Chapter 7

The Indispensability of Indexicals and the Carnap-Tarski Semantic Tradition

There was, as we have seen, a flurry of explicit theorizing about indexicality in the early years of this century. There was Husserl's treatment in the first edition of the Logical Investigations (1901), his reconsiderations in the lectures of 1908 and the second edition of 1913, Russell's "On the Nature of Acquaintance" of 1914 and Frege's "Der Gedanke" (1918). Such an output was not to be seen again for three decades; only then came Reichenbach's treatment of "token-reflexive expressions" in Elements of Symbolic Logic (1947), Arthur Burks's revival of Peirce's semiotic approach in "Icon, Index, and Symbol" (1949), Austin's "Truth," Strawson's "On Referring," and Arthur Pap's "Are Individual Concepts Necessary?" (all 1950), and finally, Bar-Hillel's "Indexical Expressions" and Nelson Goodman's treatment of "indicators" in The Structure of Appearance (both in 1954). Part of the explanation for the intervening lull in work on the peculiarities of indexical reference was the publication in 1921 of Wittgenstein's Tractatus, and its enthusiastic (if somewhat skewed and partial) reception in Vienna and England. In the heady, programmatic early days of logical positivism there was an overwhelming emphasis on the syntactic analysis of language in the service of an anti-metaphysical crusade. As we noted in Chapter 1, indexicals, as a linguistic category, are invisible to syntactic analysis—they are essentially characterized by certain semantic regularities.

But at the same time that philosophical theorizing about indexicals fell into abeyance, philosophically significant use of indexicals continued unabated. The new empiricists described the epistemological base of the linguistic structure of science in terms of a special set of sentences—the "protocol sentences" or "ostensive sentences" of the Vienna Circle, Ayer's "observation statements" and "basic propositions", etc.—examples of which (like Russell's "atomic sentences") inevitably contained indexicals: "This is white," "I am now in pain," "My pencil is black," etc.1

1 Schlick cites, as examples of Konstatierungen: "this cathedral has two spires" and "there are now two yellow lines in the visual field"; Popper gives an example of a "specific reality statement": 'This is a book'; Reichenbach calls such sentences "facts of zero level."; Carnap gives examples of "protocol sentences" such as 'I am excited now'. The original paradigm of the protocol sentence is the raw data as recorded in the professional scientist's laboratory notebook (Protokol). In actual practice what is recorded in such documents need not be sentences at all; a series of time notations paired with instrument readings may be more
It was the epistemological, not the semantic, peculiarities of these sentences which caught the attention of the syntactic analysts. Thus Ayer, in *Language, Truth and Logic*, spends several pages arguing that there are, in fact, no "ostensive or demonstrative propositions" as Schlick, and others in the Vienna Circle, had claimed. But his point is not that we could get along without the use of sentences containing ostensive or demonstrative (in the semantic sense) expressions—much less, that we in fact do get along without them; his point is that those propositions do not carry the kind of *evidential certainty* which some of the Vienna positivists claimed; they are not "demonstrative" in the epistemological sense. Later Ayer admitted that there are "basic propositions" which incorrigibly "record an immediate experience," but he continued to deny that these are the same propositions which provide evidence for factual conclusions of science, although they may be expressed using the same (context-sensitive) form of words as those evidential premises.

This chapter begins with an examination of the role indexicals play in the work of influential philosophers in the logical empiricist tradition, particularly Carnap and Tarski. We pay special attention to the contrast between what they say about language and the way they use indexicals and indexical-like devices in the presentation of their theories. This is followed by consideration of the mid-century revival of positive theorizing about indexicals. Finally we consider what can be gleaned from this period that can contribute to a theory of indexical context.

**Carnap's Call for the Elimination of Indexicality from the Language of Science**

Carnap's evolution from the early syntactic program to the later semantic program is especially instructive in regard to the hiatus in semantic investigations of indexicality; that evolution produced a methodology which strongly influenced contemporary philosophy of language, and thus played a crucial role in the direction subsequently taken by the investigation of indexicality.

In the *Aufbau*, Carnap is motivated by worries about the mismatch (earlier discussed, as we have seen, by Mill, Russell, Frege and Husserl) between what we have been characterizing as our objective epistemic and referential goals, and the apparent inadequacy of our subjective means of achieving those goals:

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typical. These raw records of course can be made into assertive sentences by the laboratory worker, but only by supplying contextual information which is taken for granted in the laboratory. In this sense, the typical examples given, while perhaps not true to the actual practice of scientist, do illustrate with their explicit indexicals the context-dependence which is typical of reports of firsthand experience.
Even though the subjective origins of all knowledge lies in the contents of experience and their connections, it is still possible, as the constructional system will show, to advance to an intersubjective, objective world, which can be conceptually comprehended and which is identical for all observers.  

He points out the crucial epistemological role of unanalyzable basic expressions, corresponding to the "contents of experience," for any system of knowledge. However, because of its "metaphysical neutrality"—its compatibility with both objective realism and phenomenalism—the Aufbau system need not take a position on what actual examples of these basic expressions might be. Carnap's official interest is in the form, rather than the content of expressive inferrential systems. But the examples he gives allow us to surmise how indexicals play a role in these statements.

Carnap recognizes the use of "ostensive definition" in ordinary language, but he claims unequivocally that "within any object domain, a unique system of definite description is in principle possible, even without the aid of ostensive definition." Contrary to Peirce's contention that "object domains"—universes of discourse—are, themselves, only fixed indexically, Carnap sees his constructional system as a proof that there is only one object domain for all of science, that every possible object of scientific inquiry can be assigned a place within that system via a "structural description," and that, therefore, all scientific referential expressions can be taken as equivalent to definite descriptions. In fact, he says, "any intersubjective, rational science presupposes this possibility." This is a very strong claim and has as a corollary, the dispensibility of indexicals, so we need to examine the arguments for it—especially since, as we shall see, a close examination of Carnap's examples and actual working methods shows that they themselves call this conclusion into question.

As an example of determinate reference via structural description, he suggests that one could refer to a specific railway station by means of a topological description which is formally identical to the actual railway network. There are two problems with this example, on Peircean intuitions. First, in order for a formal structure to be any aid in reference, its correspondence to the actual network must be established—Carnap says that he "must presuppose that by inspection of the geographical facts one can determine unequivocally whether a given network map represents a given physical entity, such as a railway system." But Peirce has argued that establishing this projective relation—e.g., by

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2 Rudolph Carnap, Der Logische Aufbau der Welt, published in English as The Logical Structure of the World, translated by Rolf A. George, p.7.

3 Ibid., pp. 24-5.
"inspection of geographical facts"—unavoidably involves indexicals, since it requires using signs which get their meaning from their "existential relations" to other physical objects (viz., dots and lines on a diagram, and railway stations and tracks). It also requires orienting and "anchoring" the diagram to the territory (e.g., by establishing that "This is North, and you are here"). Secondly, once the diagram is confirmed to have the same structure and orientation as the object system, using it in making reference to a particular station still requires picking out a point on the diagram; again, as Peirce argued, this can only be done by an index—most naturally, pointing to a dot on the diagram and saying "this station," but equivalently by using a letter or number inscribed on the diagram as an index and designating the point by a context-relative utterance of 'station B'. We will have more to say soon about such indexical uses of symbols, which merely mask, but do not eliminate, their ostensive character.

But Carnap, under the influence of a rigid anti-psychologism of the Fregean kind, has already decided that there must be a way to eliminate indexicality from scientific language because, he insists, it involves an unacceptable subjective element which violates the intention of objective reference.

For science wants to speak about what is objective, and whatever does not belong to the structure but to the material (i.e., anything that can be pointed out in a concrete ostensive definition) is, in the final analysis, subjective.\[^4\]

This, of course, is true only on a Russellian/phenomenalist view that the referents of demonstratives are ultimately private sense-data, but Carnap took the stricture against ostensive reference very seriously.

This does not mean that Carnap fails to recognize that the meanings of natural language expressions are sensitive to contextual factors. In fact he presents (§141) an outline of how a "constitutional definition" of the sign relation would need to take contextual influences into account. He notes that the sign relation cannot be assumed to be a simple matter, such that "the first occurrence of a sound will always allow us to infer its meaning"; a child does not learn 'red' from a single occurrence of it associated with a red apple. The meaning can only be determined by assigning various "weights" to possible meanings, and then adjusting these weights, in the light of further evidence, according to a set of rules. His examples show that many of these rules are contextual in nature—"The rules would say, for example, that the weight which is assigned to a physical thing, relative to a sign production, rises if

\[^4\] Ibid., p. 29.
the thing is close to the body of the sign-giver at the time of the sign production." He says, in fact, that "the most important assignment of weights to objects, albeit the most difficult one, arises when (in realistic language) a word is understood through its context." Like the child learning 'red', the scientist must use context to deduce word meanings.

Understanding how contextual factors influence ordinary word meanings can also, he thinks, play a role in solving philosophical problems; he considers (in §159) the classic Fregean "identity problem"—that distinct referring expressions can be used to make an identity statement which is both true and informative. After noting that substitution salva veritate is the logical definition of identity, he note that confusion can arise, even in science, because identity is "not always taken in the strictest sense." He illustrates this with a revealing set of examples, prominently involving the demonstrative 'this', along with the expression 'the same':

The question, "Do you already have this book, this butterfly?" does not mean the indicated object itself, but the kind, as whose representative the object is taken. This improper identification can have various different aspects, as can be seen from the following four sentences: "The public transport system in A has the same trains as that in B." "Today, I came home on the same train as yesterday, namely, on the 6:12." "This is the same train that used to run on Route 10." "I was sitting in the train you saw go by."5

Carnap explains that contextual factors allow 'train' to be use to refer to a variety of different entities, of different kinds or categories—the make and model of train, the individual train, the schedule slot, etc. Carnap uses the example to illustrate the loose way in which the notion "the same" is often used, leading to confusion about the nature of identity statements. But what the example also shows is that 'this book', 'this butterfly' and 'this train' can be used in different contexts, not only to refer to different things, but also to refer to different kinds of things. That he calls these usages "improper" reveals his attitude toward such contextual shifts in meaning. But he goes on to explain how such "improper uses" can, nonetheless lead to scientifically reputable identification of interesting types or classes of entities.

It is remarkable that occasionally the temporal sequence of concept formation is such that, first, a relation of the first-described sort is linguistically taken as identity, and that the higher-level object which justifies this usage is constructed

5 Ibid., p. 152.
only afterward. In fact, the higher-level object is constructed, as it were, precisely through the improper use of language.⁶

If it results in the creation of scientifically useful concepts, one wonders on what basis these linguistic uses are labeled "improper." Clearly these examples are meant to illustrate cases where people succeed in communicating with one another—where the contextual processes do, indeed, work to secure definite reference. So why not include such context-sensitive expressions in the official language of science? Is the problem that these uses are dependent on subjective factors and are not yet justified by a structural logical reconstruction? But remember, the purpose of Carnap's whole project in the Aufbau is supposed to be to provide improved conceptual clarity where it was lacking, not to require that all our usual concepts be held in doubt until they can be justified by eventual logical reconstructions. Just to get started on such a reconstruction one must take a system of unreconstructed concepts for granted; which concepts these are will display the motivations and the limitations of the project. It seems that Carnap has from the start set his mind on showing that a scientific description of the world can—in fact must—be given entirely in an extensional language free of subjective influence, and thus free of context-sensitive expressions.

The Thesis of Extensionality and its Decline

A serious flaw in the Aufbau program, closely related to this linguistic restriction, originates in the crucial "thesis of extensionality," presented in §43 as follows:

In every statement about a concept, this concept may be taken extensionally (i.e., it may be replaced by its extension). . . . More precisely: in every statement about a propositional function, the latter may be replaced by its extension symbol.⁷

In §45, Carnap clarifies this to the plausible—almost trivial—claim that, when 'concept' is understood to mean only propositional functions as he has conceived of them, rather than to mean "contents of a representation or thought," and when even statements about the thought expressed by (corresponding to) a propositional function are considered as statements about that propositional function, then all statements about concepts are extensional. Then, without pausing for breath, Carnap extends this modest, circumscribed

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⁶ Ibid., p. 254.
⁷ Ibid., p. 72.
result to the startling claim that all statements are extensional. But this generalization is clearly untenable unless examples of statements about "the senses of propositional functions" which he has just considered (e.g., 'I believe there are things which satisfy \(<x \text{ is a man}>\) can be reduced to extensional statements about statements or objects, and he gives no reasons to think they can.

In spite of the thin argumentation in its support, the generalized version of the extensionality thesis plays an enormous part in the official project of the Aufbau. Yet it soon becomes clear that Carnap's thesis is really a case of stipulated meaning rather than a discovery about "all statements." What lies behind the thesis is Carnap's strategic decision to aim at a theory that takes as its data only extensional statements because those are the only ones he is interested in—his constructional system is "concerned exclusively with logical, not with epistemic value [of statements]; it is purely logical, not psychological" in his particular senses of 'logical' and 'psychological'. The crucial role of a Fregean version of anti-psychologism is again apparent. The hard-line Aufbau version of the thesis does not, however, last long.

In the Logical Syntax of Language Carnap admits that there are, in fact, many intensional languages, and thus makes explicit his retreat to a more defensible version of the extensionality thesis. He says that the available arguments support, not the necessity,

but merely the possibility of an extensional language. For this reason we will now formulate the thesis of extensionality in a way which is at the same time more complete and less ambitious, namely: a universal language of science may be extensional; or, more exactly: for every given intensional language \(S_1\), an extensional language \(S_2\) may be constructed such that \(S_1\) can be translated into \(S_2\).

Carnap proceeds to give examples of potential problems—intensional sentences which have been considered by various philosophers in support of the need for intensional language for certain scientific purposes. He considers the now standard examples of propositional attitude reports and modal statements, and shows how they can be "translated" into an extensional metalanguage. He thinks the source of insistence upon intensional language is a mistaken notion of the relation between meaning and the consequence relation.

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8 Ibid., p. 84.
But the decisive point is the following: in order to determine whether or not one sentence is a consequence of another, no reference need be made to the meaning of the sentences. The mere statement of the truth values is certainly too little, but the statement of the meaning is, on the other hand, too much. It is sufficient that the syntactical design of the sentences be given. . . . It is theoretically possible to establish the logical relations (consequence-relation, compatibility, etc.) between two sentences written in Chinese without understanding their senses, provided that the syntax of the Chinese language is given. 10

The problem indexicals pose for such a view is obvious, since syntax alone is incapable of explaining the logical relations among sentences containing them. Carnap's view of the role of logic in the work of science, coupled with this syntactic picture of the nature of logic, leads naturally to the requirement that the language of science be free of indexicality.

By the time of Meaning and Necessity Carnap's attitude toward the thesis of extensionality was decidedly ambivalent. He admitted that the thesis had not been proved, and further, that proposed "translations" from intensional languages into extensional languages often alter sentences in non-trivial ways; specifically he says that a translation which is "intentionally isomorphic" is "obviously impossible between a non-extensional and an extensional sentence." 11 But at the time of the Aufbau these doubts about the expressive power of extensional languages still lay in the future. It is important, however, to see that omens of the eventual failure of that and related enterprises are clearly visible in the earliest attempts to conduct scientific discourse in a language stripped of indexical expressions.

Carnap's Attempt at Indexical-free Languages with Empirical Content

In the Aufbau, as we have seen, Carnap envisioned a language for science in which objective referential relations are conceived as structural and thus 'descriptivist' in the sense associated with his teacher Frege. Carnap summarizes his view as follows:

The representation of the world in science is fundamentally a structural description. By the definite description of an object is meant a unique characterization of that object, i.e., a characterization which allows an unequivocal identification of that object in the object domain in question. Thesis: every object of science can be uniquely characterized within its object domain through mere structural statements. Hence it is in principle possible to transform all statements of science into structural statements; indeed, this transformation is necessary if science is to

10 Ibid., pp. 258-9.
11 Meaning and Necessity. p.141.
advance from the subjective to the objective: all genuine science is structural science.\textsuperscript{12}

Contextual influences on the meanings of indexicals, as noted above, are among the subjective features of language which it was the aim of this project to eliminate.

