Notre Dame Journal of Formal Logic
Volume 28, Number 1, January 1987

The Rationalist Conception of Logic

STEVEN J. WAGNER*

The failure of Frege's foundation for mathematics in [9] led to an enduring tension in the philosophy of logic. If Frege had succeeded, almost everyone would have granted his system the title of logic in a favored, primary sense.[1] First-order logic (FOL) would, like sentential logic, have been considered an interesting special case. Stronger systems might have been called logics in view of their similarities to Frege's. But anything beyond what was needed for the general formalization of mathematics would have borne the name logic by courtesy—particularly if its principles were less evident than Frege's axioms I–V. Unfortunately, however, Russell discovered that there were no stronger systems; a generation later, Gödel showed that the truths of any RE logic could only make up a tiny part of classical mathematics. Logicians after Frege have therefore had to consider a proliferation of systems sharing to various extents the attractions of his paradigm. Since one may differ over which features, if any, can serve to pick out a logic from among the many alternatives, the "scope of logic" ([23], ch. 5) has remained in dispute. Broadly speaking, the disputants fall into two camps, one emphasizing strength as a criterion for the title of logic, the other conceptual simplicity. Stronger systems are more nearly adequate for the job of founding mathematics, yet increasing strength yields less elementary, transparent notions of logical validity and proof. Frege's system, of course, was both elementary and strong, but that was too good to be true.

This paper deals with the problem of characterizing logic that we have inherited from Frege. I will also consider a problem of Quine's. Verbally, it is the same—'what is logic?'—but Quine's motives and philosophical framework are so different that the relation of his question to Frege's is not obvious. For Quine, the determination of a speaker's ontology, which depends on a choice

*I thank Tim McCarthy for very valuable conversations. I also thank Michael Detlefsen for his advice and, particularly, for his encouragement.

Received November 20, 1985; revised December 13, 1985

of logic, replaces the logicist project as a reason for caring about the scope of logic, and his empiricism is deeply opposed to Frege's outlook. The connections between Frege's and Quine's concerns must therefore be investigated. I will argue that the two problems are in fact closely linked, and that Frege would endorse Quine's identification of logic with FOL. But the argument itself will not be Quine's. Indeed, it would appear suspect to him as well as many other philosophers. My aim is to defend, with qualifications, a version of the traditional view of logic as an instrument for reasoning. FOL is best for that, although other logics have other virtues. That view, however, is at home in a Fregean rationalism, not in Quine's empiricism and naturalism.[2] We should not be surprised to find that taking a Fregean view of logic means siding with him against the viewpoints that he most strenuously resisted; this may encourage us to reconsider these now prevailing tendencies.

1 Let me clarify some issues and review the present situation. Assume that we are given a semantically interpreted language L. To select a logic for L is to fix a relation R, holding among sets of sentences of L, that is to be understood as logical consequence. (Of course our real interests are in consequence relations in classes of languages. I follow the textbook convention of leaving this generality implicit.) What conditions must R meet to justify this understanding? Presumably, a relation associating even-numbered sentences with odd-numbered ones under an arbitrary coding will not do. Useful restrictions, however, are hard to find without making major decisions about the nature of logic.

We may begin with the abstract conditions of Gentzen's [10]:

(1) reflexivity: $R(\Sigma, \Sigma)$, where Σ is any set of sentences in L.
(2) dilution: if $R(\Sigma, \Sigma')$, then $R(\Sigma \cup \Sigma'', \Sigma')$ and $R(\Sigma, \Sigma' \cup \Sigma'')$[3]
(3) transitivity: for any sentence S, if $R(\Sigma, S)$ and $R(S, \Sigma')$, then $R(\Sigma, \Sigma')$.

Although these are trivial for FOL and many other logics, only the first holds for everything that might reasonably be called logic: without question, everything follows logically from itself. (2) is already stronger: dilution rules out inductive logics, because adding premises can weaken inductive arguments. Since we are not assuming at the start that any logic must be deductive, dilution is not immediately acceptable. Transitivity, too, can be challenged. One plausible condition on logical inference might be preserving justification: roughly, if $R(\Sigma, \Sigma')$, and if belief in Σ is justified to degree d, then belief in Σ' is justified to the same degree. (This cannot be right for inductive logics, but transitivity would have to be modified for the inductive case anyway.) The problem here is that our fallibility may prevent even perfect, deductive justification from transmitting down chains of inferences. Suppose there is a series of justification-preserving steps from S_1 to S_2, S_2 to S_3, S_3 to... to S_n. Assuming that R preserves justification, it is questionable whether $R(S_1, S_n)$ holds if the chain is too long and complex, the relation of S_1 to S_n too subtle, for anyone to grasp. One might, for example, think that any justified step from one statement to a second must be an instance of a generally reliable pattern of reasoning; and we might be as likely to go wrong as right in cases of this type. It would follow that

S_1 could be justly believed in the absence of good reasons to believe S_n, thus that the latter is no logical consequence of the former. Of course this can be disputed in various ways, but the point is that if our notion of logic is suitably general, transitivity cannot simply be taken for granted.

Because we do not want to prejudge the issue against inductive logic, we can also not immediately require R to preserve truth. Induction can lead from truths to falsehoods. On the other hand, the close connection between logical consequence and justification, which underlies the idea of an inductive logic, is disputable even if one overlooks the problem about transitivity. Second-order consequence is often considered to be logical, but correct second-order steps may be unjustified. The continuum hypothesis (CH) provides a standard illustration. There is a formula CH^* true in any second-order model M iff M contains no set larger than the natural numbers and smaller than the real numbers ([25] indicates the construction). If CH is true, CH^* is a second-order validity; analogously for $\sim CH^*$ if CH is false. $R(S, C)$ therefore holds in second-order logic (SOL) for any S and the valid member C of the pair $(CH^*, \sim CH^*)$. But S may have no epistemic relevance to C. In fact, there may be no way at all to come to hold justified belief in C—if this seems implausible for C, even more mysterious propositions of set theory will generate similar examples. So second-order consequence is quite divorced from epistemic relations.

Another approach would start with the claims that every logical validity—every logical consequence of any sentence whatever—is true, in fact necessary, and that logical consequences are necessary consequences. This by itself is consistent with taking R to be the null relation, but R becomes nontrivial if we can designate a significant body of truths as being logical. Logicism offers such a result by making the class of validities include all mathematical truths. Unfortunately, there are no obvious independent reasons to accept logicism. The most natural approach would be to introduce a notion of form, then to claim that logical truth and consequence can be defined in terms of form alone. But no *intuitive* notion of form seems to guarantee the truth of every sentence with the same form as a given mathematical truth. (Consider, e.g., CH or CH^*, or their negations.) So one can neither appeal to logicism to fix the extent of logic, nor readily derive logicism from any natural characterization of logic.

We see that very little concerning logic can be taken for granted. The result has, in recent years, been agnosticism and pluralism. Various writers have considered a variety of dimensions, formal as well as epistemic and pragmatic, along which logics may be distinguished. No consensus selecting a particular logic has emerged, and philosophers concurring on a choice of logic (most often on FOL) frequently disagree on the reasons. (For a representative sample of views, see [2], [5], [14], [15], [20], [25], [27].) Only relative to a particular context or set of interests is the title of logic seen as more than a conventional label. In a way, this assessment is impossible to dispute. The range of more or less acceptable, plausible accounts of logic proves that the concept is flexible, so that sheer intuition about what counts as logic cannot rule out a diversity of candidates. Yet I want to promote a single conception as far as this indeterminacy will allow.

Frege took over from Aristotle and Leibniz a view of logic as the basis of idealized cognition. Logic may not describe actual reasoning, but in setting out a logical language and its rules of inference, we are defining a system of thought for us to approximate as best we can. Ideally, thought (or its linguistic expression) is formulated in logical notation, and steps of reasoning follow the rules of logic. This traditional view was sometimes ambiguous between reasoning as an instrument of discovery and as an instrument of justification, hence unclear about the role of logic. Frege, however, is well-known for having distinguished sharply between discovery and justification, indeed between everyday justification and justification showing the "deepest grounds" for holding something true ([8], pp. 2–3). This was an historically important clarification, permitting a more definite classification of logic. For Frege, logic is part of the theory of ideal justification. It has no direct connection with the description of mental processes, nor with the problem of finding the truth. It is also not concerned with what should count as sufficient justification in practice. Instead logic provides the language and rules for the best justifications possible in abstraction from the limits of our intelligence. The project of [8] and [9] is to find the propositions to which we can reduce arithmetic and to give fully rigorous, explicit derivations. It is no objection if this cannot increase the certainty of arithmetic, or if humans can hardly maintain the chosen level of rigor. And it is just for this project that [7] provides the logic, that is, the standard of rigorous argument.

Thus, Frege's conception makes logic part of epistemology—of the theory of justification. What logic is depends on what counts as ideal justification. Notice that the move to *idealized* reasoning takes care of our problem about transitivity. If failures of memory and attention are not a factor, then transitivity is preserved, and we can regard logical proof as a way of obtaining conclusions that are exactly as credible as their premises. This, of course, clarifies the importance epistemologists have historically attached to logic.[4]

Many contemporary authors seem to share Frege's epistemic orientation, for epistemic considerations are often brought to bear on questions of choice of logic. Infinitary "logics," for example, are mathematically interesting, but it is widely claimed that they cannot serve as *logics* for finite beings, who cannot in general write infinite formulas or reason with infinite arguments. Moreover, this inability remains under all reasonable idealizations. But the widespread interest in SOL and other systems with nonelementary consequence relations limits our agreement with Frege, as does the specific character of his epistemic concerns. Since Frege does not seem to be after justifications in an ordinary sense, we cannot assume that a logic appropriate to his epistemic goals would suit ours. This will be discussed further below. But we can provisionally connect Frege's project with rationalist epistemology. Characteristic of rationalism are its interests in perfect rigor and in setting out our knowledge in an order reflecting not the demands of everyday exposition and argument, but rather some kind of ideal systemization. Descartes, Spinoza, and other classical rationalists pursued these aims in abstraction from practical considerations; the same seems to hold for Frege. (In Frege's case, the project is limited to systemizing mathematical knowledge, but I think he could have accepted its extension to natural science.) So I suggest that we regard Frege's conception of logic as being determined by the needs and goals of rationalistic system-building; I will call it a rationalist conception of logic (RCL). Of course, this raises a number of questions about the nature of Frege's project and about the precise connection between one's epistemology and one's logic. Although I cannot fully investigate these issues here, I hope that the rest of this paper may somewhat clarify them. Perhaps the preceding characterization of Frege will suffice for the moment, so that I may sketch the course of my discussion.

