequence but rather as an equivalent, derived characterisation of (the first-order part of) consequence as Frege understands it, it is hard to see what objection Frege would have to engaging in this kind of study, nor is there reason to think he would not find it revealing and interesting. But what of the definition of truth? It seems implicit in Ricketts' emphasis on the role of the truth-predicate for Frege that some response like this is likely to be forthcoming: 'Whatever is here defined, it is not truth, as Frege understands it. Frege's conception of truth would require him to reject this definition.'

My purpose here is not to controvert this point. For present purposes just note that this response does not involve rejecting the semantics we have just sketched: rather it denies that the inquiry analytically captures the concept of *truth*. This leaves us with an acute variation on a worry that troubled

us above: what is being excluded? Who is the opponent? Say we hold Frege to have a specific attitude toward truth as he understands it, and hold him to reject any model theory incorporating truth so understood. That may be interesting but it tells us nothing about 'the contemporary conception', if Tarski is taken to be a representative. After all, Tarski anticipated objections of the form: 'Whatever you have defined, it is not *truth*' and responded as follow:

> Referring specifically to the notion of truth, it is undoubtedly the case that in philosophical discussions – and perhaps also in every-day usage – some incipient conceptions of this notion can be found that differ essentially from [mine]. In fact, various conceptions of this sort have been discussed in the literature . . .
>
> It seems to me that none of these conceptions have been put so far in an intelligible and unequivocal form. This may change, however; a time may come when we find ourselves confronted with several incompatible, but equally clear and precise, conceptions of truth. It will then become necessary to abandon the ambiguous usage of the word "true" . . . Personally, I should not feel hurt if a future world congress of the "theoreticians of truth" should decide —by a majority of vote—to reserve the word "true" for one of the [other] conceptions, and should suggest another word, say, "*frue*" for the conception considered here. But I cannot imagine that anybody could present cogent arguments to the effect that the semantic conception is "wrong" and should be entirely abandoned.[66]

So, even if Frege does have scruples about truth, there need be no conflict with Tarski. Frege will, at worst, require that Tarskian semantics refrain from using the label 'true'. This is a minor loss, since we have seen no reason for Frege to oppose Tarski's systematic theory of fruth (defining fruth in terms of denotation, with a derivation of all instances of 'S' is frue if and only if S, etc.). One could then prove the soundness of *modus ponens*, in terms of fruth-preservingness, strive to formulate axiom systems whose completeness relative to the fruth-semantic consequence relation could be proven, etc. Again, we are left with a quite faint sense of what real conflict there might be with the modern attitude to logic, semantics and metatheory.

VI. Concluding summation

The 'no metatheory in Frege' view is based on real (although in the final analysis minor) insights and it is to Dreben's credit that he arrived at them. The formal incarnations of the ideas of soundness and validity are familiar and natural to us today in a way that they were not for Frege. Frege did emphasise that the laws of logic were to have content. Also, I have the

impression that the original impetus for Dreben's views was a family of anachronistic interpretations of Frege's project that had become established. The suggestion that Frege's conception was different from ours in the observed respects served an important function in spurring Frege inter- pretation to attain a deeper level of historical subtlety. The objective of this chapter is not to quarrel with these insights, but rather to observe that the attempts to develop the view into something more than it is have hardened into an orthodoxy that is bound up with anachronisms all of its own. Amongst these are the idea that Frege has any conception of a metatheory/ object theory distinction, and the consequent suggestion that certain sorts of arguments – like those pertaining to soundness or denotation – that we now count as 'metatheoretic' would be seen by Frege as having such a special character, requiring some special 'external perspective'.

I should not let this discussion pass without acknowledging my enthusi- astic endorsement of what Ricketts describes as: 'Burton Dreben's repeated insistence on the role of Frege's mathematical training and interests in shap- ing his philosophy'.[67] Efforts to develop Dreben's views appear to have gone astray through a failure to take this counsel sufficiently to heart. A failure to see Frege's research in its mathematical context obscures the fact that his 'new basic law' was hardly of a kind unfamiliar to him. During the many terms in which he lectured on analytic geometry, the law stared him in the face every day. Far from having a special character of a 'non-mathematical' sort, it was a slightly more general version of a paradigmatically math- ematical principle that had been known, studied and generalised for close to a century. The 'new science' that would result from working out his 'new basic law' would have straightforward, immediate mathematical applications in a context familiar to him.

