Journal of Philosophy, Inc.

What is Logic?
Author(s): Ian Hacking
Source: *The Journal of Philosophy*, Vol. 76, No. 6 (Jun., 1979), pp. 285-319
Published by: Journal of Philosophy, Inc.
Stable URL: http://www.jstor.org/stable/2025471
Accessed: 30-05-2017 20:16 UTC

REFERENCES
Linked references are available on JSTOR for this article:
http://www.jstor.org/stable/2025471?seq=1&cid=pdf-reference#references_tab_contents
You may need to log in to JSTOR to access the linked references.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at
http://about.jstor.org/terms

Journal of Philosophy, Inc. is collaborating with JSTOR to digitize, preserve and extend access to *The Journal of Philosophy*

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

THE JOURNAL OF PHILOSOPHY

VOLUME LXXVI, NO. 6, JUNE 1979

WHAT IS LOGIC? *

WHAT follows is a general theory about classical logic. It is not to be expected that logic can be or should be characterized to everyone's satisfaction, for the subject is too ancient, its workers too active, and its scope too vast for that. Instead this paper falls within one lively and enduring tradition, the one called "logicist." It is an organization and development of some of the thoughts of Leibniz, Bolzano, Frege, Russell, and Wittgenstein. The main technical device is due to Gentzen. The chief philosophical idea that has not already been widely used by many writers is taken from the *Tractatus*. The general thrust of the inquiry goes back to Frege and to Leibniz, but although I shall allude to origins I shall not trace them in any detailed way.

A theory is prompted by problems, and it is well to begin by stating some longstanding questions that this one tries to answer. My focus will be the demarcation of logic: What distinguishes logic from the extralogical? The question has been important because it is closely connected with something deeper. What has been achieved by the program, now in disarray, of trying to show that parts of mathematics are analytic? There is a third question that I hope to take up in later work: Which, if any, of the nonstandard logics can in some strict sense be regarded as alternatives to classical logic? Only two at present have sufficiently serious and far-reaching motivations to have anything in their favor. It is an important question, why intuitionist and quantum logic are at least candidates for being called "logic." But I shall not address that question now, and shall instead consider only the nature of classical logic. We

* A first version of this paper was duplicated for a seminar in Cambridge University, November, 1970, and a second version for seminars at the London School of Economics, February, 1971. I owe many debts of gratitude to people who have helped me with this project since then. I particularly wish to thank T. J. Smiley for his patient encouragement and advice.

0022-362X/79/7606/0285$03.50 © 1979 The Journal of Philosophy, Inc

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

should start by recalling the fate of Frege's project of reducing arithmetic to logic. That is what gave bite to my question, What is logic?

I. THE ANALYTIC PROGRAM

Frege tried to prove that arithmetic is analytic. "The problem," he wrote at the beginning of his *Grundlagen*, "becomes, in fact, that of finding the proof of the proposition, and of following it up right back to the primitive truths. If in carrying out this process we come only on general logical laws and on definitions, then the truth is an analytic one."[1] General logical laws and definitions are the crux. The idea is older than Frege, and the very word 'analytic' recalls Leibniz's doctrine that all necessary truths can be traced back to logical identities by finite analysis. Frege first cast this idea into a form precise enough for it to be established or refuted. Hence I speak of the *analytic* program of showing, for example, that arithmetic can be obtained from logic and definitions.

The analytic program is not primarily epistemological. It is not trying to make mathematics certain, nor to provide foundations for a shaky edifice, although some latter-day logicists had that in mind. Nor, as I understand the tradition stemming from Leibniz, is the chief interest in "what there is." There are ontological spinoffs to be sure, like the proposed answer to the question, What is a number? But remarks on certainty and on the existence of objects are by the way. The chief point is to explain the nature of at least some kinds of necessary truth.

Logicists have not agreed on whether the analytic program would work for all necessity. Leibniz argued that the necessary truths are just those which can be proved from identities by pure logic in a finite number of steps. Frege, in contrast, maintained that, although Euclidean geometry is necessarily true, it is synthetic, and founded on what Kant called intuition. Whatever we may think about such infighting, it seems that the analytic program as a whole has run into sand. Russell's paradox shook Frege's confidence that we know the basic laws of logic. Diverging set theories made it seem that propositions about sets lack the universality and apodictic certainty claimed for logic. *Principia Mathematica* was a poor tool for research in higher mathematics. Then Gödel showed that no such system is able even to analyze computable arithmetic completely.

Despite such discoveries, a philosopher might draw in his horns and admit that even though not all mathematics is analytic,

[1] *Die Grundlagen der Arithmetik* (Breslau: W. Koebner, 1884); J. Austin, trans., *The Foundations of Arithmetic* (New York: Harper, 1960), p. 4.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

analyticity remains a valuable concept. Drilling at mathematics with Frege's tools, we hit impermeable matter, but that, one might feel, is no reflection on the tools. If mathematics cannot be fully embedded in logic, that is an important discovery showing that analyticity was indeed a good investigative idea. We cannot, however, rest there. Even in 1936 Quine was launching his critique. Thirty years later most philosophers had concluded that Frege's drill would hardly make it through a bar of soap. His general laws of logic and definitions: where are they now? A whole system of philosophy has arisen around Quine's demolition of definition, synonymy, analyticity, and meaning. His comments on logic were briefer but as effective, and lead to the chief question of this paper.

II. THE DEMARCATION OF LOGIC

A logical truth, said Quine, is a truth in which only logical constants occur essentially. But, as Tarski had earlier implied, there is no delineation of the logical constants.[2] We can at best list them. It is as if we could characterize the concept *planet of the sun* only by reciting Mars, Venus, Earth, etc., and could not tell by any general principle whether the heavenly body epsilon is a planet or not. We have a laundry list of logical constants, but no characterization of what a logical constant is—except the circular one, that logical constants are those which occur essentially in analytic truths.

The question, What is a logical constant? would be unimportant were it not for the analytic program. There is no point in trying to separate logic from other science, except for that profound speculation about the nature and origin of necessary truth. But, because of that speculation, a fruitful definition of 'logical constant' is called for. This paper to some extent revives the analytic program by providing such a demarcation. Doubtless it is not the only definition that is both rigorous in application and rich in consequences, but it is curiously appropriate to the logistic tradition. It does not go as far as Frege had hoped, but it does include the ramified theory of types. This is just what the authors of *Principia* expected; for in their opinion the ramified theory is logic, but the simplified theory obtained by adding the axiom of reducibility is not logic.

The theory also seems able to resolve a difference between Leibniz and Kant. Leibniz held that all necessary truth is analytic, whereas Kant thought that even $5 + 7 = 12$ is synthetic. Kant is wrong in particular, but right in general. On my theory, truths about in-

[2] "On the Concept of Logical Consequence," trans. J. H. Woodger from the Polish and German (1936) in *Logic, Semantics, Metamathematics* (Oxford: Clarendon Press, 1956), pp. 409–420, esp. p. 420.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

dividual integers, such as $5 + 7 = 12$, are analytic, but arithmetic as a whole is synthetic. The concept *number* is not explicable by logical constants alone. Hence, even sentences of the form, "For every natural number x, then ...," are not in general analytic, for the numerical quantifier is not a logical one. The concept *number* must be given by what Kant called intuition, or at least by some extralogical practice. This is despite the fact that any particular natural number can be characterized analytically. I must add at once that this rather trivial resolution of an issue between Leibniz and Kant is only an historical oddity. For anyone interested in arithmetic it is, of course, a mere scratching of the surface.

III. WITTGENSTEIN'S "BY-PRODUCT" THEORY OF LOGIC

The interrogative, 'What is logic?' can be used to express two questions. I have just asked how to identify logic, but, having identified something, one can still ask after its essential nature. Had Frege been right, we should have reduced arithmetic to logic and shown that its truth conditions lie in logic itself. But what then is the nature of logical truth? Frege said that laws of logic are laws of thought, not in the sense that they are laws about what men do in fact take for true but because they are "the most general laws, which prescribe universally the way in which one ought to think if one is to think at all."[3] He prefers to call them laws of truth, "boundary stones set in an eternal foundation, which our thought can overflow, but never displace" (13). So it seems not to make sense to ask what makes them true, for they help constitute the nature of true thought itself.

In the *Tractatus* we find what seems to be an entirely different idea. Wittgenstein tried to explain how some classes of logical truth are a "by-product" of facts about the use of logical constants. Imagine a language of independent elementary sentences whose meanings are understood. In order to form complex sentences, let logical constants be implanted in this language, and let certain rules determine the truth conditions of such complexes as functions of their components. Then some compound sentences take the value true regardless of their components. This fact is a by-product of rules for the introduction of the logical constants. "[W]ithout bothering about sense or meaning, we construct the logical proposition out of others using only *rules that deal with signs*."[4] The fact that

[3] *Grundgesetze der Arithmetik, Begriffsschriftlich abgeleitet*, Bd. 1 (Jena: H. Pohle, 1893), partially trans. M. Furth, *The Basic Laws of Arithmetic* (Berkeley: Univ. of California Press, 1967), p. 12.

[4] *Tractatus Logico-Philosophicus*, D. F. Pears and B. F. McGuinness, trans. (New York: The Humanities Press), 6.126. For an alternative translation see the text for fn 28.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

these logical truths are a by-product of rules for signs is taken to explain the necessary, apodictic, and a priori character of some of the truths that we call logically necessary.

Recall the positivist slogan that "mathematical truths are true in virtue of the meanings of the words that express them." This came to be used in what one can only call a lighthearted way by some writers who seemed not to reflect on the complexities of mathematical reasoning. But when first propounded the slogan had real force, thanks to Frege's analytic program taken together with Wittgenstein's by-product theory. First, the meanings of mathematical expressions are to be analyzed into purely logical terminology. Secondly, the meanings of the logical constants are conveyed by their introduction rules, and these rules have as a by-product the class of logical truths. The meanings of the mathematical terms are given in terms of logic. The meanings of logical constants are conveyed by the introduction rules. Hence, if *both* Frege *and* Wittgenstein had been right, then meanings, in some imprecise sense of that word, would have been responsible for a good deal of mathematical truth. I know of no other plausible motivation for the crisp positivist slogans about "true in virtue of the meanings" or even "true by convention."

