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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 paper aims to critically examine this view, and to isolate what I take to be the core of truth in it. But I will also argue that once we have isolated this defensible kernel, the sense in which Frege was committed to rejecting "metatheory" is too narrow and uninteresting to support the conclusions the thesis has been taken by its proponents to support.

Though the main objective of this paper is the discussion of this narrowly delineated scholarly point about Frege's texts, there is a more diffuse motivation for the paper that might best be made explicit. It seems to me to be a crucial observation about everyday mathematical activity that such activity, when done productively, incorporates a kind of critical self-scrutiny. (This is true both of contemporary mathematical investigation and of the mid- to late-nineteenth-century research in geometry in which Frege was trained and in which he carried out active research.) 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. This shows itself in many ways. To list just a few, it is displayed when mathematicians make efforts to ascertain the most productive formulations of questions, or the most fruitful terms in which to pose problems, or the most illuminating general techniques, or the theoretical contexts in which the "essentials" of a problem are "best laid bare." Theorem proving, though 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 didn't practice "metatheory," though suggestions of this more narrow kind face a danger of simply collapsing into the observation that Frege was not Tarski. The objective in this paper 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 methodology.

MATHEMATICAL CONTEXT

In his *Historic Development of Logic*, written early in this 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.[2] 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."[3] This essay is now fifty years old, and it has aged as such essays will. Much of it seems whiggish and naively positivistic, and many of its assessments of broader philosophical significance seem strikingly dated. But for all that, the essay holds up quite well: the fundamental observations are plausible, and the story is acutely told. It richly deserves the recent assessment by the historian of mathematics Jeremy Gray: "a classic in the history of ideas."

The theme in both of these works is that specific developments and discoveries in early-nineteenth-century geometry put into the foreground certain formal questions that engaged with and informed developments in logic with which we are familiar. 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 "circumscribed," etc.) the result is another projective theorem. This example, which will be revisited later in this essay, exhibits a case in which certain themes familiar in contemporary discussions of logic emerged naturally in the course of nonfoundational mathematical investigations.

It is natural to ask where Frege is located in this historical current. He was a practicing geometer, and he expressed opinions on duality in various places. In a 1995 essay I argued that the concepts underwriting projective duality conformed sufficiently well to Frege's descriptions of "fruitful definitions" attained by decomposition and "drawing new boundaries" that we could reasonably count it as an example of the sort of thing he had in mind when discussing the idea of fruitfulness in general.

To form any reasoned judgment on these matters requires some immersion in the mathematical practices of Frege and his contemporaries. General mathematical customs and working assumptions of the mid- and late nineteenth century exhibit some significant differences from contemporary mathematical practice. Of course, the evident similarities are profound enough that it is of value for understanding of current mathematical activity to study Frege's relation to mathematics as he would have understood it. But we should not forget that there are delicate historical questions to be addressed in delineating Frege's mathematical setting. Some problems and techniques that would have been seen as absolutely central to any mathematician of the time have fallen into neglect today. It can require some work to identify what is being spoken of.

Nor should it be assumed that we can learn all we need about the working assumptions and salient themes in Frege's environment just from looking at Frege's writings in isolation. After all, the perceived obviousness of widespread tacit assumptions makes them less likely to be expressed explicitly. Also, we need to go beyond Frege's writings to get some sense of what he was addressing. It is, of course, always risky to rely on a writer for one's characterization of that writer's opposition, as writers on Frege often do. But such a practice is especially dangerous in Frege's case. Frege was a relatively isolated figure, much of whose correspondence and *Nachlass* has been lost. Furthermore, we should not forget that he is a strikingly uncharitable interpreter of other writers—opponents and allies alike.

It is worthwhile here to digress for a cautionary example. (Nothing in the rest of the paper directly hangs on this, so those who find these historical details tiresome should skip ahead four paragraphs.) One aspect of Frege's mathematical environment that has rightly been recently emphasized by Mark Wilson is the extension of the conception of geometry initiated by the revival of projective geometry stemming from Poncelet's research and its development by von Staudt.[5] In the next paragraphs, it will be useful to know that this subject was widely known as the geometry of *position* or *location*. (German writers used "*Lage*" or the borrowed "*position.*") In broad outline, this observation is quite revealing, but say we want to go into details. What did Frege know, how much did he know about it, and how much did he care about it? We have to be prepared for the possibility that the mathematical currents most central to Frege might not be the ones most familiar to most philosophers today.

In particular, it is worthwhile to cast a glance at the revival of Hermann Grassmann's *Ausdehnungslehre* propelled forward by Hankel's *Theorie Der Complexen Zahlensystem.* In *Grundlagen*, Frege refers to Hankel more often than any other writer, and Grassmann acknowledged Frege's "Methods of Calculation based on an Extension of the Concept of Magnitude" as a valuable contribution to the project of the *Ausdehnungslehre.*[6] There is some external evidence that Grassmann's work in general geometries was a topic of discussion during Frege's graduate education and later in his early years at Jena.[7] So say we turn to Frege's writing for some evidence of his attitude toward Grassmann/Hankel's approach of conceptually generalizing geometrical relations, and Frege's own efforts. Among our discoveries will be this passage in which Frege quotes Hankel and then brusquely dismisses an apparently quite unpromising suggestion:

> The first question to be faced, then, is whether number is definable. Hankel declares that it is not, in these words: "What we mean by thinking or putting a thing one, two, three times cannot be defined, because of the simplicity in principle of the concept of putting." But the point surely is not the putting but the once, twice, three times. If this could be defined, the concept of putting would scarcely worry us.[8]

Current philosophical readers often unreflectively treat Frege's opponents as his dismissive tone encourages us to treat them—as a sequence of intellectual stumblebums worthy of no serious attention. But if we take Frege as our sole guide in this matter, we will be led well astray. Frege misquotes Hankel here. Only one parenthetical word is omitted, but its inclusion, in context, points to a complete reinterpretation of the message Hankel is putting forward. Here is what was actually written: "What we mean by thinking or putting a thing one, two, three times cannot be defined, because of the simplicity in principle of the concept of putting (*position*)."[9]

The parenthetical addition should serve to remind us that Hankel was attempting to elaborate what we can now see to be a precursor of the contemporary notion of a vector according to which geometrical locations (positions, *lage*, . . .) and physical magnitudes could be subject to the same operations as numbers in the abstract. (We see a reflection of this idea in Frege's criticism of Hankel's appeal to a "general intuition of magnitude" at FA, 18.) The relevant contrast is between Frege's approach and a geometrical approach encompassing operations in general geometry ("the geometry of position") and algebraic calculations. Without taking the elementary precaution of checking Hankel's work itself, rather than accepting Frege's dismissal of it at face value, this rich background to an apparently incidental remark would go unnoticed. It is the kind of thing that slips away when a priori assumptions are made about how interesting Frege's mathematical setting could be for interpreters of his thought.

Of course, such historical work has, one is permitted to hope, some rewards in the illumination of noninterpretative questions as well. My objective in this essay will be in part to examine Frege's work in the spirit of the Nagel/Enriques thesis that technical developments in geometry—particularly the principle of projective duality—played an important role in spurring formal developments in logic, with the goal of deepening not only our conception of Frege's work, but also our understanding of mathematical investigation in its own right.

It should go without saying that there are differences between the practices and standards informing mathematical activity of the late nineteenth century and those informing such activity today. But there are deep similarities as well. In particular, we see in Frege the kind of sensitivity a working mathematician then or now must have to delicate questions pertaining to how a problem is best set up or of how different "ways of setting up" might relate to one another. I will take a few examples from Frege's nonfoundational writings to illustrate the point.

One way that the means of setting a problem up can matter to how that problem is worked out is in choices of coordinate systems. Frege touches on and discusses this fact in different places. For example, in the review essay on the law of inertia, he considers some ways that coordinate systems can be changed so as to give physical situations a different cast.[10] He notes that laws will be invariant under these changes (LOI, 131), that there will be no changes in "the analytic expressions of those laws in any way, except for the replacement of the old letters by the corresponding new ones" (LOI, 132) and that it is "a mathematical theorem that there are an infinite number of such coordinate systems moving relative to one another, at a uniform rate, without rotation and without change of scale." Here he seems to regard it as altogether natural and unexceptionable that issues can arise as to how to most illuminatingly represent physical situations, and that these issues can be the object of mathematical study. There can be mathematical theorems that pertain to the existence of and relations among these ways of setting problems up.

Specifically in connection with projective geometry, he discusses in a review of a textbook on analytical point and line geometry the advantages of different systems of triangular coordinates and chides the authors for failing to represent problems in the right way:

> Just as four fixed points serve to determine the triangular coordinate system, so four pairs of corresponding points determine collineation. Now with respect to this relationship the properties of figures divide into two kinds: they are called projective or metrical depending on whether they are preserved under a collinear projection. Because of the relationship that we have just brought out between triangular coordinates and collineation, projective properties are expressed in those coordinates in such a way that no determining factors of the coordinate system—e.g., the distance between the four points—enter into the formulae, which is what happens as soon as metrical properties are represented. Thus the significance of trilinear coordinates lies, first, in their great generality and adaptability, for they allow us, subject to a minor restriction, to choose arbitrarily eight constants or four fixed points, and secondly in that they do not, in spite of this generality, burden the equations in which the projective properties are expressed with constants alien to the properties themselves. Everyone who undertakes to represent the teachings of the newer projective geometry ought to be clear on this point. The authors do not even introduce trilinear coordinates in their most general form, for instead of eight arbitrary constants they choose only six. While, on the one hand, they fail to take full advantage of the generality of projective geometry, on the other hand they let the height of the generality to which they soar become an obstacle to them in their metrical investigations.

Before drawing out what is worth noting in this passage, I should assure the reader that this is not being treated as more than an incidental remark. It *is* an incidental remark: that is the point. It gives a glimpse into the sort of thing that Frege regarded as part of the stock in trade of any working mathematician. What we find is the following line of thought. A) The suggestion that one can mathematically investigate different geometries in terms of the properties preserved by different mappings. (Specifically collineations: mappings preserving the intersections of lines.) B) The suggestion that specific details of coordinate systems can be crucial to whether or not a problem has been represented in the most fruitful and revealing way. C) An indication (worked out in additional detail in the remarks preceding the quoted passage) that these properties of coordinates are themselves amenable to mathematical characterization and exploration. Such properties of coordinates were, in fact, widely studied at the time. (D) The observation that certain specific ideas (those pertaining to distance) are neither present in the basis of the more general geometry nor definable without augmenting the basis.