I will first explain why Frege's RCL encourages an identification of logic with FOL. Basically, the reasons consist in the familiar epistemic arguments against nonelementary logics, but we must address several difficulties before the identification can be confirmed. Here I should note that FOL will be defended only against a restricted class of alternatives. My main concern is with its relation to stronger competitors, e.g., SOL, not with whether (classical) FOL is already too much. Arguments to that effect, e.g., Dummett's case for intuitionistic logic, involve epistemic and semantical assumptions that cannot be dealt with here; we will instead take the usual view that FOL is transparently in order. I will also not deal with modal systems, such as alethic modal logics and tense logics. Although I think my viewpoint discourages their elevation to the status of genuine logics, I will leave this implicit. In any case, the question whether any modal logic is really logic may well contain enough of a conventional element to reduce its importance. Finally, I consider only deductive logics.[5] This is a more serious omission, for views of logic as an instrument for justification do not merely admit a generalization to the inductive case, they positively invite it. If logic defines the language and rules of argument, one would naturally suppose that there should also be nondeductive logics. But their possibility lies outside the scope of this discussion. The proposed identification of logic with FOL should therefore be viewed as holding for a special case. Ideally, we should want our conception of logic to cover appropriate inductive logics as well.

After developing a Fregean view of logic, I want to defend it in two ways. First, I will try to eliminate some competitors: attempts to identify logic with FOL for non-Fregean reasons (Quine) or to give the title to SOL or something similar (various authors). I think each of these can be shown to be untenable on internal grounds, without begging questions about the nature of logic. This appears significant, for although I have conceded that the nature of logic may be open to interpretation, the failure of rival interpretations suggests that our concept is fairly determinate after all. Second, I will examine the presuppositions of the RCL more carefully. Trying to characterize logic is pointless unless one has a viewpoint from which it matters what logic is. Of course, Frege's logicism provides one, but if we reject it, it not obvious that he could offer any deeper reasons for caring about the nature of logic. Here is where the viability of a form of rationalism becomes critical. I will defend Frege on this point and thus maintain that we have good reason to regard FOL as logic on broadly Fregean grounds—even if other viewpoints remain possible. This will amount to a substantial vindication of Frege.

2 I think that for Frege, logic must turn out to be FOL. One should not dismiss this proposal because FOL is too weak to validate logicism; or brand it as trivial because the inconsistency of Frege's views about logic (as well as in it) might lead to an identification of logic with anything one pleases. For clearly, the truth of logicism was not built into Frege's notion of logic, but was rather to be confirmed by his constructions. That is why Russell's paradox led him to abandon logicism and to seek, late in his life, geometrical foundations of mathematics. A prior conception of logic had been shown to be inadequate to the foundational task.

The main features of this conception are clear. Frege holds that logical knowledge is a priori and analytic, where the latter means at least that it is not based on any kind of spatial intuition. Further, as van Heijenoort has stressed [16], he views the *Begriffsschrift* as providing at once a *calculus ratiocinator* and a *lingua characterica*: a deductive calculus and a universal language for the expression of thought. These features are evidently tied to the rationalist programme of a comprehensive, rigorous development of our system of beliefs. We want a logic that can express all elements of this system and clarify the logical connections among them.[6]

The identification of logic with FOL then suggests itself naturally. Such fragments as propositional and monadic logic are ruled out by their expressive, representational weakness. Language exists for the expression of thought, and Fregean thoughts have no less structure than formulas of FOL. Any use of one of these fragments as a language would therefore be parasitic on the use of a full first-order language. If we can express an arbitrarily complex thought, say, that high interest rates help large corporations drive out smaller ones, using just the sentence symbol 'p', that is only because of our ability to translate from a richer background language into the sentential notation. The latter could not stand on its own. And in any case, the sentential logic prevents us from formalizing inferences universally agreed to be logical. So logic is at least FOL. Now a salient property of FOL, the existence of a complete set of rules, is implicit in the idea of a deductive calculus and the apriority condition. Although a rigorous grasp of mechanical calculation was made possible only by Turing and Post, the notion was tolerably clear to Leibniz, whom Frege follows in his concept of formal inference. Any deductive consequence C of a set Σ of statements can, on Frege's conception of logic, be mechanically calculated: a finite series of steps leads from premises in Σ to C, with each step governed by a rule the applicability of which can be recursively determined. Underlying this condition is the traditional assumption of the apodictic character of a priori knowledge. If it is a priori whether something is a proof, then that question can be settled with certainty, not merely established with some degree of probability; thus calculability is properly understood as nothing less than recursiveness. But if it is mechanically determinable whether a given argument is in fact a proof of logic, there must also be a recursive characterization of the entire set of logical rules. This suffices for the recursive enumerability of the consequence relation, that is, for completeness. Thus it is natural to identify logic with FOL, since completeness is typically lost when we go beyond it.

These observations are commonplace (cf. [3], ch 0). It is also a commonplace that they do not force the identification of logic with FOL. One large problem is that the link between logic and proof, which underlies the completeness condition, can be denied in favor of a model-theoretic notion of consequence (see section 5). An immediate difficulty, however, is that numerous complete logics essentially extend FOL, for example, Keisler's logic with the quantifier (Ux) ("there exist uncountably many x"), or the logics with so-called equicardinality or cofinality quantifiers. It seems unintuitive to call these genuine logics—

one wants to say that they belong to set theory. Yet it would beg the question against various positions, including forms of logicism, to hold that set-theoretical principles can *ipso facto* not belong to logic, so it is unclear how to rule out these extensions. Tharp faces and, I think, does not solve these difficulties in [27]. The concept of cofinality, for example, is rejected simply for being "technical" and "unpalatable" (p. 13). But Tharp's reaction to the uncountability logic hints at a better reply:

> There is a serious objection against the quantifier "for uncountably many x." One would expect that if this were a legitimate quantifier, the quantifier "for infinitely many x" would also be acceptable; but no complete logic can contain the latter quantifier... [this] seems to me to be a state of affairs sufficiently unnatural to discredit (Ux) as a logical notion. (pp. 11–12)

This is unclear, but we can make sense of it along Fregean lines. To the previously mentioned features of logic we should add that it is *fundamental*: roughly, its principles and concepts cannot depend on nonlogical ones. Frege clearly intends such a condition. It may well be necessary to acquire empirical beliefs before grasping any logical notions clearly at all, or before coming to believe anything more than trivial truths of logic. Extralogical (e.g., geometrical) mathematical thought may also be needed. But in principle, logic is independent. The point concerns not the order of belief acquisition but the order of definition and justification. Logical beliefs need rest on no others, and logical concepts are primitive or defined from logical ones alone. This leaves it open how far up logic, the foundation, goes. An identification of logic with all of set theory would be consistent with the fundamentality criterion. But fundamentality rules out gaps that would need to be filled by nonlogical cognition, as in Tharp's examples. If we admit (Ux) as a quantifier, then it is "unnatural" to omit the analogous (CTx) – "for countably many x"–because one could not, it seems, formulate or understand the Keisler logic without using the concept of countability. Yet adding the capacity to express countability–or quantifiers (Fx) and (Ix) expressing finiteness and infinity–to the logic for (Ux) destroys completeness (see [27] for discussion.) Similar arguments apply to the complete logics for other generalized quantifiers. Consider (Cx), the Chang quantifier: $(Cx)Ax$ is satisfied in a model just in case the set of things that satisfy $A(x)$ has the same cardinality as the universe of the model. Evidently, understanding (Cx) presupposes the ability to grasp "there are as many As as Bs". Yet completeness again vanishes, as Tharp observes, when our logic is augmented to express this notion. The problem is even more obvious for the cofinality quantifiers, with their dependence on the theory of ordinals.

We are assuming that logic includes the laws governing logical concepts. If having a logic for (Ux) makes countability a logical concept (because of fundamentality), we will count as logical the principles governing it, which yields incompleteness. This is not quite trivial, but it falls naturally out of Frege's viewpoint. We must claim that grasping a logical concept (or any other) is inseparable from believing certain truths involving it. To understand 'and', for example, one must know the usual rules or axioms for conjunction. Similarly for (Ux) or (CTx). This seem reasonable in any case and certainly accords with Frege's views. Frege sees our grasp of concepts as being exhibited in our acceptance of principles; this is clearest in [8], where gaining a concept of number is a matter of finding laws that define one.[7] One must, then, understand fundamentality as entailing the right kind of closure. The axioms governing (Ux) do not rest on those for (CTx) in the sense of directly requiring them for their justification. But knowing the former involves knowing the latter. If logical thought as a whole is independent of nonlogical thought, then the axioms for (CTx) would also belong to logic. Hence, the completeness of logic rules out (Ux).

The fundamentality thesis needs further clarification. Since it posits definability relations among concepts, as well as necessary connections between having a concept and holding certain beliefs, it is involved in the troubles of analyticity. Still, these troubles are not fatal. It is hard to deny that anyone who understands, say, the notion of uncountability understands countability too and shares some of our basic beliefs about cardinality. The critique of analyticity should allow such facts, and with them the fundamentality condition. Nor is the condition *ad hoc*. The traditional conception of logic obviously includes something like an idea of independence and conceptual closure that the complete extensions of FOL violate.

We thus have a basis for identifying logic with FOL. By combining the ideas of a universal language and a deductive calculus with the fundamentality condition, we can argue that logic must be at least FOL while ruling out the suggested complete extensions. This identification, however, is still both conditional and provisional. It is conditional because it presupposes Frege's general view: that logic is an instrument for formulating and checking proofs. Otherwise our epistemic constraints are unmotivated. There would be no clear reason to suppose that any proof should be recognizable as such, hence none to restrict ourselves to complete logics. Nor is fundamentality plausible apart from some view separating logic from other areas of inquiry and giving it a kind of priority. But this conditional aspect is acceptable. We will presently consider alternatives to the RCL. Now we want merely to argue that on Frege's conception, at least, FOL turns out to be logic. The provisional character of the argument is more immediately troubling. We have hardly shown that nothing essentially stronger or more expressive than FOL can meet Frege's criteria. Let us consider some specific difficulties.

Lindstrom's theorem suggests the possibility of a single argument against a range of alternatives to FOL. Lindstrom showed that any logic (in a general sense) that is either complete or compact and satisfies the Löwenheim-Skolem (L-S) theorem must be FOL. Since we have already restricted ourselves to complete logics, we would only need to find a connection between the RCL and the L-S property to clinch the matter. The standard objection, however, is that the L-S property simply lacks epistemic motivation.[8] Why should it matter, intuitively, whether any (sets of) formulas in our logic have only uncountable models? This feature is clearly linked to something of epistemic interest: it means, roughly, that the resources of logic alone do not enable us to express differences between infinite cardinalities. The distinction between countability and uncountability falls outside of logic (unlike the finite-infinite distinction, in the sense that we can write formulas with only infinite models). But why consider this desirable?