Wittgenstein tried to extend his theory of tautology in a natural way to first-order language, anticipating the theory of countable conjunction and disjunction. He had another meaning-oriented—or at least name-oriented—theory for arithmetical identities. Aside from the account of tautology, no one except Ramsey took much of this seriously. However, my demarcation of logic turns out to confirm Wittgenstein's hunch about the logical constants, using Gentzen's devices to take in the first-order quantifiers and even identity. Although Wittgenstein often alluded to the importance of rules and notational conventions of a "deep" sort, he was never able to say which ones are specifically logical. We are now able to specify a precise class of rules that fits his descriptions, yet includes the whole "logical" part of *Principia Mathematica*.

I must emphasize at once that the resulting *Tractatus*-like theory is not incompatible with the Fregean doctrine that logic constitutes the laws of truth. Nor is it incompatible with Wittgenstein's later reflections on following a rule, stated in *Remarks on the Foundations of Mathematics*. It is a matter of some delicacy to get straight the relations among these different insights. I attempt it in the concluding section of this paper, but first we require the demarcation of logic.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

IV. DEDUCIBILITY NOT LOGICAL TRUTH

What distinguishes logic from the other branches of knowledge? Quine starts his elementary textbook by implying that logic is a science of truths.[5] That is where I part company. If we must have a one-word answer, logic is the science of deduction. The point is made by Michael Dummett, who accuses Frege himself. Frege's approach was "retrograde," he says, in that it formalized "logical systems on the quite misleading analogy of an axiomatized theory." "The representation of logic as concerned with a characteristic of sentences, truth, rather than of transitions from sentences to sentences, had highly deleterious effects both in logic and in philosophy."[6] He implies that we owe the first corrective to Gerhard Gentzen.

I am less sure than Dummett that this is a corrective to *Frege*, although it is certainly a corrective to the early twentieth-century tradition in mathematical logic. When Frege speaks of logical axioms he commonly does so with some hint of qualification, and when we go back to *Begriffsschrift* we find passages such as the following:

> We have already introduced in the first chapter several principles of [pure] thought in order to transform them into rules for the application of our symbols. The rules and laws to which they correspond cannot be expressed in our "conceptual notation" because they form its basis.[7]

Tractatus 5.132 says that "Laws of inference which—as in Frege and Russell—are to justify the conclusions, are senseless and would be superfluous." This may be a legitimate objection to the primitive propositions *1.1 and *1.11 of *Principia Mathematica*. But if one had the view that anything not expressible in concept-writing is *sinnlos* and can at best be elucidated, one would find less of a difference between *Begriffsschrift* and *Tractatus* than Wittgenstein asserts.

Disregarding this historical issue, I agree entirely with Dummett that the right way to answer the question, What is logic? is to consider transitions between sentences. It is now natural to present rules for such transitions in a metalanguage. To avoid Wittgenstein's strictures, we must insist that such rules are not justifications of transitions. They are descriptions, or, perhaps, codifications of what one knows when one knows how to make certain transitions

[5] *Methods of Logic* (New York: Holt, 1950), p. xi.

[6] *Frege: Philosophy of Language* (London: Duckworth, 1973), p. 432/3.

[7] *Begriffsschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens* (Halle: L. Nebert, 1879), T. W. Bynum, trans., *Conceptual Notation* (Oxford: Clarendon Press, 1972), p. 136.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

that we call logical. In one respect this talk of metalanguage is like doing the grammar of English in English. There is evidently a distinction between two uses of the word 'justify'. On the one hand, should an author challenge an editor who has corrected a grammatical solecism, the editor may reply by citing a rule to justify the emendation. It is not to be inferred that in general the rules of grammar "justify" standard usage. They merely codify it, and are often couched in the very forms that they describe. Yet this is only an analogy; for although English grammar will alter in time and varies a little in different communities, it is an open question whether there is any real alternative to classical logic.

Deducibility comes first, I have said, and not logical truth. Today we sharply distinguish between syntactic and semantic aspects of transitions between sentences. The opening parts of my discussion will proceed by considerations about deducibility which are intended to be almost neutral between a syntactic and a semantic treatment of the idea. This is not a quirk of the exposition but a feature of the theory, which is quite in the spirit of the older logicists.

The details of my delineation of logic are so indebted to Gentzen as to make that aspect of this paper only a footnote to his work, although what I do is to turn him upside down. He took a given axiomatized entity, first-order logic, and characterized it by rules of inference.[8] I proceed in the opposite way, using rules of a certain kind to define an entity, namely "logic." I give reasons for saying that anything defined by a rule of inference like Gentzen's is a logical constant. Of course, the exact boundary depends entirely on what shall count as "like" Gentzen's rules. I shall present and defend some general propositions about deducibility and the definition of constants, all of which are in fact true of Gentzen's work, and which, in my opinion, capture its essence. I do not claim this is the only delineation; only that it is remarkably instructive.

V. THE SEQUENT CALCULUS

Gentzen invented two kinds of logical calculi, called "natural deduction" and "sequent calculus." He thought that the former gets at the heart of logical reasoning, and used the sequent calculus only as a convenient tool for proving his chief results. Dag Prawitz was able to obtain many of these results directly for the systems of natural deduction.[9] Per Martin-Löf and other workers

[8] "Untersuchungen über das logische Schliessen," *Mathematisches Zeitschrift*, XXXIX (1935): 176–210, 405–431; M. E. Szabo, trans. "Investigations into Logical Deduction," in *The Collected Papers of Gerhard Gentzen* (Amsterdam: North Holland, 1969), pp. 68–131.

[9] *Natural Deduction* (Stockholm: Almqvist & Wiksell, 1965).

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

have found them the best vehicle for investigations of higher mathematics.[10] W. W. Tait has suggested to me that the sequent calculus is right for classical logic, whereas natural deduction best fits a constructive approach. Despite all this, my exposition will without discussion employ only the sequent calculus.

In the present work the sequent calculus is regarded as a metatheory about an object language. We suppose that an object language is given, and that the class of formulas of this language is determinate. Finite (possibly empty) sets of formulas are represented by Greek capitals such as Γ and Θ. Individual formulas are represented by Roman capitals, A and B. For brevity we write, e.g., 'A,B' to stand for the set whose members are the formulas A and B, and 'Γ,A,B' for the set whose members are A,B and the members of Γ. In the metalanguage we make statements about deducibility relations in the object language, such as 'A is deducible from Γ'. The turnstile abbreviates the converse of this relation: '$\Gamma \vdash A$'. It is to be emphasized that the turnstile ($\vdash$) is a symbol of the metalanguage, and not a constant of the object language.

There is of course an intended connection between deducibility and logical consequence, so that at least $\Gamma \vdash A$ only if A is a consequence of Γ. But I shall proceed in as syntactic a way as possible, stating certain constraints that will hold on any relation that can be accounted a deducibility relation. Some readers will feel this gets the cart before the horse: we know that my constraints are appropriate to deducibility only because of our understanding of logical consequence. In my opinion, such questions of priority are a red herring. It is not necessary to define deducibility to discover that we have some constraints on this notion. Even the very simple constraints that I cull out are sufficient for a good deal of work, and, wherever they come from, it is a virtue that they can be stated in terms of '$\vdash$' alone.

The notion of deducibility is, however, extended in one slightly loaded way. Gentzen noticed that it is convenient to make statements of the form $\Gamma \vdash \Theta$, where Θ may have several members.[11] On the intended reading, this will be valid only if *some* member of Θ is assigned the value true whenever *each* member of Γ is assigned the value true. It is well known that in Gentzen's calculi, with his

[10] See, e.g., papers by Girard, Jervell, Kreisel, Martin-Löf, Prawitz, and Tait in J. E. Fenstad, ed., *Proceedings of the Second Scandinavian Logic Symposium* (Amsterdam: North Holland, 1971).

[11] A general theory is provided in D. J. Shoesmith and T. J. Smiley, *Multiple-conclusion Logic* (New York: Cambridge, 1978).

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

rules, intuitionist logic results from insisting that Θ have at most one member. I shall not discuss this seemingly magical fact here.

VI. THREE FACTS ABOUT CLASSICAL DEDUCIBILITY

When is a relation between formulas a deducibility relation? Without aiming at necessary conditions, three sufficient conditions will be readily assented to.

1. *Reflexivity:* $A \vdash A$. Leibniz believed that every logical truth is derived from identity statements by definitions. He did not work this out, because he did not have the distinction between object and metalanguage. But, with the distinction, the sequent calculus curiously confirms his hunch. The top statements in any derivation of a theorem are always instances of reflexivity. Reflexivity might be called *the deducibility of identicals*. Moreover, as we shall see in §§X and XVI, the substantive steps in the derivation may always be regarded as the application of definitions. So Leibniz's claim about the nature of logical truth is vindicated by the sequent calculus.

2. *Dilution.* If $\Gamma \vdash \Theta$, then $\Gamma, A \vdash \Theta$ and $\Gamma \vdash A, \Theta$. Adding a possibly irrelevant premise or consequent does not affect the deducibility relation. This is clear for a classical notion of deducibility and consequence. In a nonclassical language, such as might be suggested by quantum mechanics, the joint assertion of arbitrary sentences might not make sense; so this rule is here asserted only for the classical case. We may call this the *stability of classical deducibility* under the addition of arbitrary sentences. Note that it provides an essential contrast between deductive and inductive reasoning; for the introduction of a new premise may spoil an inductive inference.