Frege's attitude toward mathematical activity is no different from that of any other mathematician then or now: mathematics includes, as a crucial part of that very activity, the critical analysis and scrutiny of primitives and techniques. If Frege aims to capture the relations of ideas informing mathematical activity, he would have to include ideas like these among them. This stance would naturally lead in certain directions. So, for example, one might be led, in studying the ideas involved in the relation between coordinate systems and the geometries they describe, to a more general study 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 more general notions of definability. And, as we will see later in this paper, the striking balance displayed in the principle of duality would invite a more general study of the relations among substitutions of terms and logical deduction.

If Frege aims to have all mathematical and geometrical research represented in the Begriffsschrift, then he will have to find some room for considerations of the above sort. It is hard to believe that Frege thought, or would knowingly be committed to, the view that in a mature science of geometry, 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.

We would also expect to see Frege take some halting steps in the direction of the kinds of things we are now familiar with in model theory. Haltingly to be sure—his steps in this direction have "all the obscurity of truly great mathematics"—but he is stepping in this direction nonetheless.[13] Naturally there will be differences between Frege's work and that which is done today. Specifically, let it be stated for the record that Frege was not Tarski. But it would be surprising to learn that there were any specific commitments in his philosophy that would preclude taking this road.

Say we ask: were there types of such self-critical scrutiny that Frege rejected, or that he was committed to rejecting? If Frege's conception of his logical project had any such consequences, his conception of mathematical investigation was correspondingly impoverished, but I don't think that he held, or was committed to, anything of the sort. The purpose of the upcoming sections will be to explain why I don't accept recent claims to the effect that there are deep conflicts between the principles of Frege's philosophy and any widely accepted principles informing the attitude to metatheory as it is currently practiced. (One possible exception is that Frege takes there to be objective facts of the matter as to which principles and concepts are fundamental and which derived, while my impression is that most contemporary writers are neutral about or hostile to such claims of objective logical priority.)

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. 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 series of seminars by Burton Dreben at Harvard in the 1970s and onward. Since Dreben himself wrote little on the subject, his views were largely elaborated and developed by students and junior colleagues who passed through Harvard while these apparently quite fertile sessions were taking place. We consequently can see in the literature on these topics the signs of such incipient traditions: repeated occurrences of distinctive phrases and dialectical maneuvers suddenly popping up unexplained at crucial turns in papers by many different authors. Consider, for example, these remarks:

> 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.[14]

> 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 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.[15]

> [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

a framework more extensive than that given by logic. But even if we grant the premise that Frege adhered to a "universalist" conception of logic (as, in fact, I think we should) the conclusion only follows in a quite uninteresting and restricted sense. I'll take the next few paragraphs to explaining why this is so before moving on.

First, it should be noted 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,[24] nothing follows about Frege's views on semantic theory. It is highly unlikely that Frege would have thought that semantical investigation, or other investigations of the sort we might describe as "metatheoretic" would *require* a separate, "external" standpoint. In light of Tarski's results on the definability of truth and related discoveries, we have come to accept in the later part of this century 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. But this is a fairly new idea, 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 the theories under scrutiny. So far as I am aware, there was no suggestion that this view requires a ("broader," "external") metaperspective until these limitative results were unveiled.

Furthermore, though to pursue the point would be too much of a digression, it is worth noting that it is not even clearly correct that "semantics" 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.[25] 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."[26] This would 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 that Frege defined by way of schemata. For Frege there is no metalogical standpoint from which to interpret or assess the system (emphasis in the original).[16]

[T]he generality of [standards of correctness for Fregean judgments] 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).[17]

[Frege's] conception of judgement precludes any serious metalogical perspective and hence anything properly labeled a semantic theory.[18]

[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.[19]

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.[20]

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 logical laws as expressing an objective inference.[21]

(on Russell)
The fact that Russell does not see logic as something on which one can take a meta-theoretical 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 systematization 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.[22]

Certainly it is not mere happenstance that such idiosyncratic turns of phrase should appear unexplained in so many different essays.[23] It will be worth some effort to reconstruct the views that prompt them. First, it is worth noting a common suggestion throughout: 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 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.

Here 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 some currently common practice. III) Frege was committed to rejecting some currently common practice, and he was furthermore 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 interesting absence from Frege. As a type I claim, the observation may well be defensible and worthy of notice, though it is not an observation that supports deep conclusions about Frege's methods and philosophical attitudes. Deep conclusions will typically require type II or III claims, which will 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 exploring the soundness of inference rules is today as natural as breathing, it was not an objective that seems to have been set by Frege in *Grundgesetze*. This is an interesting and correct 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 somewhat anachronistically: "It is striking how Frege avoids even informal soundness arguments in his exposition of inference rules in *Grundgesetze*, §14–25."[30] 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 on its face 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, 57).[31]

Given that Frege says this, anyone who wants to maintain that the possibility of soundness arguments hadn't 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 held. Ricketts suggests that these sections cannot be read in the natural way because "is the true" is the translation of Frege's horizontal, and Frege's horizontal is not a truth-predicate even though Frege sometimes speaks of it as one. But even if we grant that Frege's horizontal is not a truth-predicate (which seems to me a bit of a stretch) there is nothing in these sections to indicate that Frege holds that the expression "is the True" as it occurs in *Grundgesetze* is to be regimented as the horizontal, or as the expression "( ) = the true," or as the predicate "is frue" introduced in section VIII of this paper, or anything else. Frege introduces "the True" first at § 2, and then at § 4 defines the horizontal in terms of "denotes the True." Frege does not indicate that the subsequently introduced expression for the horizontal is to be seen as superceding "is the True."

Perhaps I am missing something in Ricketts' treatment, but so far as I can ascertain the sole support he 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."[32] 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 generalizations is in his very tentative discussion of independence proofs in the third part of 'On the Foundations of Geometry [II]' pp. 426–27. He opines that these generalizations would be the laws of a new science."[33] 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 shouldn't we 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" appears to be the systematic articulation of geometrical practices with which Frege was quite familiar. The above-mentioned footnote 8 just adds reasons for taking the horizontal not to be a truth-predicate. 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. But 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 in the Begriffsschrift, he would have to regiment it in a specific way, presumably because of other commitments he had. 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 engages in have to be explained away, perhaps by arguing that they are unserious "elucidations." To argue this successfully, it cannot be enough to say that Frege didn't notice something that he clearly and explicitly states. Rather it must be argued that he was committed not to treating this with a special sort of scientific gravity. My objective in the next sections is to argue that these efforts are unsuccessful.

II. THE BASIC OBSERVATION

In *Grundgesetze*, § 31, Frege sure seems to be engaging in "metatheoretic" reasoning. He gives an argument that all the singular terms in the system 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. How could this not be counted as a metatheoretic argument? Evidently it is not enough that a proof that all singular terms denote 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?

Though we are given little to go on in the writings 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. For ease of reference, I'll 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 to verge on the 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 new opponent. "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 opponent's position entails and who, if anyone, embraces it.

We gain a foothold from a larger slice of a passage whose last sentence 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.[37]

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." We see this, for example, in an early discussion of the Lewis Carroll regress and the danger of vicious circularity which precedes the contrast with the allegedly widespread conception of logic, which sees the correctness of logical rules as reductively justified by appeal to their soundness.

> 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.[38]

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 judgment 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 to be more basic and 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 surely 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 among them.

This yields a specific, narrow thesis whose attribution to Frege is clearly 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 more 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. Frege surely 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 italicized expressions or some equivalent is 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 one or another rather stronger variations on the thesis, whose attribution to Frege is typically quite indefensible.

Bearing this in mind, the question naturally arises: who exactly does accept "the modern 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.[39] A recent collection of papers entitled *What is a Logical System?*[40] 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.[41] 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 the writings of Dummett in this connection, since he is a favored target in these writings. Indeed, an apparently 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":

> 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.[42]

But the "contemporary conception of logic" is certainly not *his*. Even a cursory acquaintance with Dummett's *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 conception of logic": Tarski and Quine.[43] As a statement about the particularities of the views of this specific pair, this may be so (though I raise doubts about the attribution to Tarski below). I am in the main content to defer to Quine's students and colleagues on questions of Quine scholarship. Of course, it is rather 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. But even in the specific case of Quine, some care is required to spell out exactly where the clashes will emerge between a Quinean conception and the assumptions needed to support the "basic observation." The basic observation applies to any view that holds that the correctness of logical principles like modus ponens can be *reduced* to model-theoretic facts, with the latter taken to be objectively more basic than the former. I have the impression (subject to correction by Quine scholars, such being the penalty for incorrect attributions to Quine) that Quine would reject the suggestion that there was some objective fact about the logical priority of semantics to proof theory, because he could make little sense of such ideas of objective conceptual priority.[44] In this regard, there is a difference with Frege, as Frege clearly does maintain that some ideas (like "concept") are fundamentally prior to others ("extension"). Hence, if a semantic theory *is* developed within set theory or a theory of extensions, Frege would see such a theory as admitting of a further reduction to a more fundamental theory of concepts. However, once we grant these differences, there need be no further differences between the Quinean position and Frege's so far as any issues relevant to the basic argument are concerned.

As a first illustration of how narrow the basic observation is, note that even if it is granted, it gives no reason to prescind from observing, and even proving, *that* modus ponens is sound, or that the soundness of modus ponens 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 for restricted fragments of logic and prove them. So, for example, this feature of Frege's position involves no commitment to rejecting the formulation and proof of the soundness of modus ponens in the propositional or predicate calculus, for example. Such proofs would use modus ponens or equivalents, but so what? If I may borrow, and 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 modus ponens (or any other logical principle or basic law).

A far-fetched example illustrates the distinctions at issue here. One fact about Frege that is rightly granted on all sides is that he unequivocally rejects "psychologism" about logic. 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 of the Begriffsschrift 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 about the relevant regions of the brain, with the reinterpreted Begriffsschrift a true descriptive account of the actions of these 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, there is nothing in Frege's view that precludes the exploration of correspondences between logical principles and various (broadly) semantic principles that might correspond to them. What is precluded is only the taking of the semantic investigations to be more basic and fundamental than the logical ones.

