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A consideration of two attacks upon the sharp analytic-synthetic distinction

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Thesis

A CONSIDERATION OF TWO ATTACKS UPON THE SHARP
ANALYTIC-SYNTHETIC DISTINCTION

by

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INTRODUCTION

The purpose of this paper is to reply to the attacks made by Quine in "Two Dogmas of Empiricism"¹ and by White in "The Analytic and the Synthetic: An Untenable Dualism"² upon the so-called sharp analytic-synthetic distinction. In particular, this essay attempts to show not only that these attacks are ill-conceived, but also that Carnap's semantic methods can be used to explain analyticity in natural languages.

The two attacks are, in effect, attacks upon the conception of the analytic as definitely different from the synthetic. Although they share the same goal and some of the same formulations, they differ in both scope and method. Quine's attack is directed primarily at three of Carnap's basic conceptions--state-description, explication, and semantical rules. These he regards (for various reasons given in chapter one) as not capable of explaining the analytic. White, accepting the sharp distinction for artificial languages only, attacks the claim that some natural language has the sharp analytic-synthetic distinction.

¹Willard V.O. Quine, From a Logical Point of View: 9 Logico-Philosophical Essays (2d ed. rev.; Cambridge:Harvard University Press, 1961), chap. ii.

²Morton White, "The Analytic and the Synthetic: An Untenable Dualism", Semantics and the Philosophy of Language, ed. Leonard Linsky, (Urbana: The University of Illinois Press, 1952).

of the artificial one. Hence, it might be said that White attacks the application of what Quine attacks in principle.

There is an important similarity between these two attacks which goes beyond the sharing of common goals and formulations. This is the mode of attack or, in other words, their basic argument. Neither Quine nor White attack the distinction itself, but only some modern reformulations of it. Hence, their attacks, if successful, amount to no more than a demonstration of the failure of a formulation or two. In point of fact, both attacks are shown to be unsuccessful in the discussion in chapter one.

Both White³ and Quine⁴ appear to argue that the failure of the formulations entails the rejection of the distinction.

But this does not seem to follow. In many cases distinctions are employed which have no theoretical formulation or none which is considered satisfactory. Metallurgy was developed long before chemistry became sophisticated enough to formulate its working distinctions. And certainly no one is going to demand an end to art criticism simply because there is no satisfactory formulation of the distinction between good and bad art. In fact, it is recognized that the able critic has developed a method of distinguishing good from bad which, if properly formulated, might provide a satisfactory formulation of the distinction.

³Ibid., p. 286.

⁴Quine, op. cit., p. 37.

In addition to this non-sequitur on the part of Quine and White, there is the problem of a replacement for the sharp distinction. Both Quine⁵ and White⁶ have mentioned some sort of gradualism, but neither has presented it. Since it is not presented, it is hardly clearly formulated. Hence, there seems no reason to prefer gradualism to the sharp distinction. And some distinction is required, for there are statements such as 'All vixens are foxes' which are not logical truths and which do not seem to require factual knowledge for the establishment of their truth-value. Unless the entire question is to be shelved, the gradualists must either give up their attack or else present their alternative and show its superiority in some manner.

There is another important reason for attempting to explain the sharp analytic-synthetic distinction: it is a historical distinction which probably influenced some of the non-philosophical thinking of the past. As such, it is as important to study it, whatever its merit, as it is to study the politics, economics, or science of earlier cultures. It is very dangerous to dismiss the concepts of the past without detailed examination. To do so is to betray an attitude toward the past that makes serious study of the origins of our own culture impossible. For such dismissal is tantamount to the consideration that earlier thinkers

⁵Ibid., p. 20.

⁶White, loc. cit.

were simply confused. And this seems to be unwarranted.

The explanation of analyticity offered in the third chapter of this essay is based upon Carnap's semantic methods. As such, it implicitly bears out the explicit rejection of Quine's and White's attacks in the first chapter. This theory, however, is based upon a revision of Carnap's basic semantical scheme in that it is extensional or referential rather than intensional. This revision does not alter any of the basic notions attacked by Quine--state-description, explication, and semantical rules. These are independent of the question of intensions or extensions.

This revision, which occupies part of the second chapter, appears to be a gain in clarity and economy over Carnap's own intensional formulation. There is also something of a gain in familiarity of some of the principles involved, since the revision replaces Carnap's own attempt to formalize talk of properties with the established language of the class calculus.

The view of the analytic presented here preserves the sharp distinction between analytic and synthetic and emphasizes the public nature of analytic statements. But it takes analytic statements as relative to a culture or a subculture, e.g., the toolmakers of the culture, and as arising out of technical and/or social orthodoxies.

This view appears to be supported by certain archeological data, e.g., divergent traditions of toolmaking in bordering areas, and by some psychological data, e.g., some

children appear to regard horns as the essential property of cows, while others choose their milk-giving ability as essential.

That is, taking a toolmaking orthodoxy as evidence of a social conception that tools must be made in a certain way, both the archeological and the psychological data appear to show that both children and primitive man have conceptions of what might be called 'necessary states of affairs'. Such necessary states, e.g., cow-having horns, tool-made-by-chipping, give rise, upon reflection, to general statements such as 'All cows have horns' and 'All tools are chipped cores'. Such statements are not empirical generalizations, but are assertions of these states of affairs which are considered necessary. Since they are not empirical generalizations, but assertions of these necessary states, they are not falsified by the occurrence of exceptions. Rather it is the exceptions which are looked upon as bogus. (Several examples of this are given in chapter three.) Hence, such statements have the character of legislations or mores.