3. *Transitivity.* If $A \vdash B$ and $B \vdash C$, then $A \vdash C$. In the context of our other facts about deducibility, this is naturally generalized to the form: If $\Gamma \vdash A, \Theta$ and $\Gamma, A \vdash \Theta$, then $\Gamma \vdash \Theta$. Gentzen called this *cut*, and it will occupy us a good deal.

These three facts about deducibility do not depend on the presence of any logical constants. The elementary sentences of the *Tractatus* were all independent, so there were no relations of deducibility between different sentences. A less ideal primitive language, even if it had no recognized logical constants, might have some relations of deducibility: "Rachel is a sister $\vdash$ Rachel is a sibling." An unanalyzed language of elementary arithmetic is rich in statements of deducibility. And even if all sentences were independent, identical sentences would be interdeducible. So the class of statements of deducibility about a "prelogical" language will never be entirely vacuous.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

VII. BUILDING RULES

A sequent calculus is a device for deriving statements of deducibility and is thus a metalinguistic instrument. The top statements are all instances of $A \vdash A$, the deducibility of identicals. All nodes in the tree are to be instances of the application of rules of derivation. These rules are of two sorts. Gentzen called one lot *structural*. Since his formulation used sequences rather than finite sets of formulas, these included rules for permutation and for contraction of identical formulas, which we may ignore. The other structural rules are precisely those which correspond to my basic facts about deducibility, namely dilution and transitivity. Thus he had rules for dilution on the left and right of the turnstile, and a rule for cut:

$$\frac{\Gamma \vdash \Theta}{\Gamma, A \vdash \Theta} \qquad \frac{\Gamma \vdash \Theta}{\Gamma \vdash A, \Theta} \qquad \frac{\Gamma \vdash A, \Theta \qquad \Gamma, A \vdash \Theta}{\Gamma \vdash \Theta}$$

These rules are to be understood as instructions: having derived the statements of the form shown above the line, one may derive the statement of the corresponding form below the line. Γ and Θ are called *side formulas* and may be empty. For brevity I shall include the deducibility of identicals as a structural rule. It allows one to start a derivation with a statement of the form $A \vdash A$. *The structural rules embody basic facts about deducibility and obtain even in a language with no logical constants at all.*

Gentzen called his other class of rules *operational*. Each of these characterizes a particular logical constant. In the case of conjunction we have, for example:

$$\frac{\Gamma, A \vdash \Theta}{\Gamma, A \,\&\, B \vdash \Theta} \qquad \frac{\Gamma, B \vdash \Theta}{\Gamma, A \,\&\, B \vdash \Theta} \qquad \frac{\Gamma \vdash A, \Theta \qquad \Gamma \vdash B, \Theta}{\Gamma \vdash A \,\&\, B, \Theta}$$

The formulas that occur in the top line but not in the bottom line are *components*. The new formula in the bottom line is the *principal formula*. We shall consider only rules with a single principal formula. Statements of deducibility occurring in the top line of a rule of derivation will be called the *premises* of that rule. Gentzen considered only rules with one or two premises, but we allow more and can even generalize to rules with countably many premises.

I shall not repeat Gentzen's calculi here. All his operational rules build up complex formulas from less complex ones. In the case of the sentential connectives, the components are literally subformulas of the principal formula. This is not the case, for example, with the rules for the universal quantifier:

$$\frac{\Gamma, Fa \vdash \Theta}{\Gamma, \forall x Fx \vdash \Theta} \qquad \frac{\Gamma \vdash Fa, \Theta}{\Gamma \vdash \forall x Fx, \Theta}$$

where a does not occur free in the bottom line

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

Even so, the principal formula is built up out of a part of the component. A generalization of this property is called the *subformula property*, and is defined elsewhere. It has the effect that principal formulas are, according to a certain complexity ordering, more complex than their components, and that they are built up out of parts of their components. *Gentzen's operational rules are all building rules:* according to a natural ordering in terms of complexity, they always pass from less complex to more complex formulas.

VIII. CUT-ELIMINATION

Only the cut rule can have a conclusion that is less complex than its premises. Hence, when cut is not used, a derivation is quite literally constructive, building up more complex formulas from their components. The theorem known as Gentzen's *Hauptsatz* shows that derivations in first-order logic can always have this property, for any statement derived using the cut rule can be obtained without it. If there are derivations of both $\Gamma \vdash A, \Theta$ and $\Gamma, A \vdash \Theta$, then a finite induction on the length and complexity of the derivations shows that they may be rearranged and combined so as to derive $\Gamma \vdash \Theta$ without cut. Haskell Curry called this the *elimination theorem*, for it shows how to eliminate A. Other writers call this result "cut-elimination" because it shows that the cut rule itself is eliminable. I shall speak of *eliminating rules* in the latter sense, and not in Curry's.

Cut-elimination is useful in proof theory. For an elementary example, we see at once why first-order logic is consistent. If it is inconsistent, there must be some formula A for which both $\vdash A$ and $A \vdash$ are derivable (the latter being interderivable with $\vdash \sim A$). Then, by cut, plain $\vdash$ is derivable. Hence $\vdash$ must be derivable without cut. That is impossible, for none of the other rules decreases complexity.

Gentzen's proof of the consistency of arithmetic uses this sort of consideration, but requires a transfinite induction up to ϵ_0. In systems of natural deduction, cut-elimination corresponds to the result that every proof can be put into a normal form in which one first analyzes certain formulas, and then synthesizes new formulas out of bits and pieces that have been obtained. During the 1960s cut-elimination and normal-form theorems yielded many valuable results in proof-theoretic investigations. The ordinal of the transfinite induction required indicates the degree of constructivity of the proofs. Here, however, we shall be concerned only with finitistic proofs of cut-elimination.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

IX. ELIMINATION THEOREMS AND DEFINITION

Gentzen's proof-theoretic work is rich in philosophical asides. For example, he had the idea that his operational rules actually define the logical constants that they introduce. This needs qualification, but first notice where it leads. We commonly require that definitions should be noncreative. Roughly speaking, if the expression w does not occur in a language fragment and is added to that fragment by means of a definition, we require that this not change what can be said in the fragment without the use of w and its definition. No more should be assertible in the fragment after defining w than could be aserted before. Nothing less should be assertible either. One is usually afraid of creating truths by definition, but one also ought not to destroy any. Wanting to guard against both possibilities, I shall say simply that definitions must be *conservative.*

Now suppose that operational rules are to be regarded as defining logical constants. Then they must be conservative. *Proving cut-elimination is one ingredient in showing that the operational rules are conservative definitions.*

How does this come about? Gentzen's rules are peculiar definitions. We start with an object language possibly but not necessarily lacking logical constants, and for which various statements of deducibility may be made in the metalanguage. Then some operational rules are added, together with syntactic rules for the logical constants thus introduced. Now what should these definitions conserve? Among other things, they should not affect the facts about deducibility that obtain for the original language fragment.

We can put this another way. We add rules for disjunction, the universal quantifier, and so forth, all couched in terms of a deducibility relation denoted by '$\vdash$'. We should ensure that adding these rules does not muck up the basic facts about the deducibility relation noted in §VI. If those facts no longer obtained, then our rules would have been destructive, and we would have no reason to think that '$\vdash$' still denotes a relation of deducibility.

I take this to be the chief moral of Arthur Prior's logical connective "tonk."[12] Prior stated inferential rules that introduced a new connective, but these rules led to absurdity. He intended this as a criticism of the very idea of introducing or defining logical connectives by rules of inference. I draw a more familiar moral. All definitions, including Gentzen-like ones, should be conservative. One may state a sequent calculus for Prior's tonk from which one can derive $A \vdash A\text{-tonk-}B$ and $A\text{-tonk-}B \vdash B$; yet there is no way to get

[12] "The Runabout Inference-Ticket," *Analysis*, XXI, 1 (October 1960): 38/39.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

$A \vdash B$. So cut-elimination does not hold. If we were to postulate Prior's rules, '$\vdash$' would no longer be transitive. That is, it would no longer be a deducibility relation. Cut-elimination assures that the operational rules, regarded as definitions of the logical constants, still leave '$\vdash$' denoting a deducibility relation.

I listed two other facts about deducibility: the deducibility of identicals, and dilution. These also must be conserved. For purposes of proof theory they are either trivial or irrelevant, but if one is to develop Gentzen's suggestion that operational rules provide definitions, then we must check them out.

Gentzen himself stated the deducibility of identicals as "$A \vdash A$ for arbitrary formulas." But a second elimination theorem is that any derivation starting from $A \vdash A$, where 'A' is complex, may be replaced by a derivation starting from the deducibility of identicals for elementary formulas. For example, if we start with $B \vee C \vdash B \vee C$, we could instead start with $B \vdash B$ and $C \vdash C$ and use the operational rules for disjunction. The requirement is, then, that the deducibility of identicals for arbitrary formulas should be eliminable. It is in fact eliminable for first-order logic.

That is a trivial fact but not a vacuous one. Consider the compound quantifier '$(\forall\exists xy)$', short for '$\forall x\exists y$'. This can be introduced by the rules:

$$\frac{\Gamma,\ Fab \vdash \Theta}{\Gamma,\ (\forall\exists xy)Fxy \vdash \Theta} \qquad \frac{\Gamma \vdash Fab,\ \Theta}{\Gamma \vdash (\forall\exists xy)Fxy,\ \Theta}$$

In the rule on the left, b does not occur free on the bottom line; for the rule on the right, a does not occur free on the bottom line. Although, assuming the deducibility of identicals, cut-elimination can be proved for this rule, there is no way to derive $(\forall\exists xy)Fxy \vdash (\forall\exists xy)Fxy$ itself. So this is a rule that does not conserve the deducibility of identicals.