In the \textit{Aufbau}, Carnap's goals were clear, but his methods were largely programmatic. By the early thirties there were signs of new ideas about how that task should be carried out. Influenced by Gödel's metamathematical success, and by Tarski's methodological innovations, Carnap began to speak of philosophy as "metalogic." The effect of this change are clearly visible in the \textit{Logical Syntax of Language} (LSL) and related papers. Firstly, he explicitly adopts the "two language" approach to linguistic analysis: an object language is explicitly distinguished, in regard to its formal properties, from a metalanguage (Carnap calls it a 'syntax-language'), which is used to describe it. This distinction makes possible Carnap's well known distinction between the "material mode" and the "formal mode" of speech which was to be the basis for identifying and dissolving metaphysical pseudo-problems. Secondly, LSL displays the solidification of the set-theoretic ideas implicit in the hierarchical constructions of the \textit{Aufbau}. These developments, together with the avowed focus on constructing a language for unified science, move the investigation further away from any interest in the referential anomalies of natural language. But issues about reference could not be entirely avoided. A system of symbols, whether formal or not, can only be given empirical content if its expressions can be linked referentially to objects. As in the \textit{Aufbau}, so in LSL Carnap thinks this relation can be characterized structurally within the syntactic system, without relying on "subjective connections" to non-linguistic entities. In fact he again explicitly excludes indexicals from consideration:

\begin{quote}
In pure syntax, only syntactical properties of expressions, in other words those that are dependent only upon the kind and order of the symbols of the expression, are dealt with. As opposed both to the symbolic languages of logistics and to the strictly scientific languages, the common word-languages contain also sentences whose logical character (for example, logical validity or being the logical consequence of another particular sentence, etc.) depends not only upon their syntactical structure but also upon extra-syntactical circumstances. . . . In the case of sentences in which words like 'I', 'you', 'here'. . . and so forth occur, the logical character is not only dependent upon the preceding sentences, but also upon the extra-linguistic situation—namely, upon the spatiotemporal position of the speaker. In what follows, we shall deal only with languages which contain \textit{no expressions which depend upon extra-syntactical factors}. . . . In the case of
\end{quote}

\textsuperscript{12} \textit{Aufbau}, p. 43.
sentences having extra-syntactical dependence, this invariance can be attained by means of the addition of person-, place-, and time-designations.\textsuperscript{13}

Carnap is here expressing the faith that a scientific language can be structured in a way that can express any legitimate empirical content ordinarily expressed using indexicals, and he gives exactly the recipe for the elimination of indexicality in favor of descriptive designation which we found in Frege and in the first edition of Husserl's \textit{Logical Investigations}. Accordingly, when Carnap talks about referring expressions, he mentions names and definite descriptions but not demonstratives. Instead, he describes a kind of quasi-demonstrative form of reference which he calls "description by means of the statement of spatiotemporal position (indirect description, so called ostension)." He says, for example, that (a particular utterance of?) the expression 'alea iacta est' can be designated "ostensively" by means of the description "Caesar's remark made at the Rubicon."\textsuperscript{14} His other examples of "so-called ostension" are especially instructive. To designate a concrete distance (something that is unavoidable if geometry is to be applied to the subject matter of physical science) he offers the following "definition in use" of a Meter: "A physical segment is said to have the length of 1 when it is congruent with the segment between the two marks on the standard meter measure in Paris." A statement of the syntax language which is supposed to play the same designative role is "'\(\text{Nu}(x)\) is to be taken as true when and only when a character which has a sufficient resemblance in design to the character occurring at such and such a place (for instance of this book) is to be found in position \(x\)." These are just the kind of designation via existential relations to physical objects which Peirce considered prime examples of indices. Both meter bars and printed shapes are extra-syntactical factors. Carnap's curious use of 'this book' only emphasizes the context-relative aspect of his example. Yet he denies that there is any indexicality here, though he characterizes these definitions as "ostensive definitions, where the term is defined by the stipulation that the objects comprehended by the term must have a certain relation (for instance congruence or likeness) to a certain indicated object." This is just what Peirce would say; the relevant object must be "indicated" by some "real connections" to the language user and a token of the expression. But Carnap continues: "In linguistic formulation the ostension takes the form of a statement of the spatiotemporal position. It is to be noted that, according to this, an ostensive definition likewise defines a symbol by

\textsuperscript{13} Ibid., p. 168.
\textsuperscript{14} Ibid., p. 155.
means of other symbols (and not by means of extra-linguistic things)."

But this is clearly not what he has just illustrated. A patch of ink on a page, even if it forms part of the inscription of a sentence, is still just as much an "extra-linguistic thing" as the meter bar in Paris is. If the meter bar were used as part of a sculpture which spelled out the word 'PARIS' it would not, thereby, become a "linguistic thing" in relation to its use as a standard object of size comparison. No more does a patch of ink being printed in a book, and having a shape reminiscent of a letter, make it a "linguistic thing" when it is functioning purely as a standard for geometric shape comparison. Furthermore, in both cases, the comparison cannot take place until the standard object and its relevant aspect are identified by the person doing the comparing, in a way which would be naturally expressed as "this bar, from here to here" or "this ink mark located here, on this page of (yes) this book." If someone does not know this, they do not understand what 'meter' or 'Nu' mean. But Carnap will not allow his scientist to express these things in such language.

What we have identified here is a by now familiar pattern. First, indexical are recognized as a problem for a view of language which takes stable meanings as paradigmatic. Then, as in Frege and the early Husserl, it is claimed that indexicals are (in principle) eliminable, at least in language intended for the use of science, in favor of descriptions based on spatiotemporal location. Because Carnap's project in LSL is purely syntactic, he feels he has done all that is needed by diagnosing a problem and recommending a cure, and goes on with his work, assured that science can get along with a purely extensional language free of context-sensitive expressions. Nevertheless, he continues to use indexicals at crucial points in the presentation of his theory.

This self-assured policy is especially apparent in the aspect of the theory most concerned with empirical content—the protocol sentences. Carnap remarks that "syntactical rules will have to be stated concerning the forms which the protocol-sentences, by means of which the results of observations are expressed, may take." Since only object languages without context sensitive expressions are under consideration, it is of course being assumed that the scientists would be able to report their observations fully without the use of indexicals. This confidence seems to be based solely on the Aufbau argument, criticized above, that it is possible to establish descriptive referential relations purely on the basis of a structural description. In LSL, this optimism is expressed via the notion of a "coordinate language" in which objects are designated by means of a (multi-dimensional) coordinate system. Carnap was clearly taken with the idea.

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15 Ibid., p. 80.
16 Ibid., p. 317.
A language which is concerned with the objects of any domain may designate these objects either by *proper names* or by systematic positional *co-ordinates* . . . for instance, house-numbers, . . . Ostwald's designation of colours by means of letters and figures, . . . the designation of geographical places by their latitude and longitude, . . . and the designation of space-time points by four co-ordinates. . . . The method of designation by proper names is the primitive one; that of positional designation corresponds to a more advanced stage of science, and has considerable methodological advantages over the former. . . . We shall take co-ordinate-languages for our object-languages.17

The view being assumed here, that structural isomorphism between a multidimensional abstract space and the actual world was sufficient, in principle, to establish determinate reference without indexicals, was conclusively refuted by C.H. Langford and Arthur Burks (see below). But even without the Peircean criticism, noted above, or the explicit refutation by Langford and Burks, there are strong indications in Carnap's own text that his view is incoherent. The only detailed application of coordinate designation he gives is in connection with his discussion of how a language can be used to describe its own syntax. Carnap tries to give examples to show how this is possible.18 He claims that by using coordinate designations in a purely formal language he can refer to specific symbols and expressions of that very language. But when he explains the connection between the designating expressions and the symbols or expressions they are to designate he always ends up resorting to one indexical device or another. For example, his formal "Language I" has, he claims, the ability to make a "descriptive syntactic statement" corresponding to the English sentence 'On page 33, line 32 of this book, an expression of the form \( \zeta = \zeta \zeta \) occurs (namely 'x=2').' In support of this claim he says:

> Since Language I has sufficient means of expression at its command for the purpose of describing the property of a domain of discrete positions, a descriptive-syntactical sentence of this kind may be formulated in I no matter whether it describes an expression of another language or an expression of I itself . . . Let us now designate the position on page 33 at which 'x=2' begins, by 'a'.

18 It may at first seen pedantic to question this possibility, since familiar natural languages are obviously capable of discussing their own syntax. The issue here, however, is whether a "language" with the formal characteristics Carnap stipulates, including the prohibition on context-sensitive expressions, could be used to make statements about its own syntax (or anything else, for that matter).
Then the aforementioned descriptive-syntactical sentence can be formulated in \( I \) in the following manner:

\[
'\text{Var} \ (a) \cdot \text{Id} \ (a') \cdot \text{LogZz} \ (a')^{19}
\]

This formula is to be read: at location \( a \) there is a variable; at location \( a' \), an identity sign; at location \( a'' \) a numerical symbol.

But that formula does not make a statement about any symbols anywhere until it is supplemented by the italicized stipulation of the designation of \( 'a' \) The "domain of discrete positions" which \( I \) can describe is a purely abstract space of formal relations, not the empirical space in which printed characters exist. The system of positional designations cannot ground reference until it is "anchored" to the physical domain of the intended objects of reference. And, as we have also seen, this anchoring always involves indexical devices. In this case, the anchoring makes use of the phrase 'the position on page 33' which, itself, is insufficient without the contextual supplementation of the earlier phrase 'of this book.' The reader is being told (in English) that the Language \( I \) symbol \( 'a' \) is to designate something located in the very book in the readers hands.\(^{20}\)

Contrary to Carnap's claim, it seems that Language \( I \) can only make descriptive syntactic statements when supplemented by (indexical containing) statements in another language—a natural language. This is not just a problem for syntactical descriptions, but seems to apply to all descriptive statements in Carnap's languages. Remember that one of his important goals is to show that a formalized language can serve the purposes of physical science. Accordingly he tries to show how physical predicates and laws can be incorporated into such a language and used in scientific reasoning. But when he gives examples, the singular statements (which would correspond to the protocol sentences) invariably contain indexicals: "This body \( a \) is of iron," "In this vessel \( b \) of volume 5000 c.c. . . . " etc. But without indexicals, how is the language to express these? Through a coordinate system? But no coordinate system can pick out an actual location without, itself, being anchored indexically.

But it might be objected that Carnap is, in fact, able to achieve determinate reference within Language \( I \) through his Gödel-style numbering system (the 'zei' functor), where


\(^{20}\) A similar situation arises in the discussion of self-referential syntactical statements in Language II (p. 131); again, the expressions are designated by positional coordinates, but this time Carnap interjects: "Example: At the places \( a \) to \( a+8 \) (indicated, say, by numbered positions on a piece of paper)." Which piece of paper? One might well ask.
numeric symbols of I are used in specified syntactic contexts to designate other symbols and expressions of I. But again the relevant sentences of I made with these numeric symbols can only be interpreted to be about the intended symbols relative to the assignment of the numbers to those symbols; in David Kaplan's terminology, if a 'zei' symbol is to function as a proper name for another symbol, a dubbing must take place. Carnap proposes to do this via an explicit extensional definition of the 'zei' relation. But such a definition—a series of statements of the form: "'α' is to be a name of 'β'"—cannot be given in I, since such a definition would already require that I have the capacity, without 'zei', to designate its own expression. The impression that the arithmeticized syntactic sentences are statements in I about symbols and expressions of I is conveyed by the fact that Carnap gives, in the surrounding text, a partial specification of 'zei'. Specifically, he provides a chart (on p. 55) in which the symbols of I are typographically aligned with their corresponding numerical symbols—their "term numbers." Although this does not make explicit use of indexicals, it is a clear example of Peircean indices, since it is the physical alignment of the symbol tokens on the page which establishes the referential relation which is to hold between the corresponding symbol types. David Kaplan has provided an analysis of this practice of "dubbing by typographical display" which makes clear its implicit demonstrative nature. It obviously involves "extra-syntactic factors" which the Language I is supposed to eschew. For some of the other constituents of the 'zei' relation, Carnap merely stipulates the correlation, but always in the surrounding natural language text, never in Language I itself; Reichenbach has shown, as we shall see, that definitions like these also involve implicit indexicality.

This problem of expressing empirical content in Carnapian formal languages using only purely syntactic resources is not just restricted to reference to objects; it also arises in regard to descriptive predicates. Carnap considers how a syntactic system is to represent predicates like colors—'Blue' or 'Red'.

We can replace them by a single [descriptive functor], say 'col', by numbering the colors in some way, and stipulating that 'col (a) = b' shall mean: the position a has the colour No. b.  

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22 Since Carnap takes predicates to designate property extensions, it is really the same problem—rather than fixing reference to a single object, what is needed is a way to fix reference to a set of objects, or to a set of sets, as in the 'zei' functor.
23 LSL, p. 55.
There is the usual problem of how "position a" will be fixed relative to the actual world, but there is an additional problem about how the colors are to be numbered "in some way." Elsewhere he talks about the three dimensional "color solid" and Ostwald's system of designating colors via co-ordinate addresses within it, so one can imagine that he was contemplating some kind of coordinate system for designating specific locations in that "color space"; analogous systems could be used for defining empirical predicates within other kinds of "quality spaces." But the same problem of "anchoring" comes up in the color coordinate system that we saw in the spatiotemporal system. In its purely formal properties, the "color space" is completely symmetrical; there is nothing in the formal structure to distinguish blue from red, black from white. Achieving determinate content would require identifying at least three points with specific colors. This can be done easily in English or German, using indexicals and samples, but not in I or II (nor, apparently, in any purely syntactic system).24

Another telling use of indexicals occurs when Carnap explains limited and unlimited quantifiers. His limited universal operator ranges over a numerically specified domain of quantification. But if quantification is to be used in descriptive statements with empirical content, this abstract domain must be linked in some way to concrete objects. Peirce, we know, claimed that the universe of discourse is always specified indexically. Carnap's example of limited quantification—"All the pieces of iron on this table are round"—seems to bear that out.25 Attempts to eliminate the reference to "this table" in favor of coordinates in an unanchored system would meet the familiar difficulties.

So Carnap's claims that it is possible to define symbols as referring to syntactical objects, or to any other kinds of entities, strictly "by means of other symbols and not by means of extra-linguistic things" is in no way established by his own attempts. Without the use of an indexical-containing metalanguage, or other context-sensitive devices, he seems unable to provide any descriptive content for his formalized languages. In Tarski's work on the properties of formal languages we will find reasons to believe that this problem is not

24 At ibid., p. 80, Carnap gives—in an example of metric geometry—the relation between the standard meter and the wavelength of a certain spectral line of cadmium. But attempts to set color standards by means of wavelength specifications fail to avoid dependence on indexicals—as Peirce explicitly argued, and as Husserl came to appreciate—since the metric scale in which the wavelengths must be specified is anchored in a specific physical structure or sample, which must ultimately be indicated indexically.

25 Ibid., p. 48 (my italics).
peculiar to Carnap's languages, but is, as Peirce claimed, endemic to any language without indexicals.
Tarski on the Expressive Limits of Formalized Languages

Alfred Tarski has exercised, directly and through Carnap, an important influence on both the aims and the methodology of philosophical semantics. In order to evaluate later applications and extensions of his methods, it is helpful to have a clear picture of his original, narrowly restricted project. As a mathematician, Tarski's main aim, as he reminds the reader often, is to clarify the foundations of "the deductive sciences." As is well known, he feels he can do this in spite of the fact that his method of formal definition and deduction cannot be carried out consistently in any natural language. The reason this cannot be done is that an analysis of the deductive sciences must unambiguously display complex chains of inference, and thus is best carried out in a language with a clearly defined formal structure. It is the lack of certain specific formal characteristics which disqualifies natural language from such a use; one such "flaw" is its expandability or "universality."

A characteristic feature of colloquial language (in contrast to various scientific languages) is its universality. It would not be in harmony with the spirit of this language if in some other language a word occurred which could not be translated into it; it could be claimed that 'if we can speak meaningfully about anything at all, we can also speak about it in colloquial language.'

Unfortunately for Tarski's project, this unlimited expressive power of natural language means there is no barrier to logical inconsistency; the resources of colloquial language unavoidably allow the construction of paradoxes such as the Liar. It is noteworthy that indexicality is involved in all the examples of the Liar which Tarski considers. The ability of natural language indexicals to refer to their own token expressions is one of the features which one would wish to eliminate from a language in order to avoid paradox and formal inconsistency. These considerations, he says, "prove emphatically that the concept of truth (as well as other semantical concepts) when applied to colloquial language in conjunction with the normal laws of logic leads inevitably to confusions and contradictions." Thus he rules out the direct application of his methods to the semantics of unmodified natural languages. Nonetheless, he holds out the hope that "exact methods" may provide some indirect benefit to our understanding of colloquial language if such a language is "split" into substructures which meet formal criteria. But he is not sanguine

27 Ibid., p. 267.
about how much this can contribute to our knowledge of the peculiar characteristics of natural language.

It may, however, be doubted whether the language of everyday life, after being 'rationalized' in this way, would still preserve its naturalness and whether it would not rather take on the characteristic features of the formalized languages.\textsuperscript{28}

Subsequent attempts at the development of a formal semantics for natural language allow us to check the accuracy of Tarski's conjecture.

Although Tarski himself participated in the growing interest philosophers of language showed in what might be achieved by applying his methods beyond the scope of the "deductive sciences," still, he remained clear about the inherent limitations that came with non-formal subject matter—in particular, the study of the actual language practices of working scientists. In his 1944 restatement of his account of truth he recognizes "the fact that in empirical research we are concerned only with natural languages and that theoretical semantics applies to these languages only with certain approximation." But this does not mean that (formal) semantic notions are not in any way involved in empirical sciences. This situation is, he says, just like "when we attempt to apply a theoretical science to empirical problems. . . . The relation between theoretical and descriptive semantics is analogous to that between pure and applied mathematics."\textsuperscript{29}

Although this indicates that there is some place for formal semantic notions in analyzing the meaning of empirical statements, it is important to see the restriction on the range of semantic conclusions imposed by this analogy. Since "the semantics of formalized languages is constructed in a purely formal way" its conclusions are of a conditional nature—given a set of intuitively clear primitive concepts (inevitably explained or stipulated in natural language) it spells out the truth conditions of all complex expressions of a formal system. It tells us, for example: \textit{given a satisfaction relation}, this is what truth amounts to. In this way, it shares the character of constructive formal systems, like those of Carnap and Goodman, in which, an extensional specification of the primitive relation is assumed; "given a pair list, a description of the world can be constructed thusly. . . ."

\textsuperscript{28} Ibid.
\textsuperscript{29} "The Semantic Conception of Truth and the Foundations of Semantics." \textit{Philosophy and Phenomenological Research}, 4 (1944), p. 365. Alanzo Church, we shall see, makes a parallel, but significantly different analogy, comparing the relation between formal and natural languages to that between applied mathematics and practical activities, such as surveying and map-making.
But clearly this is not a trivial assumption—and not one which a general theory of meaning will want to make. A specification of a satisfaction relation, or of a "pair list" which links objects with their properties, will require the meaningful use of a language. The evidence of Carnap's attempts to do this job using a language without indexicals casts doubt on this possibility, and this, in turn, leads to doubts about the completeness of a formal semantic theory which merely assumes this fundamental semantic relation. A theory cannot, we know, explain what it assumes.

On the other hand, Tarski's characterization of the subsequent stages of empirical reasoning, of the relations among the truth conditions of simple and complex statements, and of the validity of arguments relative to the presupposed meanings of simple expressions, is clearly valuable. *Once we have a simple meaningful language* Tarski's theoretical apparatus provides a way of clearly stating one of the things scientists (and all other rational reasoners) are trying to accomplish with it: valid chains of reasoning leading from simple statements to complex ones, from particular ones to general ones, etc. Carnap, Tarski, Goodman, and others working in this tradition may therefore be seen as attempting to characterize as accurately as possible an objective *goal* of human scientific activities, and of the assertive uses of language in which those activities are conducted. And we may appreciate and use these results, even if we discount what they have to say about the *means* by which language users can and do pursue those goals.\(^{30}\) We shall indeed see examples of specific model-theoretic constructions which give a good representation of specific semantic goals. There can be no objection to those representational programs, as long as the conditional nature of their results is not forgotten—and as long as the promissory note on an explanation of the meanings of simple expressions is eventually cashed out.