The origin of these mores is perhaps trial and error and need. Given the economic subsistence level of primitive man, any procedure which happens to be discovered as highly useful, e.g., in obtaining food, is heavily depended upon. Since, at the subsistence level, experimentation is liable to be suicidal, orthodoxy seems necessary for survival.

This may not be the sole source of such orthodoxies.

In fact, this explanation does not seem to apply either to primitive peoples at economic levels above subsistence or to children. A similar explanation might be offered for children by noting that their continual need for demonstrations of affection and approval indicates an emotional subsistence level. Hence, those answers to questions which appear to yield the greatest approval of their parents or teachers become the answers.

Perhaps the best explanation combines both these elements of dependence with a consideration of the methods of transmitting techniques from generation to generation. Since many of these techniques antedated the fuller development of speech, and since many such techniques require new terms for their description, it is likely that gestures and actual performance of the activity were the most common methods of teaching. The apprentice system, stressing mimicry, would probably result in an orthodoxy of method. If combined with a fear of deviation from accepted practice, prompted by the buffets of the teacher, orthodoxy is a very likely result.

The semantical system based upon Carnap's work serves as the model for this interpretation. The legislative analytic truths are taken in the semantical scheme as L-true or necessary statements. These may be said to assert necessary states of affairs, but only relative to a semantical system. L-truths are definitely different from factual or F-true statements, being true not only in such a way as to be unfalsifiable, but true in such a way as to be considered

true by anyone who knows the relevant portions of the language. Other behavioral-cultural interpretations of the semantical scheme are included in the discussion of chapter three.

It is important to recognize that this essay contains an explanation of analyticity using semantical methods. It is not an evaluation of the merit of the notion of analyticity. Such an evaluation appears to be very difficult and, in any case, it is beyond the scope of this essay.

CHAPTER I

Two attacks upon the so-called sharp analytic-synthetic distinction are considered in this section: Quine's in "Two Dogmas of Empiricism," and White's in "The Analytic and the Synthetic." In effect, both are attacks upon the notion of the analytic as public and definitely different from the synthetic.

Both essays begin by formulating a relation between analytic statements, synonymous expressions and logical truth.

White's formulation is:

'The statement "Every man is a rational animal" is analytic just in case it is the result of putting synonyms in a logical truth. . . Thus we have logical truth

(1) Every P is P

From which we may deduce by substitution:

(2) Every man is a man.

Now we put for the second occurrence of the word "man" the expression "rational animal" which is allegedly synonymous with it, and we have as our result:

(3) Every man is a rational animal.¹

Quine puts it differently, He writes,

There is. . . a second class of analytic statements typified by:

(2) No bachelor is married.

The characteristic of such a statement is that it can be turned into a logical truth by putting synonyms for synonyms; thus (2) can be turned into (1) by putting 'Unmarried man' for. . . 'bachelor'.²

The statement referred to by Quine by '(1)' is 'No un-

¹Ibid., p. 275.

²Quine, op. cit., p. 23.

'married man is married' and is supposed to be a logical truth. This is debatable, since 'un-' is not defined as a logical particle in any standard system. As such, it requires a definition if it is not to be gratuitous. Quine does label 'un-' as a logical particle, but he does not define it. He writes,

Statements which are analytic by general philosophical acclaim are not, indeed, far to seek. They fall into two classes. Those of the first class, which may be called logically true, are typified by:

(1) No unmarried man is married.

The relevant feature of this example is that it is not merely true as it stands, but remains true under any and all reinterpretations of 'man' and 'married'. If we suppose a prior inventory of logical particles, comprising 'no', 'un-', 'not', 'if', 'then', 'and', etc., then in general a logical truth is a statement which is true and remains true under all reinterpretations of its components other than logical particles.²

The problem here is what is to count as a reinterpretation. Substituting 'canny' for 'married' yields what appears to be a reinterpretation. But 'No uncanny man is canny' is false. Hence, there appears to be at least one false reinterpretation. And, hence, 'No unmarried man is married' does not seem to be a logical truth.

The problem here is to exclude such statements as reinterpretations. Quine has not provided any basis for this.

It might be thought that Quine intends 'un-' to be taken as 'non', and hence (1) is intended to be equivalent to 'No non-married man is married'. This appears to rule out the difficulties with 'canny' and 'uncanny', for there seems to be little ambiguity as to the meaning of 'non-canny.'

²Ibid., pp. 22-23

However, there is a problem as to the status of 'No non-married man is married'. While it is difficult to imagine false reinterpretations of this statement, this difficulty does not indicate that it is a logical truth. Quine's notion of logical truth will not serve unless it can be shown that all allowed reinterpretations are true. Until this is done, his contention that 'No non (or un-) married man is married' is a logical truth remains merely his contention.

There is another related difficulty here; there are recognized analytic statements which cannot properly be taken as analytic according to these formulations. In fact, the very statements which cannot be so taken are among the statements of essential predication which White says are his main worry.⁴ Such statements are, to take White's examples, 'All men are animals' and 'Every brother is a male'.