Likewise, although it is a trivial constraint that dilution should be conserved, the elimination of dilution for complex formulas is not vacuous. Consider these rules for S4:

$$\frac{\Gamma,\ A \vdash \Theta}{\Gamma,\ \Box A \vdash \Theta} \qquad \frac{\Box\Gamma \vdash A}{\Box\Gamma \vdash \Box A}$$

In the second rule the side formulas must all start with a box, as indicated. One cannot eliminate the dilution rule for arbitrary formulas even though, when the rule is added, cut-elimination is provable. In general one cannot eliminate the dilution rule (for arbitrary formulas) from systems of modal logic. Since our concern is with object-level logic rather than metalogic, dilution-elimination

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

is a convenient way of excluding metalogic and modal logic from our immediate considerations.

I shall presently qualify the suggestion that Gentzen's operational rules can be regarded as definitions. I claim here only that if we are to pursue that idea, we shall require that the definitions be conservative. Cut-elimination, dilution-elimination, and identicals-elimination (all for complex formulas) are necessary conditions for this. Thus we use elimination theorems for purposes quite different from those of the student of proof theory.

I can here forestall an objection raised by Christopher Peacocke against an earlier and more obscure draft of this paper.[13] Crudely put, it amounts to "What's so great about cut-elimination?" More precisely, if we want to define logical constants, why not just give the operational rules, plus the cut rule and any other structural rules we want? The immediate answer is that then the operational rules are certainly not, in themselves, the definitions of the logical constants. But what is wrong with just adding the operational rules and the structural rules stated for arbitrary formulas built up out of the logical constants? Nothing, except that one is not then defining logical constants in connection with some previous language fragment. Rather one is creating, as a totality, a new system of logistic. Moreover, to foreshadow one use I shall make of these ideas, this system could not have added to it rules for yet another logical constant, without then changing the structural rules once again, asserting that they are to hold for yet more complex formulas. When we add only conservative operational rules, however, we never add any structural postulates. They hold for elementary "prelogical" formulas, and are proved to hold for arbitrary complex formulas.

For a rough analogy to textbook cases of creative definition, take a system of first-order logic with identity and an operation '$\circ$' for which there is only an associative axiom: $x \circ (y \circ z) = (x \circ y) \circ z$. Try defining '$e$' by the formula $x \circ e = x$. This is not conservative, for we can now prove $(\exists y)(\forall x)(x \circ y = x)$. To caricature Peacocke's question, why not add this last sentence along with the sentence defining 'e'? There is nothing wrong with that. But it leads to a new theory of '$\circ$' which is substantively different from the old one. One has not defined 'e' for the original system with simply associative '$\circ$'.

[13] "What Is a Logical Constant?," this JOURNAL, LXXIII, 9 (May 6, 1976): 221–240, p. 231.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

I hasten to add that Peacocke's chief objection was that purely syntactic rules could not possibly be expected to be what is fundamental in characterizing logical constants. Logic is concerned with the preservation of truth, and hence with logical consequence and a semantics, so a syntactic approach ought not be basic. My doctrine is, however, that the peculiarity of the logical constants resides precisely in this: that, given a certain pure notion of truth and consequence, all the desirable semantic properties of the constants are determined, as in §XVI, by their syntactic properties. Peacocke is more favorably disposed to the subformula property (which he seems to imply is associated with cut-elimination), but I can give no defense of it as a requirement that the definitions should be conservative.

X. DEFINING LOGICAL CONSTANTS

If operational rules could be regarded as definitions, then they would have to be conservative. But can they be so regarded? Several quite different kinds of objection arise.

First, it is clear that these rules could not define the constants for a being that lacked all logical concepts. One must understand something like conjunction to apply the conjunction rule, and one must have some surrogate for some sort of quantifier to apply the rule for universal quantification. This kind of consideration particularly influences workers trying to find predicative or other constructive foundations for branches of mathematics. They look for definitions that genuinely reduce seemingly nonconstructive concepts or reasonings to ideas that are better understood. I shall not argue that the operational rules serve any such purpose.

On the contrary, the operational rules at most *characterize* the logical constants in a certain way for a person that already has some logical ideas. In fact of course there is a legitimate sense of "definition" in which a definition characterizes what one already understands. To avoid confusion, however, I shall often speak of the operational rules not as defining but only as characterizing the logical constants. This in no way weakens the demand that the rules be conservative. If they are not, the operational rules of themselves fail to characterize the logical constants.

A quite different objection arises from the fact that Gentzen wrote only at the dawn of semantic theory. Today we expect that anything worth calling a definition should imply a semantics. Since the operational rules are syntactic, they must, it will be urged, fall short of definitions. This objection is partially met by the "do-it-yourself semantics" of §XVI below. Let there be assumed a notion

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

of classical truth and falsity for elementary sentences lacking logical constants, and let the deducibility relation be complete for classical logical consequence among the class of such sentences. Then one is in a certain sense able to read off the semantics of the logical constants from the operational rules. Given the underlying notions of truth and logical consequence, the syntactic rules determine a semantics.

The operational rules do not themselves determine this semantics. It is not as if one could somehow fix what truth and logical consequence are, just by stating the operational rules. This bears on a useful observation of Hilary Putnam's. He recalls Gödel's mapping of classical sentential logic into intuitionist logic, in which, for example, classical conjunction is identified with intuitionist conjunction but classical disjunction is identified with intuitionist $\neg(\neg p \,\&^{*}\, \neg q)$. Then he writes, in italics: "*Contrary to what a number of philosophers—including recently Hacking—have asserted,* [the operational rules] *do not fix the 'meanings' of the logical connectives. Someone could accept all of those rules (and all classical tautologies, as well) and still be using the logical connectives in the nonclassical sense just described.*"[14] Putnam makes this observation in the course of urging that if the logical connectives are given a "quasi-intuitionist" interpretation one can then go on to define 'true', as he puts it, "*exactly à la Tarski.* Only 'truth' becomes provability . . ." (28). On this point my theory does not seem to diverge much from Putnam's ideas. He is avoiding the following maneuver: someone says "I'll first fix the meaning of the logical constants by giving rules of inference; then I'll define 'true' à la Tarski, so any indeterminacy in truth itself will be entirely avoided." I claim, on the contrary, that the operational rules "fix the meanings of the logical connectives" in the sense of giving a semantics, only if classical notions of truth and logical consequence are already assumed. So any avoidance of Putnam's indeterminacy would be circular. The minimum classical assumptions on truth and consequence are stated in §XV below.

XI. CATEGORICAL GRAMMAR

This discussion of definition may suggest that I want to characterize conjunction or universal quantification or some other specific constant—as if logicians did not already know what they mean! Quite the contrary. I am concerned with a general *form* of definition. Now Gentzen studied a given class of first-order constants. He pro-

[14] *Meaning and the Moral Sciences* (Boston: Routledge & Kegan Paul, 1978), p. 27.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

ceeded in the standard way, first stating the elements of his object language, then giving rules for well-formedness, and finally giving rules of inference. That is good practice for a preassigned set of logical constants, but we shall be concerned with *any* constant that can be introduced in a certain way. Hence we should think of the inferential rules introducing a constant as accompanied by syntactical rules for the grammar of the constant.

These grammatical rules should be effective. The symbol w is to be added to some well-understood language L. Suppose that not only is there an effective test for determining which strings of symbols from L are well-formed, but also that there is an effective procedure for uniquely parsing well-formed formulas of L. Then there should be grammatical rules for w, ensuring that $L + w$ has the same properties. It is well known that Russell introduced the definite-description operator so as to make its scope ambiguous; our definitions should avoid that defect.

It would be nice to state the general form for introducing a constant into any language of sufficient complexity. That would require a knowledge of the general form of language itself, and there is no such thing. But we can achieve a remarkably high degree of generality by using the categorical grammar invented by Kasimirz Ajdukiewicz.[15] He intended it as the grammar for the theory of types, but it has recently been generalized and enriched by Richard Montague.[16] We need have no views on how suitable it is for the analysis of English: categorical grammar is plainly adequate for any of the more formal languages used in logic.

This grammar assigns a category to each expression of the language. The notation for the categories indicates how new expressions are to be formed from old and also indicates the category of the resulting expression. If the primitive categories are s (for formula) and t (for term), then a singulary predicate is of category $(t)/s$, because when applied to a term it yields a sentence. Binary predicates are $(t, t)/s$. Binary sentential connectives are of category $(s, s)/s$, adjectives are $(t)/t$, and so forth. The existential and universal quantifiers of first-order logic are of category $((t)/s)/s$. Such a notation rapidly takes in some of the more complex structures even of ordinary speech. It allows, by indexing, for different kinds of

[15] "Die syntaktische Konnexität," *Studia Philosophica*, I (1935): 1–27; H. Weber, trans., "Syntactic Connection," in S. McCall, ed., *Polish Logic 1920–1939* (Oxford: Clarendon Press, 1967), pp. 207–231.

[16] "The Proper Treatment of Quantification in Ordinary English," and other papers in R. H. Thomason, ed., *Formal Philosophy: Selected Papers of Richard Montague* (New Haven, Conn.: Yale, 1974).

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

nouns, of adjectives that apply to them, and so forth. By systems of indexing it is well adapted to the theory of types, which was the point for which Ajdukiewicz designed it. I shall not elaborate the cumbersome details of this syntax, relying on the fortunate fact that there is now a reservoir of categorical grammar on which to draw. In practice we consider only language fragments of the sort familiar to logicians, the functional calculi. But I emphasize that this is an altogether general theory. We can investigate the possibility of conservative Gentzen-like rules as far as others can develop precise and unambiguous categorical grammars. Although I shall not proceed to the discussion of adverbs, for example, the road to a logic of adverbs, such as it is, lies open before us. Similarly for the logic of those second-order quantifiers, such as 'every man', retrieved by Montague from scholastic logic.