I see no full answer, but perhaps the proof of the L-S theorem provides a clue. Its essence is that countably many formulas can only require the existence of countably many objects. Of course this depends on the specific character of FOL, where formulas are built up from atomic formulas using universal and existential quantifiers. The former do not posit objects, while the latter only introduce them singly: in the proof, one expands an initial domain by adding an object chosen to satisfy each existential quantification. Since this can only increase the domain by countably many things, we never get beyond a countable model. This suggests that what matters is the countability of the language together with the elementary nature of the quantifiers. Not that the proof works just for the quantifiers of FOL. Adding, e.g., (Ix) to the language permits the argument to go through. But a variety of ways in which one might extend FOL, and which might preserve completeness (unlike (Ix)), are sufficiently infinitistic to block the proof of the L-S theorem. (Ux) is one example. This suggests a kind of correspondence between the L-S property and the fundamentality condition.

We noted that although completeness allows a variety of nonelementary logics, conjoining it with fundamentality generally blocks them. The relation of completeness to the L-S condition is apparently similar: the devices complete, nonelementary logics introduce add enough expressive power to make the proof of the L-S theorem impossible. Now the L-S condition and the fundamentality condition are entirely different. One limits the relations among infinite models of a theory, the other requires a kind of definitional closure. One has clear epistemic motivation but is unformalizable, the other is formal but unmotivated. Two distinct conditions can, however, be equivalent in the presence of a third. This may be the case here. A complete extension L' of FOL will contain an operator O interfering with the model construction in the standard proof of the L-S theorem. In order to turn L' into a logic satisfying the fundamentality condition, we must (I conjecture) add further operators that destroy completeness. The presence of O violates fundamentality in one way and the L-S condition in another. This does not mean that the latter simply inherits the motivation of the former. The L-S condition in itself remains unmotivated. But suppose we have conjectured that fundamentality blocks any complete, nonelementary logic; and that we want a formal condition that has the same effect. In the presence of completeness, the appropriate condition should rule out the same devices that fundamentality does. If the L-S condition works, and if we can (by inspecting the standard proof) understand how, then its motivation is sufficient. The epistemic motivation for completeness and our interest in fundamentality together explain why adding the L-S condition is reasonable.

These considerations, however, are vague and call for backing by a general classification of the devices that can extend FOL; one could then try to go through these and show that, for complete extensions anyway, violation of the L-S criterion is systematically correlated with violation of fundamentality. Of course, that might not happen. It would then be dogmatic to insist on the L-S criterion: if some complete extensions of FOL violate the L-S condition but not fundamentality, one might well count them as logic. In a clash between the two conditions, the one with direct epistemic motivation would have priority. But this issue is hard to discuss until some concrete case is described. And since it is hard to see how such a case could involve more than a weak extension of FOL, we may conjecture that using the L-S condition and using the fundamentality condition to supplement completeness lead to roughly the same results.

We have not touched on the logical status of operators that may appear intuitively elementary, e.g., identity or nonclassical quantifiers such as 'many', 'most', and 'few'. Some of these decisions seem to be trivial. Identity, for example, is a useful, epistemically and formally harmless addition to FOL. Further, the absolutely general applicability of identity makes natural its inclusion in a universal language for conducting proofs. The other cases cannot be discussed here, but they are surely peripheral. It we can make a reasonable case for some form of FOL, the exact boundary of logic will not be a pressing issue.[9] Hence I will turn to some points of motivation glossed over so far.

I claimed that the idea of a deductive calculus involves an effectiveness requirement on proofs—but mightn't something weaker suffice? One natural suggestion is that we do not need effective procedures for determining whether something is a proof, since procedures that settle the question with high probability would do as well in practice. But this is mistaken, since it does not define a genuine alternative from its own practical viewpoint. In reality, so-called effective procedures only yield their results with high probabilities anyway, given the possibilities of error in their application. To speak of effective procedures is already to abstract from limitations on finite memory and attention and from the possibility of mistakes in calculation and the like. The RCL offers a view of logic which, under such an abstraction, yields an effective notion of proof. Our question must be whether this is appropriate, or whether we should instead accept a logic that places weaker conditions even on ideal argumentation.

Perhaps we can grant that we should at least have the positive half of a decision procedure for the class of proofs: a way to establish mechanically that something is a proof, if it is. This is not indispensable, but it has an attractive consequence that lies at the heart of rationalism. I will call it the Cartesian condition: once a proof is given, it cannot be overthrown on logical grounds. Its premises might be false, but nothing can force us to retract any of the inferential steps, because we have already determined their correctness mechanically. In terms of the construction of an ideal order of knowledge, this means that as long as we are working from true beliefs (and logic is not responsible for that), we can continue to build without having to take anything back. If this is possible without any great sacrifice, it is clearly desirable. But what about the other half of effectiveness: why not allow the status of some alleged proofs to remain open? This leads to no obvious practical difficulty. It may be annoying not to be able to tell whether something is a proof of *P*, but we are imagining a situation in which the correctness of certain methods of proof—rules of inference, say—is already given. Our available half of a decision procedure has taken care of that. We are therefore free to look for a proof of *P* using the methods already known to be acceptable, plus any others we might discover as we go along. As far as the establishment of new results is concerned, this is not clearly worse than the situation in FOL, our usual logic.

I think this challenge can be answered, but the answer suggests a subjective element in the choice of FOL. Such an element is already present, actually, in the Cartesian condition, since there is no knock-down reply to someone who doesn't mind reversing judgments of logical correctness. Although such reversals may seem clearly unfortunate, this is to some extent a matter of epistemic taste. And taste plays a similar role in the following argument for a full effectiveness requirement.

Church's theorem rules out a decision procedure for logical implication in FOL. But we can have something weaker that is still of interest and has traditionally been taken for granted: an effective way to tell whether Q follows from *P by an immediate step.* Views connecting logic to inference and argument give the consequence relation more structure than we have so far allowed. They recognize a decidable collection of immediate inferences (e.g., universal instantiation, modus ponens, conjunction introduction) such that whenever $R(Q,P)$, there is a chain $Q, Q_1, Q_2, \ldots, Q_n, P$ in which each link follows immediately from its predecessor. This condition, together with the decidability of immediate consequence, restricts us to complete logics; but its motivation is not at once evident. What underlies the belief in smallest logical steps, and why should it be decidable whether two sentences can be linked by such a step? The best answer depends on a further assumption from the logical tradition: that immediate steps—all instances of any immediate step-type—have the epistemic property of being self-evidently, transparently correct, as directly obvious as anything can be. A logic with a consequence relation based on immediate steps then offers an epistemic gain. Once we have a proof of Q from P in such a logic, we know that the argument for Q need not and cannot be made more persuasive or clear. (Given the truth of P, of course. We might want a proof of Q from something less controversial than P, but again this kind of consideration is not an issue for logic. Also, we might still want a shorter or simpler proof from the same premise. But we are here abstracting from pragmatic problems about length, surveyability, and the like, since we are considering the situation of an ideal prover.) Again taking a Cartesian viewpoint, this is an advantage, for it shows another way in which proofs will not be superseded. And clearly, this advantage has not just been conjectured *ad hoc* to back up the RCL, but is entirely consonant with traditional views. A fully explicit logical proof is the paradigm of an unimpeachable, unimprovable argument from its premises to its conclusion.[10]

But if this clarifies the advantage of reducing consequence to chains of immediate steps, it does not yet show why immediate inferability should be decidable. Again, we might hope to manage with just the positive half of decidability; with the ability to chain together immediate steps identified with certainty as such. Although we would lack a procedure for showing that a step is not immediate, we could simply eschew the use of steps whose status was still open. (Given the elementary character of immediate steps, this is hard to imagine, so that we might well dismiss this possibility out of hand. But it will be instructive to pursue the matter.) Note, however, that this idea would reduce the significance of the distinction between a decidable immediate consequence relation and a merely enumerable one. At any time, the collection of immediate steps *we could use* would be decidable, since it would consist of the steps enumerated up to that point. So we have no argument against always working with a decidable notion of proof based on immediate steps; the difference would lie in the possibility of extending our notion over time. Now nothing in the RCL as such counts against this. It is of course opposed to the classical view of logic as being fixed, and given all at once. But that view does not follow from the connection between logic and reasoning, or even from rationalism. We could thus regard the use of FOL, our complete logic, as a stage, perhaps to be superseded by a later complete logic incorporating as yet unrecognized immediate steps. Our arguments above for identifying logic with FOL would continue to hold under a relativization to the present time.

The defender of the RCL and of our claims for FOL should be ready to concede this possibility. Yet it is hard to grasp, since no plausible evolution out of FOL suggests itself. Also, our considerations about fundamentality and Lindstrom's theorem still stand. We have no clue to the construction of a complete logic stronger than FOL that does not sacrifice desirable features. Part of the problem is that our concept of decidability seems to be absolute, even if logical consequence should turn out not to be. We can try to imagine a system *S* that, while being logic, extends FOL, but the change in logic would not change the class of effective procedures. Thus, if *S* is incomplete from our present perspective, it would remain so from the later one. Similarly, the new logic would not affect our notions of infinity and countability, which are independent of logic. To whatever extent our reasoning about extensions of FOL was plausible before, it should continue to have force. (Since we are considering only extensions that leave earlier inferences in place, we need not worry about the overthrow of this reasoning itself on logical grounds.) If it now appears that complete extensions of FOL must involve notions definitionally dependent on others that lead to an incomplete logic, then the change in logic will not alter this. So the burden of proof is heavily on those who would claim that FOL could be superseded by other logics with its epistemic properties.

This may indicate the epistemic significance of FOL. We will shortly turn to alternative approaches to characterizing logic. First, however, some remarks on a challenge to the association of the RCL with Frege.

Dummett has in effect asked whether the idea of logic developed here is so much Frege's as Gentzen's. Gentzen made logical inference the focus of his investigation, while Frege was in the first instance concerned with the extent of logical truth. According to Dummett, this points up major differences between Frege's and Gentzen's conceptions of logic, with the latter's being far superior ([4], pp. 432 ff.). Dummett might then add that my emphasis, in presenting the RCL, on the relation of logical consequence makes my approach close to Gentzen's (and that of pre-Fregean writers). Although this is strictly irrelevant to the evaluation of the RCL, it merits a few remarks on Frege's behalf.[11]

(i) Dummett contrasts the emphasis on logical truth with the emphasis on logical consequence. But this difference may reflect nothing more than Frege's and Gentzen's divergent foundational aims. A logicist, whose thesis is that mathematical truths are logical, will naturally give the generation of logical truths center stage. The central question within Hilbert's programme, on the other hand, was consistency: given a system, is a contradiction derivable in it? It was, then, natural for Gentzen to frame his project as a study of derivability. But neither approach has the consequence Dummett alleges. Frege's is not committed to the idea that logic is more concerned with truth than with consequence, and nothing in Gentzen entails the opposite conclusion.