An analytic statement such as either of the above is supposed to produce a logical truth upon a synonym substitution. Substituting 'rational animal' for 'man' in the first, the result is 'All rational animals are animals', which is true, but not logically true according to Quine's definition. Similarly, substitution of 'male sibling' for 'brother' yields 'Every male sibling is a male' which is no more a logical truth than 'All rational animals are animals'. The demonstration of this consists simply in reinterpreting

⁴Whit, op. cit., p.274

the nonlogical component 'rational' in the first and 'sibling' in the second. For example, replace 'rational' by 'toy' and 'sibling' by 'attitude'.

If it is argued that toy animals are animals and that male attitudes are male, i.e., in some metaphoric sense of 'Animal' and 'male', then it might be replied that this introduces another complication into Quinean semantics. That is, some further rule is needed indicating that statements such as these are to be taken as true. Quine has not provided such a rule.

It might be thought that these results only show that the expressions chosen as synonyms are not synonyms. Hence, to meet this objection, consider that these statements of essential predication are supposed to be the result of, or yield, logical truths via a synonym substitution. Taking the logical truths as 'Every A is A' and 'all B are B', the only way to generate 'Every brother is a male' and 'All men are animals' from those via one synonym substitution is to take as synonym pairs 'men' and 'animals', and 'brother' and 'male'. This is absurd; the pairs are not even pairs of coextensive terms.

Quine's attack upon the notion of analyticity is an attack upon what he considers separate recent attempts to explain this notion. He considers, in succession, analyticity explained by the state-description, by explication, and by semantical rule.

According to Quine, Carnap's notion of the state-description is: any exhaustive assignment of truth-values to the atomic statements of the language.⁵ This is not Carnap's formulation, which is: "a class of sentences. . . which contains for every atomic sentence either this sentence or its negation, but not both, and no other sentences."⁶

Now, it is commonly accepted among logicians that the assertion of a sentence is equivalent to asserting that it is true. Similarly, denying a sentence is accepted as equivalent to asserting its falsity. Hence, Quine's statement seems to be merely a reformulation of Carnap's.

However, Quine's conception of the state-description, or rather, of the list of all state-descriptions, is far different from Carnap's. For Quine considers this list to be a tabular criterion of analyticity. As he writes,

A statement is . . . analytic when it comes out true under every state-description. But note that this version of analyticity serves its purpose only if the atomic statements of the language are, unlike 'John is a bachelor' and 'John is married', mutually independent. Otherwise there would be a state-description which assigned truth to 'John is a bachelor' and to 'John is married', and consequently 'No bachelors are married' would turn out synthetic rather than analytic under the proposed criterion.⁷

⁵Quine, op. cit., p. 23.

⁶Rudolf Carnap, Meaning and Necessity: A Study in Semantics and Modal Logic, (2nd ed. enlarged: Chicago: The University of Chicago Press, 1956), p. 9.

⁷Quine, loc. cit.

This notion that the list of all state-descriptions is a tabular criterion on the order of a truth-table seems to be even more clearly expressed by the following statement:

The criterion in terms of state-descriptions is a reconstruction at best of logical truth, not of analyticity.⁸

That is to say, since the only truths which Quine considers to "turn out" under this so-called criterion are logical truths, the criterion is, in effect, a truth-table for some unspecified compound sentence.

However, the list of all state-descriptions is not a criterion of analyticity. If anything in Carnap's schema is to be chosen as the criterion of analyticity, it is the semantical rules. Analyticity is defined in terms of the list of all state-descriptions, but this does not make the list a criterion.

Quine's contention that the only truths which "turn out" in this list are logical truths is contradicted by Carnap's explicit assertion that there are non-logical L-truths.⁹ Since there are non-logical L-truths, the atomic sentences may contain extra-logical synonym pairs. Analytic truths do not "turn out" in the list of all state-descriptions, because no list is ever constructed or need be.

Carnap does define a sentence as L-true in a semantic system if it holds in every state-description.¹⁰ But the

⁸Ibid., pp. 23-24

¹⁰Ibid., p. 10.

⁹Carnap, op. cit., p. 15.

criterion is the semantical rules. Carnap writes,

A sentence Z_i is L-true in a semantical system S if and only if Z_i is true in S in such a way that its truth can be established on the basis of the semantical rules of the system S alone, without any reference to (extra-linguistic) facts.¹¹

That Carnap never constructs a list of all state-descriptions is something that only a survey of his works can show. That he does not need to construct such a list is evident from the above quote which shows that the list is not a criterion. It is also evident from his introduction of ' $(x)(Hx=RAx)$ ' as L-true in S_1 :¹²

the truth of the sentence ' $(x)(Hx=RAx)$ ' can be established without referring to facts by merely using the semantical rules of S_1 , . . . and the truth rules for the universal quantifier and for '='.¹³

The semantical rules referred to by Carnap are:

' Hx '--'x is human (a human being)', ' RAx '--'x is a rational animal', and "the English words. . . 'human being' and 'rational animal' mean the same."¹⁴ The rules of truth for the universal sentence and the biconditional sentence are:

$Z_i=Z_j$ holds in a state-description if and only if either both Z_i and Z_j or neither. . . hold in it; a universal sentence (e.g. ' $(x)(Px)$ ') holds in a state-description if and only if all substitution instances of its scope (' Pa ', ' Pb ', ' Pc ', etc.) hold in it.¹⁵

The atomic sentences ' Pa ', ' Pb ', ' Pc ' etc. hold in a state-description if they are members of, or belong to, it.¹⁶

¹¹Ibid.