Suppose, then, that we have a language fragment and wish to introduce a constant by means of a Gentzen-like rule. It is not enough, in general, to write down some inference schemes. Take the simplest case, a fragment L with no logical constants whatsoever, and whose expressions are all of category s. Let us add a binary connective '$*$' to this language. The steps are as follows. The grammar of L must be augmented by a category $(s,s)/s$, together with rules for sentence formation with this category. Then it is stated that '$*$' is of this category. Then operational rules are stated. Using our intuitive (but grammatically formalizable) scheme for representing the rules, we have, say:

$$\frac{\Gamma \vdash A,\Theta \qquad \Gamma \vdash B,\Theta}{\Gamma,A * B \vdash \Theta} \qquad\qquad \frac{\Gamma,A,B \vdash \Theta}{\Gamma \vdash A * B,\Theta}$$

That completes the characterization of '$*$'. Next one must check that it is conservative. Yes, the deducibility of identicals, dilution, and cut are all eliminable for formulas with '$*$'.

Perhaps no one has written down this "definition" before. What does '$*$' mean? That is the best way to put the demand that Gentzen-style definitions ought to provide a semantics. In fact the do-it-yourself semantics of §XVI does automatically provide a semantics for the above constant. One sees at once that $A * B$ is true in a model if and only if at least one of A or B is false in that model. This '$*$' is Sheffer's stroke.

The requirement that operational rules be conservative can make unexpected demands. Let us start with a language L of terms and monadic predicates. Its nonempty categories are at least s,t and $(t)/s$. To bring in a universal quantifier we must add the category $((t)/s)/s$. It is stated that the universal quantifier is of this category,

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

and one gives the usual Gentzen rules. But proof of cut-elimination is not yet possible. In the course of restructuring the derivations of $\Gamma,(\forall x)Fx \vdash \Theta$ and $\Gamma \vdash (\forall x)Fx,\Theta$, one will in general require at least one item of category t that does not occur in Γ,Θ. Since there is no upper bound on the number of terms that can occur in Γ,Θ, we shall require a stock of items of category t that cannot be exhausted. In more familiar terms, to add the universal quantifier to a language of monadic predicates, one must also add a countable class of variables. For some purposes it is doubtless true that the variables of first-order logic can be regarded as prepositions and place-holders that could in principle be dispensed with, say by a system of arrows indicating what places fall in the scope of which quantifier. The above remarks are yet another way of making the point that this is not the whole story about variables in logic.

Henceforth it will be assumed that rules for introducing a logical constant have two parts. There is the syntactic part determining the rules of well-formedness for the constant. This may include adding stocks of parameters of various categories, required to prove cut-elimination. Then there is the inferential part, giving the rules of derivability of statements in which the constant occurs.

XII. WHAT IS A LOGICAL CONSTANT?

Gentzen's rules are built around a deducibility relation. Deduction has seemed, to some, to be the essence of logic. Hence philosophers have long suggested that systems of natural deduction or the sequent calculus precisely capture "the province of logic." The phrase is that of William Kneale. Similar ideas were propounded by Karl Popper, independently of Gentzen. Paul Lorenzen's operational logic is a related syntactic development of logic which is also supposed to lead to the semantics of logical constants.[17] One basic idea of all these workers may be that *a logical constant is a constant that can be introduced, characterized, or defined in a certain way.*

What way? My answer is about the same as Kneale's: a logical constant is a constant that can be introduced by operational rules like those of Gentzen. The question becomes, "like" in what re-

[17] W. C. Kneale, "The Province of Logic," in H. D. Lewis, ed., *Contemporary British Philosophy*, Third Series (London: Allen & Unwin, 1956). Karl Popper, "New Foundations for Logic," *Mind*, LVI, 223 (July 1947): 193–235, and other papers referred to in note 1 to C. Lejewski, "Popper's Theory of Formal Inference," in P. A. Schilpp, ed., *The Philosophy of Karl Popper*, (La Salle, Ill.: Open Court, 1976), p. 669. Wolfgang Stegmüller, "Remarks on the Completeness of Logical Systems Relative to the Validity-Concepts of P. Lorenzen and K. Lorenz," *Notre Dame Journal of Formal Logic*, v, 2 (April 1964): 81–112.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

spects? Different answers will mark off different conceptions of logic. My answer is that the operational rules introducing a constant should (i) have the subformula property, and (ii) be conservative with respect to the basic facts of deducibility. The second clause means (finitistic) provability of the elimination theorems.

Why should rules for "pure logic" have the subformula property and be conservative? There are several kinds of answers. They correspond to criteria of adequacy like the following. (A) The demarcation should give the "right" logicist class of logical constants and theorems. That is, it should include the traditional (and consistent) core of what logicists said was logic and should exclude what they denied to be logic. (B) Since the demarcation is couched in terms of how logical constants are characterized, it should provide the semantics for the constants called "logical." (C) It should explicate why logic is important to the analytic program. Although (A) and (B) are important, (C) is essential. A demarcation of logic that leaves the analytic program unintelligible is of little philosophical interest (unless the point is to show that the analytic program *is* unintelligible).

I shall briefly explain how my demarcation satisfies (A)–(C), then develop these themes more fully. As regards (A), the most coherent conclusion to logicist investigations is *Principia Mathematica*. Its authors held that all of the ramified theory of types (including identity relations for various ramified types) is logic. But they regretfully concluded that the simplified theory of types is not logic. That is exactly our result. Conservative rules having the subformula property can be used to introduce the ramified quantifiers and identity, but they do not go as far as full second-order logic.[18] So criterion (A) is satisfied. In §XIII I shall discuss whether this is really desirable: is it now known that Russell excluded something truly logical or included something that is not? I don't think so.

As for criterion (B), I have already mentioned that "do-it-yourself semantics" is generated from Gentzen's operational rules for first-order logic. Indeed, what makes it work is precisely the subformula property and the elimination theorems. §XV gives some indication of how criterion (B) is satisfied.

Criterion (C) is fundamental. To delineate logic without explicating the intuitions underlying the analytic program is to forget

[18] For the ramified theory of types and second-order logic, see Prawitz, *op. cit.*, note 10, pp. 62–73. For identity, see my "On the Reality of Existence and Identity," *Canadian Journal of Philosophy*, VIII, 4 (December 1978): 613–632.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

the point of the whole exercise. Without the idea of analyticity, who cares exactly what logic *is*? Quine, in contending that logic coincides with first-order logic, is as usual entirely germane. He thinks that the intuitions underlying the analytic program are fundamentally mistaken. Hence his answer to the question, "What is logic?" does indeed satisfy criterion (C), albeit vacuously—he thinks there are few ultimately coherent intuitions to explicate. In the concluding sections of the paper I argue that the present demarcation of logic, in terms of a form of definition, does elucidate some of these intuitions in a more positive way.

XIII. FIRST- AND SECOND-ORDER

Russell said that the ramified theory of types is logic. The axiom of reducibility, the simplified theory of types, and even full second-order logic are not, then, logic. By my criterion (A), I took it as a virtue of my demarcation that it agrees with the logicist answer. But it would be no virtue if the logicists were just wrong. I now wish to examine a few putative counterexamples to show that the logicist thesis is at least not manifestly absurd.

To stop halfway between first- and second-order logic is to leave oneself in the middle of an uncomfortable slanging match. On the one hand is the position favored by Quine. He once denied that we can stake out a separate preserve for logic. Now he says that if logic is a special kind of entity, it can only be first-order logic.[19] That is not much of a shift of position. Part of the original argument was that nothing distinguishes the peculiarly logical. Now we relax: there is after all one firm distinction, namely, the distinction between first-order logic and everything else. This is buttressed by ontological considerations. Quine does not want to quantify over predicates, and so excludes second-order quantification. Then there are powerful formal results: for example, first-order logic is the strongest complete compact theory with a Löwenheim-Skolem theorem.[20]

It is well known that Richard Montague, in contrast, urged that second-order logic is the very tool for mathematical inquiries and

[19] "Existence and Quantification," in *Ontological Relativity and Other Essays*, (New York: Columbia, 1969), esp. pp. 111–113; *Philosophy of Logic* (Englewood Cliffs, N.J.: Prentice Hall, 1970), chs. 5, 6.

[20] Per Lindström, "On Extensions of Elementary Logic," *Theoria*, XXXV, 1 (1969): 1–11. I refer of course to a countable Löwenheim-Skolem theorem, and not to generalizations such as are found in Jon Barwise, Matt Kaufmann, and Michael Makkai, "Stationary Logic," *Annals of Mathematical Logic*, XIII (1978): 171–224. Note that Stationary logic also falls between first- and second-order logic. There are numerous other investigations showing either that first-order logic has all of certain desirable properties, or else is the strongest logic with certain desirable properties; e.g., J. Zucker, "The Adequacy Problem for Classical Logic," *Journal of Philosophical Logic*, VIII (1979).

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

that it is also the foundation of the categorical grammar of English. George Boolos has recently vigorously defended second-order reasoning.[21] He stands up to the ontological threats and he asks, for example, why on earth compactness, whatever its virtues, should be definitive of logic itself. My theory of logic goes between these two extremes. It appears to have some of the advantages that each party claims for its own side, while lacking some of the faults that each party finds in the other. Logic, on my account, includes the ramified theory of types, but goes no further toward a higher-order logic. This may seem an inauspicious claim, for the ramified theory is now generally held to have been a mistake. But that is because of the historical accident that the authors of *Principia* tried to find a foundation for all mathematics. They thought that logic stopped at the ramified theory, which is too weak for much set theory. Hence they added an extralogical postulate, the axiom of reducibility. As Ramsey noticed, this makes ramification redundant; since then, no one has been much interested in the ramified theory. But if we accept the fact that logic is not going to provide a foundation for mathematics and if we do not add an extralogical axiom of reducibility, then the ramified structure is untouched, and has, I believe, certain merits.