As we will see, the eventual attempt to complete the explanation of the assertive uses of language in a way that does justice to both the objective goals of assertion and to the subjective and intersubjective means by which natural language users pursue those goals will make essential reference to contextual features, and this will dramatize the limits on the usefulness of Tarski-like methods. In particular, investigations of the context-sensitivity of simple terms such as proper names and demonstratives will suggest that the use of set-theoretic models, which revolutionized the study of the structure of formal deductive

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\(^{30}\) Tarski, like Carnap, cherished the hope that problems caused by the use of natural language in scientific work could eventually be avoided if "languages with specified structure could finally replace everyday language in scientific discourse." Cf. "Semantic Conception of Truth," p. 347.
sciences, is not straightforwardly transferrable to the study of natural language. For example, the notion of an *assignment of values* to variables, which can be given a straightforward extensional definition in a system with a conceptually closed domain such as the natural numbers, becomes problematic when applied to natural language where the domain of possible referents is open-ended and conceptually amorphous (this is just a manifestation of the "universality" of colloquial language to which Tarski drew attention\(^3\)). As David Kaplan would eventually realize, specifying an "assignment of values" for the variable-like expressions of a natural language is tantamount to giving a theory of direct reference for context-sensitive expressions. And this, we might expect Tarski to agree, cannot be done in a formal language.

**Charles Morris and the (Gradual) Revival of Peircean Semiotics**

A fateful event for the treatment of indexicals and context by Anglo-American philosophers occurred in the mid-thirties, when Neurath and Carnap invited Charles Morris to join in their project to create the *International Encyclopedia of Unified Science*. Morris, an American pragmatist, was very familiar with Peirce's writings, which had finally been published between 1931 and 1935. As a co-editor of the *Encyclopedia*, Morris attempted to inject Peircean insights about the importance of the relational and contextual features of meaning into the logical positivist movement. This attempt is plain in his two contributions to the *Encyclopedia's* first volume. In his introductory essay he sets the work of formal logic into a synoptic framework which highlights its relation to other features of language:

> Formal disciplines become compatible with empiricism when they pass from the status of rival methods for the knowledge of nature to that of being formal linguistic structures available to the natural sciences as methodological tools. Logic thus rests, as Peirce maintained, on a general theory of sign, formal logic tracing the relations between signs within a language.\(^3\)

After praising the contributions of Russell, Carnap, and others to formal logic, he continues:

> This union of empiricism and methodological rationalism requires completion by one further step. . . . The introduction of such terms as 'convention', 'decision',

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\(^3\) In fact, one characteristic use of demonstratives is to expand the domain of possible referents—to refer to objects not previously within the referential reach of language.

'procedure', and 'rule' involves reference to the users of signs in addition to empirical and formal factors. . . . The introduction of pragmatic considerations avoids the extremes both of empiricism and of conventionalism in logical theory while yet doing justice to both.33

Introducing these "pragmatic considerations" will, he hopes, be one essential part of the Encyclopedia's program of making explicit the nature of science; as a first small step, Morris introduces a now familiar trichotomy:

Science, as a body of signs with certain specific relations to one another, to objects, and to practice, is at once a language, a knowledge of objects, and a type of activity; the interrelated study of syntactics, semantics, and pragmatics of the language of science in turn constitutes metascience—the science of science.34

In his other contribution to the first volume, "Foundations of the Theory of Signs," Morris expands on this trichotomy, portraying it as the outline for a Peircean program of general semiotics. In what amounts to an open invitation to anyone sympathetic to logical positivism to investigate the relevant ideas of Peirce ("whose work is second to none in the history of semiotic"35), Morris sounds all the crucial Peircean themes: the three components of semiosis—sign [vehicle], designatum and interpretant; the division of signs into icon, index and symbol; the necessary role of indexicality in all empirical statements; the role of indexicality in the meanings of bound variables; and many others (although he does not always make explicit the Peircean origins of these ideas).

Most relevant to the study of context is his reminder of Peirce's claim that:

In the end the interpretant of a symbol must reside in a habit and not in the immediate physiological reaction which the sign vehicle evoked or in the attendant images or emotions—a doctrine which prepared the way for the contemporary emphasis on rules of usage.36

What Morris had in mind here was William James's ideas about rules, and also G. H. Mead's concerns with "the social context in which such signs arise and function," but the simultaneous developments in Wittgenstein's thought, and their aftermath obviously form part of the trend Morris is at pains to point out. "There is," he insists, "a pragmatischal

33 Ibid., p. 67.
34 Ibid., p. 70, (my italics).
36 Ibid.
component in all rules," including in particular the syntactic and semantic rules which are
the proper objects of the first two divisions of the study of language. It is only when one
has seen that "the statement of the conditions under which terms are used, in so far as these
cannot be formulated in terms of syntactical and semantical rules, constitutes the
pragmatical rules for the terms in question" that one can give

the full characterization of a language:
A language in the full semiotic sense of the term is any intersubjective set of
sign vehicles whose usage is determined by syntactical, semantical and
pragmatical rules.37

Morris holds out the promise that "the concept of sign may prove as fundamental to the
sciences of man [including the science of science] as the concept of atom has been for the
physical sciences or the concept of cell for the biological sciences." His plea to the reader is
this: in order to fulfill this promising future "the unity of semiotic" must be taken seriously.
He makes this plea diplomatically—without dwelling on the earlier inadequacies stemming
from a too narrow focus on syntax and non-indexical semantics which are apparent, for
example, in Carnap's work.

This is a plea that bears repeating even now, since there continue to be strong pressures
within the philosophy of language for research in syntax, semantics, and pragmatics to
respect artificial disciplinary boundaries. Morris warned of the dangers of fastening
attention upon one dimension, while ignoring its interrelation with the other two.
Syntactics, semantics and pragmatics are interdependent, but mutually irreducible,
components of the single science of semiotics.38

Unfortunately Morris's invitation to the logical positivists to investigate what Peirce
might contribute to their project went largely unheeded; ironically the one part of Morris's
work which did become widely known, and influential, the syntax/semantics/pragmatics
trichotomy, came to be used by formally-oriented philosophers as an excuse to avoid,
rather than an impetus to engage, questions about context and context-sensitivity. An
example of this is a comment by Benson Mates in 1957:

We have all heard the wearying platitude that "you can't separate" the meaning of
a word from the entire context in which it occurs, including not only the actual
linguistic context but also the aims, feelings, beliefs, and hopes of the speaker, the
same for the listener and any bystanders, the social situation, the physical

37 Ibid., p. 113.
38 Ibid., p. 130, p. 132.
surroundings, the historical background, the rules of the game, and so on ad
infinitum. There is no doubt some truth in this, but I fail to see how it helps one
get started in an empirical investigation of language. . . . It seems to me that there
is much to be said for the well-known syntax-semantics-pragmatics division, and
that often many of the factors which ordinary language philosophers find in
common among the cases in which an expression is employed belong more to the
pragmatics of the expression than to its semantics.39

Morris's plea that the interconnection of the three dimensions of semiosis should exercise a
constant influence on practitioners of all three divisions was sadly lost, at least on one
influential segment of the philosophy of language.

In due course, nonetheless, Morris's work, along with the wider availability of Peirce's
writing, did have a positive effect, touching off a spate of new theorizing about indexicals
such as had not been seen for several decades. The obstacles to this revival are nicely
illustrated by an exchange which took place in 1948 between Irving Copi and Gustav
Bergmann. Not as reticent as Morris about criticizing the positivist trends in the
philosophy of language, Copi articulated a general critique of the "constructionist " or "ideal
language" style of philosophy, epitomized by Carnap, Reichenbach and Goodman. His
criticism focused on three objections, one of which was the problems posed by indexicals:

The requirement that an 'ideal' language be unambiguous is perhaps improper.
This would seem to follow from the fact that indexical symbols (in the sense of
Peirce) are essentially ambiguous, although in a systematic way, and that
indexical symbols cannot be eliminated from any language that is to be adequate
for expressing singular propositions.40

Bergmann, in his reply, did not deign to address this point, saying that he would only
discuss "the two which I understand."41

Reichenbach's Theory of "Token-reflexive" Expressions

we will consider the response given by Stanley Cavell to this remark by Mates. The
account of context developed there does, indeed, mentions most of the items enumerated
here, although it does not proceed ad infinitum; Mates can thus be considered to have
inadvertently made a contribution to the theory of context.
40 "Language Analysis and Metaphysical Inquiry" Philosophy of Science, XVI (1949), 65-
70, originally presented at the APA in May, 1948.
41 "Two Criteria for an Ideal Language" in ibid., pp. 71-74. Both these papers are reprinted
in Richard Rorty (ed.), The Linguistic Turn.
Unlike Bergmann, Hans Reichenbach, having read Morris, did understand that indexicals posed a serious challenge to the formalization of language for empirical scientific use. In 1947, he broke the long drought of theorizing about indexicality with a detailed consideration of what he called "token-reflexive" expressions. He had apparently been alerted to the peculiarities and importance of indexicals by Bertrand Russell, who showed him (as well as Morris) a manuscript, eventually published as An Inquiry into Meaning and Truth, in which Russell rehashes the theory of "egocentric particulars" and "logically proper names" developed in "On the Nature of Acquaintance." Reichenbach's use of the Peircean terms 'token' and 'interpretant' indicate the influence of Morris, whom Reichenbach consulted in the preparation of the manuscript, and "whose general theory of signs I used for the exposition of the sign nature of language." 43

Reichenbach's account, at §50 of his Elements of Symbolic Logic, is ostensibly a method for translating natural language statements containing indexicals into a Carnap-style extensional formal language without indexicals. Evaluating the success of any such method should begin, of course, from an understanding of the purpose for which the formalization is being proposed. Unfortunately it is not completely clear what Reichenbach's purpose is; he seems to be engaged in two not wholly harmonious enterprises. On the one hand, his book proclaims itself as providing a method of clarifying confused thinking by suggesting alternative formulations in an explicitly defined formal language. It would then be up to the reader to decide whether the proposed reformulation was adequate to the communicative intentions of the natural language original. In the process of formalization ambiguities and pseudo-problems are to be unmasked, and whatever is clear and meaningful in the original is thus to be laid out unobscured for rational scrutiny. 44 On the other hand, there are many places where, instead of saying that an expression "may be symbolized" in such-and-such a way, he says it "must be." Many features of conversational language are branded as

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42 Russell's position in this work may, at first, seem to be a repudiation of the views expressed in "On the Nature of Acquaintance," since he argues, in Ch. 7 that "egocentric particulars" can be eliminated from scientific language in favor of proper names. However Ch. 6 makes it clear that the "proper names" he is talking about are still "logically proper names" of occurrent sense data, picked out by directing acts of attention, and so retain their demonstrative character. What he has done is change his presentation to avoid the confusion between colloquial indexicals (which he considers eliminable, along with colloquial proper names) and the epistemologically and semantically primitive demonstrative names of sense data.


44 C.f. the "Preface" and the remarks on "the deficiencies of traditional grammar" at the beginning of the chapter on "Analysis of Conversational Language."
"improper"—we are told that some expression 'P' "strictly speaking, means" 'Q'; that 'R' "is to be understood as" 'S'; etc. What begins as a descriptive analysis of conversational language, offered in the hope of simulating new methods of grammatical analysis among linguists and grammarians, soon begins to slide into a program for promoting the doctrine of a "scientific philosophy" conducted through the reform of colloquial language. His main interest often seems to be demonstrating that "improper expressions" of ordinary language could (and should) be avoided altogether, or alternatively, could be retained as convenient shorthand, but only when understood as replaceable with "proper expressions" which shared the characteristics of the formalism—extensionality and reference limited to existing spatiotemporal things. This is just the muddled mix of descriptive proposal and prescriptive doctrine which Carnap had earlier pointed to as a source of pseudo-problems, but into which Carnap himself had all too often fallen. It seems only fair, then, to evaluate Reichenbach on two separate scales, corresponding to the two agendas he seems to have been pursuing.

As far as giving a description of a wide variety of indexical (and other contextual) effects, Reichenbach's account is very good. Like Russell, he points out the interdefinability of various indexicals, and like Peirce he recognizes that the expression token has a special role in the determination of meaning, and that this is a distinguishing feature of this class of expressions. He dramatizes some of the peculiarities of indexical reference by creating an artificial indexical in his symbolic language using little arrows as "token quotes" to emulate the reflexive function of such natural language expressions as 'this sentence', 'these words', 'this inscription', etc. In addition, he calls attention to the indexical significance of certain metalinguistic conventions, particularly in regard to the introduction of proper names for expression, as we can see here:

The token-quotes operation leads from a token to a token denoting that token. Let us use little arrows for the token quotes; then the sign

(I) \[\rightarrow a \leftarrow\]

represents, not a name for the token 'a' in (I) but a token for it. [a footnote explains that this last phrase is an abbreviation for another token reflexive-phrase: 'not a name for the token of (I), similar to the token \[\rightarrow a \leftarrow\], but a token for it.]

(I) is not a name because the token (I) is a reflexive token and cannot be repeated.45

He thus introduces an expression type, '→ . . . ←', which acts like a combination of the word 'that' and a pointing finger. It produces a referential event in which the physical location of an expression token between the arrows makes that very token the object of reference.

This potentially confusing passage is as important for what it shows as for what it says. One thing it shows is that, while '→ a ←' is not a name (it means something different each time it occurs), '(I)' is a name, and in each of its five occurrences in the quoted passage, names the same thing, viz. the first occurrence of '→ a ←'. This passage thus makes use of the colloquial metalinguistic device of the display line with its traditional numerical tag in creating names for expressions, and thereby demonstrates, once again, how deeply contextual cues are interwoven into the normal expressive function of language. Reichenbach is aware that he is using token-reflexivity in the metalanguage, as we can see in his explanation of a formal procedure for defining a name for an expression.

If we write the definition
\[
(3) \quad p =_{df} a \lor b
\]
or the corresponding statement using names of these symbols:
\[
(4) \quad 'p' \text{ has the same meaning as '}a \lor b'\]
this statement means, strictly speaking:

- every token similar to \( \rightarrow p \leftarrow \) is equisignificant to
- every token similar to \( \rightarrow a \lor b \leftarrow \)

we see that the application of a synonymous symbol involves application of the relation \textit{equisignificant} between tokens, and of the token quote operation.

There are two points, both familiar from Peirce, being made here. One is that every token of a symbol is an \textit{icon} of other tokens of its type (in virtue of their being recognized by language users as similar in some relevant respect). The other point is that naming, in general, involves the physical procedure of juxtaposing an object with a token of its new name in such a way that the intention to establish the name relation between the object and the name is made clear—as in the placing of a token of '→ a ←' to the right of a token of '(I)'. Peirce, of course, called this indexicality, Reichenbach calls it token-reflexivity. In the process of introducing non-reflexive names for token-reflexive expressions Reichenbach anticipates David Kaplan's theory of "dubbing" in showing just how deeply indexical the giving of names is.

In a similar way we can introduce non-reflexive tokens equisignificant to a reflexive token. For instance we can write
(6) every token similar to $\rightarrow w \leftarrow$ is equisignificant to the token $\rightarrow \rightarrow b \leftarrow \leftarrow$

this statement means that 'w' is the name of the token 'b' in (6). Incidentally, we have already used such a name for (I) by using the symbol '(I)'; writing this number symbol at the side of a reflexive token is to be interpreted in the sense of (6). [footnote: our use of the number-symbol '(I)' as the name of a token differs from the ordinary use of number-symbols for formulas because usually the number-symbols are names of symbols, i.e., refer to any token similar to the one at the side of which they are written.]

He thus provides a philosophically enlightening analysis of his own text to reveal the variety of ways, involving already conventional (traditional and habitual) practices and newly proclaimed practices, in which tokens get used as indices in the establishment of referential relations. Thinking about these features of natural language (which is, after all, the metalanguage of Reichenbach's text) can, indeed, lead to new insights into reference in general and indexical reference in particular. To that extent he succeeds in his first agenda.

In fact, the conventional token-reflexive devices for establishing the reference of names, which Reichenbach's comments brings to light, can be applied directly to Carnap's procedures in LSL. Recall Carnap's claim that the syntax of a language could be discussed within a purely formal language, free of indexical expressions; but clearly any such discussion would depend on the establishment of referential relations between expressions of that language and the primitive symbols of that language. Recall also that Carnap tried to establish these relations in two way: through explicit definition (e.g.: '15' shall be the term number of '=}') and by providing a printed table in his text in which the various primitive symbols were spatially aligned with the term numbers to which they were thereby assigned. The first of these methods (obviously in the metalanguage), although it is not explicitly token-reflexive, is, as Reichenbach has just shown, covertly so. The interpretation of term number tokens "similar to the token '15"" is being established as the type to which all tokens "similar to the token '=}" belong. And his remarks about display-naming show that Carnap's use of the printed table is, like the use of numbered display lines, token-reflexive, in the sense that its meaning is established by the physical relations existing among certain physical things. It is doubtful that Reichenbach intended his remarks to reveal inconsistencies in Carnap's early work, but that result should nonetheless count as fulfilling his agenda of clarifying our understanding of how language is used, especially by philosophers.

46 Ibid., p. 285.
Reichenbach was not equally successful in fulfilling the second part of his agenda—language reform. It is clear by the end of §50 that Reichenbach thinks he has shown that a "proper language" can get along without token-reflexive expressions. His method of elimination, once the Byzantine symbolic exposition is decoded, is fairly straightforward: Everything (i.e., everything worth talking about) which is referred to by token-reflexive expressions are spatiotemporal things or events, and thus, like any other such thing, can be given a name. This name can then be used to make a non-reflexive statement with, he thinks, the same meaning as the reflexive statement. And although giving names sometimes involves using additional token-reflexive devices (either explicitly or implicitly reflexive expressions, or the conventional physical juxtapositions which Carnap used) other cases where Reichenbach "defines" names indicates that he believes that naming can be carried out in other ways, e.g. by the more "scientifically advanced" practice of coordinate identification.