¹³Ibid.

¹⁵Ibid., p.9.

¹²Ibid., p. 15.

¹⁴Ibid., p. 4.

¹⁶Ibid.

Hence, it is clear that the introduction of non-logical analytic truths does not require, according to Carnap, construction of a list of all state-descriptions. And hence, since this is a non-logical L-truth, the atomic sentences of the language need not be independent or devoid of extralogical synonym pairs. And again, these L-truths do not "turn out", because, as noted above, there is no list in which they "turn out". They are, as this example shows very clearly, stipulations, not results.

Quine next considers Carnap's notion of explication. Hence, here too, Quine makes assertions about Carnap's semantics that are not warranted by Carnap's writings and are not detailed enough to be acceptable criticism. Quine rejects any explanation of analyticity via explication because he considers that explications rest on notions of synonymy. He writes,

In explication the purpose is not merely to paraphrase the definiendum into an outright synonym, but actually to improve upon the definiendum by refining or supplementing its meaning. But. . . explication. . . does rest nevertheless on other pre-existing synonymies.¹⁷

And this assertion, which is not Carnap's is explained by Quine in the following way:

Any word worth explicating has some contexts which, as wholes, are clear and precise enough to be useful; and the purpose of explication is to preserve the usage of these favored contexts while sharpening the usage of other contexts. In order that a given definition be suitable for purposes of explication, therefore, what is required is. . . just that each of these favored contexts of the definiendum, taken as a whole in its antecedent usage, be synonymous with the corresponding context of

¹⁷Quine, op. cit., p. 25.

the definiens.¹⁸

Carnap, on the other hand, says that explication is:

The task of making more exact a vague or not quite exact concept used in everyday life or in an earlier stage of scientific or logical development, or rather of replacing it by a newly constructed, more exact concept. . .¹⁹ This earlier concept, or sometimes the term used for it, is called the explicandum; and the new concept, or its term, is called an explicatum of the old one. Generally speaking, it is not required that an explicatum have, as nearly as possible, the same meaning as the explicandum; it should, however, correspond to the explicandum in such a way that it can be used instead of the latter.²⁰

It is important to note that Quine's statement differs from Carnap's in that Quine introduces new notions: refinement and supplementation of meaning, preservation of usage, and synonymy of context. Hence, it is difficult to take Quine's statement as a paraphrase of Carnap's. It may, however, be intended as a clarification. And certainly Carnap's statement, especially the phrase 'can be used instead of' is puzzling.

Yet, as Quine explains none of his notions, it is very difficult to take his statement as a clarification of Carnap's.

Carnap has himself offered something of a clarification of the puzzling expression 'can be used instead of' in both his various uses of explication²¹ and in a discussion of just this notion. In neither case does he approach a

¹⁸Ibid., p.25

²¹Ibid., pp.8ff.

¹⁹Carnap., op.cit., pp.7-8.

²⁰Ibid., p.8.

suggestion of synonymy of context or refinement of meaning.

Carnap does explicitly state that it is wrong to regard the requirement of similarity of explicatum to explicandum as vacuous.²² He points out that the explicandum itself must be clarified by informal discussion. And it is here that he does mention that certain usages are to be favored over others, e.g., 'true' not as in 'true friend', 'true democracy', but as in science, logic, and law as more or less having the sense of 'correct', 'not false', etc.. But these favored contexts are only those favored for clarifying the explicandum. The relation of these to the explicatum is another matter.²³

Carnap notes that the similarity of explicatum to explicandum is not always as close as even the vagueness of the explicandum permits. He says that "this requirement would be too strong, that the actual procedure of scientists is often not in agreement with it, and for good reasons.²⁴ These good reasons are: (1) "a scientific concept is more fruitful the more it can be brought into connection with other concepts on the basis of observed facts. . . .²⁵ and (2) "scientists appreciate simplicity in their concepts."²⁶

²²Rudolf Carnap, Logical Foundations of Probability (2d ed.; Chicago: The University of Chicago Press, 1962), p.4.

²³Ibid., pp.4-5.

²⁶Ibid., p.7.

²⁴Ibid., p.5.

²⁵Ibid., p.6.

Taking Carnap himself as the scientist (semanticist), and his concept of L-truth as explication of (or as explicatum of) "what Leibniz called necessary truth and Kant analytic truth."²⁷ It is possible to see that Carnap is not concerned with refinement of Kant's or Leibniz' usage. For Carnap does neither epistemology nor theology, and 'true by meanings' and 'true in all possible worlds' would seem to change their meaning somewhat without a similar surrounding structure.

At any rate, while there is no denying that Carnap's notion of explication requires further study and elaboration, especially concerning the relation of similarity between explicandum and explicatum, it is evident that Quine has not made his case.