Consider, for example, that fact that second-order logic has no chance of a completeness theorem unless one ventures into intensional entities and possible worlds. I sympathize with the extensionalist objection to these devices, and am glad that the completeness proof for ramified theory is entirely extensional. One does not need to posit any class of entities larger than the class of inscriptions demanded by the theory itself, although the intensionalist may, if he so wishes, suppose that one is quantifying over certain well-circumscribed classes of properties.[22]

Ramified theory is not adequate even for the analysis of recursive arithmetic, and the second-order theorist will object to this fact. In my opinion, this is, on the contrary, something that we have learned: arithmetic is not logic. There are many ways to do arithmetic, and not all demand second-order theorizing, but there is one new and prospering discipline that has thus far been conducted only in a second-order way. This is Montague's grammar.

Montague was of course a champion of second-order theory in mathematical investigations as well as in the sphere of grammar.

[21] "On Second-Order Logic," this JOURNAL, LXXII, 16 (Sept. 18, 1975): 509–526.

[22] There is an unpublished extensional semantics for ramified theory, due to David Kaplan.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

Let us keep these separate. Although second-order theory provides a powerful grammar for all sorts of fragments of English, we should not at once jump to conclusions. We should consider whether we need the full force of second-order logic to effect Montague's analysis of bits of ordinary language that we well understand. I tentatively propose that there is nothing in the more accessible parts of the analysis which cannot also be provided by a ramified theory.

A Löwenheim-Skolem theorem holds for anything which, on my delineation, is logic. It follows from Per Lindström's result that logic is not compact. Defending second-order theory, Boolos asks why logic should be compact? Compactness is a handy property, but there is no argument that it is of the essence of logic. On my account, however, classical logic has a property that we might call *proof-compactness*. I allow countably many premises. The question then arises whether we could obtain additional theorems by allowing proofs with more than finitely many steps, using an ω-rule. The answer is that no new theorems arise in this way. This property of proof-compactness has a venerable tradition behind it. It was Leibniz's original demarcation of the logically necessary: that which can be proved in finitely many steps.

The second-order logician has one telling objection against his first-order antagonist. Leon Henkin[23] proved that there is no first-order treatment of branching quantifiers such as

$$\begin{matrix}(x)(\exists y)\\ \\ (z)(\exists w)\end{matrix} \qquad (Pxy \;\&\; Qzw \;\&\; Ryw) \tag{1}$$

Branching quantifiers do not seem to involve any idea that is fundamentally different from ordinary quantification; so why, it is asked, should we exclude them from the province of logic? We understand them, it is said, in just the way that we understand ordinary universal quantification. The second-order logician does have a rendering of branching quantifiers, namely:

$$(\exists f)(\exists g)(x)(z)(P(x, fx) \;\&\; Q(z, gz) \;\&\; R(fx, gz)) \tag{2}$$

The question of branching quantifiers leads also to another rival theory on the nature of logic, the game-theoretic semantics being developed by Jaakko Hintikka and his students.[24] Hintikka claims

[23] "Some Remarks on Infinitely Long Formulas," in *Infinitistic Methods: Proceedings of a Symposium on Foundations of Mathematics* (New York: Pergamon, 1961).

[24] *Logic, Language Games and Information* (Oxford: Clarendon Press, 1973), ch. III; "Quantifiers vs. Quantification Theory," *Linguistic Inquiry*, V (1974): 153–177.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

it as a virtue of his theory that it provides an analysis of real sentences of English. To use an example due to Janet Beehner,

(3) Some melody of every big band from the forties is copied in some hit song of every rock'n roll star.

Students of English grammar are by no means agreed on whether such sentences do bear the branching quantifier reading.[25] Such current issues will take a number of years to settle down, and here I wish only to warn against too speedy a slide: "If you look at these sentences, you see they involve no idea basically different from the sort of thing we do in first-order logic; we can indeed explain everything in terms of individuals. So you must count branching quantifiers as much a part of 'logic' as first-order logic." Then comes: "the natural abstraction of the idea of branching quantifiers produces a theory formally as strong as full second-order logic." I would interject that there is nothing that we understand in (3) that cannot be understood by (2) where f and g are restricted to very low-grade ramified functions. I don't know whether (3) has to have a branching reading in English syntax. But, if it does, then it can be represented in a way that stops short of full second-order logic. To express matters in a modest way, the ramified theory once again shows itself to be a buffer between first- and second-order logic.

XIV. OTHER LOGICS

A demarcation can be bad in two distinct ways. It can be too strait, denying the existence of borderline cases, or it can be too vague, muttering that there will always be a messy border. A good criterion is one which is sharp but which can also be relaxed in various ways. For example, a good criterion of "grammatical sentence" is one that provides a natural measure of degrees of being grammatical. Likewise, we should allow that a core definition of "logic" can forge out in various directions. Here are some examples of how my demarcation does this.

I say that a Gentzen-like rule should have the subformula property. Now a number of workers have prepared Gentzen versions of Hilbert's ϵ-calculus, where cut-elimination does not hold. Linda Wessells has recently offered a cut-free sequent calculus for Hilbert's ϵ, but her rules do not, strictly speaking, have a subformula

[25] For some relevant considerations on these tentative matters I am indebted to Laurie Carlsen and Alice ter Meulen, "Informational Independence in Intensional Contexts," in E. Saarinen *et al.*, eds., "Essays in Honour of Jaakko Hintikka" (Boston: Reidel, 1979), pp. 61–72; and to an unpublished paper by Jon Barwise.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

property.[26] (It may be possible to find a more encompassing but not merely *ad hoc* property that they do have.) On present evidence we should say not that Hilbert's ϵ is not logical but that it fails to fall under the logicist conception of logic in an entirely specific and well defined way.

A much better understood extension of the logicist conception of logic concerns the power of proofs of cut-elimination. I have considered only finitistically provable cut-elimination. Transfinite induction up to limit ordinals provides more and more generous classifications of logic. Many readers will say that is how we ought to go, for the restriction to finitistic proofs is provincial or old-fashioned. There are many things to say on that score. One logicist defense might recall Leibniz's idea that a proof of a necessary proposition must have a finite number of steps. Now take a theory like arithmetic for which one proves an elimination theorem using transfinite induction. That does not imply that cut-free proofs in the object-level theory itself have to be infinite in length; for any particular use of the cut rule can be dealt with in a finite number of steps. So Gentzen's object-level theory of arithmetic is not to be faulted on that Leibnizian score. But there is also a logicist conception that logic is the very canon of consistency. If, for Leibnizian reasons, proofs in logic are things that stop, then only when logic is construed in terms of finitistically provable elimination theorems do we have a theory whose consistency can be established by logic itself.

Modal logics provide yet another direction in which to extend logic. Obviously the principles underlying this extension must be quite different from those which lead on to arithmetic. Our demarcation displays this very nicely. It is not provability of cut-elimination that excludes modal logic, but dilution-elimination, as was shown in §IX. The serious modal logics such as *T*, *S4*, and *S5* have cut-free sequent-calculus formalizations, but the rules place restrictions on side formulas. Gentzen's rules for sentential connections are all "local" in that they concern only the components from which the principal formula is built up, and place no restrictions on the side formulas. Gentzen's own first-order rules, though not strictly local, are equivalent to local ones. That is why dilution-elimination goes through for first-order logic but not for modal

[26] "Cut-elimination in a Gentzen-style ϵ-Calculus without Identity," *Zeitschrift für mathematische Logik und Grundlagen der Mathematik*, XXIII (1977): 527–538. For a debate on whether Hilbert's 'ϵ' is a logical constant, see Thomas Bjurlöf, "A Note on Logical Constants," *Analysis*, XXXVIII (1978): 119–121, and Christopher Peacocke, "A Reply to Bjurlöf's Objection," *idib*, 122–124.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

logics. What does this formal difference amount to? The meaning is clearer when one thinks of natural-deduction formulations. To place a restriction on side formulas is to insist that whether a step in a proof is valid depends on the history of the proof and on the forms of the sentences that occur higher up in the proof. Modal logic is in this sense metalogical, and our criterion picks out object-level classical logic. This is not to deny that modal logic is in some general sense logic, but rather to specify precisely a formal respect in which it differs from first-order and ramified logic. Thus the subformula property, finitistic cut-elimination, and dilution-elimination may all be relaxed to provide different kinds of extension of my core logicist demarcation of logic. That is as it should be.

The difference between first-order and modal logic shows up in a more familiar way at the semantic level. Models for modal logic use domains of possible worlds. The seriously motivated nonclassical logics, such as are inspired by intuitionist mathematics or quantum physics, also have different semantics. I believe that the difference between such nonclassical logics and classical logics lies in the deducibility relation itself and in the corresponding semantic relation of logical consequence. The nonclassical logics assume a different underlying semantic framework. I shall develop this idea in the next two sections.

XV. SEMANTIC FRAMEWORKS

Suppose that Gentzen-like operational rules are to be added to a language fragment. If the initial bit of language is something one can understand, then it ought to be possible to understand this fragment augmented by the constants introduced by the rules. If not, the rules are not definitions.

We must, then, provide a semantics for the new logical constants, presupposing a semantics for the original language fragment. That assumes some substantive and precise analysis of the fragment. The assumption has two parts: first, a general assumption about the form that the semantics should take, and, second, a specific account of the language fragment to hand.

By the form of the semantics I mean an initial abstraction about the nature of truth and logical consequence. What I have to say in detail about this abstraction does not depend on any philosophy, but it is well to offer one philosophical setting, which the reader need not accept. I do not believe that English is by its nature classical or intuitionistic or whatever. Classical and intuitionistic truth are both abstractions made by logicians. We all know that logicians attend to numerous different objects that might be served by uttering

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

'if . . . then . . .' sentences. The abstractions that result include the material conditional, several kinds of strict implication, and a panoply of stronger connectives of which relevant implication is the current favorite. In a more ambitious way classical and some serious nonclassical logics abstract from the very point of making statements. Classical logic is one of several interesting abstractions. If the point of uttering sentences (perhaps only with some selected topics of conversation) were to say what can be established, or to say what makes sense in microphysics, then one is drawn to nonclassical abstractions.