This allows us to draw one moral: we should not assume that we can settle debates as to what Frege would have regarded as natural without genuine scholarly effort. Perhaps the idea that certain questions are smooth outgrowths of ongoing mathematical practice and others are merely orient- ing puzzles that need to be set aside when the real science begins is of value in studying Frege. But we can only get a confident grip on where Frege would see that distinction as falling if we have some sense of the problems he took mathematics to be addressing. Without serious immersion in Frege's mathematical environment, consulting our own intuitions about how we would draw the line if we were Frege is unlikely to serve as a reliable guide.[68]

Notes

1 This chapter is significantly abbreviated and slightly expanded from the article that was originally published in *Philosophical Topics*. The slight expansion con- sists of some material added to take into account more recent work by proponents of the school criticised here, although of course only so much can be done

without completely rewriting the article. There has been a great deal of work in these areas since the original article appeared, so I have restricted myself to just a few observations that will not disrupt the overall flow of argument and will fill gaps left by the omission of material from the original version. More detailed critical scrutiny is available in my more recent work. The abridgements – responding to the request of the editors to cut the article down in length – come in two varieties. In some cases I have omitted discussions that seemed in retrospect to address relatively minor side issues. The second variety of omission sharpens the focus of the chapter and changes its character somewhat. The original article was intended both as a generally negative critical assessment of the line of interpretation of Frege that suggests his views are somehow incompatible with 'contemporary metatheory' and as a positive discussion of some of the features of the nineteenth-century mathematical context that make metatheory a natural and inescapable outgrowth of a cluster of problems confronted by nineteenth-century mathematicians. The second objective was intertwined with a more general metadiscussion of the importance – for our understanding of Frege – of genuine mathematical history rather than armchair caricature. I have developed and deepened the positive account in more recent work, and I will refer the reader there for the updated account of the emergence of metatheory from the problems of nineteenth-century mathematics. The revised article is restricted to the critical assessment of the 'no metatheory' interpretation of Frege, setting out only those details of nineteenth-century mathematics needed for this purpose. (For the mathematical history, plus further critical examination of the 'no metatheory' view, see my *Frege and the Emergence of Modern Mathematics* (working title) to appear with Oxford University Press. Some details of Frege's attitudes to independence proofs, and the mathematical context for these investigations, appears in my 'Frege on Axioms, Indirect Proof and Independence Arguments in Geometry: Did Frege Reject Independence Proofs?', *Notre Dame Journal of Formal Logic* 41(3) (2000).

2 Ernst Nagel, 'The Formation of Modern Conceptions of Formal Logic in the Development of Geometry', in *Teleology Revisited and Other Essay*, 1939, New York: Columbia University Press 1979.

3 Otto Hesse, 'Ein Übertragungsprinzip', *Journal für die Reine und Angewante Mathematik* 66 (1866): 531–538. ([1897] *Gess. Werke*, pp. 531–538.)

4 Otto Hesse, 'Vier Vorlesungen aus der Analytischen Geometrie', *Zeitschrift für Mathematik und Physik* 11 (1866): 369–425.

5 'On A Geometrical Representation of Imaginary Forms in the Plane', in *Collected Papers* (henceforth [IFP]), p. 3. I have changed the translation of 'übertragen' from 'carry over' to 'transfer' so as to conform to the usual choice in English translations of German mathematical works. I do not want to rest argumentative weight on the choice of expression. In fact, Frege probably meant his remarks to echo Hesse's by then standard technical usage, but that is less important than the fact that Frege appreciates the underlying idea. (Still, it is worth noting that of the many words Frege could have chosen here, he chooses an expression that is relatively little used in everyday language, but that had at the time a specific and well-known technical use.)

6 Felix Klein, 'Über die Sogennante Nicht-Euclidische Geometrie', *Mathematische Annalen* VI (1873): 314:

> Die Untersuchungen der Nicht – Euklidischen Geometrie haben durchaus nicht den Zweck, über die Gültigkeit des Parallelenaxioms zu entscheiden, sondern es handelt sich in denselben nur um die Frage: of

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das Parallelenaxiom eine mathematische Folge der übrigen bei Euklid aufgeführten Axiome ist; Ein Frage, die durch die fraglichen Untersuchungen definitiv mit *Nein* beangtwortet wird.