Quine himself expresses his understanding of the notion of explication rather differently in a later writing. He writes, that in explication,

We do not claim synonymy. We do not claim to make clear and explicit what the users of the unclear expression had unconsciously in mind all along. We do not expose hidden meanings. . . we supply lacks. We fix on the particular functions of the unclear expression that make it worth troubling about, and then devise a substitute, clear and couched in terms to our liking, that fills those functions. Beyond those conditions of partial agreement, dictated by our interests and purposes, any traits of the explicans core under the head of "don't cares". . . Under this head we are free to allow the explicans all manner of novel connotations never associated with the explicandum.²⁸

²⁷ Carnap, Meaning and Necessity, p.8.

²⁸ Willard V.O. Quine, Word and Object (New York: John Wiley & Sons, Inc., 1960), pp.258-259.

Here Quine is using 'explicans' where Carnap uses 'explicatum'. It appears that Quine has abandoned his earlier insistence on synonymy of context, refinement of meaning and preservation of usage. He suggests, rather, conditions of partial agreement and some partial similarity of function. While the matter is still somewhat clouded, it seems that Quine has given up, by allowing all manner of new connotations to the explicans, his earlier notion that an explication is a kind of definition or redefinition of the explicandum.²⁹ This is suggested by his terminological shift from writing 'definiendum', writing 'explicandum', as well as in the other differences noted above.

The third of Carnap's notions which Quine discusses in this essay is that of semantical rules. Since this notion figured in the discussion of state-descriptions, it is relevant to note that this separate discussion of each notion, i.e., state-description, semantical rules, and explication, indicates a misconception by Quine of Carnap's structuring of the analytic. These are not separate defences of explanations of analyticity as Carnap presents them; they are parts of one presentation of a schema in which analytic statements have a proper place.

Quine's attack upon semantical rules, however, is far better than any of the other formulations or attacks considered here. He raises an important issue succinctly in the following. He writes,

²⁹Quine, From a Logical Point of View, p.25.

Appeal to hypothetical languages of an artificially simple kind could conceivably be useful in clarifying analyticity, if the mental or behavioral or cultural factors relevant to analyticity--whatever they may be--were somehow sketched into the simplified model.³⁰

But he does not follow his own suggestion and sketch. Rather, he laments the absence of consideration of these factors and says,

Semantical rules are distinguishable, apparently, only by the fact of appearing on a page under the heading 'Semantical Rules'; and this heading is in itself then meaningless.³¹

And this is not so, as a glance at Meaning and Necessity reveals.

The semantical rules include, according to Carnap:

- (i) Rules of formation, on the basis of a classification of the signs; these rules constitute a definition of 'sentence'.
- (ii) Rules of designation for the primitive descriptive constants, namely, individual constants and predicates.
- (iii) Rules of truth.
- (iv) Rules of ranges.³²

Each of these kinds of rules has a definite function, and although no page is headed 'Semantical Rules' such a heading would be no more meaningless than a page heading such as 'List of Definitions' or 'List of Theorems and Metatheorems' or, for that matter 'List of Parts'. Semantical rules, like theorems, metatheorems, gears, plugs, and tubes, are distinguishable according to what they do, i.e., according to their function.

³⁰Ibid., p.36.

³¹Ibid., p.34.

³²Carnap, Meaning & Necessity, p.169.

However, these errors should not be allowed to obscure what is valuable in Quine's attack upon semantical rules. For what Quine appears to be saying is that Carnap's semantical rules are purely formal and unrelated to any behavioral or cultural phenomena that might be considered as relevant to the presence of analytic statements in some natural language. In short, Quine appears to be asking for a behavioral-cultural equivalent of a semantical rule. Such a demand is valuable, as it points directly at the purpose of formal semantic analysis: explaining the phenomena of natural language.

It is unfortunate that Quine does not press and explain his demand more fully. Certainly one of the major problems of logical semantic analysis is the relation of the logico-semantical schemas to the data of natural language. Quine appears to be rejecting Carnap's purely formal approach to semantics, and to be proposing a more applied one of some sort, but exactly what sort is not clear.

It may be replied to this that Quine is, after all, merely writing a critical essay and not a book. Yet, such a suggestive criticism would benefit from an elaboration of its implicit point of view. Such an elaboration need not itself be a full theory of analyticity. Rather, it could be limited to the manner in which these behavioral-cultural factors are sketched into the model language.

What Quine seems to miss in this attack is that, while Carnap is doing neither epistemology nor anthropology nor sociology of language, his constructions, or ones very like them, can be used to in the very behavioral-cultural explanation he so vigorously demands. This will be sketched in the last section of this paper.

White's essay is primarily an attack upon investigations of the sort mentioned in this last paragraph. Or, to put it better, it is an attack upon attempts to say that "people using natural languages behave as if they had made rules for their language. . ."³³

White asks those who undertake such attempts "How do we establish when people behave as if they had done something which they haven't done?"³⁴ This dogmatic assertion about what people haven't done is weakly supported by White's assertion,

I suppose it would be granted that those who use natural language do not make conventions and rules of definition by making a linguistic contract at the dawn of history.³⁵

White then turns to a consideration of three replies to his question. These replies consist simply in the offering of criterion of analytic statements in ordinary language and are most certainly not the only or the best replies that could be given.