These remarks about abstraction are intended chiefly as a flag, posting quicksand. It might well be the case that there are decisive arguments in favor of classical, intuitionist, or quantum logic (to mention only views that are widely attributed to Kripke, Dummett, and Putnam in turn). I here contend only that none of these is the logic of English. When I speak of a semantic framework for a language fragment, I mean an abstraction from the point of uttering declarative sentences. It is art, not nature, that determines the semantics of natural languages.

A semantic framework is to be the general form of the semantics of a language. It may well be that I include too little in the framework, and then smuggle in more assumptions as part of the semantics of specific languages. Perhaps I focus too much on what has been the subject of recent discussion. But I shall make two assumptions only, one about truth, and the other about logical consequence. The strongest version of the assumption about truth is that every sentence of the language fragment should be assigned the value true or false but not both. The second assumption is that a set of sentences Θ is a logical consequence of the set of sentences Γ if no matter what values are assigned to the members of Γ, Θ, some member of Θ is true when every member of Γ is true. We make only these two assumptions. The language fragment might have a rich field of logical consequence, or it might be a "prelogical" language of independent elementary sentences like that envisaged by the *Tractatus*, in which there are no nontrivial consequences.

There is an important connection between the structural rules and this classical semantic framework. Let us say that a set of rules of deducibility is *complete* with respect to a language with a classical semantic framework, when $\Gamma \vdash \Theta$ is derivable if and only if Θ is a logical consequence of Γ. *Then the structural rules are complete with respect to a prelogical classical language.* Or to put it differently, Θ

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

is a logical consequence of Γ in any classical language whatsoever, if and only if $\Gamma \vdash \Theta$ is derivable by the structural rules alone.

I began in §VI writing out general constraints on anything that could count as a relation of deducibility. These constraints had already been recognized by Gentzen as the structural rules of his sequent calculus. They also pick out the minimum facts of logical consequence that obtain from a classical point of view. Thus the structural rules formalize the "pure" theory of classical logical consequence. It is my contention, not developed here, that a nonclassical logic has a right to be called logic (and not merely another logical algebra) just if there is a different semantic framework, with respect to which some nonclassical structural rules are complete.

XVI. DO-IT-YOURSELF SEMANTICS

The rules for logical constants always take the following form. Since the requirement of dilution-elimination means that all rules are local in the sense of placing no restrictions on side formulas, side formulas may be disregarded. Omitting side formulas, the form of the rules is:

$$\frac{\{\Pi_i \vdash \Sigma_i\}}{A \vdash} \qquad \frac{\{\Pi_j \vdash \Sigma_j\}}{\vdash A}$$

The formulas in Π and Σ are components of A. A set of statements $\{\Pi_j \vdash \Sigma_j\}$ from which one may derive $\vdash A$ is called a *right protopremise*; $\{\Pi_i \vdash \Sigma_i\}$ above is a *left protopremise*. Consider a Gentzen-like rule which is to add a constant to a language fragment. A semantics and a class of models for the language is assumed. A model assigns every sentence of this language the values T or F. We are concerned with how to assign values to complex sentences in terms of a model for the original language. Thanks to the subformula property an inductive definition may be used.

> A takes the value T in a model iff there is a right protopremise for A, $\{\Pi_j \vdash \Sigma_j\}$, such that for each j some member of Σ_j takes T or some member of Π_j takes F.

Thanks to the elimination theorems, it is possible to establish a matching assertion,

> A takes the value F iff there is a left protopremise for A, $\{\Pi_i \vdash \Sigma_i\}$ such that for each i some member of Σ_i takes T or some member of Π_i takes F.

There is nothing more to be said about sentential logic. More complicated languages have more structured models. In §XI it was pointed out that provability of cut-elimination for first-order logic might require the addition of countably many items of category t,

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

i.e., an infinite stock of variables. This applies only to languages which already have some terms and predicates and for which we assume models with a nonempty domain of individuals. For each model M of the original language, we now need all models obtained by assigning the new variables to individuals in the domain of M. This indicates a general technique for ramified logic, and is possible because variables of a given category are added only to a language that already has items of that category and, hence, whose models already have a nonempty class of objects for items of that category.

This semantics is not directly applicable to Gentzen's quantifier rules. His rule for the universal quantifier is stated on the left below. Instead we use the equivalent ω-rule on the right, which is local in the sense of §XIV, i.e., places no restrictions on the side formulas. (That such a rule is equivalent is implied already by Gentzen's Lemma 3.10 "redesignating of free object variables.")

$$\frac{\Gamma \vdash Fa, \Theta}{\Gamma \vdash \forall x Fx, \Theta} \text{ where } a \text{ does not occur in the bottom line} \qquad \frac{\{\Gamma \vdash Ft_i, \Theta\}_i}{\Gamma \vdash \forall x Fx, \Theta} \text{ where } t_i \text{ runs over all terms}$$

Such devices may be immediately generalized to ramified logic. Charles Parsons points out that such ω-rules precisely parallel the *Tractatus* account of the first-order quantifiers.

The completeness theorem states that, under the above semantics, $\Gamma \vdash \Theta$ is derivable if and only if Θ is a logical consequence of Γ. I have elsewhere stated some details of the proof.[27] This do-it-yourself semantics is of course a fraud. It purports to be a mechanical way of providing the semantics of the logical constants. But in fact it needs a thoroughly hand-crafted semantics for the original language fragment in order for the whole thing to work. In the case of the quantifiers with the ω-rule above, one would have to understand something like arithmetic, or at least counting, to deploy the semantics. This is not a problem for the present paper, for I have tried only to find a demarcation of the logical constants. The claim is that the constants are those which can be introduced into a language in a certain way. They can be introduced by rules of a certain form. The rules are syntactic in character. Yet they are such that if strong semantic assumptions of a general kind are made, then the specific semantics of the individual logical constants is thereby determined. The rule for '*' in §XI will illustrate this. What are the truth conditions for '$A * B$'? The rules stated above show that $A * B$

[27] "Do-It-Yourself Semantics for Classical Sequent Calculi, Including Ramified Type Theory," in R. Butts and J. Hintikka, eds., *Logic, Foundations of Mathematics, and Computability Theory* (Boston: Reidel, 1977), pp. 371–390.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

takes the value T if and only if at least one of A or B is F. So the do-it-yourself semantics is not vacuous; for one could well have failed to understand what the '*' meant, and then found it out by these semantics.

XVII. PRELOGICAL LANGUAGE

The *Tractatus* imagines a language of elementary sentences that lack logical constants, which has in addition sentential connectives for forming compound sentences. The truth tables exhibit the character of these connectives and show how the truth conditions for complex sentences may be explained in terms of simpler sentences. One could carry this myth one step further, and consider the fragment of the language consisting just of elementary sentences. Then one could imagine the logical connectives being added to that language, and their use being conveyed by syntactic rules and the truth tables. The point of this extra myth would not be to show how the connectives are in fact learned, but rather to display something about the logical connectives—"they are words of this peculiar sort, that one can imagine them being planted in the language in this way."

There is no part of the *Tractatus* of which Wittgenstein became more scornful than the myth of elementary sentences. But having been taught some of the lessons of later philosophy, one can still usefully deploy the myth. It is an instructive myth because it is the purest form of the idea of a purely descriptive part of language.

This myth, and the task of distinguishing nondescriptive logical constants, is foreshadowed by the schoolmen. The signs of quantity, 'some', 'all', and 'no', were central examples of syncategorematical terms. But what, it was asked, distinguishes them from descriptive terms? It was natural to contrast *velus* (meaning 'veil') and *vel* (meaning something like inclusive disjunction) but there was never an agreed basis to the distinction. In general one tried to say as much, or more, about what it is to be descriptive as about what it is to be syncategorematical. Much the same holds for similar dichotomies in judgments, say between the empirical and the a priori, or in faculties, say between sensibility and the understanding. It is a virtue of my demarcation that it enables one to characterize the logical constants without being forced to say what is on the other side of the dichotomy. One does not have to say what pure descriptive constants are. A logical constant is a constant that can be added to any language of a certain sort.

Yet the myth of the prelogical or entirely descriptive language still has some force. It enables us to ask, What are the minimum

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

conditions on a language, in order that logical constants should be added by means of operational rules? If the rules are to be regarded as something like definitions, then we suppose the formulas of a prelogical language are themselves intelligible. Such understanding might be achieved by providing a theory of truth for the language, or a formal semantics, or: the language is simply understood. A number of philosophers have recently been reinforcing the point that the stating of meanings comes to an end, and languages are in the end just understood.

A fact about the terminology of the *Tractatus* has been widely overlooked. Wittgenstein speaks of *truth conditions* for compound sentences. He shows how truth conditions of compounds are determined by their components. But he does not speak of truth conditions for elementary sentences. He does speak of their truth possibilities, but not of their truth conditions.[28] This thought could be combined with a recent doctrine of Putnam's. He urges that the theory of understanding a language is not to be couched in terms of truth conditions and correspondence, although those ideas are important for explaining the success of discourse. Without taking a stand on such matters, one could urge that understanding the prelogical language is indeed not to be explained in terms of truth conditions. Complex sentences, obtained by adding logical constants, however, may have their truth conditions explained in terms of more elementary sentences.