(ii) Axiomatic and Gentzen-type formalizations correspond in obvious ways. The latter yield a class of logical truths, the sentences derivable from null hypotheses, while axioms can straightforwardly be converted into rules. In spite of this, Dummett claims that Frege's choice of formalization was pernicious. His reason seems to be that it led to a preoccupation with logical truth as a special kind of truth, and to related preoccupations with analytic truth, necessary truth, and their opposites. But—setting aside the correctness of Dummett's historical story and critique of the history—recognizing axioms of logic carries no commitment at all to any particular view of logical truth. Of course, unless 'logic' is simply an arbitrary designation, we must say something about what makes a given truth logical. But essentially the same problem will arise in Gentzen's framework, as a question about the status of the rules and of the logical theorems they generate. In any case, axiomatic formulations say nothing about whether logical truths are analytic or necessary. Frege thought they were both, and the same view harmonizes as well with Gentzen's logical theory as with Frege's. (Consider, on the question of analyticity, the common idea that Gentzen-style rules give the meanings of the connectives.) But this is simply another matter. Even less is there any implication, in Frege's approach, that logical truth differs from other kinds of truth in any more fundamental way—as one might hold if one believed that truth was correspondence for nonlogical truths, but essentially conventional or linguistic in logic. Many philosophers after Frege did believe that, but he is blameless in this, and his use of axioms particularly so.

(iii) In any case, the author of [7], [8], and [9] was plainly interested in proof above all. This concern is inseparable from an interest in logical consequence. Thus, even if Dummett is somehow right in stressing the primacy of consequence over truth in logic, no criticism of Frege on this score is appropriate.

Quite possibly, the RCL is more readily formulated in the light of post-Fregean logical developments—a body of work in which Gentzen's writings are prominent. But its main features can still be found in Frege.

To argue that an epistemic conception selects FOL as logic is not, of course, to make an absolute case for FOL. I turn to other viewpoints.

3 Two routes to a nontraditional characterization of logic have been developed. The first is to look for technical criteria that pick out a certain logic or class of logics in some natural way. For example, Quine has proclaimed FOL a "solid and significant unity" for satisfying a remarkable group of metatheorems ([20]; [23], ch. 6). But while Quine is clearly right in this, not everyone reads the technical data the same way. Tharp's discussion is inconclusive (FOL seems to win, but not decisively) and Hacking argues that natural criteria, framed in terms of the sequent calculus, pick out an extension that includes ramified type logic. Boolos finds a respectable case for drawing the line no lower than SOL. We will look at some of these issues below. For the moment, we note that the technical strategy at least tends to yield an identification of logic with FOL, so that there is a rough extensional agreement with the traditional view. Not so for the second main strategy. This alternative, best presented in [25], makes our choice of logic turn on adequacy of expressive power. (Some of [2] also invites this reading, and the idea is well-known among logicians.) The basic fact is that first-order axioms fail to characterize most mathematical notions, while second-order ones suffice. This is usually illustrated by the contrast between first- and second-order arithmetic: only the latter determinately expresses our concept of the number sequence. An important further example, on which Shapiro rightly dwells, is *set*. Although it is not quite true, at least without further restrictions, that second-order set theory is categorical, the second-order axioms improve by ruling out nonstandard models, leaving open only the height of the universe. Shapiro therefore concludes that mathematicians need second-order principles to express their theories. The first-order formulations are mere surrogates, the interest of which is parasitic on the second-order cases. Thus, Shapiro argues that the logic of mathematical practice should be (at least) second order.

Simply looking for technical differences among various logics is a strictly mathematical project. Moreover, technical features alone could not confer a distinguished status, for familiar, trivial reasons. Any logic will have numerous distinguishing features. A good many logics will have enough of them to count as "solid and significant unities". Unless one is content simply to classify a range of systems, one needs some prior selection of important criteria. Yet no such selection could be made on purely technical grounds, since the importance or interest of technical features is not itself a purely technical question. And in fact, the criteria at issue usually seem to be interesting in virtue of a connection to cognition. To review some examples, this is clear for completeness, with its relevance to determining validity, and compactness, which helps to guarantee the existence of finite proofs. Tharp's continuity condition in [27] suggests that the first-order quantifiers are conceptually elementary, minimal extensions of non-quantificational operators. Hacking [15] tries to ground the demarcation of logic in a view of how we come to understand certain operators. And although the epistemic significance of the L-S property is problematic, it does seem to be connected to the relatively finitary character of our logic, which is presumably important because our minds are finite. All this is obvious and may lead to the suspicion that the technical strategy is a straw man. Yet a number of remarks in the literature seem to suggest it.

Suppose, however, that a purely technical approach to characterizing logic is not really being pursued; that the real issue is whether any logic has conspicuously many features of epistemic interest. This could be motivated in two ways.

One might, first, deny that logic is epistemically significant, yet hold that traditional philosophers of logic did succeed in picking out *something*. One would then argue that what we call logic has interesting features that stand as technical analogues to epistemic properties, in the way that recursiveness is close to psychological notions of computation or calculation. Thus, one would see earlier philosophers as being on to something but misdescribing it—as mistaking a technically important body of doctrine for an epistemically important one. The analogies between the mathematical concepts and their epistemic-psychological counterparts would explain this confusion. The characterization of logic would, then, still be a purely mathematical project, but one with interesting extramathematical roots.

One can find this viewpoint in Quine. Quine denies that logic is a priori or necessary, that it provides a foundation of knowledge, that it is in any sense a theory of inference, and so forth. Very little remains of the assumptions behind the RCL and other traditional views. But still, FOL is given a special place for its completeness and compactness, for the coincidence of various intuitive definitions of logical truth in its case, and for confining itself to relatively elementary concepts—all features related to presumably outdated reasons for selecting FOL.

As far as our *present* goals are concerned, this is no different from the purely technical approach. Defining logic serves no philosophical purpose. If one does not take this line, then one seems to be forced back to an epistemic characterization of logic, which then needs to be spelled out and justified. One must explain the cognitive significance of the properties of logic and connect them to a general view of the place of logic. To do less is to maintain an untenable intermediate position: to hold that one's interest is more than purely technical, yet decline to make sense of it. I think that in the end Quine avoids this trap. For in spite of his official relation to the logical tradition, important elements in his philosophy depend on a more philosophically committal view of logic. I will explain this with reference to his views on existence and quantification in [20] and [23].

4 Quine holds that to discern what a person P believes in—her ontology—we must first represent her beliefs in FOL. By way of illustration, he considers someone advancing a theory in Henkin's branched-quantifier logic (HL).[12] If this formulation uses only first-order quantifiers, the theorist may appear to believe only in the objects in their intended domain (whatever it might be), but for Quine this is an illusion. Since the equivalent formulation of her theory using linear quantifiers will quantify over functions on that domain, he holds that such functions likewise belong to her ontology. Quine bases this view on the remarkable metatheorems for FOL: the solid and significant unity of FOL recommends its choice as a measure of ontology. Quine adds, however, that should there be more or less reasonable alternatives, "it seems clearest and simplest to say that deviant concepts of existence exist along with them" ([20], p. 113).

For familiar reasons, Quine should not be able to speak of P's having definite theoretical beliefs at all. His writings on translation imply, roughly, that any two representations of P's theory alike in their observational consequences will be equally correct [22]; and it seems clear that wide variation in what would intuitively count as content is compatible with this kind of empirical equivalence. Thus, there should be no sense in asking what P believes in, except relative to an arbitrary (in principle) translation scheme. This is also a direct consequence of the thesis of ontological relativity. But just because these doctrines do away with the whole idea of a persons's ontology, it is pointless to consider them here. We are concerned with a thesis in the philosophy of logic that presupposes the sensicality and value of ontological inquires. Although elements in Quine's philosophy contradict this presupposition, the thesis has independent interest and deserves to be evaluated on its own terms. Further, such an evaluation seems to be required in order to do justice to the parts of Quine's thought that assume the significance of ontological questions, notably his insistence on parsimony and his attempts to simplify our ontology via reductions. So we may consider these parts in partial isolation from their Quinian context.

These views certainly raise difficulties. It is, for example, hardly clear why users of HL would have a deviant concept of existence if HL were a "reasonable alternative" to FOL. A deviant logic, perhaps, but why a deviant concept of existence? And granting that for a moment, it is also puzzling why the *reasonableness* of HL should matter. If the use of HL involves a deviant concept of existence, that should be so even if preferring HL is somehow unreasonable. In order to sort these problems out, it is best to begin with Quine's underlying motivation.

I assume that Quine's interest in ontological improvement is basic here: in getting empirically adequate theories with the smallest, clearest ontologies. A dependence of ontology on logic would threaten this Occamite project. If one could avoid belief in a class of objects not by refining one's theory, but simply by changing logics, then moving to another logic would seem to offer the advantages of theft over honest toil. Consider, for example, a theorist who has posited a certain domain of objects plus functions on that domain. If she suspects that the functions may be unnecessary, she ought to look for a clever way to explain the same data without them. If she can instead simply switch to HL and claim to have reduced her commitments in that way, then the chance for genuine insight has been lost and ontological reform threatens to become irrelevant.[13] To be precise, we may distinguish two dangers:

(1) the possibility of avoiding a commitment by changing logics, as just described.

(2) a relativization of ontology to logic. We would say that our ontology is indeterminate absolutely; that it may contain *F*s relative to logic *L* but no *F*s (although perhaps *G*s instead) relative to *L*′. (Of course, this is close to what Quine does embrace in [19], but we are setting that aside.) On this view, one could still try to be parsimonious within an arbitrarily chosen logic, but the interest of that would hardly be clear.

I see two possible escapes from these problems. One would posit an association between logics and criteria of ontology that makes ontology invariant. Thus, if the criterion (QC) identifying the ontology with the intended domain of quantification were correct for theories formalized in FOL, while the criterion (HC) that adds functions on that domain were to apply to theories in HL, then changing from one logic to the other would leave ontology the same. This would block (1) and (2) and help to ensure that ontological progress could come only though meaningful improvements in theory. But this idea faces difficulties. Once we raise the question of how criteria for ontology are paired with logics, it is mysterious why the answer should come out in Quine's favor. Of course it seems intuitively right to rule out ontological reforms accomplished by changing one's logic, but this appeal to intuition begs the question against (1) and (2).

And even assuming invariance, why should QC go with FOL instead of, say, HL? Quine dwells on the attractive formal properties of FOL, but they look completely irrelevant here. FOL can be as solid and significant as you please, but that does not show why ontology should be read off of the quantifiers in this logic rather than in some other one. Also, the idea of a multiplicity of criteria leaves out of account Quine's insistence on the triviality of QC. He rightly regards the (objectual) existential quantifier as no more than the formal device for asserting existence, the logician's translation of 'there are . . . '. QC would then seem to be the only reasonable criterion. Trivially, what we believe there is is what we believe there is, so no matter what our logic, the intended range of our quantifiers should reveal our ontology. But then the suggested escape from (1) and (2) is blocked.