The first reply is that of C.I. Lewis. He appeals to

³³White, op.cit., p.278. ³⁵Ibid., p.279.

³⁴Ibid., pp.278-279.

an intuitive insight or imaginative experiment as to whether one criterion in mind includes another, e.g., whether the criterion in mind for man includes the criterion in mind of rational animal. The result of this experiment is supposed to be that "I cannot consistently think of, . . . cannot conceive of, a man who is not a rational animal."³⁶ White disposes of this criterion on the grounds that this early retreat to intuition is not satisfactory.³⁷

The remaining two replies are considered in more detail. The first of these is: analytic statements are those whose denials are self-contradictory. White notes that not all the denials of allegedly analytic statements are of the form of syntactic, or logical, self-contradictions. He further notes that an appeal to the senses of the component terms to establish the status of a sentence as a self-contradiction is question-begging.³⁸ This does not seem necessary. In fact, White's claim is one that requires substantiation.

White discusses a non-question-begging criterion of so-called 'self-contradiction' with interesting results. He relaxes the syntactic requirement for self-contradiction and allows in all statements which produce feeling or horror or queerness on the part of the people who use the language.

³⁶Ibid., p.279.

³⁸Ibid.

³⁷Ibid., p.280.

Noting that not all the members of the language-using community will experience horror at such denials, White asks "Who, then?"³⁹ He also notes that the horror experienced at the denials of analytic statements must somehow be distinguished from the horror at the denials of firmly believed synthetic statements.

All this is sensible enough, but it in no way warrants White's conclusion that this criterion is going to yield a gradualism between analytic and synthetic statements. It is as unwarranted as the claim that because water occurs as ice at various temperatures in nature, that there is no freezing point but only a gradation of freezingness. White himself has suggested this analogy and he replies to criticisms as the one above,

But it should be pointed out that a conception according to which "analytic" is simply the higher region of a scale on which "synthetic" is the lower region, breaks down the radical separation of the analytic and the synthetic as expressive of different kinds of knowledge.⁴⁰

Again, while this reply is vague and metaphorical, it can clearly be seen that what it says need not be so. A scale could be constructed on which all synthetic statements have numbers less than zero, and all analytic ones numbers greater than or equal to zero. The kinds of knowledge expressed by each kind of statement could be as different as you please. This is obviously not what White wants, but he has not shown why this could not be the case.

³⁹Ibid., p.2⁹¹.

⁴⁰Ibid., p.2⁹².

The second criterion receiving White's detailed consideration is the term-withholding criterion. This is apparently a criterion of synonymy. It is stated as: if we were presented with something that wasn't a rational animal, we would not call it a man. After again noting that not all the members of the language community would withhold the term, White imagines two experiments involving native subjects. These imagined experiments are supposed to show something about the proposed criterion. In fact, they serve only to illustrate White's methodological naiveté.

The first of these imagined experiments involves the checking of a translation by anthropologists. These anthropologists have concluded from their investigations that whereas 'man' and 'rational animal' are synonymous in the native language, 'man' and 'featherless biped' are only coextensive. White imagines himself checking this by the simple procedure of showing the natives various things, saying the native word for 'man' and awaiting native assent or dissent. And since during this imagined investigation, the natives called, i.e., assented to the calling of by White, neither non-featherless bipeds nor non-rational animals, White concludes that (1) ". . . such crude behaviorism will not avail." and, puzzlingly, that (2) "the criterion, therefore, is one that will not help us make the distinction."⁴¹ The distinction here is, apparently,

⁴¹Ibid., p.283.

that between synonymous and coextensive terms.

What is puzzling about White's second conclusion as to the merit of the distinction is that if the first conclusion is sound, and it seems to be, the second is not. For if the procedures for applying the criterion are unsound, the criterion has not been properly applied, and no conclusion can be drawn as to its merit.

That such procedures as White employs in his imagined experiment may well be unsound can be seen not only from the consideration of limitations of pointing, e.g., it is ambiguous as to spatial and temporal spread, but also from consideration of the structure of native language. White is saying single native expressions, not whole native sentences. Or, rather White is using so-called native words as single word sentences such as '(this is a) man'. It may be that the structure of the native language is such that the same expression used as a sentence differs from its use in a more than single word sentence. White's experiment is carried on in utter ignorance of such considerations.

White's second imagined experiment has slightly different conditions in that he is able to converse with natives. He asks them, "Aren't you surer in concluding that something is not a man from the fact that it is not a rational animal, than you are in concluding it from the fact that it is not a featherless biped?"⁴² And getting an

⁴²Ibid.

affirmative answer (which is surprising considering the length and difficulty of the question), White concludes he has,

a criterion which makes of the distinction a matter of degree. Not being a rational animal is simply a better sign of the absence of manhood than is the property of not being a featherless biped. . . .⁴³

Aside from the two non-sequiturs, i.e., concluding from an answer a criterion and a gradualistic notion of synonymy or analyticity, the assertion that not being a rational animal is simply a better sign of the absence of manhood that is not being a featherless biped is absurd. Bipedity is an easily observable property; children of two years notice it regularly. But rationality, far from being easily observable, itself requires a criterion. If, to follow White solely for the purpose of argument, rationality is taken as an observable property, e.g., coherent and appropriate behavior, then it is difficult to see why those featherless bipeds (on this planet) who act incoherently are commonly taken as erring or ill human beings and not merely as animals.