**Goodman's "Improved" Coordinate Systems**

Reichenbach's attitude toward elimination of indexicals—the programmatic motivations for it and the procedure for carrying it out are succinctly stated by Nelson Goodman (using 'indicator' in place of Reichenbach's 'token-reflexive').

Although indicators are of enormous practical utility, they are likely to be awkward for formal discourse. Various remedies may be applied.

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47 David Kaplan's critique of Reichenbach's theory of token reflexives sees in it an underlying flaw shared by all coordinate replacement theories. Although the coordinate description may have the same content as the indexical, it does not have the same meaning, since it does not achieve its reference in the same way—it may, for example require different contextual supplementation; in Kaplan's terminology, it does not have the same character.

48 It should be noted that Reichenbach's elimination of token-reflexivity in §50 is of a piece with his claims to have eliminated reference to a whole series of non-extensional uses of ordinary language in §§46-49. The pattern he follows is to reduce putative examples of references to these "fictitious objects" to references to mental states of speakers, and then to reduce these to bodily states of speakers, and then, presumably, to spatiotemporal coordinate reference to the occurrences of those states. The whole account, thus relies on a strong commitment to a "type identity" physicalist theory of mental states. He does not rely explicitly on this reduction in §50, but he does rely on the notion of 'equisignificance' which, as he had explained in §49, seems to involve objectionable intensional features of just the kind which provoked the reduction elsewhere.

In the final analysis all these remedies rely on either coordinate description or implicit recourse to natural language referential terms. Let us see how this prescription grows out of the internal needs of his theory.

Goodman's perspective is interesting because of his intimate knowledge of the problems and methods of the representational systems of constructionist philosophy. In such systems the coordinate structures used to express information are intended to grow out of a single basic primitive relation. Goodman criticizes Carnap's choice of basic relation as follows:

We might, as Carnap does in the *Aufbau*, suppose some degree of similarity to be taken as standard, and adopt as basic the similarity predicate that applies between two qualities if and only if they are similar to at least that standard degree. In this way, we get a symmetric 2-place predicate as a basis, but only by resorting to the fiction that a certain degree of similarity is taken as standard. Since ordinary language provides no ready way of describing definite degrees of similarity, the standard is presumably to be fixed as the interval between two arbitrarily designated qualia—and two qualia must be selected for this purpose from each of the different categories [e.g., colors, odors, spatial distance, temporal duration, etc.]. The predicate is then presumably to be explained in a roundabout way, as applying between two qualia if and only if they are colors more alike than $c$ and $c'$, or sounds more alike than $s$ and $s'$, and so on.

Note that this designation of samples cannot be done in the formal language itself, since the coordinate-system which would allow descriptive reference to them is exactly what is to be constructed on the basis of the primitive similarity relation, which must, itself already have been defined via the samples. This is just Peirce's point about the need to anchor all coordinate systems by non-coordinate designations—Greenwich cannot be designated as the prime meridian by giving its longitude. Goodman realizes that this circularity is fatal to a system constructed like Carnap's, and he claims to have avoided this (and several other) problem by choosing as a primitive relation what he calls 'matching' between qualia; but similar problems appear when he explains how this relation is to be specified:

Facts concerning the matching of qualia may be thought of as first expressed by statements in which the qualia compared are picked out by descriptions; e.g., "the color of the left-hand one of the two round patches now near the center of my visual field matches the color of the right-hand one." On the basis of all such information at our command, we construct a map that assigns a position to each of the described qualia. Quale names may then be treated as indicating positions
on this map. Indeed, to order a category of qualia amounts to defining a set of quale names in terms of relative position. 50

As with any constructional system, Goodman can't claim that his is anything but an uninterpreted calculus unless he can link symbols with objects of experience. This passage is where he claims to do that, and we need only note that it is done using indexicals: 'left', 'right' and 'my' which Goodman has said ought to be dispensed with "in formal discourse." Thus, the contentfullness of his whole system is riding on the remedy for indexicality which Goodman claims to have.

And this apparent reliance on indexicals is not just a problem for setting up the system; if it were, one might hope that once the system was in place, determinate reference to specific entities (in Goodman's case, specific qualia) could then be achieved via the completed 'maps'. But remember, these maps, like any other, can only be used to state facts when oriented to the territory occupied by the objects to which they are intended to refer. The color-solid for example, as a purely formal structure, is simply a symmetrical arrangement of abstract relations. Goodman realizes the general need for such orientation:

The problem of defining the earlier-later direction along the [time] line . . . has close analogues with respect to all the other categories. If some two individual moments such that one is known to be earlier than the other can be systematically defined, the problem of defining 'earlier than' quite generally is readily solved. The problem of defining the direction thus resolves itself into one of defining the requisite individual names. The same holds true for the problem of defining the black-white direction in the series of grays or the left-right direction along a horizontal line of places in the visual field. 51

But how to define these "requisite names"? He cannot get away with calling them 'this' and 'that.' He realizes that he must make good on the suggestion that there are "remedies" for the awkwardness of including indexicals ("indicators") in formal discourse; when we look for his account of how to do this, we see that he considers two such remedies:

One lies in supplying a freely repeatable name (or description) of the indicator. . . For example, a given 'now' might be identified by any 'The 937th word uttered by George Washington in 1776.' . . . Using such a name we can readily arrive at a repeatable translation of the indicator. . . . Or we may seek a translation that contains no name of the indicator itself, but rather another name for what the

50 Ibid., p. 278.
51 Ibid., p. 357.
indicator names. Thus a certain 'here' is translated by any 'Philadelphia'; and a certain 'ran' is translated by any 'runs [tenseless] on Jan. 7, 1948 at noon E.S.T.'

In other words the remedy is this: using conventional methods for designating spatiotemporal locations—either proper names, coordinate designations or a combination of the two—replace the indexical with a description, either of the token itself or of the object to which the indexical referred. This, of course, is the antithesis of the Peirce-Morris-Copi thesis, mentioned above, of the necessity of indexicals for any language with determinate empirical content. Goodman's attempts at elimination of indexicals—like Reichenbach's and Carnap's—can only succeed if either proper names or coordinate designations can be given determinate content without using indexicals. Presently we will turn to a general critique of coordinate reference schemes. We consider Kaplan's account of proper names in the next chapter.

**Burks on Peirce**

What influence Peirce has had on contemporary discussions of indexicals is largely due to Arthur Burks's 1949 paper, "Icon, Index and Symbol." Burks gives a simplified overview of Peirce's semiotic theory, beginning with Peirce's claim that signs are constituted by a three way relation involving a *sign*, an *object*, and an *interpretant*. He then presents Peirce's division of signs into icon, index and symbol. He notes the crucial Peircean insight that being a sign is not an intrinsic property of an object but an essentially triadic relational one—something is a sign only in so far as it is interpreted significantly. The existence of semiotic properties thus depends on actual acts of interpretation. For example, in the case of *icons*, although it is in virtue of its intrinsic properties that a sign can be an icon of its object, it is only in the actual recognition of similarity by an interpreter that the iconic relation exists. Burks says a sign is not iconic unless the interpreter recognizes it as such. In the case of *symbols*, the link between the sign and its object is by way of a socially established rule or convention. Again the reality of the symbol depends on actual cases of interpreters recognizing it as a case governed by that rule. There are no intrinsic physical properties that identify something as a symbol, or as symbolizing an object, except insofar as the symbol and its object enter into actual human acts of symbolizing. This applies also in the case of indices. The connection between an *index* and its object is, Peirce tells us, direct, physical and automatic. One thing is an index of something else because of their

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52 Ibid., p. 369.
"existential relation" to each other. And yet, even in this case the interpretant is essential to the nature of the index; after all, every object in the universe is related to every other object in some way or other. The index relation is constituted by the relation between the index and its object being of just the right kind to cause a semiotic reaction in an interpreter. Burks also points out the important fact that signs typically combine features of different sign types; onomatopoeic words and demonstratives may be considered as primarily iconic and indexical respectively, but they are nonetheless both words, and thus symbolic as well.53

Burks then states Pierces crucial thesis that "all three kinds of signs are required for a satisfactory general purpose language."54 This thesis implies, of course, what we have been calling the "indispensability thesis"—that indexicals are indispensable in any language capable of making factual statements, or as Burks says, capable of "formulating the procedures and results of the empirical sciences."55 He then gives arguments for both the "practical" and "theoretical" indispensability of indexicals.

His arguments for practical indispensability are by now familiar, but he gives compact formulations which are worth examining. He gives several reasons why indexicals cannot, in general, be replaced by definite descriptions.56 First, if the description is in terms of the intrinsic properties of the object, we must know that it uniquely fulfills that description; but this requires an impossible knowledge of the entire universe. It is his second argument that is aimed at identification by coordinate systems, as proposed by Carnap, Reichenbach, Goodman, etc. His objection is what we have been calling the "anchor problem."

Setting up a coordinate system without the use of indices is subject to the same difficulty, for the interpretant must now know how to characterize uniquely his origin and axes solely in terms of [non-indexical] properties.57

Third, even if reference to an object via a coordinate system could be achieved, it is doubtful that the proposed replacement would really have the same meaning as the

53 Based on examples showing that icons and indices are typically also symbols, Burks goes on to draw the unjustified conclusion that symbols are therefore the most basic kind of sign, though he fails to give any examples of pure symbols.
55 Ibid., p. 677.
56 He mentions replacement by names as well, but does not give explicit arguments for the dependence of proper names on indexicals; for that we must await Kaplan's account of "dubbing."
57 Ibid., p. 684.
indexical original. Both Burks and those of his contemporaries who advocated coordinate replacement favored quasi-verificationist notions of meaning according to which "the meaning of a sentence is whatever must be understood in order to be able to verify that sentence." But "if a person is present when ['This book is red'] is uttered he can verify it by direct observation whereas he would have to determine his location to verify ['The book at x, y, z, and r is red.']." Thus they cannot have the same meaning; there is something that the first expresses that the second cannot. Stripped of the verificationist vocabulary, the same objection can be made with the Frege-like observation that a competent language user could accept the first as true and the second as false, so the two must mean different things.

To refute the claims of Carnap and others that indexicals can be eliminated from a language usable for scientific purposes it is enough to show that such an elimination is impossible in practice. But Burks goes further; he argues, that there are reasons to think that such elimination is not even a theoretical possibility. The gist of his argument, adapted from an argument by C. H. Langford is this: Consider a world divided in half in such a way that one side was the mirror-image of the other—this could be as simple as a world containing only two neutrons. Corresponding objects on the two sides would have exactly the same intrinsic and (non-indexical) relational properties. Using indexicals we can easily say things about any object in such a world (e.g., "This box is yellow," "The neutron on the left is spinning."); but if we were restricted to descriptive reference, we could never refer to any object without, at the same time referring to its Doppelgänger. Since such a world is possible, and since talking about what is possible is one of the uses of language, there are things science ought to be able to say that cannot, in principle, be said without indexicals.

Burks' presentation and extension of key Peircean ideas about indexicals made an important contribution to reviving research in this area, but it also contains some serious mistakes, which have led to misunderstandings about Peirce's original views. I will mention only a few of the most troubling. Burks doesn't seem to understand the role of "existential connections" in the operation of Peircean indices. He complains that Peirce confused the index relation with the cause and effect relation. It is, indeed, essential to Peirce’s point that the index-relation includes as

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58 At this point Burks introduces an distinction between meaning and information which is very close to Kaplan's distinction between character and content, and thus makes an important contribution to the analysis of indexical meaning. John Perry cites Burks as an influence on his own work.

59 Ibid., p. 679.
a component a physical causal relation; what Burks doesn’t see is that that is not all that it includes. In order to constitute an index, that causal relation must be such that it has a particular effect on an interpreter. It is this, to cite one of Peirce's examples, that makes a weathercock an index. It is constructed and placed in such a way that its causal relation to the wind allows it to have a separate causal effect on someone who knows how weathercocks work, and that second effect is just the one that the direction of the wind would have if it were perceived directly. Burks compares the case of the weathercock with the idea that "clouds are a sign of rain“ and argues that clouds are not really a "sign“ in Peirce’s sense since they are not "used by an interpretant to represent or denote anything. “ But here again he is mistaken. It is true that clouds accompany rain according to a causal connection which is there whether or not anyone ever notices them. And it is true that this fact alone does not make a cloud a sign of rain. It is only when someone knows the connection between clouds and rain, and thus can use that connection in their thinking that a cloud does indeed become a sign, and, on Peirce's classification of signs, an index. People who know that clouds (of a certain kind) are accompanied by rain will automatically ("habitually") use the appearance of those clouds in their thinking in ways exactly analogous to their use of the appearance of rain itself. In other words, the clouds will represent rain in their thinking. If, in addition, those people use language, the fact that the clouds represent rain in their thinking will allow them to use the 'clouds' to represent rain in their speech (e.g., "You better cover that hay before those clouds get here."). The case is simpler with Peirce's examples of the barometer and the weathercock. The construction of these devices presupposes that those who made them have some understanding of the causal link between them and the physical processes of which they act as indices; that people habitually use them indicates that they understand that there is such a link. Any regular physical correlation is potentially an index if it can be used as such by people who understand how to do this; it will not actually be an index until it is so used.

Although Burks's commentary created some misunderstandings about Pierce, it did keep his name and terminology in circulation. But the influence of Peirce's actual ideas was transmitted more forcefully, if less explicitly, in two other papers from this very fertile period, John Austin's "Truth" and Peter Strawson's "On Referring"(both published in 1950).
Austin and Strawson Continue the Peircean Revival

Austin had already read Morris's "Foundations" by 1940, and generally approved of his "attacks" on "those who think of 'a meaning' as a definite something which is 'simply located' somewhere: . . . a kind of entity which can be described wholly without reference to the total activity of 'semiosis'." Austin eventually added much to our understanding of that "total activity." And while dividing explanations of meaning only into syntactic and semantic components, he is clearly engaging in what Morris had called pragmatics; Austin includes under "semantics" contextualized accounts of the use of words, which require reference to language users.

Now the reason why I cannot say 'The cat is on the mat and I do not believe it' is not that it offends against syntactics in the sense of being in some way 'self-contradictory'. What prevents my saying it is rather some semantic convention (implicit, of course), about the way we use words in situations.

By "situations" here he means, of course, contexts.

By 1950 Austin had accepted Morris's "open invitation," and read Peirce for himself. He quibbles that Peirce's distinctions do not go far enough—that the distinction between type and token does not prise apart all the meanings of 'word', and that "with all his 66 divisions of signs, Peirce does not, I believe, distinguish between a sentence and a statement" He then sets out to extend Peirce's work by offering his own set of distinctions. Nonetheless he has assimilated, and now forcefully restates as a central thesis of "Truth," the Peirce/Morris claim of the essential and indispensable contribution of indexicality to factual uses of language. He notes, using the type/token vocabulary, that one sentence can be used to make different statements and vice versa. He then goes on:

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61 This foreshadows David Kaplan's concerns about the inadequacy of the Carnapian version of Morris's trichotomy, in regard to the "pre-semantic" functions of context in the referential workings of proper names. These and related worries have led Kaplan to call for a "semantics of use" in the spirit of Strawson and Austin (and so, of Morris and Peirce).
63 "Truth," in *Philosophical Papers*, p. 119. Austin is, of course, wrong about this; Peirce carefully distinguished between sentence tokens and the assertive acts with which they were associated, and between these assertive acts, as real events, and the ideal objective contents which these acts express. Those contents are what Austin and Strawson call statements.
A statement is [something which we say is] made and its making is an historic event, the utterance by a certain speaker or writer of certain words (a sentence) to an audience with reference to an historic situation, event or what not. . . . The same sentence is used in making different statements (I say 'It is mine', you say 'It is mine'); it may also be used on two occasions or by two person in making the same statement, but for this the utterance must be made with reference to the same situation or event.64

A description of language which covers only the syntactic and semantic behavior of sentences will be unable to account for the distinction between sentences and the statements they are used to make. To account for the complex interrelation between sentences and statements, Austin thinks we must examine what is required "if there is to be communication of the sort that we achieve by language at all." This is just the place where Peirce had said that any semiotic investigation needed to start—with generalizations about the nature of assertion as such—generalizations which, once stated, would seem obvious to any user of language. Austin says that among these conditions of the possibility of assertive communication as such is the requirement that there be two particular sets of conventions:

Descriptive conventions correlating the words (=sentences) with the types of situation, thing, event, &c., to be found in the world.

Demonstrative conventions correlating the words (=statements) with the historic situations, &c., to be found in the world.