It may well be asked whether a prelogical language could be understood by a being that had no logical concepts. I doubt that there are any compelling arguments one way or the other, and the question is idle because prelogical language is a myth. Beneath any "elementary" sentence—be it "this is black," said pointing at this letter '*r*'—there is a swarming underworld of logically germane sentences that bear on the sentence in more than a merely empirical or inductive way. The best metaphor may be that of an hourglass, of endless constructions which converge on a collection of what are at one level simple sentences, but which, when they pass through the narrow point of the hourglass, open up again in endless complexity. The elementary sentences of the *Tractatus* are merely the elements that occur at a narrowing in an hourglass, and are not ultimate. Logic, depth grammar, structuralism, and the like should postulate points of convergence or condensation, not atoms.

[28] For *Wahrheitsbedingungen*, see, 4.431, 4.442, 4.45, 4.46, 4.461, 4.463; for *Wahrheitsmöglichkeiten*, see 4.3, 4.31, 4.4, 4.41, 4.42, 4.43, 4.431, 4.44, 4.442, 4.45, 4.46, 5.101. Note that in 4.063 (b) the word sometimes translated as "conditions" is *Umständen*.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

XVIII. LOGICAL TRUTH

My criterion (C) insists that a demarcation of logical constants should connect with the analytic program. The immediate Fregean aim had been to reduce arithmetic to logic, proving thereby that arithmetic is not synthetic. The results are negative. Recursive arithmetic cannot be reduced to logic, strictly construed. If, however, one were to extend the definition of logic to allow for transfinite proofs of cut-elimination, then arithmetic does get included as "logic." In §XIV I have expressed some reservations about this strategy.

The strict demarcation of logic provides some vindication for both Leibniz and Kant. In the ramified hierarchy cumbersome definitions enable one to prove that $5 + 7 = 12$. So, on my explication, Kant's immediate claim about arithmetic is wrong. *That* sum is not synthetic. But although Leibniz may win this battle he loses the war, for the concept *natural number* cannot itself be categorically characterized in pure logic. We can say only that the natural numbers are those which come in the sequence 1, 2, 3, $\cdots$. We do have an intuition of this sequence. Perhaps, as Kant supposed, it is connected to the intuition of succession in time.

Arithmetic was a special project for the analytic program. More generally, it aimed at understanding the nature of logical truth. A. J. Ayer could say, in the great days of enthusiasm, that "there is nothing mysterious about the apodictic certainty of logic and mathematics. Our knowledge that no observation can ever confute the proposition '$7 + 5 = 12$' depends simply on the fact that the symbolic expression '$7 + 5$' is synonymous with '12', just as our knowledge that every oculist is an eye doctor depends on the fact that the symbol 'eye-doctor' is synonymous with 'oculist'. And the same explanation holds good for every other a priori truth."[29] No one would dare say this sort of thing any more, but there is a lingering belief in the maxim, "Necessary truths are true in virtue of the meanings of the words used to express them."

The by-product theory of logical truth, which in §III I attributed to the *Tractatus*, gives some credibility to that idea. Intended originally for the truth-functional connectives, it is readily extended to any constant introduced by Gentzen-like rules satisfying my criteria. "Without troubling ourselves about a sense and a reference, we form the logical theorems out of others by rules that deal with signs alone." That fits the operational rules rather well. Rules for a notation are provided, in consequence of which certain complex

[29] *Language, Truth and Logic*, 2nd edn. (London: Gollancz, 1946), p. 85.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

sentences come out as theorems. As the *Tractatus* continues, "All theorems of logic are of equal status; there are not some that are essentially primitive and others deduced from these." "Logic can always be conceived so that every theorem is its own proof."[30] In the sequent calculus every theorem has, indeed, its own analysis, and no theorems are primitive. Moreover this analysis answers rather exactly to Leibniz's idea of the finitely long analysis of necessary propositions.

This way of thinking makes the existence of theorems in logic look like a by-product of the rules that convey the use of logical constants. It guards against the supposition that theorems represent constraints on how the world must be. The only constraints arise from the system for forming complex sentences. Some complexes that are built up using logical constants describe complex possibilities, but theorems and contradictions are only limiting cases that arise from the rules for the notation itself. As Leibniz well understood, the concept *logically necessary proposition* is to be explained in terms of proof, and not in terms of truth in all possible worlds (as if necessity represented some constraint on what worlds can be created).

It is not to be inferred that logical truths are in any instructive sense true by convention. Imagine that notational conventions in the form of operational rules were added to a "prelogical" language. Even in that mythical case convention would not completely account for logical truth. There would remain the question prompted by Lewis Carroll's Achilles and the Tortoise,[31] and elaborated by Quine long ago.[32] By what conventions, it would be asked, do we apply the rules as we do when we derive theorems in the calculus? Even when we deliberately adopt a convention (say with the rule that turned out to introduce Sheffer's stroke in §XI) that convention is not the *complete* account of the logical theorems that result.

To reject truth by convention is not to reject truth in virtue of meaning. The operational rules display certain features of logical constants. These constants do have a "meaning," aspects of which are displayed by these rules. The rules do not produce or create or justify logical truths, but set out what it is about their meaning that is connected with logical truth. Note that a particular objection to "truth in virtue of meaning" is irrelevant here. It is rightly

[30] *Tractatus* 6.126, 6.127, 6.1265; for this discussion one must translate *Satz* as 'theorem'.

[31] "What the Tortoise Said to Achilles," *Mind*, IV (1895): 278–280.

[32] "Truth by Convention" (1936), in W. V. Quine, *The Ways of Paradox* (New York: Random House, 1966), pp. 70–99.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

observed that 'It is raining or it is not raining' cannot be true in virtue of the meaning of the words, because the proposition expressed would be true even if we used quite different words. Yet this tautology is still a product of meanings, not (except adventitiously) of the English signs 'or' and 'not', but of the abstract "meaning" that is conveyed by the operational rules that we happen to label as disjunction and negation. Whatever be the signs we associate with those rules, there will be a tautology corresponding.

These considerations are all of a negative sort: logical truth has no source outside language and whatever makes language possible. There still remain the familiar questions as to what it is to grasp the meaning. Undoubtedly we understand some constants before formulating any rules, and, even if we did not, there would be questions about following the rules. It may be, however, that certain positive conclusions are to be drawn from the theory of operational rules. For it is not as if they justify talk of logical *truth*. All we get from applying the rules are some sentences with the deducibility sign in front. To construe these as truths we require a semantics.

I have contended that the semantic framework has nothing to do with particular logical constants. Given a semantic framework for a language, one may use operational rules to characterize logical constants; the do-it-yourself semantics shows how the semantics for the language works out for the constants that are thus introduced. This leads one to recall some remarks of Frege quoted earlier.[33] Analytic truths are those whose proofs may be traced back to "general logical laws." But these general laws were not to be primitive propositions written down in the object language. What are these "most general laws, which prescribe universally the way in which one ought to think if one is to think at all"? I believe that in the end they are not about particular logical ideas, such as the quantifier or the conditional. They must be what Frege calls "laws of truth." A law of truth would concern the nature of the semantic framework itself.

The analytic program has come full circle. In an attempt to understand the idea of analyticity, one considered the logical truths. A logical truth is a truth in which only logical constants occur essentially. So one searched for a demarcation of the logical constants. Having used the theory of deducibility to find it, we then see that

[33] References for Frege's remarks are given in notes 1, 3, and 7. Frege himself appears to have abandoned the analytic program late in life; see his, "Neuer Versuch der Grundlegung der Arithmetik," in H. Hermes *et al.*, eds., *Nachgelassene Schriften* (Hamburg: Felix Meiner, 1969), p. 298.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms

although the existence of particular theorems of logic may be explained in terms of rules that define individual constants, the notion of logical truth depends on the notion of truth for a language. If a nonstandard logic is possible, in a way that is not parasitic upon classical logic, then a nonclassical notion of truth and consequence is possible. But if a nonstandard logic must ultimately be explained using classical logic, then indeed we would have found something that "our thought can overflow, but never displace."

IAN HACKING

Stanford University

COUNTERPART-THEORETIC SEMANTICS FOR MODAL LOGIC *

DAVID LEWIS's proposals [1] for the analysis of *de re* modal locutions [2] represent a major advance in thinking on the subject, but have not had the amount of critical study they deserve. In this paper I will inquire whether they constitute an adequate semantic theory of *de re* modality. In doing so I will not discuss Lewis's notorious metaphysical realism with respect to possible worlds. The semantic theory embodied in his postulates for counterpart theory and his schema for translating a modal language into a counterpart-theoretic one are compatible with any of a wide range of metaphysical theories about possible worlds and the nature of possibility; arguments about which of these metaphysical theories is correct are therefore irrelevant to the semantic issue. In particular, adoption of a counterpart-theoretic semantics is compatible with taking possible worlds to be purely abstract structures, with the

* This paper is a counterpart of a portion of my dissertation (University of Pittsburgh, 1977), but has benefited from comments on an earlier version. I would like to thank my advisor, Joseph Camp, and also David Lewis, Robert Purdy, and Jay Hartman Hazen for their various kinds of help and encouragement.

[1] In "Counterpart Theory and Quantified Modal Logic," this JOURNAL, LXV, 5 (March 7, 1968): 113–126. Acquaintance with this paper is presupposed; references to Lewis, unless otherwise noted, are to it. Cf. also his "Counterparts of Persons and Their Bodies," this JOURNAL, LXVIII, 7 (April 8, 1971): 203–211.

[2] This paper considers only the first-order logic of necessity and possibility, treated by Lewis in the papers cited, with the addition of an actuality operator. Counterpart-theoretic semantics have also been proposed for other (more often employed?) intensional locutions, such as counterfactuals. Cf. Lewis, *Counterfactuals* (Cambridge, Mass.: Harvard, 1973), pp. 36–43.

0022-362X/79/7606/0319$02.00 © 1979 The Journal of Philosophy, Inc.

This content downloaded from 217.112.157.133 on Tue, 30 May 2017 20:16:35 UTC
All use subject to http://about.jstor.org/terms