If this last point is taken seriously, then Quine needs to show that our logic is in fact FOL and that we cannot change it.[14] Although this is a strong thesis, it is not the absurd denial of transcriptions from one logic to another. One can, for example, obviously go back and forth between statements of a theory with linear and with branching quantifiers. But the thesis is that these transcriptions, while constituting different representations of various bodies of doctrine, do not change a speaker's underlying logic: that they occur within a fixed background logic. Suppose we try to axiomatize all of our theories in HL. That is, we present each theory T as a set A of HL-axioms plus the statement that its truths are the consequences of A. The Quinian should claim that in spite of this apparent commitment to HL, our logic is still FOL because our use of A is really embedded in a broader first-order theory that includes a discussion of A. The illusion of nonelementary discourse will vanish once this background is made explicit.

Quine's remarks on solidity and significance may be intended to explain our hypothesized adherence to FOL by showing why we would never give up this logic. His comment on reasonable alternatives at the end of [20] would then concede that differences over logic might make certain ontological disputes rationally intractable. For if our disagreement with P can be traced to her use of HL instead of FOL, and if there is no strong argument for preferring one logic over the other, then we must leave our dispute unresolved – which may be what the (obscure) attribution of a "deviant concept of existence" is supposed to mean. The trouble with this perspective is that the appeal to solidity and significance is far too weak, since many logics will be solid and significant unities in one way or another. Further, viewing our logic as a matter of choice opens the door to the relativity we want to avoid; it is hard to see how one could prove that nothing besides FOL could ever reasonably be chosen. Quine concedes this result with equanimity (maybe because he wants to affirm ontological relativity on other grounds anyway), but we can hardly follow him. So Quine seems in the end to have no satisfactory account of the status of FOL and its relation to ontology.

Quine would fare better with a Fregean defense. If we regard logic as the basis of idealized argument, then what counts as logic will depend on our ideal practices. The relevant idealizations are not arbitrary. Although they abstract from our usual memory limitations and the existence of competing demands on our time, they leave intact such deeper features as the finiteness of our minds. They also assume no changes in what might be called our fundamental capacity for logical insight. Creatures who can simply see whether a formula of FOL is valid, or to whom the truth value of CH is immediately clear, would presumably have a different logic; but since these powers are not simply a matter of having more time or memory, we do not attribute them to our idealized selves. So the idealization remains tied to the most general facts about our mental powers. Because we cannot even imagine how to alter these, a Fregean viewpoint makes changes of logic impossible in a strong sense. And if we can argue that, given these facts, our logic must be FOL, then we have just the result that Quine wants. The inescapability of FOL, plus the trivial correctness of QC, ensures a uniform standard of ontology.

By way of illustration, consider again the attempt to convert all our theories to HL. Let T be given via some axiomatization A. We may imagine a theorist P who, trying to avoid FOL, applies some (incomplete) set of HL-rules to A in order to find further elements of T. Our claim, however, is that everything she gets in this way could equally be obtained by first-order arguments from premises she necessarily accepts. Instead of applying HL-rules to A, she can reason from the statement that the elements of T are the sentences true in every HL-model of A. Since this is a first-order statement, and since the entire model theory of T can be given in a first-order language, there is no need for P to go beyond FOL in order to develop T. She can simply use first-order model theory and set theory to discover what besides A holds in every model of T. Moreover, the incompleteness of HL forces her to view T model-theoretically; incompleteness just means the absence of any substitute for the model-theoretic notion of consequence. So the introduction of rules in HL gains nothing. And since P must have a model theory for T, no possibility of ontological gain can be connected with the use of A. The model theory will reintroduce the functions that seemed to vanish with the change to HL.[15]

But why be certain that the first-order approach will yield everything that P could get by working in HL? The alternative would be to posit a set of rules R for HL and a sentence S such that

(i) P can prove S from A, using R.
(ii) P cannot prove that S is true in all models of A within her (first-order) model theory.

Note that (ii) plus the semantic counterpart to (i), the claim that A semantically entails S, is reasonable. Many semantic consequences of A can presumably be derived neither within our present model theory, nor from any we are ever likely to be justified in believing. But if (i) and (ii) hold, then P finds evident principles about HL that do not follow from her statable principles about models of HL. This would seem to amount to a kind of direct insight into consequence in HL. Now direct logical insight is not objectionable in principle. We must claim it ourselves for elementary logic. But we seem to lack it for HL, and that cannot be remedied by more computing time or any imaginable sharpening of our wits. If P is like us, then (i) and (ii) must be jointly impossible for her, too. She can therefore not—on the view I an taking—claim HL as her logic. Whatever fragments of HL she might use are reducible to, and derivable from, first-order statements and rules.

Quine might dislike this defense, with its involvement in psychology and traditional epistemology. But since his alternative seems to fail, we may conjecture that FOL is special just insofar as we connect the nature of logic to questions about human reasoning and ideal systems of knowledge. For Quine as for Frege, this is what the issue must come down to, in spite of the differences in their assumptions and interests. Of course, much of Quine's motivation stems from a distrust of traditional views of logic, so that we can hardly dismiss his approach without a closer look at what a Fregean might be committed to. That is the business of section 6. First, let us take up the case for SOL sketched in section 3.

5 Boolos has convincingly rebutted various common objections to SOL [2]. I also think that the attempt to dismiss SOL (as logic) because of its existence assumptions (e.g., [6]) fails. Although logic can clearly not demonstrate the existence of any spatiotemporal individual, certain abstract objects might well be said to exist on logical grounds. If we recognize abstract things at all, at least some of them will appear to exist necessarily. Among the necessary existents, some will arguably exist as a matter of logic alone. Perhaps this is so for all properties, if their existence does not require their instantiation, or perhaps only for certain ones (e.g., "logically necessary" properties like thinking-or-not-thinking). I am not asserting anything of this sort (see instead [1]). But since such claims cannot be dismissed on intuitive grounds, the ontology of SOL does not seem to be a serious drawback.[16]

Since the positive reasons for preferring SOL are widely known and have been excellently detailed in [25], we may be brief. They are also epistemic, but instead of reflecting a concern with reasoning and argument, they focus on the problem of characterizing mathematical concepts. Consider the two versions of the induction axiom:

(1) $F(0) \,\&\, (x)(F(x) \rightarrow F(x+1)) \rightarrow (x)F(x)$
(2) $(AF)(F(0) \,\&\, (x)(F(x) \rightarrow F(x+1)) \rightarrow (x)F(x))$.

(1) is a schema. To obtain its instances, we replace '*F*' by appropriate open sentences of first-order number theory. In (2), the variable '*F*' ranges over all subsets of the domain of natural numbers. (Instead of (1) we could use an orthographic copy of (2) in which the main quantifier is restricted to first-order definable subsets of this domain.) The widespread use of (1) in logic is due to the technical fact that the well-developed, relatively tractable theory of first-order models applies to number theories employing (1). It seems, however, that any discussion of number must depend on a grasp of the notion defined by (2). (2) describes the numbers up to isomorphism, whereas (1) allows models with "nonstandard" numbers, i.e., elements that aren't numbers at all. Further, as Shapiro emphasizes, (1) suffers from language-relativity. Since it is schematic, any change in our language will yield a new axiom. Thus there is really a multiplicity of first-order induction schemas corresponding to the variously expressive number-theoretic languages we might use. Since we seem to have a single number concept, and since it seems arbitrary to select any one of these languages as being primary or basic, is mysterious how our understanding of number could reside in any form of (1). What links the different schemas is rather that they are all restrictions of (2). So (2), besides being categorical, is in an important way absolute.

Similar remarks apply to the axiomatizations of real number and set. One might of course hold that the concepts they define are not genuinely absolute, since they still depend on a notion of subset (or set) that may itself be relative. Some mathematicians have thought that a model of a theory given one construal of 'subset', hence of the second-order quantifier, may fail on another, equally legitimate, interpretation. But most foundational workers find the notion of subset determinate. And the second-order formulations retain an advantage in any case. If the notion of set is indeterminate, we may simply have to live with a fundamental ambiguity in our thinking, one infecting our concepts in all mathematical domains. The relativity that comes with the first-order axiom schemas, however, is additional, since it derives from the need to make an arbitrary choice of language even after we have settled on our notion of set.

These observations certainly suggest that second-order formulations of (many) mathematical theories are basic. From this we obtain an argument for SOL. A second-order language and consequence relation must apparently be used to set out principal mathematical concepts and their associated theories. The truths of arithmetic, for example, must be described as the set of all consequences of a certain second-order sentence *PA*; similarly for analysis and set theory. So mathematical discourse seems to be essentially second order. First-order theories of numbers or sets are interesting objects of study, but they only partially formalize our mathematical beliefs. This view forms a counterpart to the one proposed on Quine's behalf at the end of section 4: it is a way of saying that we think in SOL, whatever notations we may use.

This defense of SOL does not deal with arguments, thus differing from the defense of FOL given in section 2. This omission is not obviously a weakness. Although Shapiro says nothing about how to discover second-order consequences, nothing in his view prevents such discoveries. Since we know many truths of arithmetic, we can indeed establish many consequences of *PA*. We lack a mechanical procedure for generating all of them, but the defender of FOL cannot offer such a procedure either, so the two positions seem to be on a par. The difference lies in the relation between derivation and logical consequence. For Shapiro, of course, there is no method for deriving all the logical consequences of any given sentence. Thus, there is no guarantee that any logical consequence of *S* can be obtained from *S* by finitely many obvious, elementary steps. But Shapiro can deny that logical implication must have this feature. A reply to him cannot simply beg the question in favor of the RCL. As things stand, Shapiro will not even allow that there are two reasonable notions of logic, one defined by his criteria, the other inferential. He has, as just noted, an argument to the effect that our discourse is fundamentally second order. From this he infers that the logical consequences of our sentences are their second-order consequences, so that there is no place for the RCL to get a foothold. Thus:

> I suggest that the considerations of the present article preempt such reasoning [i.e., the arguments for a complete or compact logic]... all things equal, it would be desirable to have a recursively axiomatized, compact logic with fewer presuppositions... however, the purpose of logic is to study and codify correct inference. Since one cannot codify the correct inferences of a second-order language with a first-order logic, it follows that the logic of mathematics cannot be first-order.[17]

Shapiro's objections to the usual first-order axiomatizations are convincing. The argument for SOL, however, has a large hole that I believe he cannot close. (2) need not be read as a second-order statement. One can equally well take it to be part of a first-order language allowing quantification over sets. Looking at the English that (2) formalizes—'every set of numbers that contains 0 and is closed under successor contains all numbers'—makes it clear that the question whether (2) is first or second order is unreal. Inspection of this statement in isolation from its linguistic background tells us nothing. (Compare the situation in section 4, where the appearance of working in HL was removed when we considered the background theory.) For all that has been said so far, it appears possible to view Shapiro's representations not as supporting SOL, but as showing that mathematics must be presented in first-order theories with set quantifiers. And proponents of the RCL can then add that since the gains Shapiro wants can be obtained with first-order theories, the supposedly preempted arguments for FOL come back into play.