7 S. Feferman, 'Working Foundations', in *In the Light of Logic*, Oxford: Oxford University Press, 1998, pp. 105–124.

8 J. van Heijenoort, 'Logic as Calculus and Logic as Language', *Synthese* 17 (1967): 324–330.

9 Warren Goldfarb, 'Logic in the Twenties: the Nature of the Quantifier', *Journal of Symbolic Logic* 44 (1979): 353.

10 Warren Goldfarb, 'Logicism and Logical Truth', *Journal of Philosophy* (1982): 694.

11 James Conant, 'The Search for Logically Alien Thought: Descartes, Kant, Frege and the *Tractatus', Philosophical Topics* 20 (Fall 1991): 171, n. 58.

12 Thomas E. Ricketts, 'Objectivity and Objecthood: Frege's Metaphysics of Judgment', in *Frege Synthesized*, L. Haaperanta and J. Hintikka (eds), Dordrecht: D. Reidel, 1986, p. 80.

13 *Ibid.*, p. 76.

14 *Ibid.*, p. 67.

15 B. Dreben and J. van Heijenoort, 'Introductory Note to Gödel 1929, 1930, 1930a', in *Kurt Gödel: Collected Works*, New York: Oxford University Press, 1986, pp. 44–60. I am indebted to Richard Heck and Ian Proops for drawing this passage to my attention.

16 Joan Weiner, *Frege in Perspective*, Ithaca, N.Y.: Cornell University Press, 1990, p. 227. In the original article I mistakenly omitted the word 'primitive' from this quote.

17 P. Hylton, *Russell, Idealism and the Emergence of Analytic Philosophy*, Oxford: Oxford University Press, 1990, p. 203.

18 In the original article, I was inclined to grant the point about 'universalism' but in the interim my colleague Ian Proops has shown that the idea of the 'universalist conception of logic', as it is deployed in Russell and Frege scholarship, is unexpectedly shaky. See his 'Russell and the "Universalist" Conception of Logic' forthcoming in *Nous*.

19 Warren Goldfarb, 'Logicism and Logical Truth'; James Conant, 'The Search for Logically Alien Thought'.

20 So, for example, in the account sketched in Saul Kripke's 'Outline of a Theory of Truth', *Journal of Philosophy* (1975), the truth-predicate can be contained in the theory it is a truth-predicate for. (This specific theory has partially defined predicates, which conflicts with Frege's sharp boundaries requirement.)

21 The unmodified remarks alluded to are: 'The general standards for the judgements of a discipline are not provided by statements about the discipline. They are provided by judgements *within* the discipline.' Ricketts, Objectivity and Objecthood', p. 80.

22 Gottlob Frege, *Basic Laws of Arithmetic: Exposition of the System*, M. Furth (trans.), Berkeley: University of California Press, 1964, p. 130; hereafter referred to as *BLA*.

23 See, for example, Gottlob Frege, 'Formal Theories of Arithmetic', in *Collected Papers*, B. McGuinness (ed.), M. Black *et al.* (trans.), Oxford: Blackwell, 1984, pp. 112 and 114; hereafter referred to as *FTA*.

24 J. Floyd, 'Frege, Semantics and the Double Definition Stroke', *The Story of Analytic Philosophy: Plot and Heroes*, A. Biletzki and A. Matar (eds), London: Routledge, 1998, p. 151.

25 Ricketts, 'Logic and Truth in Frege', p. 128.

26 *Ibid.*, p. 136.

27 Here, I am especially indebted to Jason Stanley for drawing this section of *BLA* to my attention.

28 This point in the original article should be adjusted. I suggested that there is no reason to accept Ricketts' view that the horizontal is the translation of 'is the True'. Joan Weiner has pointed out in an unpublished paper a strong reason to say the horizontal is *not* the representation of 'is the true', since in 'On Concept and Object', Frege argues that expressions of the form 'is the K', where 'the K' denotes an object, must be regimented '= k' (pp. 183–185), where 'k' is a name of the K. So the most plausible suggestion is that 'is the True', should be regimented '= T'. As a result, the reasoning involving 'is the True' is straightforwardly representable in the *Grundgesetze* system but, of course, 'is the True' is not a predicate of thoughts with the logical properties of the truth predicate in the 'Foundations of Geometry II' treatment of metatheory.