More delicate issues arise if we ask whether we could *justify* logical claims on the basis of empirical observations were we to have made such a discovery. Say that it were observed that whenever we contemplated a particular unproven Begriffsschrift sentence the "logically true" region of the brain became stimulated in the way that only logical truths tend to provoke. Could we take this as a reason for thinking the formula true? Of course we could. It would be a pretty meager reason, of course, but it would give a reason. Obviously any such justification would be less than optimal, in that it would provide only weak empirical support for a claim that could in principle be given a logical proof. It would not be right to take empirical discovery of this sort as doing the work of a logical proof. But it would be a reason for believing the claim in question, and to that extent a justification for it. A justification can be worth having even if it falls short of the best justification possible.

Now of course this example is too fanciful to serve for anything but illustration. But an analogous example is directly relevant to Frege's interest and research: the geometric interpretation of complex numbers. Frege was of course aware that the complex numbers could be interpreted in terms of the two-dimensional plane. Hence, of course, claims about complex numbers could be supported by synthetic arguments in plane geometry. What is Frege's attitude toward such research? He *does* feel that a geometric argument leaves more to be done. Thus for example he remarks:

> [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, 116–17)

Similarly in FA:

> 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 in to arithmetic. (FA, 113–14)

The "geometric interpretation" cannot be relied on as fundamental. The diagnostic job is not completed until facts about complex numbers are demonstrated in purely logical terms. But this does not mean that such representations cannot be coherently worked out, or that they are not worth studying. As early as his Ph.D. thesis, we find Frege appreciating that very fine distinctions and principled comparisons can be made among different ways of representing the complex numbers geometrically. So, for example, he closes his thesis with the sketch of a generalization of Gauss's representation, and evaluates both the value of the generalization and its intuitive relationship to the special case it generalizes.

> 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.[45]

What we seem to find in this case is a perfect example of a body of knowledge which can provide illumination and diagnosis, with more and less fruitful versions. Furthermore, the relationships in virtue of which some are more and some less fruitful can be studied. Indeed, one could presumably also have provisional arguments based on these representations: if one proved in synthetic geometry that a certain sentence is true in the geometric interpretation of complex numbers, that would give some reason to think that the corresponding sentence of complex arithmetic is true. Of course, this wouldn't end the job: it would still be necessary to prove the theorem logically to know its truth with the proper "extent of validity." That is, one can in this system provide arguments and reasons for belief, though not reductions to basic principles. Thus, if one reserves the word "justification" for such ultimate proofs, then there is no justification of theorems of complex number theory in this system. But the system is still well worth studying, and it has a great potential for diagnostic illumination.

The point here is that there is nothing in Frege's views forbidding him to engage in semantical investigation in this vein, if a consequence relation could be defined. The relation of logical consequence would not thereby be *reduced* to semantic consequence, even if it were to turn out that the two notions were equivalent. (If, for example, whenever one sentence expressing a thought is a semantic consequence of other sentences expressing thoughts, the thought expressed by the first sentence would follow logically from the thoughts expressed by the others and conversely.) Anything proveable semantically would be provisionally justified, in the sense that one would have reasons to believe it, but it would not be fully justified unless a derivation in non-semantic terms were given. And, as with the geometric interpretation of complex numbers, there could be any number of reasons why one might want to carry out such investigations. Certainly such investigations are fully compatible with the "basic observation."

One attempt to widen the scope of the "basic argument" in Frege's case turns upon Frege's attitude toward the *full explicitness* needed for proofs to be adequately "gap-free." The natural answer to this suggestion is implicit in what has been said above. 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 countersuggestion appealing to "full-explicitness" considerations would reject that Frege has room for this: if modus ponens must be sound, then it must be proven to be sound. But this suggestion doesn't survive comparison with Frege's practice: he explicitly states that the rigor 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, 9). It is not just Frege's words, but also his practice, that indicates his attitude in this regard: he does engage in various contortions to ensure that every singular term will denote. He clearly does regard it as essential to full rigor 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.

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"/"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 characterization of his conception of logical truth "not being defined by schemata." First, to establish a benchmark, it is worth noting that Frege had no objection to the *use* of schemata in presenting, discussing, and using a logical system. If a "schematic letter" is just a single letter for which more complicated expressions can be substituted, then it is textually quite untenable to suggest that Frege rejected the use of schemata, or took them to be of small moment for his logic.[46]

Working out what is at issue will again require some digging, as the full scope of the view attributed to Frege typically appears only indirectly, when it is tacitly called upon in the course of arguments against opposing interpretations. Two points emerge. First of all, there is an appeal to the fact when this sort of talk is completely regimented, this talk will be represented in quantificational terms, and, second, the claims at issue are only tenable if one stresses the idea of "defined by" in a certain way. I'll first consider the issue of quantification briefly, though most of my attention will be directed at the force of the second point.

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 confronts directly a "schematic" presentation of geometries in general in terms of axioms with collections of 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 at the end of "On the Foundations of Geometry: First Series."[47] He emphasizes that one could (so long as various articles of logical hygiene were 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 a battery of logical complications that might ensue, but it is apparent that he has no objection to the basic approach. That is, though 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. The point seems to reduce to the fact that Frege takes "set" to be derivative and "function" to be basic, which is indeed a difference between Frege and most people today. But it is not clear that anything more than that is at issue. I'll take up the priority of concept to set again later in this essay.

The way "defined by" needs to be stressed comes out if we consider the spin given to passages in which Frege seems to say the kinds of things that, on this line, he shouldn't say. One appears in the dispute with Hilbert in FG II. Frege considers how one might mathematically address the relations of dependence and independence among thoughts. His treatment is careful, and he suggests that the upshot would be a new discipline, but nonetheless he does suggest that this science can be developed mathematically, and 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, 336)

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

The answer given in this line of interpretation emphasizes that when fully regimented there will be appeals to quantification in places where, it is suggested, contemporary writers would rely on unquantified schemata. I am not sure that much hangs on this, but it is, I think, quite right. Another point raised in this connection concerns Frege's use of the truth-predicate. I'll set that point aside for consideration later in the paper. Right now I'll consider a third point that is put forward: Though Frege appears to be laying the groundwork for proving something in the neighborhood of 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 by applying logical laws and inferences.

This is, of course, true: that is how Frege defines these notions. But 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 in FG II, provably equivalent to the one he defines. Frege is of course aware that there will typically be many logically equivalent definitions of notions. So he might accept that there could be an equivalent definition of consequence of a more recognizably semantic sort. The only reservation he would have would be that the equivalent definition would not be the basic one.

We have arrived at a point where we need to examine in more detail a point that has been alluded to several times already. In Frege's view, not all logically equivalent claims or concepts are equal: it can be an objective fact that one of a pair of equivalent notions is basic and the other derived. Most centrally for our purposes, Frege holds the notion of concept to be prior to that of set/extension of a concept:

> I do, in fact, maintain that the concept is logically prior to its extension; and I regard as futile the attempt to take the extension of a concept as a class, and make it rest, not on the concept, but on single things.[48]

> If arithmetic is to be independent of all particular properties of things, this must also hold true of its ultimate building blocks: they must be of a purely logical nature. From this there follows the requirement that everything arithmetical be reducible to logic by means of definitions. So, for example, I have replaced the expression 'set' which is frequently used by mathematicians, with the expression customary in logic: 'concept'. (FTA, 114)

Frege makes it evident that he takes the identification of such objectively fundamental ideas and principles to be a central goal of mathematics and science, and that he views scientific, mathematical, and logical investigation as required to ascertain what these ideas are.[49] (Indeed, the question of status of such diagnostic and analytic investigations have in Frege's eyes is one of the points of disagreement I have with this line of interpretation.) Here are some remarks that make Frege's stance apparent:

> I should like to subscribe to [Lange's] statement 'that elementary concepts are not the original data of a science', or as I should like to express it, that they must first be discovered by logical analysis. Similarly, the chemical elements are not the original data of chemistry, but their discovery indicates an advanced stage in the development of the science. What comes first in the logical and objective order is not what comes first in the psychological and historical order. (LOI, 135–36)[50]

> What is simple cannot be decomposed and what is logically simple cannot have a proper definition. Now something logically simple is no more given to us at the outset than most of the chemical elements are; it is reached only by means of scientific work.

> . . . If something has been discovered that is simple, or at least must count as simple for the time being, we shall have to coin a term for it, since language will not originally contain an expression that exactly answers. On the introduction of a name containing something logically simple, a definition is not possible; there is nothing for it but to lead the reader or hearer, by means of hints, to understand the word as intended.[51]

These remarks, especially Frege's talk of "hints," bring us to one of the key disputes I have with this interpretation. Under dispute is the status of the "scientific work" or "logical analysis." Before explaining my disagreement, I'll indicate some points of concord. Ricketts notes that Frege sets aside a special category of intellectual activity that Frege calls "elucidation."[52] Elucidations are remarks aimed at bringing someone to understand a principle or primitive notion that cannot be further reduced. The success or failure of an elucidation is to be assessed solely by whether or not the requisite mutual understanding is attained. If you "catch on" to the concept/object distinction through Frege's attempts at elucidation, the remarks have served their sole purpose (and should, presumably, be set aside). If you have not caught on, the words have not served their purpose (and, presumably, should be repeated at twice the volume). I think that this is clearly right: Frege did have such a category, he discusses it repeatedly, and he is quite clear about its nature and scope. To repeat: Elucidations are propaedeutic remarks aimed at securing general understanding and communication. One place where Frege makes this point will be useful for the upcoming discussion:

> Are Hilbertian definitions, then, elucidations? Elucidations will generally be propositions that contain the expression in question, perhaps even several such expressions. . . . If Hilbertian definitions were to serve only the mutual understanding of the investigators and the communication of the science, not its construction, then they could be considered elucidations in the sense noted above. . . . [However] it is not intended that they belong to the propaedeutic but rather that they serve as cornerstones of the science: as premises of inferences. (FG II, 301)

He then adds several sentences maintaining that if they were understood as elucidations, Hilbert's axioms would not be very good elucidations. Then he continues:

> Let us turn to proper definitions! They, too, serve mutual understanding, but they achieve it in a much more perfect manner than the elucidations in that they leave nothing to guesswork. . . . And if, like an elucidation, a definition were to serve only mutual understanding and the communication of the science, then in this case it would indeed be superfluous. But that is an advantage gained only incidentally. The real importance of a definition lies in its logical construction out of primitive elements. . . . The insight it permits into the logical structure is not

> only valuable in itself, but also is a condition for insight into the logical linkage of truths. (FG II, 302)

Frege then distinguishes, in familiar chemical terms, two sorts of mental processes through which one can arrive at definitions:

> The mental activities leading to the formulation of a definition may be of two kinds: analytic or synthetic. This is familiar to the activities of the chemist, who either analyses a given substance into its elements or lets given elements combine to form a new substance. In both cases, we come to know the composition of a substance. . . . But the mental work preceding the formulation of a definition does not appear in the systematic structure of mathematics; only its result, the definition, does. (FG II, 302)

Two things should be noted in this quotation and the preceding one: Frege dismisses from mathematics any questions of antecedent mental activity, and he endorses the idea that well-chosen definitions can yield insight. The remark which is most pertinent for those who adhere to the "no metatheory" interpretation is the one banishing, from a finished science, the "mental work preceding the formulation of a definition." This is the sort of psychological baggage which Frege feels has no place in the study of logic as such. This is the kind of thing which is only useful insofar as it can contribute to elucidation, it would be maintained. Here too I agree: where I diverge from these readers is in my assessment of the attitude which informs the remarks about the potential insight gained into logical structure from properly chosen definition. There, what is at issue is not psychological, but rather it is available for the same sort of study as any other object of mathematics admits. This is the kind of thing I take to support an interesting kind of "metatheory," of a sort that Frege indicates no objection to and some inclination to explore.