To return to the non-sequiturs in White's argument, it is obvious that an affirmative answer to this question does not entail the making of a criterion whether of degree or not.

Degrees of felt certainty, and that is what is called for here, are compatible with as deep an analytic-synthetic

⁴³Ibid.

or synonymy-coextensivity distinction as is imaginable. One may be uncertain or sure about anything for many reasons other than those relevant to White's criterion. Ignorance, anger, a desire to play god or demon, etc. can prompt unlimited amounts of dogmatism. Similarly, some fear dogmatism to the point of never deciding even simple matters without great deliberation. White has not shown that this test, if it is that, shows what he claims it shows.

The notion that statements of essential predication or non-logical analytic statements generally merely express better signs than generalizations such as 'All men are featherless bipeds' is, as noted above, a poor one. Common-sense reflection upon the history of human social and political life in this century alone should suffice to prompt the recognition that man's rationality is a sometime thing. So long as 'rationality' means 'agreeability to argument', White's apparent claim that rationality is one of the better observable signs of manhood runs contrary to the statements of history.

A much more obvious approach to explaining analyticity consists in taking the label 'statements of essential predication', which tradition applies to non-logical analytic statements, at face value. Quine mentions just such a construction in passing.

The Aristotelian notion of essence was the forerunner,

no doubt, of the modern notion of intension or meaning. For Aristotle it was essential in men to be rational, accidental to be two-legged. Meaning is what essence becomes when it is divorced from the object of reference and wedded to the word.⁴⁴

As statements predicating essences, non-logical analytic statements are statements of a kind of concepts. Hence, it would seem that to follow the Quinean demand for the consideration of the behavioral or cultural factors relevant to analyticity is to undertake a study of the behavioral or cultural bases of this kind of formation. Of course to do so is only to satisfy part of this demand; these factors have also to be sketched into the model language.

It is interesting to note that Quine's own study of the psychology of language in Word and Object neglects the topic of concept-formation. He writes all around it, mentioning the relativity of ontology and of sentence significance to a given theory:

Unless pretty firmly and directly conditioned to sensory stimulation, a sentence S is meaningless except relative to its own theory; meaningless intertheoretically.⁴⁵ The statements are about posited entities, are significant only in relation to a surrounding body of theory, and are justifiable only by supplementing observation with scientific method. . . .⁴⁶

He describes the origins of speech in the child as reinforced operant behavior producing mimicry⁴⁷, and the development from this basis of one-word or occasion sentences. He notes the difference between the child's resulting

⁴⁴Quine, From a Logical Point of View, p.22. ⁴⁶Ibid.

⁴⁵Quine, Word and Object, p.24. ⁴⁷Ibid., p.80f.

conceptual scheme and the adult conceptual scheme of mobile and enduring physical objects, which are the referents of the terms in adult language.⁴⁸ But he does not explain either the origins of, nor the changes in, the adult conceptual scheme. Nor, for that matter, is there any account of the changes in the child's conceptual scheme as he gradually progresses toward a grasp of adult language and concepts.

Quine does mention both the synonymy-coextensivity distinction for terms and the analytic-synthetic distinction for sentences, but merely as by-products of language learning.⁴⁹ He admits that "these are still not behavioristic reconstructions of intuitive semantics, but only a behavioristic ersatz."⁵⁰

This is a pretty obvious indication that Quine is dissatisfied with his explanation of analyticity. Why he did not abandon it and attempt another on different lines is puzzling. Certainly, as can be seen from the citations in this chapter, it is not because the problem is uninteresting to him.

At any rate, consideration of these two attacks has shown that they are not successful as attacks. As sources of suggestions and as philosophical experiments of a sort they are very useful. Quine's suggestion that the behavioral and cultural factors relevant to analyticity be sketched into

⁴⁸ Ibid., pp.92-93.

⁵⁰ Ibid., p.66.

⁴⁹ Ibid., pp.66-67.

the simplified model language is followed in the discussion of the last chapter. And White's experiments, in their very lack of success, suggest that this naive method of directly questioning native subjects is not useful.

One useful method, which combines Quine's suggestion with White's comment that analytic statements are statements of essential predication, appears to consist in studying the behavioral and cultural data relevant to the conception of essential properties and in attempting to sketch such data into a model language.

The next chapter is devoted to the building of the model language. The last, to the consideration and interpretation of the data as well as the sketching of it into the model.

CHAPTER II

Carnap

Carnap's semantic analysis, unlike those considered previously in this paper, proceeds by the construction of artificial formal language systems. This formality and artificiality do not exclude consideration of ordinary name languages such as English, for the metalanguage of some of these artificial systems, e.g., S1, is a portion of the English Language. Nor, taking Carnap at his word, does the formality appear to exclude consideration of the epistemology of language. In fact, the distinction between intensions and extensions is apparently epistemological. That is, intensions and extensions are distinguished on the basis of conditions of knowledge. More specifically, whereas understanding the designator is a sufficient condition for understanding the intension of the designator, it is not sufficient for understanding its extension. Carnap writes,

A designator stands primarily for its intension; the intension is what is actually conveyed by the designator from the speaker to the listener, it is what the listener understands. The reference to the extension. . . is secondary; the extension concerns the location of application of the designator, so that, in general, it cannot be determined by the listener merely on the basis of his understanding of the designator, but only with the help of factual knowledge.¹

¹Carnap, Meaning and Necessity, p.157.