A statement is said to be true when the historic state of affairs to which it is correlated by the demonstrative conventions . . . is of a type with which the sentence used in making it is correlated by the descriptive conventions.65

Austin cautions that care needs to be taken in identifying the descriptive and the demonstrative components in a given statement. Not only do many verbal forms serve both descriptive and demonstrative purposes,

many demonstrative conventions are non-verbal, (pointing, &c.) and using these we can make a statement in a single word which is not a 'sentence'. Thus, 'languages' like that of (traffic, &c.) signs use quite distinct media for their descriptive and demonstrative elements (the sign on the post, the site of the post). And however many verbal demonstrative devices we use as auxiliaries, there must

64 Ibid., pp. 119-20.
65 Ibid., p. 122.
always be a non-verbal origin for these co-ordinates, which is the point of utterance of the statement.66

With these observations Austin introduced the essentials of both the method and results of Peircean semiotics at the very base of an influential contemporary stream of the philosophy of language.67

Peter Strawson agreed with Austin that this distinction—between a sentence and the statements which could be made with it—is essential to a full description of the general features of natural language.68 In "On Referring" he talks about a parallel distinction in regard to singular terms: the expressions of language and their uses in making reference to objects for the purpose of making statements about them. He argues that Russell's account of singular terms, including his notion of logically proper names, his reduction of ordinary names to covert descriptions, and finally his reduction of definite descriptions to statements containing quantified variables, is vitiated by the failure to make these crucial distinctions. Russell, says Strawson, in opposing Meinongian arguments that the meaningfulness of statements about non-existent objects implied that those objects "in some sense" existed, unwittingly joined Meinong in making two illegitimate assumptions on which the latter's arguments were based. First, he assumed that if a sentence is meaningful, it must be either true or false; second he assumed that if a sentence is meaningful and of subject-predicate form, there must be something which it is about, which could make it true or false. Strawson points out that it is not sentences which can be true or false, or which can be about something, but only the statements made using sentences; sentences themselves can be neither. On the other hand, a sentence can have a meaning, whether it is ever used to make a true statement or not, since "to give the meaning of an expression (type) . . . is to give general directions for its use to refer to or mention particular objects or persons; to give the meaning of a sentence is to give general directions for its use in making true or false assertions."69 He goes on, in this Peircean vein, to say more about what

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66 Ibid., footnote 3.
67 For an interesting examination of the contrast between the fundamental features of the Austinian notion of truth, based on the Peircean distinction between demonstrative and descriptive elements, and the very different notion of truth growing out of Tarski's work see Jon Barwise and John Etchemendy, *The Liar; an Essay on Truth and Circularity*, New York: Oxford University Press, 1987.
68 C.f., "On Referring," p. 6. Strawson, like Austin, notes that one sentence (type) may be used to make different statements and that different utterances may make the same statement—in the terminology he introduces "have the same use."
69 Ibid., p. 9.
"giving the meaning" involves. "To talk about the meaning of an expression or sentence is not to talk about its use on a particular occasion, but about the rules, habits, conventions governing its correct use, on all occasions, to refer or assert."

In giving the meanings of singular terms, making reference to context turns out to be unavoidable.

The expressions which can in fact occur as singular logical subjects . . . demonstratives, substantival phrases, proper names, pronouns . . ., together with context (in the widest sense), are what one uses to make unique references. The point of the conventions governing the uses of such expressions is, along with the situation of utterance, to secure uniqueness of reference. . . . The actual unique reference made, if any, is a matter of the particular use in the particular context; the significance of the expression used is the set of rules or conventions which permit such references to be made.\textsuperscript{70}

Strawson does not go as far as Austin in demanding demonstrative and descriptive elements in all statements, but he does highlight the importance of these two features—Strawson calls them the "identifying" or "referring" role and the "attributive" or "ascriptive" role. Context is essentially involved in the meanings of the expressions which play identifying roles.

The fulfillment of more or less precisely stateable contextual conditions in conventionally (or in a wide sense of the word, logically) required for the correct referring use of expressions in the sense in which it is not true of ascriptive uses. . . . The requirement for the correct application of an expression in its referring use to a certain thing is something over and above any requirement derived from such ascriptive meaning as the expression may have; it is, namely, the requirement that the thing should be in a certain relation to the speaker and to the context of utterance. Let me call this the contextual requirement.\textsuperscript{71}

Again, as in Austin, these observations are in complete agreement with Peirce's notion of the indexical aspect of reference and assertion. Strawson even gives his own example of statements using non-verbal indices—"uttering a single word or attributive phrase in the conspicuous presence of the object referred to; or the painting of the words 'unsafe for

\textsuperscript{70} Ibid., p. 17.
\textsuperscript{71} Ibid., p. 19. Strawson's insight into the conventional role of context illustrates the fact, noted later by Kaplan, that if semantics is understood as the study of the meanings words have by convention, then contextual factors and context-sensitivity must find a place in semantic theory.
lorries' on a bridge, or the tying of a label reading 'first prize' on a vegetable barrow.\textsuperscript{72}

These are just variants of Peirce's shipping crate stamped with 'GLASS'.

Having made explicit the role of context in the meanings of referring expressions, Strawson does the additional service of giving some indication of what context must include to do this job.

What in general is required for making a unique reference is, obviously, some device, or devices, for showing both that a unique reference is intended and what unique reference it is; some device requiring and enabling the hearer or reader to identify what is being talked about. In securing this result, the context of utterance is of an importance which is almost impossible to exaggerate; and by 'context' I mean, at least, the time, the place, the situation, the identity of the speaker, the subjects which form the immediate focus of interest, and the personal histories of both the speaker and those he is addressing.\textsuperscript{73}

In Chapter 9 we will have occasion to consider the list of contextual features given by Strawson here. For now we should just note the similarity of his list and the one quoted above from Benson Mates—the list which he fears will go on "\textit{ad infinitum}." The difference in attitude between Mates and Strawson in regard to the possibility of a theory of context could not be more striking. This difference is amusingly summed up by Austin:

\begin{quote}
I think we should not despair too easily and talk, as people are apt to do, about the infinite uses of language. Philosophers will do this when they have listed as many, let us say, as seventeen; but even if there were something like ten thousand uses of language, surely we could list them all in time. This, after all is no larger that the number of species of beetle that entomologists have taken pains to list.\textsuperscript{74}
\end{quote}

In chapter 9 we shall see that we can make a good start at listing the most important features of context without going to the lengths of the entomologist. In doing this we can take advantage of the contributions to the understanding of context made by some other philosophers, such as Grice and Searle, who were influenced by the Peircean picture of meaning enunciated by Austin and Strawson.

We should note that Austin's and Strawson's ideas, perhaps because of some of the polemical pronouncements attached to them, had little influence on the (more formally-oriented) stream of semantic analysis which we will mainly consider in the remainder of

\textsuperscript{72} Ibid., p. 18.
\textsuperscript{73} Ibid., p. 19.
\textsuperscript{74} "Performative Utterances," in \textit{Philosophical Papers}, p. 234.
this chapter. Interestingly, David Kaplan and others trained in the formal stream have recently taken inspiration from some key Strawsonian ideas. An important conduit of these ideas was Kieth Donnellan, whose distinction between referential and attributive uses of descriptions was a development of Strawson's general distinction between referring and attributive uses of expressions. We will see these two streams—the Peircean and the Carnapian—rejoin in Kaplan's work, to be examined in the next chapter.

**Bar-Hillel: Limited Indispensability and Extensional Models of Context**

Yehoshua Bar-Hillel surveyed the whole period of activity we have been considering in his "Indexical Expressions" of 1954. He agrees with Burks and Strawson, contra Reichenbach, that indexicals are not eliminable from language—that a philosophically adequate account of language must include an account of the direct meaningfulness of context-sensitive expressions. He differs, however, on the reasons for this. He disputes the Peircean arguments (versions of which are found in Burks, and Russell) against elimination of indexicals in factual statements via coordinate systems; this is the now familiar claim that coordinate systems can only make determinate factual statements when the origin, orientation and scale of the system are indexically anchored; Bar-Hillel's interesting counter-argument deserves attention, and we will examine it shortly.

But even if science can be done without indexicals, he thinks an investigation of indexicals is still mandatory because there are many things that language does besides making factual scientific statements; there are many "cognitive contents" which can only be expressed indexically, and so for many purposes, including doing philosophy, indexicals can be shown to be indispensable. This is a point that has been developed and insisted upon more recently by John Perry and David Kaplan.

The case for indispensability would be even stronger if, contrary to Bar-Hillel's claim, indexicals are also required for making definite factual statements. Let us, therefore, consider his argument to the effect that this is not the case. The claim that coordinate systems need to be anchored indexically is, he says, based on a confusion between what is required to learn an expression, and what is required to use that expression.

There can be little doubt that learning how to use coordinates, just as learning how to use words like 'red', involves the use of indexical signs. But, nevertheless, a coordinate, just as the word 'red', is non-indexical in this clear and definite sense... in its reference being independent of the pragmatic context of its production. A token of 'This book is red' will not be understood in the way intended by its producer by anybody who does not know the context of its production, even if he has an encyclopedic knowledge and an arsenal of tools; a token of 'The book
at location $l_1$ and time $t_1$ is red' will be understood in exactly the same way by anybody having a certain knowledge (which he might have got with the help of indexical signs) and perhaps other tools.\textsuperscript{75}

Bar-Hillel's argument is based on an analogy between the way 'red' is used and the way locational coordinates are used. But does this analogy hold? He is correct that when we use 'red' the interpretation of our sentence (typically) requires no knowledge of the context in which it was used, but does the same hold for coordinate descriptions? He says we can interpret 'at location $l_1$ and time $t_1$' given "certain knowledge, and perhaps other tools" but without knowledge of the context. Deciding whether this is true would be easier if we had a clear distinction between what knowledge counts as contextual and what does not; such a distinction will eventually grow out of a theory of context. But we can get a preliminary answer, without having to decide that thorny issue, by examining the mechanism of coordinate specification. Suppose an astronomical observation had been reported using this designation of spatiotemporal location: Latitude: 47.15.84 N., Longitude: 106.45.89 W.; 7:01 p.m.; March 12, 1994." What would one need to know to understand what was being reported? The first thing to notice is what has been suppressed in the above description: the latitude may be assumed to north of the equator; the longitude, west of Greenwich; the time, relative to Greenwich Mean Time (or is it local time?); the date, relative to the currently accepted version of the Gregorian calendar, etc. Because of the long-standing and uniform standards involved, it is easy to forget these implicit components. But a moment's consideration of an earlier age when, depending on the nationality of the cartographer, longitude might have been measured from Paris, Rome or Moscow, as easily as from Greenwich, when all time was non-standard "local time," and when there was no universally accepted calendar, makes it clear that without a good deal of implicit or explicit supplementation, the above description is meaningless. In that earlier age the need for contextual supplementation was obvious. And once we see this, it is clear that the above location is being specified, not in terms of its intrinsic properties, but in terms of its "existential relations" to other locations—and that reference to those other physical locations (the equator, Greenwich, etc.) is carried along in the meaning of the coordinate specification. Furthermore, as the investigation of proper names will show, the names being used refer to those locations only because of certain "existential relations" between those locations and tokens of those names.

\textsuperscript{75} "Indexical Expressions," \textit{Mind}, v, 63 (1954), p. 374.
Having seen this, we can uncover an underlying semantic disanalogy between coordinate designations and quality words like 'red'. It is true that physical samples are used in learning quality words, just as relations to physical origin points are used in learning coordinate systems; this far Bar-Hillel's analogy holds. The general point he has missed is this: concept words like 'red' are (typically) used in a way that is intended to be free of any dependence on the circumstances in which they were learned—my use of 'red' (learned via samples a, b and c) is intended to mean exactly the same as my teacher's use of 'red' (learned via samples d, e and f). So the samples are relevant to the context of learning, but they play no role in the context of use. Proper names, by contrast (including the names of origin points in coordinate systems), are typically used with the intention of referring to the very thing in connection with which the name was learned. Even though the named object may be remote from an utterance of a name, it remains a relevant part of the context of that utterance in the sense that it is a non-linguistic factor involved in the determination of meaning—if the name had been learned in connection with a different object, an utterance of that name would have a different meaning. When I give a longitude west of Greenwich, I intend to specify an angular displacement relative to the very place from which my teacher measured angular displacements. The meaning convention for such referring terms includes ineliminable reference to a specific object, in a way which the meaning conventions for concept words specifically exclude. In Recanati's terminology, the identities of the samples used to learn a concept word are "truth conditionally irrelevant" to statements made with that word; the identity of the origin point used to learn a coordinate system is not. A coordinate description depends on the meaning of proper names of its origin points, not just in learning, but in every use. So the context-sensitivity of the proper names will transfer to the coordinate system. We shall return to the details of the context-sensitivity involved in the meaning conventions for proper names when we examine the "directly referential" peculiarities of these conventions which have, of course, been the subject of much recent semantic research. While it may be true that proper names can be established without explicit use of indexical words, narrowly construed (although they are normally established using them) the crucial point is that the same contextual structures and influences that fix indexical reference are required to fix the reference of proper names. It is in this sense that the uses of indices are indispensable to reference via coordinate systems. 

So it appears that the analogy between context-neutral quality words and coordinate designations does not hold. Nonetheless, Bar-Hillel's argument, though based on a flawed analogy, is enlightening because it makes explicit a usually unarticulated view that has
persisted for a long time among many in the semantic tradition growing out of the work of Tarski and Carnap.

Bar-Hillel also points out a new difficulty raised by indexicals. Theories of truth which are judged by the Tarski criterion to be "adequate" for non-context-sensitive languages (or "language fragments") will not be adequate for languages containing indexicals, since, in general, it is not the case that 'I am hungry' is true if and only if I am hungry.\textsuperscript{76} He does not, however, stop to explore the reasons for this, or its implications for semantic theory.

Bar-Hillel's positive proposal concerning the symbolic treatment of indexical-containing expressions begins with the observation that reference and truth are not, generally speaking, assigned to sentence tokens alone, but only to tokens-in-a-context.\textsuperscript{77} Specifically, he distinguishes between sentences (types and tokens) and judgements, which he identifies with ordered pairs consisting of a sentence token and a context (note that these "judgements" are to be thought of as extensionally defined set-theoretic objects). Then, on this basis, he divides all sentences into two groups—indexical sentences and statements—by means of the fact that the former occur in judgements with variable meanings and truth-values, while the latter do not. Bar-Hillel's formal innovation of using "context" as a component of a model-theoretic representation sets the stage for later developments by philosophers such as Montague and Kaplan. These developments would eventually uncover its inadequacies, but for over a decade Bar-Hillel's sketchy treatment seems to have satisfied his colleagues that indexicals were under control—could be adequately represented within an extensional formalism.

\textbf{Carnap's Later Position on Pragmatics}

Bar-Hillel's attempt to incorporate context into a formal semantic theory was a marked departure from the usual practice among formal theorists faced with bothersome contextual influences: compartmentalize and ignore. This practice is visible in the way questions about formal structure were kept separate from questions of application. When Goodman considered how the primitive similarity relation, which played such a crucial role in both his system and in Carnap's \textit{Aufbau}, is to be applied to actual human experience, he remarked

\begin{quote}
Carnap would perhaps say that it makes no difference what degree of similarity is required so long as it is fixed. One might thus regard two color-spots as
\end{quote}

\textsuperscript{76} Ibid., p. 363.
\textsuperscript{77} This is the earliest occurrence I have found of the notion of "true in a context"—a notion which will be examined in Ch. 9.
similar if and only if they are at least as much alike as some arbitrarily designated pair. A standard pair of qualities of each other kind would also have to be designated. Such a method of arriving at a relation list seems rather awkward and unreliable; but this, of course, does not affect the actual constructions once a relation list is available.\textsuperscript{78}

The job of constructing a formal language is, he is suggesting, to be seen as separable from the job of figuring out how to plug empirical content into it. After surveying several problems with the application of Carnap's favored "recognizably-similar-to" relation, Goodman recommends that

the best course is simply to admit that the whole epistemological argument in terms of memory images and the nature of recognition is irrelevant, and adopt [the primitive relation] as the relation that obtains between every two [basic experiences] \(x\) and \(y\) if \(x\) is part similar to and earlier than \(y\). This in effect is Carnap's procedure throughout most of the \([\textit{Aufbau}]\).\textsuperscript{79}

In other words, don't get bogged down in problems about explaining what experiences correspond to the primitive relation—explanations which always seem to involve context-sensitive expressions. Just specify formal requirements which that relation is assumed to satisfy, and move on. The thinking seems to be that conditional results are better than no results. But for someone like Carnap who held out-hope for actual expressive application of artificial languages with empirical content,\textsuperscript{80} the epistemological issues were not irrelevant—they could be postponed, but not permanently evaded. From the beginning, Carnap's official doctrine had been that structural isomorphism between the formal system and the target domain would eventually secure reference to objects of experience. When purely syntactic structure seemed incapable of serving the purpose, an extensional semantics was added. As time went on Carnap increasingly allowed for the possibility that an intensional object language, or even an intensional metalanguage, would be needed to achieve a system that was complete enough to assure that the 'intended interpretation' was the only possible one. We will see that his eventual expression of interest in the aspects of

\textsuperscript{78} \textit{Structure of Appearance}, p. 171 (italics added).
\textsuperscript{79} Ibid. 173.
\textsuperscript{80} In the preface to \textit{Introduction to Semantics} he notes that Tarski's work was long unavailable to non-Polish scholars because of the language barrier, and then remarks, "This fact, incidentally, confirms once more the urgent need for an international auxiliary language, especially for scientific purposes."
language which he had long sought to compartmentalize as "pragmatics" shows a gradual realization that more than structural isomorphism would be required.

In Introduction to Semantics Carnap is careful to distinguish between the formal concepts of 'truth' and 'probability' and the corresponding pragmatic concepts of 'verification' and 'degree of confirmation'. The latter two are merely mentioned in the course of being put to one side, out of the range of proper theoretical concern. This is partly explained by Carnap's adoption of a version of Morris's semiotic trichotomy which assigns to pragmatics any investigation in which "explicit reference is made to the . . . user of language." Carnap includes in this category the "study of the procedures applied by scientists in recording the results of experiments, etc." He says that syntax and semantics each have both pure and applied (descriptive) departments, although, strictly speaking, their descriptive parts, which deal with the practices of actual people and cultures, are to be classed as parts of pragmatics. But this classification leaves no room for what eventually would be called "pure pragmatics," the study of features of language which do not have to do with any particular language users (or other particular contextual features), but rather with the role played in language as such by language users (and context) as such.

Others saw that an account of the link between language, as abstract system, and the language user, as socially conditioned intentional communicator, would eventually need to be addressed in the process of explaining the linguistic basis of scientific practice, and that this would involve talking about the common general features of "the procedures applied by scientist" in their work. This was indeed the point of Morris's remarks in the Encyclopedia about the need for a unified theory of signs, sensitive to the interaction among the syntactical, semantic and pragmatic dimensions of language. Austin and Strawson were concerned with these issues as well; so were Hempel and Quine, as we shall soon see.