Unlike the psychological facts about mental processes, the logical facts about the structures of definition are not preliminary, or part of any propaedeutic to "the real thing." The sorts of questions concerning how a theorem can best be generalized, or what the relations between the number of points needed to determine a collineation and the number needed to fix a set of triangular coordinates are, or what the relations between different definitions of the same idea might be, or whether a given subject exhibits the kind of parallel deductive structure exhibited by projective duality are now, and were then, the kinds of questions that are addressed and answered as part of ongoing mathematical practice. They remain on the table even after agreement on primitives is reached.

Hence I cannot see that the issues about "logical structure" or the fruits of the "scientific work" and "logical analysis" that are involved for Frege in identifying the primitive notions of mathematics should be consigned to dismissable propaedeutics. They are themselves part of the subject matter. Hence it is also crucial that, although Frege did indeed think that logical truths were to be *defined as* those thoughts which followed from logical laws by means of logical inferences, this indicates no hesitation about exploring 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. But that does not mean that Frege wouldn't regard it as a handy thing to know and use.

Of course, for Frege, any proof in which the premises, or the terms in which the premises are formulated, are not as basic or fundamental as they could be leaves a task undone, however cogent the proof may otherwise be. So, in particular, any argument from premises formulated in terms of sets cannot be accepted as settling the questions proved unless there is a further reduction to premises formulated in terms of concepts and functions. If this is what is at issue, then it is clearly correct to say that Frege does not have a "schematic" view of logic. However, beyond the well-known fact that Frege sees talk of sets/aggregates/extensions/etc. as needing reduction to talk of concepts and functions, it is hard to see what the "schematic"/"substantive" contrast adds.

The contrast does mark a difference from how things are done today, but it is important to be clear on what it is. Modern writers who discuss model theory and semantics are in the main silent and presumably neutral on whether there is an "ultimate" basis of what they are doing, and (if there is) what this basis might be. This is, no doubt, a shortcoming of contemporary attitudes and a respect in which Frege is more systematic and philosophically thorough. But it does not indicate any respect in which Frege differs from the mass of contemporary logicians over the admissibility of semantic methods. In particular, the fact that no semantic definition of consequence could be *basic* for Frege does not mean it must be consigned to the realm of a ladder that is ultimately to be kicked away. (I will return to this point in the next section.)

Before moving on I should pause to note one respect in which the work considered here is correct, and, I think, quite important. Much of the exegetical work that explores variations on the themes under scrutiny here looks forward to certain dark doctrines of the *Tractatus:* Tractarian remarks on elucidations, the inexpressibility of logical form, the say/show distinction, and other nonsense. The suggestion (advanced independently by Geach and others) is that these themes in the *Tractatus* reflect Wittgenstein's attempts to come to grips with threads he perceived to run through Frege's writing.

I am a long way from a scholarly understanding of the *Tractatus*, and so my opinion should be given little weight, but for what it is worth I will say that I do find this interpretation of the early Wittgenstein fairly convincing.

The conjecture that Wittgenstein was moved by a certain (mis)reading of Frege's treatment of the "concept horse" problem does seem to me to shed much light on the "say/show" distinction and related mysteries. That Wittgenstein was reading Frege through spectacles darkly tinted with Russellian doctrine help to explain why he would arrive at this point of view on Frege's work.[53] But of course the fact that Wittgenstein may have, at one stage in his life, read Frege a certain way does not mean that this reading is correct. My point here is that the connection with Wittgenstein leads us astray in reading Frege: it leads us to read backwards into Frege distinctions and attitudes that just aren't there.

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 whose vocabulary 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, 337–38)

The answer Frege gives to this question is yes, if the translation leaves (what we would now call) logical constants untouched. He does not give a criterion of what it is to be a logical notion. He just lists a few notions that are to count. But since there does not seem to be much agreement on the characteristics of logical constants even today, 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 bodies with respect to their chemical properties does not constitute a hindrance to their mutual replacement. To logic, for example, there belong the following: negation, identity, subsumption, subordination of concepts. And here logic brooks no replacement. It is true that in an inference we can replace Charlemagne by Sahara, and the concept *king* by the concept *desert*, 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 G′. (FG II, 338)

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 suspicious or illegitimate about what he is doing, but rather because he is attempting to correct what he takes to be an unacceptably loose way of talking by Hilbert. He is also careful not to use uninterpreted symbols: rather he speaks in terms of replacing some *interpreted* symbols with other interpreted symbols. But despite these niceties, it is hard to see what isn't "schematic" about what Frege is saying here. His point is that one can arrive at general statements about consequence and logical dependence by considering the possibilities of interchanging non-logical vocabulary while holding the logical vocabulary fixed. 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 hard to see what hangs on this fact. Neither Frege nor the interpreters under consideration here give any indication of what that significance might be. This point is perhaps worth lingering over. Today, thanks largely to Quine, the question of whether or not a claim involves a quantifier is seen as 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 this is not a matter which Frege expresses no opinion: there is nothing in his discussion of quantification to indicate any inclination toward the kind of significance that is attributed to it today. Without such indications on Frege's part, it is hard to see how the mere use of quantification can properly be taken to indicate a deep division between Frege and his contemporary successors.

To delve into the issue of how the consequence relation is defined, it will be useful to detour through a few other issues. This discussion will also provide the occasion for bringing in a few more details from Frege's writing *en passant.* Consider these remarks from Ricketts' "Truth-Values and Courses-of-Value":

> The syntactic codification of quantificational inference makes metamathematics possible, in particular the mathematically rigorous investigation of formal derivability in formalizations of various mathematical theories. But Frege introduces his formalism in order to use it to state the gap-free proofs that will establish the logicist conjecture. He shows no inclination to treat his formalism as an object of mathematical investigation. As I noted at the outset, Frege's Begriffsschrift is a framework for universal science: with the addition of the requisite vocabulary, the laws and facts of the special sciences are expressible in it. For Frege, truth is scientific truth: there are no truths not expressible in this framework. Nothing in Frege's philosophy precludes, as we would put it, formalizing the logical syntax of the Begriffsschrift within the framework of the Begriffsschrift, treating the constructions of the Begriffsschrift as a notational game. Frege, however, would see little point to this exercise. Sentences, considered only as series of marks or sounds, are of no interest to Frege. They are of interest only in that they express thoughts and so, when produced with asserting force, may be used to manifest publicly the acknowledgment of the truth of a thought.[54]

It is true that Frege is not interested in "mere notational games," but that is not the issue: the question is whether he is interested in studying the formal relations among the expressions in sentences that *do* express thoughts. In the passage we have just seen, Frege clearly does show an "inclination to treat his formalism as the object of mathematical investigation." He *is* using mappings, etc., on the structure of language as reflecting the deductive structure of thoughts. Frege of course made it plain in his polemics against formalists that he was interested in sentences only insofar as they express thoughts, but it simply does not follow that he would have had no interest in the structure of the sentences that express thoughts. Quite the contrary; Frege repeatedly stresses, in both his early and late writings, that the study of the structure of sentences is a guide to the structure of thoughts, since the former "mirrors" the latter.[55]

Of course, we might define consequence in terms of following by logical laws, but what are the "logical laws"? 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 consequence, 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 characterization 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.[56]

It is true that Frege nowhere gives a criterion of the logical, though this could simply reflect that he had not arrived at one. Following his definition of dependence, Frege acknowledges the absence of a delineation of the logical, in words that seem to suggest that he regards this task as difficult but not in principle impossible.

> 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, 339, emphasis mine)[57]

This passage reveals several interesting things: note in particular that Frege describes the translation principle he has just sketched as underwritten by a (presumably heretofore unformulated) "basic law." I will develop some of the ramifications after reflecting on what these remarks indicate about Ricketts' observations.

Of course, it is interesting and well worth pointing out that Frege never managed to formulate a criterion for a principle to be a logical law. It isn't clear from Ricketts' paper whether he thinks that the absence of a characterization of logical truths reflects merely a failure on Frege's part to attain a goal that he takes to be in principle attainable or reflects some deeper commitments in Frege's conception of logic. It is hard to conclude much from the fact that Frege stops where he does in the definition of "dependence." Since he was at this time attempting to patch up the system of *Grundgesetze*, it is not as if he didn't have enough work to do. The Fregean words just quoted suggest the "merely a failure" interpretation, though the quotation is not decisive.

At one point in Ricketts' "Logic and Truth in Frege" there does appear to be an argument that Frege holds the stronger thesis that there can be no criterion of "the logical." He states (without referring to any texts as support): "Frege aims to arrive at a surveyable group of logical principles that, as he says, in *Foundations of Arithmetic*, § 91, 'suffice for all cases'. Frege is clear that the comprehensiveness of a group of logical principles is subject only to 'experimental' test."[58]

I don't think Frege *is* clear on this point. Quite the opposite, unless by 'experimental' we mean all reasoning short of certainty, he doesn't seem to say anything that even hints in this direction. Frege does say at FA, § 90, that the efforts of the *Grundlagen* only make it "probable" that arithmetic is analytic, but his remarks point in a quite different direction: they suggest that one can have reasoned investigation of mathematical truths that falls short of certainty or completely cogent reductive proof.