29 Ricketts, 'Logic and Truth in Frege', p. 136.

30 *Ibid.*

31 Letter to Russell, *Philosophical and Mathematical Correspondence*, G. Gabriel, H. Hermes, F. Kambartel, C. Thiel and A. Verhaart (eds), abridged from the German edn. by B. McGuinness, H. Kaal (trans.) Chicago: University of Chicago Press, 1980, p. 132; hereafter referred to as *Corr*.

32 Lewis Carroll, 'What the Tortoise Said to Achilles', *Mind* 1895.

33 Ricketts, 'Logic and Truth in Frege', p. 136.

34 Ricketts, 'Objectivity and Objecthood', p. 83.

35 D. Gabbay (ed.), *What is a Logical System?*, Oxford: Oxford University Press, 1994.

36 Ian Hacking, 'What is Logic?', in *What is a Logical System?*, Gabbay (ed.).

37 Ricketts, *Logic and Truth in Frege*, p. 123; Ricketts writes:

> Michael Dummett asserts, 'Reality cannot be said to obey a law of logic; it is our thinking about reality that obeys such a law or flouts it.' However correct this precept may be for some contemporary views of logic, it is false of Frege's. It has long been established that Frege has a universalist conception of logic.

(Ricketts adds a footnote that reads: 'Goldfarb, "Logic in the Twenties: the Nature of the Quantifier," *Journal of Symbolic Logic* 44 (1979). Goldfarb observes that Dummett anachronistically assimilates Frege's conception of logic to a post-Gödelian one.')

38 Ricketts, 'Objectivity and Objecthood', p. 76.

39 'Frege's Conception of Logic', in S. Sheih and J. Floyd, *Futures Past*, Oxford: Oxford University Press, 2001.

40 Goldfarb, 'Frege's Conception of Logic', pp. 25–26. The two works of Quine that Goldfarb cites are Quine's *Elementary Logic*, Boston: Ginn, 1941, and *Methods of Logic* New York: Holt, 1950.

41 Say we stipulate, for the sake of the discussion, that in the cited writings Quine does articulate what Ricketts and Goldfarb call 'the contemporary conception'. After laying out the position, Goldfarb addresses the question of what, if any, conflict there is between Frege's view and the 'contemporary conception'. He arrives at this point: on the contemporary conception, there cannot in principle be a universal set (as opposed to a proper class) as a domain of quantification, while for Frege the quantifiers range unrestrictedly (Goldfarb, 'Frege's Conception of Logic', pp. 38–39). Goldfarb presents this as a forced result of

following out the implications of the 'schematic conception' rather than as an independent development. For the sake of argument, let us grant that the attribution to Frege is correct. The concern with the size of the universe is of course a post-paradox development without clear relevance to earlier writings like *Grundgesetze*, but let us set that aside. Then we do seem to have a point of disagreement between Frege and many contemporary researchers today. Yes, indeed, although some recent work has tried to revive the idea of a universal domain of quantification, it is not a stretch to suggest that *almost everyone* accepts that a universal domain would have to be a proper class. Almost everyone, that is . . . except Quine, who has a universal set as part of his system *NF*! (See, for example, 'New Foundations for Mathematical Logic', in *From a Logical Point of View*, 2nd edn, New York: Harper and Row, 1961.) Whatever 'schematic conception' may be implicit in Quine, it clearly does not entail that the domain of quantification has growing pains. So even the one supposed canonical representative of the purported 'contemporary schematic conception' does not fit the story.

Obviously, if we hopscotch across the range of contemporary views, taking a part from here and a part from there, perhaps with the assistance of a lightning storm, we can assemble a Frankenstein's monster position from which Frege (as much as anyone else) would recoil in horror. But we should not think that the possibility of such a diversion tells us anything about the work of actual researchers as they occur in nature.

42 Gottlob Frege, *The Foundations of Arithmetic*, J. L. Austin (trans.), 2nd rev. edn, Evanston: Northwestern University Press, 1980, pp. 113–114; hereafter referred to as *FA*.

43 'On a Geometrical Representation of Imaginary Forms in the Plane', in *Collected Papers*, p. 55.