By the mid-fifties, Carnap himself had come to appreciate the need for incorporating what he had been calling "pragmatics" into any—even a purely logical—account of language. Using the distinction between descriptive and pure semiotics, Morris suggested, in 1963, that what was needed for the continued development of Carnap's program was a "pure pragmatics" to supplement pure syntax and pure semantics. Carnap agreed:

Morris stresses the importance of pragmatics, the theory of the use of language by human beings, as a field of semiotics on a par with logical syntax and

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82 Ibid., p. 10
83 Ibid.
semantics. I think he is right in saying that although I have recognized all three fields of semiotics, I have dealt with them in different ways. In syntax and semantics, my main interest and practically all my constructive work was directed toward the non-empirical, purely logical side, called "pure syntax" and "pure semantics." In the case of pragmatics, I was mainly thinking of empirical investigations. Today I would agree with Morris that there is an urgent need to develop pure pragmatics, which would supply the framework for descriptive pragmatics. . . . At about the same time, and independently of each other, Morris and I said almost literally the same thing; Morris writes: "An explicit concern with pure pragmatics becomes an urgent task," and I said: "there is an urgent need for a system of theoretical pragmatics (in [1955-6])."^84

This suggests a change in the taxonomy of semiotics: "descriptive syntax" and "descriptive semantics" could still be considered part of (descriptive) pragmatics. But now, perhaps, "pure pragmatics,"—thought of as the study of the relation between meaning and "language users as such"—should be considered part of "pure semantics." Most of those influenced by Carnap continued, however, to think in terms of the old simple trichotomy as Carnap had originally taken it from Morris. Eventually, as we shall see, Kaplan would be compelled to abandon Carnap's original version because of what he called "pre-semantic" and straightforwardly semantic influences of context. Others have followed Kaplan's move, although the simple trichotomy continues to exert a strong influence.

What Carnap says about his earlier avoidance of pragmatics is, as Morris points out, not quite accurate. Notions such as 'verification,' 'confirmation' and 'testability' which played a crucial role in the Vienna Circle discussions of meaning are all, as Carnap noted, pragmatic concepts. Those discussions of the form of protocol sentences considered both the laboratory procedures of actual scientists and the activity of "making an observation" as such—as presupposed in the empirical content of any conceivable language. The former might well be classified as "descriptive pragmatics"; the latter seems to be just what Morris has in mind when he talks about "pure pragmatics"—it is certainly what Peirce had in mind in including indexicality in the universal structure of language as such.

There may have been another factor in Carnap's late interest in pragmatics. Discussion of pragmatic issues, both pure and descriptive, are unavoidable in any program aimed at creating artificial languages for independent practical use. As late as 1963 Carnap was still insisting that an artificial language could be learned as a first language (e.g., "by the Berlitz method of immersion") and "thereby refute the wide-spread view that constructed

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languages are not autonomous, but essentially parasitic, based on natural languages." He did not admit any inconsistency between this hope and the requirement that constructed languages be free of demonstratives and other context-sensitive devices but perhaps he realized that there was an inconsistency, and this was a source of the expressed "urgent need" for a pure pragmatics.

**Hempel and Quine on Another Kind of Context-sensitive Meaning**

Others had already been thinking about what needed to be taken into account in explaining the meaningfulness of terms used in actual scientific discourse. For example, when Carl Hempel tried to reconcile the empiricist intuition that theoretical terms can only have meaning if they are somehow linked to experience, he was led to the conclusion that the "experiential meaning" of scientific terms and statements must be evaluated, not individually, but in the context of a linguistic and theoretical framework.

Thus, e.g., the relativistic theory of the deflection of light rays in the gravitational field of the sun entails assertions about observable phenomena only if it is conjoined with a considerable body of astronomical and optical theory as well as a large number of specific statements about the instruments used in those observations of solar eclipses which serve to test the hypothesis in question.

Terms like 'gravitational field', 'atom', 'gene', 'electron', etc., will have different meanings in different theoretical settings, and disconnected from theory they have no meaning at all.

Quine's "Two Dogmas of Empiricism" generalized Hempel's contextualization of meaning to cover, not just theoretical terms, but the whole vocabulary of factual discourse. Quine summarizes the progression of empiricist theories of meaning—the struggle to explain how experience is linked to the truth of factual statements—from the term-atomism of Locke and Hume, in which individual terms were seen as linked to "ideas" obtained

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85 Ibid., p. 938. Carnap refers here to Herbert Bohnert's article in the same volume, where Bohnert describes a child learning a formalized language "adequate to the purpose of the natural sciences." Bohnert says: "A child could not, of course, learn the language from its definition. . . He would learn to speak and understand by pointings, contexts, and trials in the way one learns any language initially." Eventually, Bohnert thinks, the child's initial intuitive grasp of the language would be honed by learning the explicit definitional structure of the language, until, by the same process as adding postulates to an axiomatized system, the "range of possible interpretations is progressively cut down." (p. 420).

through perception, to the sentence-atomism of Russell and Frege, where the sentence, expressing a fact or state of affairs is the basic unit of factual meaning, finally to his own position (deriving from Hertz and Duhem) of theory-holism:

My present suggestion is that it is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement. Taken collectively, science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one. [Russell's] idea of defining a symbol in use was . . . an advance over the impossible term-by-term empiricism of Locke and Hume. The statement, rather than the term, came with Frege to be recognized as the unit accountable to an empiricist critique. But what I am now urging is that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science.87

With this suggestion came the problematic notion that meaning is to be thought of only relative to an entire "conceptual scheme." Later we will review well know objections to this claim on our way to a more moderate solution to the problem of the basic unit of meaning (viz., an utterance in a context). It will be argued that a 'conceptual scheme' is best understood as a special case of a context. For now we need only note that while Quine was comfortable with the idea that the meanings of expressions in scientific discourse are influenced by their theoretical context, he was not happy with the prospect of those meanings being influenced by the physical and conversational context—he was willing to accept a grand contextualism while rejecting a modest one.

Quine, Davidson and Perry on Dispensability

Burks's and Bar-Hillel's proofs of indispensability did not put to rest the issue of the place of indexical in meaningful language. Quine is an example of how one could be a "theoretical contextualist" and still believe in the dispensibility of indexicals in a language with empirical content. Let us look, then, at Quine's influential statements on dispensibility and then at the contrasting views of Davidson and Perry.

Quine's attitude toward indexical expressions is similar to Carnap's —indexicals occur in epistemologically basic statements, but are intractable to syntactic methods, so that any hope of a logical theory based on recursive syntactic methods must provide a method of eliminating context-sensitive expressions in favor of non-context-sensitive ones. The main

87 "Two Dogmas of Empiricism." in From a Logical Point of View, end of §V.
issues come out in connection with the logic of natural language arguments. At first Quine sounds very much like Strawson, but deep differences soon appear.

Strictly speaking, what admit of truth and falsity are not statements as repeatable patterns of utterance, but individual events of statement utterance. For, utterances that sound alike can vary in meaning with the occasion of the utterance. This is due . . . to systematic ambiguities which are essential to the nature of language. The pronoun 'I' changes its reference with every change of speaker; 'here' changes its reference with every significant movement through space. . . . The critical point of contact between description and reality is to be sought in the utterance of a statement on the occasion of an experience which that statement utterance directly reports. The seeing of a green patch, and the simultaneous utterance 'Green patch now', constitutes the sort of composite event which, on its rare occurrences, gladdens the heart of the epistemologist. . . . However, it is a great source of simplification in logical theory to talk of statements in abstraction from the individual occasions of their utterance; and this abstraction, if made in full awareness and subject to a certain precaution offers no difficulties. The precaution is merely that we must not apply our logical techniques to examples in which one and the same statement recurs several times with changed meanings, owing to variations in immediate context. But such examples are easily enough adjusted to the purposes of logic by some preliminary paraphrasing, by way of bringing the implicit shifts of meaning into explicit form. 88

Certainly for short arguments in informal circumstances this is plausible enough. But when one begins to consider "the whole of science" it is no longer clear that we can avoid applying logic to context-sensitive expressions; and if we set out to paraphrase them away, we need to know what all the "implicit shifts" are, and how to bring them all into "explicit form." When the whole of science is at stake "preliminary paraphrasing" becomes a daunting task.

But Quine, undaunted, gives an extended account of such paraphrase techniques in *Word and Object*, relying heavily on coordinate designation, and replacement of indexicals by proper names. Quine professes a liberal attitude toward the suitability of these

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88 *Methods of Logic*, 3rd ed., p. 1, p. 4-5. In *Methods of Logic*, Quine restricts attention to ambiguities which occur within arguments, and thus he can set aside concerns about indexical meaning in general. "This is why words of ambiguous reference such as 'I', 'you', 'here', Smith', and 'Elm Street' are ordinarily allowable in logical arguments without qualification; their interpretation is indifferent to the logical soundness of an argument, provided merely that it stays the same throughout the space of the argument." pp. 48-9. The broader concern of *Word and Object*—the entire language of science—makes it impossible to avoid the issue of how indexicals should be treated in a regimented language.
paraphrases—each language user is free to accept or offer modifications of such paraphrases, as required by our "evolving expressive needs"—but for his part, he is convinced that "all traits of reality worthy of the name can be set down in" a regimented language from which all indexicals have been paraphrased away. This last claim is the one which is most troubling, and reveals that Quine has already decided what traits of reality are "worthy of the name."

Davidson echoes Quine's methodological worries about the difficulties posed by indexical expressions but he is not convinced that elimination is an option. In "Truth and Meaning" he articulates his well known thesis that a theory which would imply statements of the truth conditions of all sentences of a language, in just the way specified by Tarski's 'Convention T', would amount to a theory of meaning for that language since "to give truth conditions is a way of giving the meaning of a sentence." He is conscious of Tarski's doubts about extending his kind of account to natural language (doubts which become palpable, for example, in Bar-Hillel's observation that Convention T is just not applicable to sentences containing indexicals), but he holds out hope that such doubts can be overcome. For one thing, he thinks the field of inquiry can be restricted to "fragments" of language which do not contain troublesome features; for example, the set-theoretic paradoxes can be avoided because "most of the problems of general philosophical interest arise within a fragment of the relevant natural language that may be conceived as containing very little set theory." Nonetheless, he admits that indexicals represent a "very large fly in the ointment." Both friends and foes of formal semantics seem agreed that "formal semantics and logic are incompetent to deal with disturbances caused by demonstratives." He realizes that provisionally eliminating indexicals in favors of constants (as in Carnap, Quine, etc.), is unacceptable in an account which claims to be a theory of meaning for natural language. "Clearly demonstrative cannot be eliminated from natural language without loss or radical change." Specifically, in response to previous paraphrase schemes, he says that "it could be fairly pointed out that part of understanding demonstratives is knowing the rules by which they adjust their reference to circumstance; assimilating demonstratives to constant terms obliterates this feature." In other words, a theory of meaning should tell us not only what words mean, but also how they mean—the

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89 *Word and Object*, p. 228.
91 Ibid., p. 78.
92 Ibid., p. 81.
different ways that meaning is achieved. He thought the problem needed to be taken seriously, but he also thought, at that point, that a "technically trivial" solution could be found. Here he introduces a version of Bar-Hillel's notion of "truth-in-a-context." It is simplest, he says, just to view truth as a relation between a sentence, a person, and a time. As a first approximation, the theory might entail sentences like the following: 'That book was stolen' is true as (potentially) spoken by $p$ at $t$ if and only if the book demonstrated by $p$ at $t$ is stolen prior to $t$.93

But making such an account work was not as "technically trivial" as Davidson thought (he admits this in a footnote note added in 1982). A few paragraphs later he notes that one of the outstanding difficulties for the kind of theory he has in mind is giving an account of "verbs of action that imply purpose"—e.g. 'to demonstrate' in the sense required for his just mentioned treatment of 'that book'.94 A full treatment of natural language would have to wait on a theory of action. In Chapter 9 we will see the important sense in which this is true.

As time went on, Davidson saw more and more evidence of the importance of demonstratives for the communicative functions of natural language. In fact, he employs a novel use of demonstrative reference in his account of indirect discourse. In "On Saying That" he tries to avoid appealing to non-standard references for words used in indirect discourse—e.g., the 'senses' or 'intension' used by Frege, Church and Carnap—and also to avoid the semantically opaque quotational blocks of Quine and Scheffler, which leave it a mystery how we ever learn the meanings of those content sentences. We can avoid these tactics, he says, if we take the 'that' of indirect discourse to function as a demonstrative, whose referent is the content sentence, and which, in this context, indicates the intention to "say the same as" the speaker referred to by the subject of the main clause. Thus 'Galileo said that the earth moves' is to be read as two separate sentences with independent truth conditions: 'Galileo says that. The earth moves'. The demonstrative in the first sentence is to be taken as referring to the second. The first makes the claim that the speaker is about to

93 Ibid. In a footnote here, Davidson says "There is more than an intimation of this approach to demonstratives and truth in Austin's 1950 article 'Truth.'"
94 The teleological aspects of 'demonstration', 'reference', 'saying' etc., become especially important when we consider language, not just as an activity of an individual, but as a social practice built on intentional activities of many language users. We have already noted the importance of Mill's "intention to refer to external objects," and Frege's "intention to affirm the identical objective thought." The theory of context of Chapter 9 attempts to satisfy Davidson's requirement for an account of language use as purposeful action, specifically by seeing it as a part of a communal intentional practice.
"say the same thing" that Galileo did; the second purports to make the first true by saying the very thing which Galileo is being claimed to have said. The second has exactly the truth conditions it has when said in direct discourse (thus restoring pre-Fregean "semantic innocence," but at the price of admitting the need for a theory of demonstratives). The semantic difference (which accounts for the failure of substitutivity) lies, not in the meanings of the words, but rather in the way the words are used in this particular communicative context. "Language is the instrument it is because the same expression, with semantic features (meaning) unchanged, can serve countless purposes."\textsuperscript{95}

But a theory of language should, for this very reason, be expected to provide, not just a theory of meaning (in the Davidsonian sense of truth conditions) but also a theory of use. We want to know what it is about 'that' that permits it to be used in the way Davidson describes. One might then conclude that, since making factual claims is but one of the uses of language, the theory of truth-conditional meaning will be, not the whole, but only a proper part of the theory of use. The importance of this expansion of the range of semantic theory is all the more evident when we see the way that demonstratives can be used to explain such important and troubling constructions as indirect discourse (and, as Davidson suggests, intensional contexts generally).

If Davidson is right about this, then an adequate theory of language should not seek to eliminate indexicals, either from its official vocabulary or from the object languages it considers, but should incorporate the study of indexical meanings on a par with truth-conditional semantics, in a move toward a unified semiotic theory. Developing Davidson's explanation of indirect discourse in terms of a demonstrative use of 'that' will, for example, require saying how that particular way of using 'that' counts as a conventional part of English. This will mean specifying the rule that a language learner masters in coming to understand sentences that use 'that' in this way. This use presupposes, among other things, the sophisticated notion of "saying the same thing" in a sense that allows cross-language identification of content. The issues about translation and interpretation which are thus raised may seem, at first, to be extraneous complications best set aside in the attempt to survey the most basic structures of linguistic meaning. This conclusion should be resisted since, as we will see in Chapter 9, the dramatic interpretive situation of "radical translation" between different languages must be explained by appeal to the same basic abilities and contextual influences that come into play in the more subtle situations involving a single language—e.g., first language acquisition, linguistic change, and

conversational disambiguation. Explanation of the subtle and familiar can be aided by examination of the unusual and dramatic. A complete explanation of indirect discourse in this style will require building a picture of the language user as situated in a social and communicative context in which she has a fairly complex but specifiable body of knowledge, skills and motivations. This kind of "hidden indexical" approach to intensional constructions has continued to interest philosophers of language, and serves to emphasize the important general role which the theory of indexicality, and thus also the theory of context may be called upon to play.

Davidson's appeal to hidden demonstratives is motivated by the technical needs of semantic theory as he conceives it. Others have argued that there are also pre-theoretic intuitions about how language works that lead us to give indexicals an irreducible role in any theory of language.

John Perry has eloquently updated Peirce's important insights into the indispensable role that indexicality plays in the semantics of natural language. In "The Problem of the Essential Indexical," and elsewhere, he has argued that indexicals are essential to psychological explanation—explaining behavior of language users in terms of what they say and hear. There is, for example, something conveyed by 'The meeting starts now' that is not conveyed by the truth-conditionally equivalent 'The meeting starts at noon'—something that can cause a person who thinks it is only 11:30 to jump up and run down the hall. And it is part of the conventional meaning of 'now' that allow it to convey whatever content that is. Open-minded observation of how natural language works reveals that indexicals capture things about the world which are true and important and apparently cannot be specified without them. Theories that exclude them from consideration will be the poorer for it. Recently Perry has begun to formulate a picture of context that helps make sense of this action-explaining aspect of indexicality, and we will see what can be learned from that work in Chapter 9.

Undertaking Infinite Tasks with Finite Means

Let us take stock of the series of philosophical insights, central to the agenda of the philosophy of language in this century, which should motivate an investigation of indexicality and context. Mill, reacting to empiricist skepticism about the objectivity of knowledge and wishing to establish the objectivity of the contents of scientific statements as well as of the logical relations among them, proposed a principle of semantic objectivity: We normally intend that our words refer to external objects and facts, not to our ideas of those objects and facts.
Russell, wishing to consolidate Mill's defense of logic, while remaining true to empiricist intuitions, realized that the fulfillment of our Millian intentions of objective reference and content was dependent on limited perceptual and cognitive resources. This led him to a pessimistic restriction on the sphere of applicability of referential terms: we can only refer directly to objects with which we are acquainted. Complicating matters was his recognition of a fundamentally subjective aspect of this acquaintance relation: two people can never know that they are acquainted with the same object in the same way at the same time. The slide back into solipsistic skepticism threatened, since the content of empirical statements, on this view, derived exclusively from singular references to sense data using the logically proper names 'this' and 'that'.