> § 90 I do not claim to have made the analytic character of arithmetical propositions more than probable, because it can still be doubted whether they are deducible solely from purely logical laws, or whether some other type of premiss is not involved at some point in their proof without noticing it. This misgiving will not be completely allayed even by the indications I have given of the proof of some of the propositions; it can only be removed by producing a chain of deductions with no link missing, such that no step in it is taken without our noticing it. (FA, 102)

Elsewhere, Frege's remarks seem to underwrite the impression that he feels that a proof of the comprehensiveness of his principles would be desirable, though again he has only provided reasoned support for the view:

> The fundamental principle of reducing the number of primitive laws as far as possible wouldn't be fully satisfied without a demonstration *(nachweis)* that the few left are also sufficient. It is this consideration which determined the form of the second and third sections of [B] . . . it wasn't my intention to provide a sample of how to carry out such derivations in a brief and prac-

tical way: it was to show that I can manage throughout with my basic laws. Of course, the fact that I managed with them in several cases could not render this more than probable (*Wahrscheinlich*). But it wasn't a matter of indifference which example I chose for my demonstration. So as not to overlook the arguments which are of value in scientific use, I chose the step-by-step derivation of a sentence which, it seems to me, is indispensable to arithmetic. (BLC, 37–38)

Here too, we see Frege accepting that one can have reasoned support that falls short of certainty. By addressing an example which is fruitful in a specific way, he takes himself to have provided such support. Here too the absence of a general criterion of "the logical" seems to reflect only that Frege hadn't managed to formulate one. There is nothing to indicate that he regarded such a thing as impossible.

It was mentioned above that Frege appears to regard the translation principle he sketched in FG II as one that would have to be supported by a (presumably new) "basic law." This does support Ricketts' view to the extent that it indicates Frege regards his list of basic laws as, to some extent, open-ended. But the claim is a double-edged sword: Frege also seems to be embracing the possibility of a "new science" underwritten by at least one evidently "metatheoretic" basic law. Here too we see that Frege approaches the deductive structure of thoughts by studying the relations exhibited by the expressions in the sentences that express the thoughts.

However, a more significant observation pertains to the sort of view 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 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 extremely familiar to geometers of the late nineteenth century. Frege is, as it happens, describing precisely the format that projective plane 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. To get a sense of this, see the reproduced pages from Cremona's 1893 textbook.[59] Not only are 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 internal evidence in the "Foundations of Geometry II" essay itself indicating that Frege made any connection with projective geometry and the principle of duality. However, I think it is not at all speculative to suggest that he *must* have recognized that projective duality was an evident realization 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

CHAPTER XVII.

DESARGUES' THEOREM.

183. THEOREM. *Any transversal whatever meets a conic and the opposite sides of an inscribed quadrangle in three conjugate pairs of points of an involution.*

This is known as DESARGUES' theorem *.

Let $QRST$ (Fig. 122) be a quadrangle inscribed in a conic, and let s be any transversal cutting the conic in P and P', and the sides QT, RS, QR, TS of the

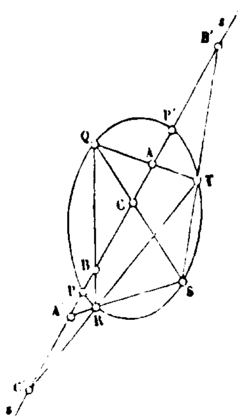

Fig. 122.

CORRELATIVE THEOREM. *The tangents from an arbitrary point to a conic and the straight lines which join the same point to the opposite vertices of any circumscribed quadrilateral form three conjugate pairs of rays of an involution.*

Let $qrst$ (Fig. 123) be a quadrilateral circumscribed about a conic; from any point S let tangents p, p' be drawn to the conic, and let the straight lines

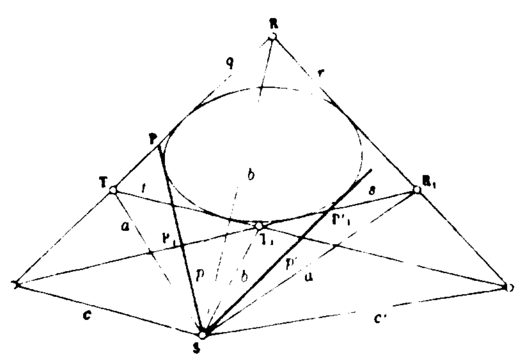

Fig. 123.

* DESARGUES, *loc. cit.*, pp. 171, 176.

FIGURE 1

quadrangle in A, A', B, B' respectively.

The two pencils which join the points P, R, P', T of the conic to Q and S respectively are projective with one another (Art. 149), and the same is therefore true of the groups of points in which these pencils are cut by the transversal. That is, the group of points $PBP'A$ is projective with the group $PA'P'B'$, and therefore (Art. 45) with $P'B'PA'$; consequently (Art. 123) the three pairs of points

$$PP',\ AA',\ BB'$$

are in involution.

184. This theorem, like that of Pascal (Art. 153, right), enables us to construct by points a conic of which five points P, Q, R, S, T are given. For if (Fig. 122) an arbitrary transversal s be drawn through P, cutting QT, RS, QR, TS in A, A', B, B' respectively; and if (as in Art. 134) the point P' be found, conjugate to P in the involution determined by the pairs of points A, A' and B, B'; then will P' be another point on the conic to be constructed.

185. The pair of points C, C' in which the transversal cuts the diagonals QS and RT of the inscribed quadrangle belong also (Art. 131, left) to the involution determined by the points A, A' and B, B'.

Moreover, since the points A, A' and B, B' suffice to determine the involution, the points

a, a', b, b' be drawn which join S to the vertices qt, rs, qr, ts of the quadrilateral respectively.

The two groups of points in which q and s are cut by the tangents p, r, p', t are projective with one another (Art. 149), and the same is therefore true of the pencils formed by joining these points to S. That is, the group of rays $pbp'a$ is projective with the group $pa'p'b'$, and therefore (Art. 45) with $p'b'pa'$; consequently (Art. 123) the three pairs of rays

$$pp',\ aa',\ bb'$$

are in involution.

This theorem, like that of Brianchon (Art. 153, left), enables us to construct by tangents a conic of which five tangents p, q, r, s, t are given. For if (Fig. 123) an arbitrary point S be taken on p, and this point be joined to the points qt, rs, qr, ts respectively by the rays a, a', b, b'; and if (Art. 134) the ray p' be constructed, conjugate to p in the involution determined by the pairs of rays a, a' and b, b'; then will p' be another tangent to the conic to be constructed.

The pair of rays c, c' which connect S with the points of intersection qs and rt of the opposite sides of the circumscribed quadrilateral belong also (Art. 131, right) to the involution determined by the rays a, a' and b, b'.

Moreover, since the rays a, a' and b, b' suffice to determine the involution, the rays p, p' are a

FIGURE 2

very first topic covered in the graduate lectures Frege attended on geometry, for example.[60] 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, I think, almost inconceivable that Frege would not have made the connection.

To follow up on this observation, I want to consider an example where Frege does explicitly discuss the principle of duality. What the discussion reveals is 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.

> 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. (RGW, 95)

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, in non-logical domains, already being done. For example, even at the time, duality principles in geometry were of crucial importance in studying the geometric structures of symmetric crystals.[61] The relations between the dualities of the theorems about crystals and the corresponding symmetries in the crystals described were recognized as important and as the basis for further study as objects in their own right. 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 the Begriffsschrift, he would see that as a basis for adding yet another new basic law, rather than as consigning such studies to the realm of inexpressible propaedeutic.

It will be useful at this point to consider Quine for a few paragraphs before moving on to the next section. Not much will hang on these closing words, but they may help illuminate how difficult it can be to identify "the modern conception of logic" spoken of in these writings, and they may clarify what one should take to be at issue between Quine and Frege. As mentioned above, Quine is one of the only figures cited as a representative of "the modern conception," so it is worth checking what Quine does say in the passages to which Ricketts' footnotes refer us.[62] We are directed to three places by footnote 16 of Ricketts' "Objectivity and Objecthood," only one of which is a discussion of schemata and logical truth: Quine's *Philosophy of Logic*.[63] Here Quine gives several definitions of logical truth that he puts forward as equivalent: one is in terms of structure, and one is in terms of substitution of sentences: "A *logical truth*, then, is definable *as a sentence from which we get only truths when we substitute sentences for its simple sentences*."[64]

Apart from the fact that Frege would take truth to be fundamentally a property of thoughts and only derivatively applicable to sentences, it is hard to see that Frege would have any objection whatever to this definition. Substitution seems to be one of the things counted as legitimate in Frege's discussion of his new basic law.

Logical schemata are then defined as convenient intermediaries:

> Sometimes this definition of logical truth is given in two stages, mediated by the notion of a *valid logical schema*. . . . A logical schema is *valid* if every sentence obtainable from it by substituting sentences for simple sentence schemata is true. A *logical truth*, finally, is a truth thus obtainable from a valid logical schema. The reason for the two-step version is just that the notion of a schema is of further utility. Because of their freedom from subject matter, schemata are the natural medium for logical laws and proofs.[65]

This suggests less that there is a basic notion of schema from which logical truth can be defined as that there is a basic definition of logical truth by substitution of truths into truths and a derived, schematic shorthand that is convenient for some purposes. Here it is hard to see what the clash with Frege could be. It does not, at any rate, seem that there is some basic, nonderivative appeal to an unreduced notion of schema that is being used to define "logical truth" here.

Of course, on the very next page, Quine proceeds to display the conceptual opportunism that is one of his trademarks. Why rest content with one definition when we can shoot a whole brace of them? "But I shall have much more to say of validity. The definition of validity now before us refers to substitution; a schema is valid if substitution in it yields only true sentences. A very different definition of validity is also worth knowing: one that makes use of set theory."[66] Quine then gives a model-theoretic definition, and a couple of pages later alludes to a proof that for sufficiently rich languages the definitions are equivalent. The point is, of course, that for Quine there is nothing but simple convenience at issue in the choice of which of these definitions to embrace. The suggestion that there might be no fact of the matter about which of a collection of equivalent axiomatizations might be uniquely basic is not completely foreign to Frege. He acknowledges (at least in the later writings) that whether or not a sentence is an axiom can depend on which of two equivalent, equally good systems one chooses to adopt.[67] But at least on the subject of concept and extension, he doesn't seem to have wavered in his commitment to the view that the latter was objectively derivative from the former.

This allows us once again to sharpen our view of what is at issue here between Frege and "the modern conception," at least insofar as the cited passages from Quine are taken to delineate it. It appears here at least that the difference is not that Quine and Frege are committed to different conceptions as that Quine is (in principle) neutral among (possibly) demonstratively equivalent conceptions, while Frege is committed to regarding one specific conception as basic, and others derived. This has, no doubt, some philosophical consequences (though just what these are would have to be spelled out). But the inadmissability of metatheory for Frege is not among them.