44 One attempt to widen the scope of the 'basic argument' in Frege's case might turn upon Frege's attitude toward the *full explicitness* needed for proofs to be adequately 'gap-free'. Say we count *modus ponens* as a basic rule. For proofs appealing to *modus ponens* to be gap-free and correct, it is only necessary that *modus ponens* actually *be* sound, not that it be proved sound. The counter-suggestion appealing to 'full-explicitness' considerations would reject that Frege has room for this: if *modus ponens* must *be* sound for proofs using it to be cogent, then it must be *proven* to be sound in any proof using *modus ponens* to be gap-free. Thus, the infinite regress argument would be smuggled back in through a back entrance. But this suggestion – unlikely on face value – does not survive comparison with the texts: Frege explicitly states that the rigour of proofs can depend on a principle without that principle being itself proven and included in any proofs that depend on it. Once again, a key is the early sections of *Grundgesetze*. After discussing various bits of the work that can be skimmed on a first go-through, Frege remarks that when this first sweep is completed:

> [the reader] may reread the Exposition of the Begriffsschrift as a connected whole, keeping in mind that the stipulations that are not made use of later and hence seem superfluous serve to carry out the basic principle that every correctly-formed name is to denote something, a principle that is essential for full rigor.
>
> (*BLA*, p. 9)

Frege's practice reinforces the point: he does engage in various contortions to ensure that every singular term will denote. He clearly does regard it as essential to full rigour that every singular term in his system denotes. He thinks that, for

the specific system of *Grundgesetze*, it can be *proven*. But he does not think that proofs in the system have gaps unless the proof at §31 is tacked on. To suggest otherwise just misunderstands what is involved, for Frege, in providing gap-free proofs.

45 Gottlob Frege, 'On the Foundations of Geometry: First Series', in *Collected Papers*, B. McGuinness (ed.), pp. 282–284; hereafter referred to as *FG*. 'On the Foundations of Geometry: Second Series' is also in *Collected Papers* and is hereafter referred to as *FG II*.

46 Richard Heck in 'Frege and Semantics', in T. Ricketts (ed.), *Cambridge Companion to Frege*, forthcoming; Boolos and Stanley in conversation.

47 Ricketts, 'Logic and Truth in Frege', p. 124.

48 *FA*, p. 6.

49 I have omitted one paragraph with additional questions. It is possible that the 'these questions' refers to them, and not the questions in both. I think it is the latter, and that anyway nothing much hangs on this, but do not want the ellipsis to mislead.

50 Here, I have edited out a discussion of an argument that Ricketts hinted at in the writings I discussed, and develops further in more recent writings: that Frege holds the comprehensiveness of a formulation of logic to admit of only what Ricketts calls 'experimental' support. Since I discuss the point in more recent work, I will not expand the early discussion here.

51 In the original article, I reproduced two pages from Cremona's projective geometry textbook of 1893 as an illustration; I have omitted them here for the sake of space. Readers seeking the full visual experience can look up the original of this article or any projective geometry textbook written in the nineteenth century.

52 A. Clebsch, *Vorlesungen über Geometrie*, Vol. 1, Leipzig, 1876. (I discuss the significance of this source in 'Geometry and Generality in Frege's Philosophy of Arithmetic'.)

53 The next few paragraphs were added to the new version of the article.

54 Quoted in Ricketts, 'Frege's 1906 Foray into Metalogic, *Philosophical Topics* 25(2) (Fall 1997): 181. (I have interpolated one sentence omitted by ellipsis.)

55 Ricketts, 'Frege's 1906 Foray . . .', p. 181. Here, there appears to be a certain slipperiness between the thesis that, for Frege, the scope of logic is *exclusively* thoughts, which is obviously false, and that the scope of logic *includes* thoughts (along with everything else), which is clearly true.

56 If, in a discussion of Emmanuel Lasker, I say that world chess champions have not otherwise been mathematicians of the first rank, I unambiguously convey that Lasker *was* such a mathematician, although he was unusual in that regard. What work would 'otherwise' otherwise be doing?

57 *Grundgesetze der Arithmetik*, Vol. II, Jena, 1903 (henceforth *Gz II*), pp. 171–172:

> Bei der Aufstellung dieser Definition habe ich mich bemüht, nur die nothwendigen Bestimmungen aufzunehmen und nur solche, die von einander unabhängig sind. Dass dies gelungen sei, kann freilich nicht bewiesen werden, wird aber wahrscheinlich, wenn mehrfach Versuche misslingen, einige dieser Bestimmungen auf andere zurückzuführen. Insbesondere scheint es nicht möglich zu sein, die Ziele [formula; omitted here] zu entbehren.