Frege, equally committed to the defense of objective contents, but not so impressed as Russell by the individuality of our perceptual access to objects, was nonetheless clear that our reference to objects was mediated by some mode of access—the various ways that objects of different kinds can be given to us. But Frege saw no reason that some of the modes of presentation cannot be just as objective as the objects they present. An object's being presented in the same way to different people could provide the required means of objective reference to fulfill our Millian referential intentions. This defense of objectivity can be formulated in a pair of principles. The first explains the intended objectivity of reference as being fundamentally intersubjective: we intend to talk about the same things that others talk about—aiming at a "common treasure of knowledge." The second principle resonates with Russellian worries about the means of access available to fulfill our intentions: the way a thing is given comes into the meaning of thoughts and statements about that thing. Linking these two principles with the plausible assumption that objects are sometimes presented to different individuals in the same way provides a way of grounding objective reference in (shared) experience.

As attractive as this Fregean solution may be to a defender of objective empirical content, worries remain. Do we have good reasons to believe that the possibility of shared modes of access to objects is ever realized? If not, we are still left without dependable means for fulfilling our objective referential intentions. In Peirce we find both an illuminating statement of the predicament and a clue to its solution. Concrete acts of meaning are indeed dependent for their content on non-repeatable, non-shareable perspectivally individuated facts about the circumstances in which that act takes place. This is Peirce's point about the pervasive role of indexicality in natural language: the meaning of every utterance depends to some extent upon concrete non-repeatable relations among speakers, hearers and whatever is being talked about. Facts about the
communicative situation—for example about perspective and perception—allow us to approach but never achieve the identical token contents for which we aim. But the distinctness of content tokens need not prevent sameness of type; sameness of type is grounded in shared habits of thought and action associated with specific combinations of words and experiences—exactly the habits that constitute sharing a language. Specifically, distinct tokens of a perceptual or linguistic contents may be of the same type insofar as they are aimed at the same object or fact as part of their role in a collective activity. This notion of distinct concrete acts being joined together by their striving to express a common object—their teleological commonality—is expressed in Peirce's asymptote principle: the (Millian/Fregean) goal of identical objective meaning functions as a regulative norm, having the real effect of guiding natural language. A community's being guided by a common goal is, Peirce thought, just what scientific and semantic objectivity amounts to.

Husserl at first thought that, however difficult it might in fact be, that in principle there was no obstacle to fully achieving the objective reference to empirical objects which is a characteristic aim of science. This he credited to the "unlimited range of human reason." Later, however, he came to appreciate the rift between the finite means available to language users and the infinite task which objective empirical meaning seems to represent. This mismatch became apparent when he tried to spell out how perceptual evidence establishes the truth of empirical sentences. When truth is understood as adaequatio intellectus et rei, the perceptual evidence needed to establish truth would seem to aim at "adequacy" (i.e. complete correspondence) to the object. But our perspectival perceptions of objects wear on their faces their inadequacy to their objects. Each view of an object reveals something new about it, and together they form a series which forever promises more to come. In Husserl's extended struggle to explain how external objects come to be knowable for us as given in the subjective flow of experience, he provides the material needed for characterizing the parallel move toward objectivity of linguistic reference built on limited subjective access. The object itself—free of viewer relative limitation—is the ideal limit at which a series of actual perceptions aims. The objective entity functions as a regulative goal in perception. It unites distinct perceptual acts as being of the same object. The goal of a perception will be achieved if no future perception disconfirms it. But of course we cannot know this to be the case without having the infinitely many perceptions in question. We know what it is to have an objective perception, but we also know that our finitude prevents us from ever (knowingly) reaching this infinite goal. The same structure of infinite goal and finite means is present too in acts of reference which depend on perception for their content. 'This' can be used with the intention to refer to and speak about a public
object of concurrent common perception. The content of the expression is then intended to be the "ideal" content of the accompanying perception. It is the fact that we share a language which includes in the very meaning of its referential terms the intention that they refer to the object which is the limit of all the perceptions of those using that term that provides the common element joining our individual perceptual intentions to the same "ideal" content—a content which is thus intersubjective and so, to that extent, objective. In his late work Husserl described the particular human experience of being joined with others in a life-world of common objects, activities, practices (including language) and values, striving toward common, though unreachable goals. For Husserl as for Peirce, an evolving but objective science is a paradigm of this striving.

**Cleaning as an "Infinite Task"

But can we make sense of this seemingly paradoxical notion of a finite human process aimed at an unattainable goal? Actually this structure is fairly common in human activities. Consider, for example, the ordinary notion of **cleaning**. To be clean is to be free of contamination. When a person sets about cleaning a window, their goal is to remove all the foreign matter from the surface of the glass. We know how people learn this notion; they learn the activities of scrubbing, squeegeeing and rubbing the glass, and the connection between those actions and claims of "that's clean." Unfortunately, such a claim is often followed by the discovery of an unnoticed streak or smudge. The concept 'is clean' grows up along with the concept 'looks clean (but really isn't)'. But these two are not the same. In a way, the objective concept 'clean' depends on a mistaken application of the word 'clean' to mere approximations of cleanliness. We may, in fact, never experience any surfaces which are totally free of contamination; and if we did, we could never know it, being unable to infallibly distinguish the property 'looks clean' from the property 'is clean'. And yet, a person does not really understand what it is to clean (as opposed to what it means to scrub and squeegee) until they know that the goal of cleaning is to produce a surface free of contamination. In learning to squeegee one **does** experience actual examples of squeegeeing—'looks like squeegeeing' and 'is squeegeeing' typically apply to exactly the same actions. But in learning to clean, one need never experience an example of the goal. And lest it be objected that the goal of cleaning is that the window look clean, consider what the cleaner does when the offending smudge is pointed out. To look clean is perspectival; the smudge demonstrates that what did (and still does) looked clean from one perspective does not look clean from another. The fact that the window still looks clean
from the outside is no excuse for leaving the smudge which is visible from the inside. In cleaning a window, that it looks clean is just a step toward the goal—i.e., that it be clean.

So shouldn't we just conclude that 'clean' is always used improperly? Not at all. This would imply that even before we understand how certain words work we have a test to tell what "proper uses" are. That language would contain and transmit words which are always used improperly is extremely implausible. Cleaning has as its goal a state which cannot be known to be achieved, but it is a state which is nonetheless aimed at by actual examples of cleaning. To understand what cleaning is involves two thing: understanding the objective goal and understanding the practical activities (including perspectival evaluation of surfaces) involved in pursuing that goal. Although everything we call clean may turn out on further evidence to have merely seemed clean, we cannot do without (provisionally) applying the word to actual examples and also using it to describe the elusive goal of cleaning. The meaning of 'clean' is not exhausted by either its absolute sense as goal, or by its relative sense as provisional property of actual window panes. Its meaning must embrace both these senses and the teleological relation between them. The skeptic who says that we can have no such concept as 'clean' since we have no actual examples by means of which to learn it has just misunderstood the way such words work. The same goes for other goal-oriented concepts such as 'smooth', 'uniform', 'simultaneously', 'immediately', 'instantly' and, of course, 'perfect'.

Let us now, as Husserl's mature views suggest, apply this goal/means structure to the practice of referring demonstratively to an object by means of perceptions of that object. The intentional content of an ongoing perception of a tree contains as a component the external object which is the goal of that perception, the tree itself, in its independent completeness. But another part of the content is the way the tree is presented, in its variegated and shifting aspects. It is always possible that what presents itself as 'this tree' may, on further inspection, turn out to be something else (the reflection of a tree in a spotless window perhaps). But the possibility that it may have only seemed to be this tree does not make it improper to call it a perception of a tree. Nor does the fallibility of the subjective content of the perception impugn the objectivity of the tree itself, which is the goal of the perception, and which marks it as being a perception of a tree. In fact, discovery of perceptual failure confirms that objectivity is a constitutive part of perceptual content, since what distinguishes hallucination from perception is the absence of the tree, not of the image of a tree. In Millian language, we intend that our sensory perceptions be of external objects, not of our ideas of objects. What is important in our account of the
content of the perception, as in our account of the meaning of 'clean', is to recognize both
the non-relative goal and the perspectival, fallible means directed at that goal.

This structure, slightly modified, repeats itself when I say 'that tree' to someone in order
to refer to the tree. Again, the content is partially constituted by an intention to achieve an
objective goal. The modification is that in using language the relevant intention is that the
word refer, not to the object which is the goal of just my perceptions, but to one which
would be the goal of anybody's perceptions who is in this same communicative context,
including those of the person I'm addressing. so again the structure of relative means to an
absolute goal is apparent in the notions of reference and truth. As Mill said, we intend our
words to refer to objects, not to our ideas. As Frege said, we intend our assertions to
affirm thought contents which are the identical possessions of all language users. As Pierce
and Husserl said, it is our association with others in a language community—others who
Teach and correct us—that allows us to keep our individual perceptual and communicative
activities aimed at these common goals.

These remarks about the goal-oriented nature of reference grow out of a picture of
language articulated by Pierce and Husserl, but connect with Davidson's realization that the
a theory of language must take seriously the fact that language use is fundamentally
teleological. Philosophers working on this aspect of language have been repeatedly drawn
to the investigation of indexicals. The role of perception in empirical content and the
intentional interplay between perceptual content and indexical content provides a reason
why this should be so. This is enough to lead from an interest in objective reference to an
interest in indexicality. But the fact that recently investigations of indexicals have also
emerged from a tradition that explicitly tried to break the link between individual intentions
and linguistic meaning should make us wonder what contributions extensionalist formal
semantic theory might make to an account of indexical reference couched in the language of
goals and means. Those contributions are, I believe, substantial.

Model-theoretic Semantics as a Characterization of Linguistic Goals

By the early fifties, as the use of formalized languages became an established part of
philosophical practice, a variety of opinions developed about their philosophical
significance. In contrast to Carnap's quixotic pursuit of artificial languages for the actual
use of conducting and reporting scientific research, Alonzo Church expressed the view that
formal languages could aid our understanding of, but should not be expected to function as,
languages for actual communication. The success of any formalization of language must,
Church notes, be judged relative to the purpose for which it was designed.

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Let us take it as our purpose to provide an abstract theory of the actual use of
language for human communication—not a factual or historical report of what
has been observed to take place but a norm to which we may regard everyday
linguistic behavior as an imprecise approximation, in the same way that, e.g.,
elementary (applied) geometry is a norm to which we may regard as imprecise
approximations the practical activity of the land-surveyor in laying out a plot of
ground.\textsuperscript{96}

The analogy with geometry provides a useful way to think about the role of formal theory
in the overall theory of meaning.

A formal replacement language could claim to be a theory of meaning for natural language
if it could do everything natural language does in its obscure and confusing way, and do it
within a transparently understandable structure. This is roughly the sense in which set
theory can claim to provide a theory of arithmetic. This is the apparent sense of Tarski's
analogy between formal languages and natural language—that it is like the relation between
pure and applied mathematics. But formal languages like Carnap's \textit{cannot} do several crucial
things natural languages do—crucially, they cannot make assertive statements about
individual empirical objects—without using contextual resources. Church is suggesting, on
the other hand, that formal languages, even if they do not themselves provide a complete
theory of meaning, can still have a role to play; they can describe the goal at which natural
language aims. This is the role illustrated by his analogy between natural language and
surveying.

Geometry can give the surveyor only conditional advice; it can say that \textit{if} benchmark B
is located in the southwest corner of a square plot \textit{x} feet on a side, \textit{then} the opposite corner
will be \textit{2x} feet N.W. of that benchmark. Geometry does not tell the surveyor the location
of benchmark B, how long a foot is, or what direction is North, but it does make plain why
those are things the surveyor wants to know, and what the surveyor will do with that
knowledge. It is by means of those things that the surveyor attempts to reproduce on the
actual landscape the formal relations specified in the legal description. A complete
explanation of surveying will cover the abstract formal geometric relations which play a role
in the legal description, plus the general features of the techniques for projecting those
relations onto the surface of the earth. Church suggests that formal languages can play a
similar role in explaining meaning.

\textsuperscript{96} "The Need for Abstract Entities," \textit{American Academy of Arts and Sciences Proceedings},
80 (1951), p. 100.
Church's analogy, though suggestive, is vague; be can find, in the recent developments of formal semantics, examples of just the kind of thing his analogy suggests. The idea of a norm is a modal notion, so it is not surprising that it is in intensional logic that we find the clearest illustrations. When Jaakko Hintikka describes how possible worlds semantics developed out of the Carnap-Wittgenstein notion of 'state descriptions', he illuminates how possible worlds can play a role in explaining linguistic norms. Hintikka says we can think of state descriptions as

descriptions of the different possible states of affairs or courses of events (in short, 'possible worlds') in which a speaker of the language in question could conceivably find himself, and which he could in principle distinguish conceptually from each other. From this answer it is only a short step to the crucial idea that the rules for using the language will have to be shown—in principle—by the way a well informed speaker would use it in these different circumstances according to the rules, i.e., by the extensions which the expressions of the language would have in those several 'possible worlds'. This is all we need to arrive at the basic ideas of possible-worlds semantics.97

Here the use of possible worlds to characterize the ideal outcome of the "well informed" application of the rules of language is clearly articulated. The connection is especially clear with concept words; the intension of 'red'—the function which picks out all the red objects in each possible world—precisely represents the goal of every competent user of that word: that in every conceivably situation it apply to all and only the things that are red. Hintikka also show how this representational technique applies to the long-standing problem raised by Frege and Husserl about the ways of being given (modes of presentation) that characterize our connections with objects. Hintikka sees this as representable by a function in which an expression is mapped to an object. The basic idea of possible world semantics is, he says,

not very far from Frege and Husserl. Frege said that in his notion of Sinn or sense more is involved than the reference. It includes also the way in which the reference is given (die Art des Gegebenseins). Now possible-worlds semantics arises when it is realized that all such talk of 'ways of being given' is functional, that the only reasonable way of understanding Frege's statement in the last analysis is to interpret the sense or Sinn as the function which gives us the reference, by means of which we can find this reference. . . . It is not so far from Husserl, either, for what Husserl was interested in was precisely those 'vehicles of directedness', the noemata, which

97 "Carnap's Heritage," in The Intentions of Intentionality, p. 79.
enable us to intend or refer to objects. This is precisely what the meaning functions do on the possible-worlds conception.98

Hintikka realizes that he is stretching the comparison a bit, since to get "ways of being given" to work as meaning functions we have to "most liberally allow which worlds can be the arguments on which these functions depend." The functions, for example, take whole worlds as arguments; actual language users never respond to more that a small part of a world. But taken with the caveat implied in the "in principle," the comparison is instructive.99 Again, it seems plausible to identify the formal semantic analogue with the the ideal goal of actual linguistic practices. In the full explanation of how expressions are used by finite, non-perfectly-informed language users "the relevant arguments must somehow be only certain 'parts' or 'aspects' of the world in question."100 Nevertheless, our intention in using a name is that it pick out a single particular individual in whatever situation he exists, and in whatever situation we encounter him. The fact that our experiences of objects do not measure up the the situation portrayed by the function will be part of the explanation of the informativeness of identity statements involving different names of a single object.

The usefulness of this approach is clear in the way Montague and Kalish applied model theoretic semantics to the failure of substitutivity in indirect discourse. The problem arises, as Frege noted, because an object can be referred to with different expressions, and because language users may not know that those expressions refer to the same object—that one object is being given in two different ways. Montague and Kalish modeled the ways of being given—the connections between objects and expressions—with 'naming functions'.

Naming functions do not explain how we know what names attach to what objects, when different expressions attach to the same object, or when a single expression (on different occasions) attaches to different objects. They do, however, provide a good model of the goal of our use of names—we want our referring expressions to connect with objects in such a way that the uses of the name will get both speaker and hearer to the same object. This goal is precisely expressed by a naming function. In so far as language is a rule governed activity, it is directed toward this kind of goal.

In his later work Montague began to work specifically on indexicals and provided increasingly general formal analogues to natural language context-sensitivities. Formally,

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98 Ibid., p. 235.
99 Cf., ibid., p. xiv.
100 Ibid., xv.
these are devices for relativizing the translations and interpretations of sentences to various model-theoretic analogs of contextual parameters—just as designating expressions in 'that' clauses had been translated into formal analogues which were to be interpreted "relative to a naming function." In "Pragmatics" he explicitly suggests extending the Tarskian model-theoretic concept of truth relative to a model or to an interpretation to a notion of "truth relative to a context of use" as a way of dealing with indexical expressions in natural language.\textsuperscript{101} As in the case of naming functions, it will not do to complain that Montague tells us nothing about how users of ordinary language interpret sentences absent a list of possible object, exhaustive descriptions of possible worlds, or a list of which of those objects or worlds exist relative to their "point of reference." In fact it seems much of our language use is motivated just by the fact that we don't have such things, though often we wish we did. These are issues for a general theory of natural language but they are just not issues that Montague's account is intended to address. What we can ask of constructions like Montague's is how well they describe the state of knowledge and the accompanying interpretive results at which our assertive uses of ordinary language aim. Results like Montague's thus find a place within a larger theory which seeks to explain both our semantic goals and the conventional means we employ in pursuit of those goals. Major contributions to such an account are to be found in the work of David Kaplan, to which we now turn.

\textsuperscript{101} Ibid., Ch. 3, p. 96.
Fossils

Scott gives other bits of good advice that are relevant to any assessment of formal models of context-sensitivity. He notes that contextual indices are to be thought of as "fixed in advance," meaning that such a model cannot express the ways in which contexts grow and change during the course of a period of discourse. The distance of such a model-theoretic interpretation from normal human cognitive realities is clear from the fact that Scott's complete context is "some God-awful multi-tuple, which has hidden in it all the information about the domains, indices, and non-logical symbols. Once we know [this interpretation] we know everything."102 And yet it is just this distance from linguistic reality, this idealized character, that makes this part of the characterization of our semantic goals. the "God-awful multi-tuple" represents the ideal state of knowledge in which our language fulfills every communicative aspiration.

.....Kaplan/Kripke, proper names as part of the specification of possible worlds--possible worlds represent relevant alternatives, constructed to serve deliberative purposes.