V. INTERPRETATION

Also worth notice are the above-cited remarks in Goldfarb's 1982 "Logicism and Logical Truth" and Conant's 1992 "The Search for Logically Alien Thought" that for Frege, "questions concerning reinterpretation . . . cannot/do not arise" with reference to the Begriffsschrift. As they stand, these remarks occur without supporting argument, and so some reconstruction will be necessary. On the face of it, one might think that Frege's mathematical work makes such a suggestion rather unlikely. If the Begriffsschrift is a device to regiment mathematical investigation, this presumably must include those investigations in which reinterpretation functions as a key proof device. Not only was Frege quite familiar with such investigations: his published writings on geometry reveal him to have engaged in some himself. That is, Frege not only thought it was acceptable to consider reinterpretations of the languages he studied, he even carried out and studied such reinterpretations in those of his own researches that were devoted to purely geometrical work. Elsewhere in this paper I have discussed the geometrical representation of complex numbers, and so I will stick with that example here.[68] As we've seen, Frege does speak of the "interpretation" of i in geometric terms. In practice, he doesn't require that a different symbol be used when $\sqrt{1}$ receives a denotation in a synthetically presented plane and when it doesn't.

There are some places where, on a hasty reading, one might take Frege to be rejecting interpretations. So, for example, he remarks in his controversy with Hilbert that: "The word 'interpretation' is objectionable, for when properly expressed, a thought leaves no room for different interpretations. We have seen that ambiguity simply must be rejected and how it may seem necessary only because of insufficient logical insight. I merely recall what I have said about the use of letters above, on p. 377." (FG II, 315)

It might seem that in the first sentence and the first half of the second, Frege is rejecting both the idea of interpretation *tout court* and dismissing all interpretations as a kind of "ambiguity," but this would be to take the remark rather drastically out of context. The third sentence and the final part of the second refer back to a preceding discussion suggesting that what Frege has in mind by "ambiguity" is not the reassigning of a new sense and denotation to an expression, replacing the old. Rather, Frege is objecting to taking a single expression or sense to be (what one might call) "multiply-denoting." These remarks do clearly fit into a theme that occurs elsewhere in Frege's writings, which is that only signs, and not thoughts, can be spoken of as interpreted.[69] But that does not conflict with anything said here. What is at issue here is the study of reinterpretations of signs as a means of studying the corresponding thoughts.

Frege does make some remarks a little later in this essay that could be taken to provide some support to the view that even signs are not to be reinterpreted (contrary to his own informal practice). I don't think that they carry much weight, but I am not altogether sure what conclusion to draw from them. I'll explain what I make of these remarks after I cite them:

> [Korselt writes]: 'In this way, one sequence of formal inferences can sometimes be interpreted in different ways.'
>
> What can be interpreted is perhaps a sign or group of signs, though the univocity of the signs—which we must retain at all cost—excludes different interpretations. (FG II, 318)

I am inclined to think that this is also an instance where what Frege is rejecting, in his embrace of the "univocity of signs," is that no sign can have, ambiguously, more than one interpretation at a time. That is, it is acceptable to interpret $\sqrt{1}$ as some logical object, and then later, for some specific purpose, reinterpret it as a point in the plane, just so long as it is not understood to refer to two things "indifferently" under a single interpretation.[70]

I am not as convinced of this particular point as I am about most of what I have written here. However, I don't think that much hangs on it. Perhaps whenever a sign is given a new interpretation, one must introduce a new sign (perhaps with a subscript on the old one, as one reading of the FG, 284 remarks would suggest) and set aside the old one. So understood, the principle of not allowing changes of interpretation seems to be just a rule of careful, organized thinking for Frege—an article of prudent logical hygiene. I don't see any reason to think such a principle would flow from any deeper aspect of Frege's philosophy, such as any "universalism about logic." Even if the "no ambiguity" principle is in fact a "no reinterpretation of interpreted signs" principle, nothing else in Frege's broader views would be affected if it were given up.

VI. EXPLANATION AND JUSTIFICATION

Of course, Frege sees deductive justification in terms of basic laws as very important. A deductive proof of a claim provides not only grounds for believing it, but also serves assorted diagnostic functions. But how much of the activity of science is bound up with giving justifications, where these are understood as reductive logical proofs? One dialectical strategy in this line of interpretation seems to break down into variations on the theme of stretching the basic observation just a step or two beyond what it will actually support. This can be brought out through examining a few more passages from Ricketts' more recent elaborations of this interpretation. Consider first this embellishment, revisiting familiar themes of a fragment of Frege's anti-psychologist polemic:

> The basic idea here is clear enough: as every science draws on logic, no science can provide a foundation for logic. . . . More than a language for the maximally general science of logic, Frege conceives his Begriffsschrift as a framework for universal science. He envisions that, simply with the addition of the requisite specialized vocabulary, any fact or law uncovered by the special sciences can be expressed in it. Explanations in the mature sciences are provided by proofs in this framework of one truth from more basic truths. In this framework, there will be no proofs, no explanations, for basic logical laws. The very attempt would, from Frege's viewpoint, be vitiated by vicious circularity. . . . There is no realm of truths not subject to logic, not expressible within the framework of the Begriffsschrift.[71]

In several ways, this passage pushes the basic observation too far. Note first the remarks about explanation: no textual evidence is given for the suggestion that Frege holds that all scientific explanation must be, as suggested in Ricketts' gloss, deductive reductions of some claims to other, more basic claims. In fact, such textual evidence is hard to come by: apart from a few incidental remarks, Frege says very little about explanation. It will be illuminating to consider one of the few places where Frege does express an opinion. At first sight this might seem to provide some slight support for Ricketts' attribution, but on closer scrutiny the remarks tell against it: "Indeed the essence of explanation [*Erklärung*] lies precisely in the fact that a wide, possibly unsurveyable manifold is governed by one or a few sentences. The value of an explanation can be directly measured by this condensation and simplification: it is zero if the number of assumptions is as great as the number of facts to be explained" (BLC, 36).

Even in these remarks, which mark the closest Frege comes in any writings to endorsing the view that Ricketts attributes to him, Frege does not put any weight on the identification of, and reduction to, the *basic* facts. All that is emphasized is the "condensation and simplification." In fact, Frege says that only the reduction in number is what matters in explanation, and he says nothing about not whether the reduction is to the most basic facts. According to this criterion, a reduction of a battery of claims about concepts to a short list of claims about extensions of concepts could be a valuable explanation, even though the more basic was explained in terms of the less basic. To be clear, I should reemphasize that I am not denying that Frege thought that some claims and notions were objectively more basic than others, and that an objective of mathematics and science should be to prove the more basic in terms of the less. However, there is nothing in the lines just given to suggest that he holds *explanation* [*Erklärung*] as exhausted by this objective. Quite the opposite: his "direct measure" of the value of an explanation is neutral on the issue of more and less basic. So either we have to conclude that the remark, and its "direct measure" is not altogether serious or that it is serious and that explanations need not be proofs that reduce the more to the less basic. Either way, Ricketts' spin on "explanation" gets no textual support here.

Elsewhere, when Frege uses expressions that can be naturally translated "explanation" he shows no reluctance to apply them to the kinds of arguments that Ricketts suggests would be vitiated by "vicious circularity." One striking example occurs in the fateful response to Russell's letter considered above. In speaking of the argument of § 31 that all singular terms denote, Frege writes that this lesson can be derived from Russell's paradox: "it seems as if my explanations [*Ausführungen*] in section § 31 do not suffice to secure a denotation for my combinations of signs in all cases" (Corr, 132).[72]

Moreover, in some cases, Frege speaks of "explanations" when he is talking about arguments that he openly denies are demonstrative reductions: "Kerry contests my definition of 'concept'. I would remark, in the first place, that my explanation [*Erklärung*] was not meant as a proper definition" (CO, 182).

Casting the net more broadly to bring in additional places where Frege discusses explanation merely confirms the impression that Frege probably has no settled, thought-out view of what "explanation" comes to. Furthermore, if he does have such a settled view, the textual evidence sets us on a different course from the one Ricketts pursues. Explanations, for Frege, are not exclusively deductive reductions of statements to more basic statements. The addition of "explanation" to "justification" serves only to add the illusion that the basic observation entails more than it actually does about Frege's attitudes to logical and mathematical inquiry. Just as Frege was not Tarski, he may not have been Hempel either.

This is not to say that deductive reductions to more basic truths don't represent important scientific successes for Frege. Of course they do. It is, after all, characteristic of mathematical practice "always to prefer proof, where proof is possible"(FA, 2). Proving a claim on the basis of more primitive truths also has an important diagnostic role in revealing the "extent of validity" of a particular claim. In such cases, what is at issue is not "condensation and simplification," but rather logical priority. The reduction of the arithmetic of complex numbers to logical principles instead of geometric ones would be diagnostically valuable even if there were a *greater* number and variety of logical principles involved. That is, even if "condensation and simplification" were better effected by the geometric representation, the logical proof would be desirable. Here it seems that explanation, in this "condensation and simplification" sense, and reduction would satisfy different desiderata. The point is that the attempt to nudge the basic observation one step further is really without textual grounding. We are left with what we began with: Frege would have thought that a certain kind of reduction—a proof of logical principles by reducing them to *more basic* principles—would be impossible. The point does not extend any further than that, and in particular it does not entail any conclusions about Frege's attitude toward explanation.

Of course, one might maintain, independent of any facets of one's Frege exegesis, that explanations are, *as a matter of fact*, always deductive reductions of the sort precluded by the basic observation. (Or perhaps the weaker position might be maintained that explanations in sciences like mathematics and logic in which claims can be shown to hold *necessarily* are invariably such reductions.) If this is what underwrites Ricketts' parenthetical addition of "explanation," then of course it need not matter what Frege took explanation to consist in. But such a thesis about explanation in general, or mathematical and logical explanation in particular, is not at all obvious. Since it would be a significant digression to take up this issue here, I'll merely leave off by observing that no argument is given, in any of the writings under discussion, for the view that explanations must have this character. It is certainly not a claim that can be merely presupposed.