A Quinian scenario may clarify the situation. Suppose that we land on an island and hear a native N uttering (2) by way of characterizing number. What is her logic? If (2) characterizes number, we know that '(F)' must be a set quantifier, but that does not settle the question. Yet Shapiro's argument boils down to the fact that such a quantifier is needed to block nonstandard models. If that leaves the question of logic unanswered, then Shapiro's reasoning cannot bear on it, and we may take into account other facts about N, such as her inferential practices. If it seems that FOL adequately codifies her inferences, that is evidence that N is presenting her mathematics in a first-order framework. And as usual, considerations about the foreign native reflect on us. Shapiro claims that SOL is needed to formalize our mathematical discourse; but a first-order representation seems to be equally good from his viewpoint and better overall.

Shapiro replies that if we advocate the first-order reading, we cannot take the concept of set (or subset) to given by any formalization. First-order set theory admits ill-founded models, which adequate conceptions of set presumably rule out. So we must hold that when we present mathematics in FOL, set is a concept fixed and understood (in a "standard" way) in advance, to one to be elucidated. But this is no reply in itself, because Shapiro presupposes the same thing. There is, for him, no way to explain or elucidate the meaning of the second-order quantifiers. If second-order presentations of mathematical theories are basic, then these devices must simply be clear from the start—and they involve the concept of set.[18] Hence Shapiro tries to argue that despite these evident parallels, the presuppositions of the second-order approach are smaller and more elementary. He observes that a second-order axiomatization presupposes only subsets of (or functions and relations on) a given domain (the domain of numbers, for example, in the case of second-order arithmetic). In contrast, Shapiro claims, the first-order approach with standard set variables presupposes a grasp of a set theoretic hierarchy. When we use such an axiom system, we must be working within a background language L that includes a theory of sets. L will therefore contain a power set operator and will hence carry commitment to the power set of any set it discusses, the power set of that set, and so on. This is far more than one needs for any branch of analysis (assuming one starts with at least the domain of numbers). If L has the resources for transfinite iterations of the power set operation, as it very well may, so much the worse. Shapiro concludes that instead of a single layer of sets on top of a presumably well understood domain, the first-order approach introduces a large iterative universe.

This reply is unfair. The difference between the first- and second-order readings of (2) concerns the logic of the background language. (2) itself has exactly the same conceptual and ontological presuppositions against either background. In each case, we must grasp and believe in the ("absolute") power set of the natural numbers. If understanding set requires belief in power sets of sets one believes in, then the second-order theorist is committed to as many sets as her first-order rival. If set can be understood prior to any strong theory of sets, then the first-order theorist can equally well get by with minimal commitments. In short, both theorists presuppose a standard notion of set, whatever that may involve. There is absolutely no difference.

Having rejected Shapiro's position, we may ask what underlies it: what considerations, not necessarily operative for any particular recent defender of SOL, might make it seem natural? I will suggest an answer that leads us back to Frege.

For a Hilbert-style formalist, axiom systems are self-contained presentations of mathematical concepts. Understanding only logic and syntactical notions in advance, one should on this view be able to grasp set or number via an appropriate axiomatization; logic and axioms suffice to fix any mathematical subject matter. Incompleteness shows this to be impossible if the logic is first order. But if one holds to this ideal anyway, uncritically and perhaps unconsciously, one is led to identify SOL with logic.[19] Another formalist idea is also relevant here: that the process of understanding mathematical concepts should follow the order of the strength or complexity of the corresponding theories (e.g., that *counting number* (arithmetic) should be prior to *real number* (analysis), and this in turn prior to *set* (set theory)). From this it follows that one shouldn't need a standard notion of set in order to understand number. My first-order approach violates that. Of course, the second-order treatment of number theory likewise violates it, by presupposing a standard grasp of the set quantifiers. But if one has decided that the second-order quantifiers are logical, and also that logical concepts are admissible as foundations for our grasp of number, then one may find the second-order way superior. This would be confused, since one preserves one's view of the order of mathematical concepts only by building the principal concepts at issue into logic. Yet a confusion that is obvious enough when exposed can shape our views as long as it remains implicit.

The truth behind this confusion is that arithmetic is more elementary than set theory, conceptually as well as proof-theoretically. The concepts of arithmetic are simpler in any reasonable sense, and various basic principles about sets have turned out be far more debatable that any axiom of arithmetic. We may, however, still need to know something about sets to have any concept of number. Probably nothing of the principles that give set theory most of its power: replacement, choice, and strong power set and abstraction axioms. But arithmetic may be inseparable from thinking about relations between collections. Then it is perfectly natural to suppose that number might need a set-theoretic definition.

The connection between arithmetical and set-theoretic thinking was of course a main logicist insight. The "bottom up" tendency, on the other hand, has roots in an empiricist tradition opposed to the rationalistic and Kantian elements in logicism. Empiricists have been concerned to minimize assumptions and to show how simpler concepts or systems of thought can be built up without presupposing anything from more advanced levels.[20] We can now see that this difference also bears on the philosophy of logic. Where Frege's own views lead, as I have argued, to an identification of logic with FOL, the main current arguments for choosing SOL seem to rest on formalist and empiricist ideas. The fact that these arguments turn out to have no force would not have surprised Frege and can be seen as supporting his own conception of logic.

My response to Shapiro exploits a shared assumption about logic and reasoning. Without his agreement that "the purpose of logic is to study and codify correct inference," I could not have supported the first-order reading of (2) against him. Indeed, the quickest way to make a case for SOL is to assert the irrelevance of proof to logical consequence. If one starts from a semantic viewpoint, defining validity as truth in all models and admitting no reason why implications should be humanly computable, then FOL will look arbitrary. The flat rejection of an epistemic view of logic preempts counterarguments based on the epistemic advantages of FOL. Should Shapiro have taken this line? To my mind, his actual procedure indicates the strong conceptual tie between logic and inference. It would be easy to promote SOL by jettisoning epistemic conditions. Instead, Shapiro tries to show that his conception of logic satisfies them as well as one could hope. They appear hard to ignore. Perhaps the moral is that if one respects them at all, one cannot avoid choosing FOL; any concession to an inferential view blocks the move to a stronger logic.

Resisting all concessions, I think, simply yields a different conception of logic. I have nothing against that, but wonder whether the designation 'logic' still fits. The pull of the inferential view is universal: virtually every significant position in the philosophy of logic admits some connection between logic and proof. The purely model-theoretic alternative therefore has no serious claim to being part of *our* conception of logic.

We have outlined a Fregean view of logic and argued that it singles out FOL. Further, we have rejected Quine's way of singling out FOL, and the support for SOL has dissolved. But we need to reexamine our rationalist assumptions. The relation of FOL to inference is, as we shall see, open to attacks of a sort quite different from those considered so far.

6 Frege's view of logic clashes with the naturalism that now dominates epistemology. (Actually, the dominant perspective combines elements from naturalism, empiricism, and pragmatism. But I will emphasize the naturalism here.) One form of naturalism, found both in [21] and among Frege's contemporaries, rejects normative epistemology in favor of purely descriptive studies of cognition. Frege rightly saw this as a threat to the foundation of his enterprise: logic cannot be part of the theory of ideal justification if we are not supposed to talk about justification at all. But this challenge, important as it is, must fail. Normative thinking seems to be ineliminable, and [21] (the best source) offers no real argument against it in epistemology (cf. [18]). Less extreme naturalistic ideas, however, may lead to more plausible challenges. I will consider one due largely to Goldman [12],[13] and variously known as the "externalist" or "reliabilist" view of justification. Although I cannot fully describe this position here, some main points can fairly readily be brought to bear on the RCL.

The externalist takes justified beliefs to be those formed by reliable processes—processes likely to yield true beliefs. Notoriously, this statement alone is unclear and cannot distinguish externalism from the traditional views it is intended to oppose. There are problems about the identification and individuation of processes; and Descartes, too, thought that good reasoning should generally lead to the truth. In spite of such difficulties, a distinctive feature emerges when we ask how likelihood—the likelihood that a process will lead to the truth—is to be assessed. A Cartesian canon of method would ideally consist of ways to find the truth under all circumstances. Descartes recognized this to be impossible and needed a benevolent God to restrict the possible relations between thought and reality. Nonetheless, he aimed for methods of scientific discovery that would work in all circumstances short of the global forms of deception represented by madness, the Evil Demon, and the like. In contrast, externalism strongly ties reliability to the context of a particular inquirer (or maybe a community or species of inquirers). Someone's methods are taken to be reliable, hence justification-conferring, if they lead her and persons rather like her to the truth in her circumstances and rather similar ones (cf. Goldman's famous example of the barns in [11]). Of course this is still vague, but it definitely departs from the Cartesian viewpoint. The idea of a universal canon of method is replaced by the search for guidelines sensitive to the character of their user and context of use. This brings out the naturalism in Goldman's approach. If he is right, we need knowledge of our epistemic situation to find the correct theory of justification for us. Epistemology must therefore go hand in hand with cognitive psychology, the study of error in measurement and sampling, the sociology of science, perhaps even politics and economics. All of these contribute to the description of ourselves as knowers and the assessment of various epistemic policies. If this does not make epistemology itself another empirical science (e.g., because it remains normative), it certainly suggests that any a priori epistemology would have to be a sterile, limited discipline. In any case, it radically revises traditional views of the nature and point of epistemology.

These remarks focus on discovery and the processes by which beliefs are formed. The problems about justification commonly addressed by Goldman and others lie in that area. But the same ideas apply to justification as it concerned Frege: the providing of convincing, illuminating arguments. From an externalist viewpoint, a good method of argument would presumably tend to produce belief in truths, assuming that true premises were used. Arguments conducted by this method should indeed have true conclusions when they convince, while ones leading from truths to falsehoods should readily be seen to be fallacious. No doubt the externalist would add further conditions pertaining to the role of arguments in producing insight. For example, good arguments should be surveyable and should bring out relevant relations among the propositions appearing in them. An efficiency requirement will also be important: fallible methods tending to produce insight quickly on important questions might be better than more accurate but cumbersome alternatives. This is in line with the idea that our methods should be the best for us, relative to our epistemic abilities and goals, in worlds like our own.