Scott and Transition to Kaplan

But if Indexicals are indispensable to language, semantic theory must offer an account of them. This was recognized in a general way even by Reichenbach. But developments in formal semantics tended to treat them, as Russell and Frege had done, as marginal cases. Again, this was encouraged by a focus on mathematical and theoretical uses of language in science. But with Montague came a clear realization that natural language semantics/pragmatics demands a formal treatment suited to its own peculiarities, and is distorted if shoehorned into a semantics designed for purely formal languages. Dana Scott/// set out the outlines of a program for development of more flexible, and thus more

102 Ibid.p.163.
appropriate semantic theories, and this program bore fruits in David Kaplan's logic of Demonstratives.

Quine's problems about de re readings--a special close connection between a term and an object.
The new Kaplan 97///- philosophical semantics should provide a "theory of use," not just a "theory of meanings". This first becomes evident with indexicals. Applying the semantic analysis of 'eternal sentences' in terms of their truth conditions proves inadequate for indexical sentences. But that this would be so can already be suspected from the different ways we give "dictionary definitions." For property content words like 'feral' and 'fortnight' we look for synonymous expressions, ones which have the same meaning. For indexicals (and expressives, such as 'hello' and 'goodbye') we look for information about how they are used. There are arguments to the effect that indexicals and expressives are more basic or primitive in language, and that descriptive, factual uses of language should be looked upon as one among a range of uses. If so, then definition by synonym will be seen as a special case of definition by rule of use--'fortnight' is used in just the same way as 'period of fourteen days.'

If the general aim of a philosophical theory of semantics for natural language is to describe and understand how language is used, and if one of the uses of language is to make objective statements about determinate objects, it becomes clear why an extensional model of context is inadequate. Consider an explanation of the use of 'this' in making a statement about a specific object. A plausible first approximation of a description of such a use is: 'this' can be used to refer to a specific object in the context of the speaker and hearer, the object which is most relevant, given gestures, previous statements and other contextual clues available to the agents. This characterization contains two common features of such rules of referential use: a specification of the goal--what is to be referred to--and a specification of the means available to reach that goal--the contextual information available to the agents involved. Even in the first approximation, it also provides a list of some of the contextual features required--agents with common access to the object, surrounding expressions, gestures, etc.
The two parts of this characterization of use correspond to the apparently conflicting intuitions about linguistic reference surveyed above. We intend to make objective reference using subjective means. Once we accept that this tension is endemic to meaning--that it is part of what meaning is--we can get on with the job of fleshing out the two parts of the account. Since to goal of referential uses of language is the object itself, unconditioned by any perspectival limitations on its identity, a model theoretical semantics is well suited to give a description of this aspect of meaning. An utterance of "This is red" has as its goal to make a truth-evaluable claim about a particular object, and that claim is that it belongs to a particular class of objects which contains all and only those objects in all possible worlds which are red. This is just what the sophisticated set-theoretic models of indexical reference tell us. What they do not tell us is how, on a given occasion, a language speaker goes about trying to achieve that goal. To flesh out this second part of the rule of use for the sentence 'this is red' we look to the second part of the characterization of the use of 'this'--the speaker and hearer's access to contextual features. These features don't enter into what the speaker means to say, but they enter into how the speaker means to say it. Can this component of the semantic explanation be adequately using an extensional model of context? There are reasons to suspect that it cannot. The components of context relevant to how referential goals are achieved must be cognitively effective on the language users. A contextual factor can only play a role in fixing reference in so far as it is available to and operational on the actual language users. It makes sense to say that a language user could be mistaken about "what was said" by an utterance of 'this is red'

//To say that the meaning of an utterance of 'this' is a (set-theoretically defined) function from an extensionally described context to an object leaves unexplained the fallibility of indexical reference. This shortcoming is solved when we give the meaning of 'this' as a rule of use intended to take speakers from a perceptually apparent object to an extensionally definite object about which something objectively true can be said.
Fossils:
Carnap on the hierarchy of language

///Carnap's avowed aim is to present a description of an extensional object language, capable of stating scientific claims, and to do so using an extensional metalanguage. One thing this involves, he thinks, is eliminating context-sensitive expressions and the psychological contamination they bring. He realizes this brings with it certain restrictions, growing out of the peculiarities of the object-language / metalanguage relation which had recently been uncovered by Tarski and Gödel, but he sees no obstacle to creating the required formal metalanguages.

For every term which is stated in any unambiguous way in a word-language, there exists a formal definition in an appropriate [formal] language. . . But there exists neither a [formal?] language in which all arithmetical terms can be defined nor one in which all arithmetical sentences are resoluable. . . . In other words, everything mathematical cannot be exhausted by one system; it requires an infinite series of ever richer languages.  

Goodman:

He considers an objection that could be raised by someone who thinks that a specification of a time or place via an indexical cannot be automatically replaced by a standard date or place name. A statement using 'now' does not say the same thing as a truth functionally equivalent tenseless statement using 'October 17, 1948 at 10 p.m., E.S.T.'

unless we know in addition that the time of the utterance is October 17, 1948 at 10 p.m., E.S.T. Since—the argument runs—we recognize the tenseless sentence as a translation of the tensed one only in the light of outside knowledge, we have here no genuine translation at all. But this seems to me no more cogent than would the parallel argument that 'L'Angleterre' is not a genuine translation of 'England' because we recognize it as a translation only if we know that L'Angleterre is England. (370)

Whether there is a notion of 'translation' which requires more that just the preservation of truth value, is of course a much discussed question, but we will examining cases where it

103 P. 222
seems that we do want "translations' to reflect, for example, the cognitive significance of the statements being translated for those using the translation, or for those whose words are being translated.

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Fossil Intro:

Post-war Anglo-American Philosophy of language was not generally a hospitable environment for the elaboration of theories about context sensitivity of meaning. Those parts of Frege's and Russell's programs which dealt with formal logical relations among expressions with fixed meanings provided the basis for tremendous growth of analytic precision, but problems raised by ambiguous expression and the psychological significance of expressions tended to be set to one side. The ongoing reception of Wittgenstein's Tractatus provides an interesting illustration. The Tractatus was generally read for its formal core; its theory of "application" (the content of ordinary language) and the limits of formal analysis being treating as obscure and dispensable at best, at worst "metaphysical" or insane.

There were, however, some developments which eventually play a role in the blossoming of interest in indexicality heralded by the work of Montague and Kaplan. A brief mention of these developments will set the stage both for understanding the pivotal place of Kaplan's work in setting the agenda for theories of indexicality, and thus of indexical context, and also for understanding what are, as I will argue, fundamental inadequacies in many of the products of that agenda. Apparent progress in the analysis certain forms of context-sensitive meaning in natural language was achieved at the expense of ignoring aspects of context-sensitivity which had been identified as essential to meaning by earlier philosophers, particularly Peirce and Husserl. Intervening developments in the philosophy of language help to explain why it was thought that these aspects of context-sensitivity could be safely ignored.
The consideration of what we called, in chapter 1, "core examples" of indexical uses become especially important, both in appreciating that something essential was being passed over in the development of precise formal models of language, and in evaluating the later attempts to extend those formal models to account for indexical meaning.

Francois Recanati, building on the work of John Perry, Kent Bach et. al., has described the problems for philosophical semantics caused by

Burks .... also underestimates the importance of indexicals as "anchors“ in ostensive definitions . . . 679 }

Montague's Frankly Unpragmatic Pragmatics

Nowhere do we see more clearly the deliberate focus on the purely formal properties of natural language—to the exclusion of concern about its implementation in concrete language users—then in the work of Richard Montague. Drawing on the set-theoretic semantic techniques pioneered by Tarski, along with categorial analysis of expressions which Tarski had taken over from Husserl, and Frege's uncompromising anti-psycologism, Montague sought to map the abstract (algebraic) structures which constitute the space of possibilities within which languages develop and function. For Montague, "the syntax, semantics, and pragmatics of natural languages are branches of mathematics, not of psychology."104

At the same time, Montague's work opened the door for subsequent important advances in the appreciation of the importance and complexity of context-sensitivity, leading to the work of David Kaplan on demonstrative reference. The key to the application of formal techniques to intensional (including context-sensitive uses) uses of language, was the development of possible worlds semantics, which allow the modeling of interpretations of syntactically fixed expressions to vary relative to the circumstances at which they are

104 Richmond Thomason's characterization, in his editor's introduction to Formal Philosophy, citation ///p. 2.
evaluated. With possible world semantics comes a bifurcation between theoretical interest in formal structure and the question of application in actual language use—the bifurcation which Carnap and Church had characterized with the analogy with pure and applied mathematics. Richmond Thomason sums up this problem as follows:

Very little is known, for instance, about the actual geometry of physical space, but this does not call geometrical theory into question. The theory is applicable, but owing to experimental difficulties we do not know exactly how to apply it. The case in possible worlds semantics, however, is worse. Not only has no one ever constructed anything like a suitable candidate for the set of all possible worlds for a rich fragment of English, but it is extremely unclear what kinds of considerations would be used to guide the construction of possible worlds, or to decide between rival candidates for the set of all possible worlds, once constructed. (p.51)

In one perfectly straightforward sense we do know a great deal about the actual "geometry" of physical space—we spend our whole lives in it; what is problematic is the application of an abstract formal structure—a pure geometry—to the space of ordinary experience. But given a variety of alternative geometries, the development of applications to experienced space is an enlightening way of clarifying our intuitive grasp of the too familiar shape of that space. Similarly we all know a great deal about how to use a language rich in context-sensitive expressions, but that intuitive familiarity—a "working knowledge"—does not constitute a clear conceptual grasp of the general structural features of the language we use, or of language as such. A variety of pure formal structures which emulate natural language features provides a tool for clarification, but only when combined with a theory of application/implementation which connects it with a recognizable account of ordinary human capacities and motivations. Nonetheless, development of the formal tools often proceeds most smoothly when questions of applicability are postponed or discounted.

By enlarging the notion of a model to include indexed sets of situations, relative to which the expression is to be interpreted, Montague opened the door to explicit investigation of the contextual structures involved in fixing the meanings of particular uses of indexicals. It
should be noted, however, that Montague's technique, following the practice established by Carnap, involves modeling intensional phenomena within an extensional system. As we will see, this imposes certain limitations on the range of the investigation, particularly on the natural language contextual features which it can successfully explain.

Whether a program such as Montague's is judged successful or not will depend of course on what the philosophical purpose of the program is taken to be; on this question Montague has little to say. Clearly he is motivated by glaring failures of the existing formal systems to provide plausible formalizations of some important natural language patterns which meet even the basic criteria assigning the right truth values to individual sentences and correctly displaying valid inferential relations among sentences.

One difficulty for Montague's treatment of indexicals is his narrow focus on syntactic categories—the recursive rules of his grammar and the parallel semantics are founded on a strict set of these categories. So while indexicals as a class are, as we have seen, characterized by semantic peculiarities, they must be handled piecemeal in a syntactic system like Montague's, so that their interdefinability and interaction among contextual features are blurred. Nonetheless, Montague's approach represents an important steps toward solving this problem by breaking away from the tradition of trying to apply a single semantic pattern to all expressions; instead his approach emphasizes that different kinds of meanings can attach to different kinds of expressions, and attach in different ways. This seems to have had a liberating effect, allowing other investigators, such as Kaplan, to take seriously the wide range of meaning-patterns displayed by natural language.

One stumbling block for earlier formal systems, which Montague and Kalish set out to remedy in their paper "That" is the well known failure of substitutivity in 'that' clauses following psychological verbs. When Frege first noticed this he took it as an indication of something deeper: the distinction between truth value and the epistemic value of a sentence. Frege had noted that substitutivity fails in certain contexts when a single object can be
designated in different ways; this led him to posit a (non-extensional) aspect of meaning. This move is just what Montague wants to avoid. To explain failure of substitutivity within an extensional formalism, he introduces "naming functions" which map objects to 'designating expressions'. Designating expressions within 'that' clauses are then evaluated relative to a particular naming function as specified by the formal translation of 'that.' For this maneuver to help, he must also require that sentences containing 'that' clauses be restructured in the formal translation into sentences about sentences. Thus 'It is provable in arithmetic that 9 is greater than 5' plausibly becomes "9 > 5" is provable in arithmetic'; 'It is necessary that 9=9' becomes '9=9' is a necessary sentence'. The problems come into focus, however, when 'Kepler was aware that the number of planets is greater than 5' gets translated as 'Kepler was aware of [the truth of?] "the number of planets is greater than 5"'. Not only is this translation awkward, and transparently different in content from the original, it could not even be true unless Kepler had prescient knowledge of modern English. However, if one accepts these translations, the maneuver does fix the problem—substitutions of coreferential terms relative to a given naming function does produce valid inferences. This is a very big 'if' and Montague might be criticized for treating the symptom while encouraging neglect of the causes.

His treatment does not shed any light on the underlying aspects of Frege's problem—the difference in knowledge content between '9=9' and 'The number of planets is 9'—although it does give us another name for the problem: difference in naming function. But even this may be of some help.

/////copy to section on examples of goals///An Example of a Model theoretical GOAL--

**Naming function**

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Naming functions do not explain how we know what names attach to what objects, when different expressions attach to the same object, or when a single expression (on different occasions) attaches to different objects. It does, however, provide a good model of the goal of our use of names—we want our referring expressions to connect with objects in such a way that the given use will get both speaker and hearer to the same object. This goal is precisely expressed by a naming function. In so far as language is a rule governed activity, it is goal directed; and if naming functions help to characterize that goal, they serve a scientifically useful purpose. They do not, of course, tell us how that goal is achieved, nor why that particular goal should characterize the activity.///

In his later work, Montague provides increasingly general formal analogues to natural language context-sensitivities. Formally, these are devices for relativizing the translations and interpretations of sentences to various model-theoretic analogs of contextual parameters—just as designating expressions in 'that' clauses had been translated into formal analogues which were to be interpreted "relative to a naming function." In "Pragmatics" he explicitly suggests extending the Tarskian model-theoretic concept of truth relative to a model or to an interpretation to a notion of "truth relative to a context of use" as a way of dealing with indexical expressions in natural language.106 As in the case of naming functions, it will not do to complain that Montague tells us nothing about how ordinary users of ordinary language interpret sentences absent a list of possible object, exhaustive descriptions of possible worlds or a list of which of those objects or worlds exist relative to their "point of reference." In fact it seems mush of our language use is motivated just by the fact that we don't have such things, though often we wish we did. What we can ask of constructions like Montague's is how well they describe the state of knowledge and the accompanying interpretive results at which our assertive uses of ordinary language aim. ///

106 Ibid., (Ch. 3). p. 96.
**Dana Scott's Advice**

The full power and generality of possible-worlds semantics and its natural language application growing out of Montague's work is nicely articulated by Dana Scott. He saw that it was possible to extend the idea of a possible world to cover any dimension of an occasion of use which might be found to affect meaning, starting from the structure of tense logic where sentences are indexed by $i \in I$ representing instants of time, and then generalizing.

For more general situations one must not think of the $i \in I$ as anything as simple as instants of time, or even possible worlds. In general we will have

$$i = (w, t, p, a, \ldots)$$

Where the index $i$ has become many coordinates; for example, $w$ is a world, $t$ is a *time*, $p = (x, y, z)$ is a (three dimensional) *position* in the world, $a$ is an agent, etc. All these coordinates can be varied, possibly independently, and thus effect the truth values of statements which have indirect references to these coordinates.\(^{107}\)

Thus Scott gives a general recipe for context in its most general sense—a structure into which any conceivable contextual parameter is to be plugged.

At the same time Scott displays some terminological confusion which has continued to mask the connections between formal treatments of natural language and much earlier philosophical concerns. When he says that his treatment is an extension of the principle of "indexical expressions," he identifies that phrase with Carnap and Bar-Hillel without mentioning its transmission to them by Morris, let alone its source in Peirce. When Scott then says that the core of his extension of the formalism is the suggestion that "any system of structures can be indexed by the elements of some suitable set" he gives the impression that it is the indexed representational requirement that characterizes indexical expressions. Gone is any trace of the crucial Peircean insight into the pervasive presence of the indexical dimension in all assertive use of language.

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\(^{107}\) "Advice on Model Logic," p. 149.
Scott gives other bits of good advice that are relevant to any assessment of formal models of context-sensitivity. He notes that contextual indices are to be thought of as "fixed in advance," meaning that such a model cannot express the ways in which contexts grow and change during the course of a period of discourse. The distance of such a model-theoretic interpretation from normal human cognitive realities is clear from the fact that Scott's complete context is "some God-awful multi-tuple, which has hidden in it all the information about the domains, indices, and non-logical symbols. Once we know [this interpretation] we know everything." And yet it is just this distance from linguistic reality, this idealized character, that makes this part of the characterization of our semantic goals. the "God-awful multi-tuple" represents the ideal state of knowledge in which our language fulfills every communicative aspiration.

Scott's reflections on the basic purposes of the formal semantics program, namely "conceptual clarification," was healthful in provoking others to be explicit about the metholological decisions they were making, and to justify them in terms of their philosophical goals. Kaplan, for example, responded to Scott's thoughts by cautioning him of the dangers of ignoring "the primacy of extensional contexts," dangers which, to Kaplan, seemed to creep in when individuals are confused with their intensional correlates, individual concepts.

It is technically more convenient to treat all constants as intensional simply because we can represent the extensional ones as a subset. But one of the main aims of doing intensional logic in the way we do is to describe intensional object languages in extensional metalanguages.109 Interestingly, Kaplan's view seems to have changed over time, but the comment is revealing of the self-image of intensional logic of the late 60's.

It is an important question how far the project of formal semantics does require that intensional object languages be analyzed in extensional metalanguages, and what limitations

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108 Ibid., p. 163.
109 Ibid., "Postscript" containing comments from Kaplan and Montague. p. 172.
this puts on the range of "conceptual clarification" that can be achieved by these methods. One central fault of this type of semantic theory, which comes to the fore especially in the consideration of indexical reference, is that intentionality, in the form of belief states, the directing of attention, and the intention to refer, are not, and apparently cannot be fully represented in extensional models.\textsuperscript{110}