VII. TRUTH AND SEMANTICS

A further point that is marshalled in support of the thesis that Frege could not have endorsed "semantic metatheory" in Ricketts' writing revolves about Frege's attitude toward truth and truth-attributions. So, for example, in an early paper he remarks:

> 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.[73]

That is, Frege is precluded from taking up the "contemporary view" because that view requires the ineliminable use of a truth-predicate, and Frege was committed to rejecting such a use. 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[74] 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[75]

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 up to this point has indicated that these give no reason to think that soundness could not be stateable in the Begriffsschrift, so long as the soundness of a rule is not treated as a more basic fact to which the correctness of inferences according to the rule is to be reduced. We have seen no reason to think that Frege would have thought such soundness statements could not be regimented so as to participate in "rigorous, explicit construction of proofs in Begriffsschrift." In these remarks we see a third consideration, turning on Frege's attitude to the truth-predicate. What does this additional consideration add?

There are two questions that should be distinguished: I) is Ricketts' account of Frege's treatment of the truth-predicate correct? II) if we grant for the sake of argument that Ricketts' account 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 handy to ask this question about the historical Tarski in particular, though the point is independent of the specific attribution to Tarski.)

I do, in fact, differ with Ricketts on I), but I will leave that aside here. On this point I have little to add to the discussion in Stanley (1996) so I will refer the reader there.[76] What I will address is II): I want to suggest that, as far as Tarski's conception of logical consequence is concerned, Frege's views of the truth-predicate can only be a red herring.

A first observation to set the point up is that, *pace* Ricketts, on the Tarskian approach, the truth-predicate is given an *eliminative* definition, in terms of set-membership. (Among other things, this is why Tarski could not rest content with a recursive definition of the truth-predicate but took the additional step of transforming it into an explicit one.) Consider a set theory text like Kunen's *Set Theory*,[77] where we have set-membership already at hand in the basic vocabulary. The standard move of defining model theory for (fragments of) set theory does not require the addition of an extra primitive predicate. Far from it: the Tarskian truth definition, and the definition of consequence, requires no vocabulary in addition to the set-theoretic vocabulary already present.

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. To avoid worries about limitative results, say we try to develop this semantical representation within the framework of the Begriffsschrift as a whole.[78] There should be no objection to functions from names to what those things name. Recall that Frege seems to have no reluctance to try 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 horse" problem. However, Frege does accept that one can approach concepts by talking about signs, though certain niceties have to be 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. 141–42)

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, 186). Fair enough. But say that our concern is not to capture "the real situation" but only to work out a model-theoretic definition of truth that will satisfy Tarski's formal correctness and material-adequacy conditions, and which will allow a characterization of semantic consequence such that all and only those sentences which 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. Also, the specification of courses-of-values will be derived from statements about functions via some appropriately weakened version of basic law V. As mentioned above, this would not set our project apart from the kinds of things that are done today: there is no shortage of presentations of the (countable) model theory for first-order arithmetic within second-order arithmetic with a sufficiently strong comprehension axiom. The resulting definition would be, strictly speaking, 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 characterization 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."

Fair enough: as I have indicated, my purpose here is not to controvert this specific point about Frege's views on truth. But it is important to note that this response does not involve rejecting the semantic inquiry we have just sketched: rather it denies that this inquiry captures the notion of *truth*. This may well be right, but it leaves us with an acute variation on a worry that troubled us above: just what is being excluded here? Who is the opponent? After all, Tarski anticipated objections of the general family: "Whatever you have defined, it is sure not *truth*" and responded to them as follows:

> Referring specifically to the notion of truth, it is undoubtedly the case that in philosophical discussions—and perhaps also in everyday 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 votes—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.[79]

So even if Frege does have specific scruples about the nature of truth, there need be no conflict with Tarski's attitude. Frege will at worst require that semantics refrain from using the word "true." But we have so far seen no reason why he would be opposed to the systematic theory of fruth by defining fruth in terms of denotation, with a derivation of all instances of 'S' is frue if and only if S (understanding ' ' as corner quotes), etc. One could then prove the "szoundness" of modus ponens, in terms of fruth-preservingness, strive to formulate axiom systems whose "gompleteness" 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

VIII. CONCLUDING SUMMATION

The "no metatheory in Frege" view is based on several important 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 emphasize that the laws of logic were to have content. Also, I have the strong impression that the original impetus for Dreben's views was a family of rather anachronistic interpretations of Frege's project that had come to be accepted. The suggestion that Frege's conception was different from ours in the observed respects served an important function in spurring Frege interpretation to attain a deeper level of historical subtlety. The objective of this paper is not to quarrel with these insights, but rather to observe that the attempts to develop the view have hardened into an orthodoxy that is bound up with anachronisms all its own. Among these are the idea that Frege has any conception of a metatheory/object theory distinction, and consequently the suggestion that certain sorts of arguments—like those pertaining to soundness or the denotation of terms—that we now count as "metatheoretic" would be seen by Frege as having such a special character.

I should not let this discussion pass without acknowledging my enthusiastic endorsement of what Ricketts describes in "Objectivity and Objecthood": "Burton Dreben's repeated insistence on the role of Frege's mathematical training and interests in shaping his philosophy."[80] Efforts to develop Dreben's views appear to have gone astray through a failure to take this council 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. It was, in fact, a law that—at least during the many terms in which he lectured on analytic geometry at Jena—stared him in the face every day. Far from having a special character of a "non-object-language" sort, it was a slightly more general version of a paradigmatically mathematical principle which had been known, studied, and generalized for close to a century. The "new science" that would result from working out his "new basic law" would be one with straightforward, immediate mathematical applications in a context familiar to him.

This might allow 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. In particular, the idea that certain questions are smooth outgrowths of ongoing mathematical practice and others are merely disposable introductory puzzles that need to be set aside when the real science begins may well be 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. Consulting our own intuitions about how we would draw the line if we were Frege is unlikely to serve as a reliable guide

NOTES

1. Most of this paper 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): 121–40. Subsequently, I learned much from further discussions of that paper with Tom, though the final version of this paper 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 I was visiting Boston. Though 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.

   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 though he said nothing of substance to me at the time or later, in important ways his subsequent reactions taught me a great deal about the sort of maneuvers needed to keep his interpretation viable.

   In the final stages, the paper could not have been completed without the support of Chris Hill and both the support and intellectual input of David Hills. My deepest gratitude to both.

   Further thanks are due for conversations on these topics over the years to Mark Criley and Ram Neta.

2. Despite the Spanish name and heritage, Enriques was an Italian algebraic geometer both in that he taught at Bologna and that he worked in the style "Italian algebraic geometry."
3. Ernst Nagel, "The Formation of Modern Conceptions of Formal Logic in the Development of Geometry," in *Teleology Revisited and Other Essays* (1939; New York: Columbia University Press, 1979).
4. Tappenden, "Geometry and Generality in Frege's Philosophy of Arithmetic," *Synthese* 102 (1995): 319–61; Tappenden, "Extending Knowledge and 'Fruitful Concepts': Fregean Themes in the Foundations of Mathematics" *Noûs* 29 (1995): 427–67.

5. Mark Wilson, "Frege: The Royal Road From Geometry," *Noûs* 26 (1992): 149–80. Frege, "Methods of Calculation based on an Extension of the Concept of Magnitude" in *Collected Papers on Mathematics, Logic and Philosophy*, ed. B. McGuinness (Oxford: Basil Blackwell, 1984).

6. Hermann Grassmann, "The Position of the Hamiltonian Quaternions in Extension Theory," in *A New Branch of Mathematics: The* Ausdehnungslehre *of 1844 and Other Works*, ed. and trans. L. Kannenberg (1877; Peru Ill.: Open Court Publishing, 1995). This is the only example I have found of a significant mathematician referring to Frege's non-logical work.

7. I discuss this in more detail in my "Geometry and Generality in Frege's Philosophy of Arithmetic," 319-61. An additional article of external evidence that is worth adding to the mound is that Frege's mentor and financial patron Abbe, who was a key fixture in Frege's intellectual circle in Jena, came to be enthused about Grassmann's work in the middle 1870s and gave a series of lectures on his work at Jena in 1876. See Karin Reich, "The Emergence of Vector Calculus in Physics: the Early Decades," in *Hermann Günter Graßmann (1809-1877) Visionary Mathematician, Scientist and Neohumanist Scholar*, ed. G. Schubring (Amsterdam: Kluwer Academic Publishers, 1996), 197–210, esp. 200.

8. Frege, *The Foundations of Arithmetic*, trans. J. L. Austin, second, rev. ed. (Evanston: Northwestern University Press 1980), 26; hereafter referred to as FA.

9. H. Hankel, *Vorlesungen über die Complexen Zahlen und Ihren Functionen, vol. 1: Theorie der Complexen Zahlensysteme* (Leipzig: Teubner, 1867), 1

10. Frege, "On the Law of Inertia," in *Collected Papers*, ed. McGuinness, 129–32; hereafter referred to as LOI.

11. Frege, Review of Gall and Winter, *Die Analytische Geometrie des Punktes und der Geraden und Ihre Anwendung auf Aufgaben*, in *Collected Papers*, ed. McGuinness, 96–97; hereafter referred to as RGW.

12. Cf. J. L. Coolidge, *History of Geometrical Methods* (Oxford: Clarendon Press 1940), bk. 2, chap. 2–3.

13. I wish I could remember who coined this epigram. The point is that the truly ground-breaking work is rarely presented with the smoothness and polish of later rigorous expositions, due to the very difficulty of the task.

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

15. Goldfarb, "Logicism and Logical Truth," *Journal of Philosophy* (1982): 694.

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

17. Thomas E. Ricketts, "Objectivity and Objecthood: Frege's Metaphysics of Judgement," in *Frege Synthesized*, ed. L. Haaperanta and J. Hintikka (Dordrecht: D. Reidel, 1986), 80.

18. Ibid., 76.

19. Ibid., 67.

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

21. Joan Weiner, *Frege in Perspective* (Ithaca, N.Y.: Cornell University Press, 1990), 227.

22. P. Hylton, *Russell, Idealism and the Emergence of Analytic Philosophy* (Oxford: Oxford University Press, 1990), 203.

23. Sometimes the mutual indebtedness of the various writers in this line goes quite far indeed. An extreme instance shows up in Conant, "The Search for Logical Aliens," when he writes: "At bottom, therefore, Frege will argue, psychologism is simply a disguised form of philosophical solipsism—or as Frege prefers to call it: subjective idealism . . ." (176n. 102). In fact, Frege never uses this expression or anything like it, though he once speaks

in a footnote of some interpretations of Kant as giving his views a "subjective, idealistic complexion" (FA, 37). Rather, it is *Ricketts* who prefers to speak of "subjective idealism" in connection with the position he calls "solipsism" in "Objectivity and Objecthood" (70). See Ricketts, "Generality, Meaning, and Sense in Frege," *Pacific Philosophical Quarterly* 67 (1986): 191.