It is this relativity, in particular, that casts doubt both on the RCL and on the status of FOL. Arguments in FOL seem largely irrelevant to our ordinary practices of justification. The notation of FOL is almost useless even for the conduct of mathematics, and the standards of rigor set in [7] are impossibly burdensome. Here it is not enough to reply that FOL is supposed to be a source of *ideal* patterns of justification, patterns optimal only insofar as we abstract from limits on memory, attention, and available time. To be sure, naturalism will permit us our interest in ideals, both in descriptive science and in normative theory. But it may change our ideals. The ideal of arguments conducted in FOL does seem to be associated with Cartesian inquiry. Descartes self-consciously set aside the pragmatic reasons for limiting the time and care spent on any piece of reasoning, and he was willing to pay a high price in efficiency. The recognition of many truths could be postponed a long time to avoid any false belief. Now the externalist will not only regard this as an unattainable goal, but will dispute it as an ideal. Ideals function as standards: we remain under practical constraints of course, but the closer we can get, the better. To an externalist, concerned to recognize our contingent limits and needs, the Cartesian ideal will appear irrelevant. It is perhaps appropriate for ideal cognizers, who have unlimited memory and attention and can take their time when necessary. They, too, will not always be able to carry out proofs according to the standards of FOL, since they also have practical concerns, but they can at least attain the ideal when circumstances warrant it. We, on the other hand, are quickly exhausted by the demands of rigorous proof and can seldom escape the pressures to increase efficiency, hence fallibility. As compensation for this, we can tolerate considerable error, particularly in theoretical areas where proof matters. These contrasts between ideal cognizers and ourselves suggest that only the former should be concerned with the *Begriffsschrift's* standard of proof.

Naturally, we are free to have multiple ideals, and to be guided by different ones at different times. Arguments in FOL, or close approximations to them, clearly do have a place. But the externalist can still ask why FOL should provide more than one (possibly not very important) ideal among many.

These externalist objections may appear overly concerned with the cumbersome notation of FOL, thus still allowing first-order consequence a special status. But we singled out first-order consequence just by defining it in terms of chains of elementary inferential steps. If their interest is called into question, then so is the relevance of FOL. It is not obvious what an externalist view of logic and logical consequence would be, but, offhand, I do not see that externalists must be particularly interested in first-order implication. If empirical research were to demonstrate a significant advantage to, say, intuitionistic consequence—which is readily conceivable—then intuitionistic logic might be preferred.[21] "Logic" might even turn out to be something quite unlike any of the systems logicians now study. We have seen something of the interplay between one's conception of logic and one's epistemology and philosophical projects. If externalism alters these significantly, it is unlikely to let our traditional choice of logic stand.

I suggest that these (and similar) doubts are implicit in our resistance to a Fregean conception of logic. That would be historically appropriate. I have argued that opposition to Frege from one direction is rooted in formalism and a view of mathematical concept formation that has formalist overtones. It now appears that naturalism, and the associated attempts to give epistemology a more empirical and pragmatic footing, underlie a second principal brand of opposition. Non-Fregean views of logic must be understood with reference to the broader issues separating him from other philosophers of the recent past. Conversely, Frege's philosophy of logic fits his overall perspective.

That is one mark of a great philosopher: his views turn out to be systematic in unobvious ways. But it raises a difficulty. I have tried to defend the RCL, but isn't our naturalism superior to Frege's viewpoint? How then can the RCL be maintained?

In fact I am sympathetic to Frege's philosophical tendencies. Such large questions as the status of naturalism or the dispute between empiricism and rationalism cannot, however, be taken up here.[22] Instead I want to suggest that Frege's view of logic can be defended with the help of one idea that, although foreign to him, can be grafted onto his philosophy without harm: a social conception of knowledge. By this I mean not the platitude that acquiring significant amounts of knowledge depends on social cooperation. Even Descartes believed that. It is rather the idea that knowledge is a property of social groups or cultures. This does not rule out parallel, individualistic conceptions of knowledge. But one may hold that some items of knowledge, particularly in well-developed disciplines such as the sciences, history, and philosophy, belong primarily to groups and only derivatively to their members. In the first instance, the group is the knower. I think the epistemic role Frege gives logic can be understood from such a viewpoint.

Any social account of knowledge will need considerable clarification and defense. Here, however, I will elaborate very briefly, assuming that such accounts are not unfamiliar to the reader and hoping for some degree of sympathy. I am interested in the knowledge of certain kinds of groups, of which modern scientific communities, for example, those of professional mathematicians or cognitive psychologists, are paradigmatic. Among their distinctive features are their historical character (they evolve and accumulate results over a period exceeding the lifetime of any individual), the existence of institutions for exchanging and pooling information (journals, books, and libraries), and the existence of recognized community boundaries and qualifications for membership (roughly, academic affiliation). We may identify the beliefs of such a community with the results its members recognize as being established. This leaves room for questions both about how widely a group belief needs to be accepted and about what counts as establishing a proposition, but the effect of this identification should be reasonably clear. The community of set theorists believes the usual theorems of set theory. It also believes in the truth, or correctness, of the axiom of choice, even though this is not a (nontrivial) theorem and even though it is sometimes questioned. It does not, I would say, believe in the falsity of CH, for even though CH is widely doubted, no argument against it meets normal mathematical standards for plausibility arguments, let alone for proof. Nor does the community believe unreported results of individual workers, or propositions about which there is significant controversy. Taking these remarks to suffice by way of illustration, we may add that some community beliefs will also be justified or known. The latter will be true, and both will satisfy suitable analogs of the conditions for individual knowledge or justified belief. I assume that if psychological concepts can be extended to communities, the extension of these conditions will not bring major additional problems. Thus, an epistemology for scientific communities should be possible. And it appears that these communities, if not their members, are agents for which a Fregean view of logic and justification is appropriate, because they are sufficiently free from the relevant cognitive limits. Let us reconsider the externalist's doubts.

There was, first, the problem of memory and attention. FOL seemed not even to present an ideal of argumentation, because no human can follow more than a few moderately long proofs in FOL. But communities can exploit the combined resources of many individuals, and they can take their time. Of course they, like individuals, will recognize practical constraints. The mathematical community writes out very few of its proofs in FOL. But this ideal of proof is relevant for the community in a way that it is not for individuals. If there were a point in doing so, most of the business of proof could be conducted in rigorous first-order formalizations. Everything would take longer, and individual mathematicians would find it harder to survey their field, but it would be perfectly possible for the community to assemble the body of proofs that constitute established mathematics in this way. (Russell and Whitehead did something like this for much of the mathematics of eighty years ago.) So the community can approach the ideal of a system of proofs set out in FOL as nearly as it pleases. Similarly, factors limiting the rigor with which individuals can do mathematics tend to matter less to the community. Insofar as the community at any time is interested in the long-term growth of a field, not in immediate results, it can proceed with all due care and rigor. Barring historical upheavals of a rare order, the field should develop indefinitely, unimpeded by the distractions and limits of individual life. The community can therefore slow this development as much as seems appropriate, secure in the knowledge that the job will eventually get done. The length and complexity of FOL proofs, then, is no barrier to their use in justifying community beliefs.

A second main problem was efficiency—the fact that rigorous avoidance of error comes at too high a price in the acquisition of truths. It seems that individuals and communities will assess such trade-offs differently, for scientific communities adopt new beliefs slowly when any possibility of doubt attaches to them. Recall that community beliefs as I characterized them are accepted by almost all relevant scientists. Yet it is well known that controversial ideas in science may undergo decades of discussion before reaching this stage. Communities are conservative in their adoption of beliefs, much more so than individuals, and for good reason: this attitude makes possible the construction of a nearly cummulative body of scientific doctrine over time. Although the community, too, can err, and although occasionally the discovery of an error requires extensive backtracking, the once standard view of scientific progress still seems *roughly* correct to me: scientists build on the results of earlier generations. And only very conservative procedures for acceptance make this possible. In any case, since the conservativism of communal epistemic policy is a plain fact, the conservativism implicit in accepting FOL as an ideal of justification is no objection.

These remarks suggest a reply to a further challenge. The externalist might well accept normative concepts in principle, yet ask why justification should be very important to us. Its value in everyday life is unclear: individuals are notoriously poor at justifying their beliefs, and as long as one's beliefs are true, one need generally not worry about their justification anyway. Such considerations might raise doubts about the importance of logic in our overall scheme, even if they do not threaten our conception of logic. But justification matters much more to communities, due to their epistemic conservativism. If we will not adopt a belief unless we are fairly sure it is true, and if making sure means subjecting it to stringent criticism, then the community needs access to the grounds on which the belief might be held. Hence justifications must be publicly presented along with our intended conclusions. That, of course, is why evidence and methods matter so much more in science than in everyday life. Conservativism aside, there is another reason for communities to be more concerned with justification. An individual, having enough important beliefs to remember, could never manage to retain very many of their justifications too. But communities have the resources to keep track of reasons along with beliefs. Justification is more relevant for communities because it is cognitively far more affordable.

Human communities do not extend an individual's cognitive powers *too* far. They can handle infinite formulas and arguments no better and have no superior insight into nonelementary logical consequence. Thus, going social yields no case for drawing the limits of logic higher than FOL.[23] But if we are finite groups of finite beings, FOL might already seem to be too much. The communities possible in our universe can grasp only the beginning of the infinitude of proofs in FOL. Why, then, allow proofs of any finite length? Suppose, for example, that Σ is unsatisfiable, but that the shortest derivation of a contradiction from Σ is 10^{150} steps long. Since no community could give that derivation, let alone recognize it as a proof, why should Σ count as being *logically* inconsistent? There is no clear naturalist justification for such an empirically unconstrained notion of logical proof. Setting a bound on the lengths of derivations, however, would entail incompleteness, thus seeming to invite a renewed challenge from proponents of such incomplete logics as SOL.

But on inspection, the case for staying within FOL stands up. The argument from our finiteness does not undermine the reasons for doing mathematics in a first-order language, or for taking proofs to be chains of elementary, first-order steps. Nor has the connection of logic to inference been weakened. Sections IV–V above are therefore still in order. The real issue here is between FOL and some fragment L' admitting only finitely many proofs, perhaps only those that a physically possible community in our universe could grasp. (Let us call the proofs in question "short"; the precise bound and its precise justification do not matter.) In other words: is P a logical consequence of Σ if it is provable from Σ in FOL? Or must the proof be short? This question may not be critical. The answer makes no difference to the main arguments of this paper, as just noted. The status of any concrete proof will also be unaffected, since proofs we can actually give are acceptable either way. Thus, after having advocated FOL at considerable length, I would not seriously resist a naturalistically motivated restriction to L'. But the choice of FOL yields a more appealing classification. If we call Σ in the example above logically inconsistent, we see Σ as being similar to sets of sentences that allow short proofs of contradictions. If we decide on any particular L', we are basing a significant division on a partly arbitrary cutoff. The former choice seems far more natural. One might wonder why—after all, ω is in principle as arbitrary a limit as any finite ordinal. Yet the finite proofs seem analogous to a natural kind. We find them essentially akin to each other and unlike their infinite counterparts. From our viewpoint, ω marks a major boundary. I think this reflects a sense of relative necessity. We are inclined to view it as accidental whether communities can grasp proofs of length 10^{10}, or 10^{100}, or 10^{1000}. Infinitary reasoning, on the other hand, seems more radically impossible, ruled out by fundamental aspects of reality. And in deciding what to call logic, we are guided by this sense, not by the natural limits to our capacities. So Quine's remark applies here: a perception of solidity and significant unity underlies our choice of logic. Whether this perception is available to a strict naturalist is another question.