58 Anmerkung zu §175 S. 172 Erste Spalte:

Dass die Unabhängigkeit der angeführten Bestimmungen von einander nicht bewiesen werden könne, soll nicht unbedingt hingestellt werden. Denkbar ist es ja, dass man Klassen von Relationen auffinden könnte, für welche immer Bestimmungen bis auf eine zuträfen, sodass jede von diesen in einem der Beispiele nicht zuträfe. Aber ob es an dieser Stelle der Untersuchung ohne voraussetzung der Geometrie oder der gebrochenen, negativen und irrationalen Zahlen oder von Erfahrungsthatsachen möglich sei, solche Beispiele zu geben, ist zu bezweifeln.

59 Gottlob Frege, Review of Gall and Winter, *Die Analytische Geometrie des Punktes und der Geraden und Ihre Anwendung auf Aufgaben*, in *Collected Papers*, B. McGuinness (ed.), p. 95.
60 *Cf.* E. Scholz, *Symmetrie Gruppe Dualität*, Basel: Birkhauser, 1989.
61 Here I have omitted a section discussing the question of whether or not explanation for Frege consists of demonstrative reduction to more basic principles. The conclusion is (i) Frege probably did not have a settled, thought out view of what explanation consists in and (ii) what little textual evidence there is indicates that if Frege did have a view of what explanation consists in, he did not take explanation to consist in deductive reductions to more basic terms.
62 Ricketts, 'Objectivity and Objecthood', p. 76.
63 Ricketts, 'Objectivity and Objecthood', and Ricketts, 'Frege, the *Tractatus* and the Logocentric Predicament', *Noûs* 19 (1985): 3–15.
64 Ricketts, 'Truth-Values', p. 203.
65 Stanley, 'Truth and Metatheory'; forthcoming work of Richard Heck is also of interest in this connection.
66 A. Tarski, 'The Semantic Conception of Truth and the Foundations of Semantics', repr. in *Semantics and the Philosophy of Language*, Linsky, L. (ed.), Urbana: University of Illinois Press, 1952, p. 28.
67 Ricketts, 'Objectivity and Objecthood', p. 95, n. 43.
68 This chapter has been long in gestation and it has developed through many forms. At each stage in development, I have accumulated debts that it is now my pleasure to acknowledge. I owe a great debt to Hans Sluga, for helping me in my first halting steps toward fitting Frege in his historical setting. I have learned the most about the topic of these papers in conversations with Richard Heck and Jason Stanley. Jason Stanley's 'Truth and Metatheory in Frege', *Pacific Philosophical Quarterly* 77 (1996): 45–70, is similar in topic and content to this paper, and could be read as a companion to it. On topics where I feel Jason has already covered the ground adequately, I have tried to avoid duplication. In particular, I give the topic of Frege's regress argument on the truth-predicate little notice, since most of what I would say has already been said clearly and cogently by Jason.
   I was first introduced to the interpretation discussed here in a graduate seminar I co-taught with James Conant at the University of Pittsburgh. Jim was a tireless guide to this family of views and their characteristic dialectical patterns and I learned much from him. Subsequently, I had several illuminating discussions with Joan Weiner that cleared up many issues for me. I am grateful to Joan for her time and openness. A sketch that might be seen as a first draft of this paper began as comments on a draft of Tom Ricketts' 'Logic and Truth in Frege', *Aristotelian Society* Suppl. Vol. (1997): 121–140. Subsequently, I learned much from further discussions of that paper with Tom, although the final version of this article and that one will give a fair estimate of the extent of the remaining

differences between us. The richest encounters for me were two graduate seminars with Burton Dreben, during a year when I was visiting Boston. Although we appear to disagree on how to read virtually every line Frege wrote, I found the experience of hashing these things out to be immensely exciting and instructive. I remember our conversations with warmth and gratitude. Also, both in and out of the seminar, I enjoyed and learned from discussions with Ian Proops, Juliet Floyd and Steven Gross. Conversations with Susan Sterrett and one of her unpublished papers (currently online at the Pittsburgh philosophy of science archive at philsci-archive.pitt.edu/archive/00000723/00/SterrettFregeHilbert1994.pdf) was important in sparking my early thoughts on the Hilbert-Frege correspondence.

The first sections of the 'Many Faces of Metatheory' section were incorporated into a talk given at Harvard in the spring of 1995. Warren Goldfarb was in the audience and although he said nothing of substance to me at the time or later, in important ways his subsequent manoeuvres taught me a great deal about the devices needed to keep his interpretation viable.

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