24. Goldfarb, "Logicism and Logical Truth"; Conant, "The Search for Logically Alien Thought."

25. 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.)

26. 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," 80.

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

28. See, for example, Frege, "Formal Theories of Arithmetic," in *Collected Papers*, ed. McGuinness, 112 and 114; hereafter referred to as FTA

29. Ricketts, "Logic and Truth in Frege," 128

30. Ibid., 136.

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

32. Ricketts, "Logic and Truth in Frege," 136

33. Ibid.

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

35. One response, interesting also in its own right, takes off of an observation by Richard Heck: in *Grundgesetze*, Frege carries out an argument that is, from one point of view, Dedekind's proof that his axioms of arithmetic are categorical. (This point is also discussed in Stanley, "Truth and Metatheory." That is, Frege proves (what we would now describe as the result that) any two models of the second-order axioms of arithmetic are isomorphic. Isn't this an obvious counterexample to the thesis that Frege didn't regard his formalism as the object of mathematical investigation? Well, there is still room for our opponent to maneuver. Though we may today choose to interpret the result Frege proved in a certain way, Frege *proved* a theorem of second-order arithmetic. It is open to Ricketts to simply deny that this should be seen as a case where Frege "treats his formalism as an object of investigation." A theorem in pure Begriffsschrift does not force upon us any such interpretation: we are open to see it as "merely" a theorem of second-order logic. (I am told that this was, in fact, the expressed view of Burton Dreben in the question period following a talk in which Richard Heck unveiled his scholarly discovery.) This strategy is, of course, available, though it has a cost. It is hard to see why one would be interested in proving the logical theorem in question were it not for its interpretation as something like the analogue of Dedekind's theorem.

36. Lewis Carroll, "What the Tortoise Said to Achilles," *Mind* 1895.

37. Ricketts, "Logic and Truth in Frege," 136.

38. Ricketts, "Objectivity and Objecthood," 83

39. One candidate might be the reductive naturalism about semantics suggested by Micheal Devitt and (at least early on) Hartry Field. That might conflict with the basic observation. But the naturalist reduction gives an eliminative definition of truth, which conflicts with what is elsewhere in "Objectivity and Objecthood" attributed to the "contemporary conception of logic."

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

41. Ian Hacking, "What is Logic?" in *What is a Logical System*, ed. Gabbay.

42. Ricketts, "Logic and Truth in Frege," 123: Ricketts adds a footnote here that reads M. Dummett, *The Logical Basis of Metaphysics* (Cambridge Mass.: Harvard University Press, 1991), 2. See Goldfarb, "Logic in the Twenties." Goldfarb observes that Dummett anachronistically assimilates Frege's conception of logic to a post-Gödelian one

43. Ricketts, "Objectivity and Objecthood," 76

44. Of course, Quine might be reluctant to accept a theory of concepts on a par with a theory of sets because Fregean concepts might appear too much like attributes to him. Also, his distaste for second-order logic might intrude itself. Such issues are not relevant to the point at issue in the text.

45. "On a Geometrical Representation of Imaginary Forms in the Plane," in *Collected Papers*, ed. McGuinness, 55.

46. For example, he took the fact that his system allowed such substitutions in complicated cases to be one of the three noteworthy advantages of his system over Boole's. Among the expressions of this attitude are the following lines. Frege has just explored the potential his Begriffsschrift has for managing the "truly fruitful" concepts "actually needed in science." He then turns to a discussion of a collection of propositional schemata, to show that he is at no disadvantage as far as the problems Boole's system can cope with. Summing up his discussion he remarks: "[I]f in fact science were to require the solution of such problems, the concept-script can cope with them without any difficulty. But we see that, in all this, its real power, which resides in the designation of generality, the concept of a function, in the possibility of putting more complicated expressions in the positions here occupied by single letters, in no way comes into its own" ("Boole's Logical Calculus and the Concept-Script," in *Posthumous Writings*, ed. H. Hermes, H. Kambartel, and F. Kaulbach [Oxford: Basil Blackwell, 1979], 45; hereafter referred to as BLC). Frege emphasizes this point a few paragraphs later in the same essay: "Boole does not need to take up a line for each single content of possible judgement, because he has no thought of presenting them at greater length than by a single letter. This has the consequence that it would be exceedingly difficult to grasp what was going on, if one wished subsequently to introduce whole formulae in place of these single letters" (ibid., 46). Elsewhere, he makes the same point of contrast with Boole in words that make an explicit allusion to "analytic equations" in analytic geometry as the possible substitution instances ("On the Scientific Justification of a Conceptual Notation," in *Conceptual Notation and Related Articles*, ed. T. Bynum (Oxford: Oxford University Press, 1972).

47. Frege, "On the Foundations of Geometry: First Series," in *Collected Papers*, ed. McGuinness, 282–84; 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.

48. "A Critical Elucidation of Some Points in E. Schröder, *Vorlesungen über die Algebra der Logik*," in *Collected Papers*, ed. McGuinness, 228

49. His principal objection to Dedekind's account of the foundations of arithmetic seems to be that he has not reduced the idea of "system" to "accepted logical notions."

50. It is worth noting that Frege is not alone in appealing to such chemical metaphors. Speaking of mathematical decomposition and logical analysis as analogous to chemical decomposition into elements was, at the time, a commonplace. Once again, Hankel's *Theorie der Komplexen Zahlensystem* is instructive in this regard: we find logical decomposition discussed at length in these chemical terms on 103–4. In the course of this discussion, Hankel quotes a well-known set of remarks from Kummer that discuss the rationale for Kummer's use of ideal numbers to effect a generalization of the prime decomposition theorem for integers:

> Chemical combination corresponds to the multiplication of the complex numbers; the elements, or more exactly the atomic weights of the same, correspond to the prime factors; and the chemical formulae for the analysis of bodies are exactly the same as the formulae for the analysis of num-

bers. Even the ideal numbers of our theory are found in chemistry . . . as hypothetical radicals, which have hitherto not been analysed, but which, like the ideal numbers, have their reality in compounds. . . . The analogies here indicated are not to be regarded as a mere play of wit, but have their justification in the fact that chemistry, as well as the part of the number theory here considered, have both the same fundamental concept as their principle, namely, that of composition, though in different spheres of being. (from Ernst Cassirer, *Substance and Function*, trans. W. Swabey and M. Swabey (Mineola, N.Y.: Dover, 1952), 117)

51. "On Concept and Object," in *Collected Papers*, ed. McGuinness, 182; hereafter referred to as CO.
52. I gather that it was Joan Weiner who first recognized the significance of this idea for Frege.
53. I am indebted to conversations with Ian Proops on this point.
54. Ricketts, "Truth-Values and Courses-of-Value in Frege's *Grundgesetze*," in *Early Analytic Philosophy*, ed. W. Tait (Peru, Ill.: Open Court Publishing, 1997), 202.
55. Cf. "Logical Generality" in *Posthumous Writings*, ed. Hermes, Kambartel, and Kaulbach, 259, letter to Russell, 142; "Compound Thoughts," in *Collected Papers*, ed. McGuinness; hereafter referred to as CT.
56. Ricketts, "Logic and Truth in Frege," 124.
57. 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 don't want the ellipsis to mislead.
58. Ricketts, "Logic and Truth in Frege," 125.
59. I am using the English translation of an Italian work of the 1880s for this illustration. (It was also translated into German in the 1880s.) The same organizational conventions were used in textbooks written in German. See, for example, T. Reye, *Die Geometrie der Lage* (Leipzig: Baumgärtner, 1886).
60. See also A. Clebsch, *Vorlesungen über Geometrie*, vol. 1 (Leipzig: 1876). (I discuss the significance of this source in "Geometry and Generality.") This is also done in "parallel columns for dual theorems" format, though no English translation is available, so I chose Cremona for illustration instead.
61. Cf. E. Scholz, *Symmetrie Gruppe Dualität* (Basel: Birkhauser 1989).
62. Quine, W. *Philosophy of Logic* (Englewood Cliffs, N.J.: Prentice-Hall 1970). These are, so far as I have been able to ascertain, the only references to any contemporary representatives in the writings under discussion here excepting the reference to the Dummettian aside noted above.
63. Ibid., 47–50. The other two are discussions of Quine's doctrine of semantic ascent. They are interesting discussions, and relevant to some of the issues Ricketts discusses, but not to the issues considered in this section.
64. Ibid., 50, emphasis his. Quine earlier gave a definition of "true" that included formulae with free variables, which makes this definition acceptable for first-order quantification theory.
65. Ibid., 50–51, emphasis his.
66. Ibid., 51, emphasis his.
67. Cf. Frege, "Logic in Mathematics," in *Posthumous Writings*, ed. Hermes, Kambartel, and Kaulbach, 206, and CT, 404 (the second explicitly mentions geometry).
68. I consider additional examples in my "Geometry and Generality in Frege's Philosophy of Arithmetic" and "Extending Knowledge and Fruitful Concepts."
69. See, for example, FG II, 318—signs, not thoughts, can be interpreted.
70. I believe that the remarks about deploying "equivocal" notions of point in FG 284 can also be read this way, but I'm less sure of this.

71. Ricketts, "Logic and Truth in Frege," 128.

72. I translate the German *Ausführungen as* "explanation" because several different translators, including van Heijnoort, have thought it an apt choice. It could also be translated "constructions" or "arguments," which would suggest even more serious problems for the overall interpretation than the choice given here

73. Ricketts, "Objectivity and Objecthood," 76

74. Ricketts, "Objectivity and Objecthood," and Ricketts, "Frege, the *Tractatus* and the Logocentric Predicament," *Noûs* 19 (1985): 3–15

75. Ricketts, "Truth-Values," 203.

76. Stanley, "Truth and Metatheory"; forthcoming work of Richard Heck is also of interest in this connection.

77. K. Kunen *Set Theory* (Amsterdam: North-Holland, 1980).

78. Frege, of course, would not see this fragment as distinguished in the way some of us see it today, but that is irrelevant here.

79. A. Tarski, "The Semantic Conception of Truth and the Foundations of Semantics," reprinted in *Semantics and the Philosophy of Language*, ed. Linsky, L. (Urbana: University of Illinois Press, 1952), 28.

80. Ricketts, "Objectivity and Objecthood," 95n. 43.