I have conceded the naturalist little. The cognizers rationalist epistemology postulates must be possible; and we appeal to facts about scientific communities to establish that. This does not make epistemology, let alone logic, empirical in any way. The empirical fact is part of the explanation of why logic matters to us as Frege thought it should. And in making this reply, we do not offer what the naturalist will demand: a full naturalistic justification of our conception of logic and our interest in FOL. I have tried to indicate how the naturalist can make sense of standards of rigor and systematicity that are inappropriate for individuals. This does not explain our interest in developing a *calculus ratiocinator* and a *lingua characterica*, in a relatively Cartesian notion of proof, or in identifying a body of doctrine meeting the fundamentality condition. Yet it was just these ideas that provided Frege's starting point and led to our selection of FOL. Further work along the lines of this paper would therefore have to clarify the assumptions and motives underlying Frege's interests, and to evaluate their relation to naturalism. If they turn out to have no place in any naturalist programme, that might be more the naturalist's problem than Frege's. For although much remains to be said about the RCL, I think the power and interest of Frege's vision of logic is clear. These issues must be left for another occasion. In the meantime, we should not assume that a social conception of knowledge can fully reconcile rationalism with naturalism. A hard choice may lie ahead.[24]

I have admitted conventional elements in the designation of logic. But FOL stands up well in spite of that. FOL has interesting, important epistemic properties. Its choice follows from a natural elaboration of traditional ideas on logic and argument. Further, the best existing alternatives to this choice do not withstand scrutiny; we have no present reason to take the possibility of challenges to FOL seriously. I conclude that although Frege was wrong about the foundations of mathematics, he was right about logic. Yet he did not have the last word. Defending him against his naturalistic opponents may force us to draw on the tradition, due above all to Hegel and Marx, that subordinates individual to social consciousness—a viewpoint deeply alien to the one Frege inherited from Leibniz and Kant. Although this goes beyond Frege, it would be a happy result, a synthesis of two great contributions of the nineteenth century. The prospect of this marriage may make up for the brevity with which I have proposed it here.

NOTES

1. I will sometimes mean logic in such a sense by 'logic', as the context should make clear.

2. My view of Frege's historical place generally follows [26]. I interpret his theory of number in this light in [29].

3. Following Gentzen's convention, this last formula means "at least one member of $\Sigma' \cup \Sigma''$ follows from Σ." Also, I write 'S' for '$\{S\}$' in (3).

4. From this epistemic viewpoint, the requirement of truth preservation is not rooted in our conception of logic as such. It rather has to do with the favored application of logic: to a discipline, mathematics, in which probabilistic argument is generally not accepted.

5. I am speaking intuitively, since we cannot take the notion of "deductive" logic for granted here. There are various plausible attempts to explain the distinction at issue without assuming too much. E.g., deductive logics are characterized by dilution and the necessitation of conclusions by premises, inductive logics by the use of probabilistic notions.

6. Frege also maintains that logic studies the laws of truth. This important idea is a side issue for us; it played a role in Frege's battle against psychologism (the view that logic describes thought) but cannot settle the dispute between FOL and its rivals.

7. See also the discussion of the priority of judgments over concepts in [26].

8. Here I am particularly indebted to Tharp.

9. Other approaches may be useful, e.g., the attempt to identify logic via "topic neutrality" and the like. See esp. [17]. I am neglecting them because they do not seem to help with the choice between FOL and its various standard extensions, such as SOL. But they may complement my approach and show things that it cannot.

10. Michael Detlefsen remarks in a letter that "Frege wanted rigor . . . not so much for reasons of 'certainty,' but rather to provide a strict control on the type of information used in a proof [as a prerequisite for establishing logicism]." That is basically right: Frege emphasizes the role of logical formalization in identifying assumptions (or gaps) that might otherwise be overlooked. Still, our Cartesian conditions are central to the standard view of proof—a view that hardly changed from Aristotle's time to 1900, and that Frege certainly shared (e.g., a Fregean proof is clearly intended not to be provisional in any way). These conditions are also relevant to Frege's explicit concerns. Suppose there were a question about the clarity or force of a step in an arithmetical proof. If geometrical intuition, and only geometrical intuition, could help there, then the claim of logicism would have to be qualified.

11. What follows is not a full critique of Dummett's discussion, which seems to me to misrepresent Frege in more than one way.

12. [20] gives a brief introduction to HL.

13. Cf. Quine's description of SOL as "set theory in sheep's clothing" and as "hiding staggering existential assumptions" ([23], pp. 66–68).

14. Note that "change of logic" is ambiguous between a change in the logical principles we accept, as when we decide to reject excluded middle or add an infinitary rule, and a change in what counts as *logic* for us. The latter is the main issue here.

15. Of course working in FOL will not allow an enumeration of the truths of T, in spite of the completeness of FOL. We will miss intended elements of T that fail only in nonstandard models of A when we try to generate T from the first-order model theory. This could be gotten round only if the notion of a standard model of an HL theory were fully formalizable, which it is not. (But we can do better and better by refining our first-order theories.) Similarly for the remarks below on doing arithmetic within a first-order set theory.

16. Unless one is a nominalist. But then one will reject SOL anyway, independently of any views about the connection between logic and existence.

17. These remarks are misleading in that Shapiro otherwise says very little about inference and how it is to be studied and codified. Also, it may be question-begging to define the correct *inferences* in a language in terms of second-order logical consequence.

18. Which must of course be taken to be standard, lest we admit the Henkin models and lose the categoricity Shapiro values.

19. Various passages in [2] and [25] echo the desire to characterize key mathematical notions within logic.

20. Compare [24], where Quine's project is to build up set theory step by step, using the smallest possible assumptions at each stage.

21. Here I am indebted to Richard Grandy.

22. I discuss these questions in a book in progress (working title: *Truth, Pragmatism, and Ultimate Theory*), on which this essay draws.

23. Reflection shows that infinitary reasoning would pose difficulties even for an infinite collection of finite beings. In any case, such collections are impossible in a strong sense, hence irrelevant to the limits of our logic.

24. [29] pursues questions about rationalism and communal knowledge.

REFERENCES

[1] Bealer, George, *Quality and Concept*, Oxford University Press, Oxford, 1982.

[2] Boolos, George, "On second-order logic," *Journal of Philosophy*, vol. 72 (1975), pp. 509–527.

[3] Church, Alonzo, *Introduction to Mathematical Logic*, Princeton University Press, Princeton, New Jersey, 1956.

[4] Dummett, Michael, *Frege: Philosophy of Language*, Harper & Row, New York, 1973.

[5] Field, Hartry, *Science without Numbers*, Princeton University Press, Princeton, 1981.

[6] Field, Hartry, "Is mathematical knowledge just logical knowledge?", *Philosophical Review*, vol. 93 (1984), pp. 509-552.

[7] Frege, Gottlob, *Begriffsschrift*, Halle, 1879, translated in Heijenoort, Jan van (ed.), *From Frege to Gödel*, Harvard University Press, Cambridge, 1967.

[8] Frege, Gottlob, *The Foundations of Arithmetic* (*Die Grundlagen der Arithmetik*, with facing English text), translated by J. L. Austin, Northwestern University Press, Evanston, Illinois, 1974.

[9] Frege, Gottlob, *Die Grundgesetze der Arithmetik*, Olms, Darmstadt and Hildesheim, 1962.

[10] Gentzen, Gerhard, "Untersuchungen über das logische Schließen," *Mathematische Zeitschrift*, vol. 39 (1935), pp. 176-210, 405-431; translated in *The Collected Papers of Gerhard Gentzen*, ed. by M. E. Szabo, North Holland, Amsterdam, 1969.

[11] Goldman, Alvin, "Discrimination and perceptual knowledge," *Journal of Philosophy*, vol. 73 (1976), pp. 771-791.

[12] Goldman, Alvin, "What is justified belief?," in Pappas (ed.), *Justification and Knowledge*, Reidel, Dordrecht, 1979.

[13] Goldman, Alvin, "The internalist conception of justification," *Midwest Studies in Philosophy* V (1980), pp. 27-51.

[14] Grandy, Richard, "Philosophy of logic," ms., 1983.

[15] Hacking, Ian, "What is logic?", *Journal of Philosophy*, vol. 76 (1979), pp. 285-319.

[16] Heijenoort, Jan van, "Logic as calculus and logic as language," *Synthese*, vol. 17 (1967), pp. 324-330.

[17] McCarthy, Timothy, "The idea of a logical constant," *Journal of Philosophy*, vol. 78 (1981), pp. 499-523.

[18] Putnam, Hilary, "Why reason can't be naturalized," *Synthese*, vol. 52 (1982), pp. 3-23.

[19] Quine, W. V., *Ontological Relativity and Other Essays*, Columbia University Press, New York, 1969.

[20] Quine, W. V., "Existence and quantification," in [19].

[21] Quine, W. V., "Epistemology naturalized," in [19].

[22] Quine, W. V., "On the reasons for indeterminacy of translation," *Journal of Philosophy*, vol. 67 (1970), pp. 178-183.

[23] Quine, W. V., *Philosophy of Logic*, Prentice-Hall, Englewood Cliffs, New Jersey, 1970.

[24] Quine, W. V., *Set Theory and Its Logic*, Harvard University Press, Cambridge, 1970.

[25] Shapiro, Stewart, "Second-order languages and mathematical practice," forthcoming in *The Journal of Symbolic Logic*, vol. 50 (1985), pp. 714-742.

[26] Sluga, Hans, *Gottlob Frege*, Routledge and Kegan Paul, London, Henly, and Boston, 1980.

[27] Tharp, Leslie, "Which logic is the right logic?", *Synthese*, vol. 31 (1975), pp. 1–21.

[28] Wagner, Steven, "Cartesian epistomology," ms., 1983.

[29] Wagner, Steven, "Frege's definition of number," *Notre Dame Journal of Formal Logic*, vol. 24 (1983), pp. 1–21.

Department of Philosophy
University of Illinois, Urbana-Champaign
Urbana IL 61801