However, Carnap, once having made this distinction, neglects epistemology to the detriment of his analysis. It is one of the purposes of this section to show that Carnap cannot consistently neglect or dismiss epistemology while doing a semantic analysis which rests on such a distinction.

Before showing this, however, there is some preparatory of background material that must be presented.

Intensions and extensions can be properly introduced only after discussion of formal language systems, metalanguage, semantical rules, and state-descriptions.

A simple example of a formal language system is S1. S1 consists of a portion of the notation of symbolic logic, plus nonlogical constants of both individual(subject) and predicate types. Truth-functional connectives for negation, disjunction, conjunction, material implication, and material equivalence, plus individual variables and universal and existential quantifiers complete the basic notation. In addition, there is the iota-operator for individual descriptions and the lambda-operator for property or class expressions.²

In order for the shapes of S1 to be used to make descriptive expressions, including statements, it must be supplemented by M. M is the metalanguage of S1. It contains portions of English suitable not only for the descriptive interpretation of S1, but for the discussion of the expressions of S1 and its interpretation as well. Hence,

²Ibid., pp. 3-4.

M contains, in addition to the descriptive interpretations of the non-logical constants of S1, names of the descriptive interpretations, names of the expressions of S1, and certain semantical terms. The descriptive interpretation of the nonlogical constants of S1 is given by the rules of designation:

's' is a symbolic translation of 'Walter Scott'
 'w' -- '(the book) Waverly'
 'Hx'--'x is human (a human being)'
 'Rax'--'x is a rational animal'
 'Fx'--'x is (naturally) featherless'
 'Bx'--'x is a biped'
 'Axy'--'x is an author of y'

In addition, Carnap indicates that "The English words used here are supposed to be understood in such a way that 'human being' and 'rational animal' mean the same."³

This last does not appear either in form or in function to be like the other rules of designation. That is, it neither states explicitly a rule of symbolic translation nor contains any of the symbols of S1. The point is that 'Hx' and 'Rax' are both to be translated as 'x is human (a human being)' and 'x is a rational animal'. This could be done simply by listing both translations opposite both 'Hx' and 'Rax'. Alternatively, since the purpose of this rule is to legitimize '(x)(Hx=Rax)' as true by the semantical rules of S1 alone, this rule need not be taken as a designation rule but as some other kind of semantical rule of range for '(x)(Hs=Rax)'. This alternative will be

³Ibid., p.4.

presented in the discussion of rules of ranges below.

An atomic sentence is defined as: any predicate of degree N , i.e., having free occurrences of N different variables, followed by N individual constants.⁴ Carnap does not give specific rules of formation, such as this one, for non-atomic sentences, but the examples used throughout the book indicate that the common rules of formation apply.

The rule of truth for atomic sentences of degree 1 is: an atomic sentence of degree 1 is true if the individual to which the individual constant refers has the property to which the predicate refers. This rule obviously presupposes the rules of designation, since there is no referring to individuals or properties without such rules. More complex sentences, both atomic (of degree greater than 1) and non-atomic, have similar rules corresponding to their truth-tables. Quantified sentences will be discussed shortly.

Having more or less in principle introduced the notion of the truth of a denial of an atomic sentence and the notion of the truth of a material biconditional sentence, Carnap is in a position to introduce the semantical terms 'false' and 'equivalent'. A sentence is false if its negation is true. Two sentences are equivalent if their material biconditional is true.⁵

⁴Ibid., p.5.

⁵Ibid., p.6.

The L-concepts are introduced next along with the concepts of state-description and range. The L-concepts are explicata for certain concepts which, according to Carnap, have been used by philosophers for many years without being satisfactorily defined. For example, L-truth is the explicatum for truth based on purely logical reasons or on meanings, rather than facts.⁶

A state-description in S1 is a class of sentences of S1 which contains for every atomic sentence either this sentence or its negation but not both, and no other sentences.⁷

That class of all those state-descriptions in which a sentence holds is its range. Generally, sentences are said to hold in a state-description if they would be true if all the sentences of the state-description were true. For atomic sentences and the denials of atomic sentences, holding in a state-description is the same as belonging to it. Or, since no state-description has both an atomic sentence and its denial, the denial can be said to hold if the sentence does not. Disjunctions of atomic sentences and/or denials of atomic sentences hold if both or neither hold. Universally quantified sentences, treated as conjunctions of their substitution instances, are true in a state-description if all their substitution instances hold.

The range of an L-truth (in S1) is every state-description (in S1). Tautologies such as 'Hs V -Hs' and

⁶Ibid., p.10.

⁷Ibid., p.9.

'(x) (Bx=Bx)' can be seen to be L-true, since wherever 'Hs' fails to hold, '-Hs' holds and vice-versa; and wherever 'Bs' or 'Bw' holds or fails to hold, 'Bs' or 'Bw' holds or fails to hold.

The ease with which such logical truths can be seen to be L-truths can be misleading in at least two ways. First, it can be taken as indicating that there are no problems about the notion of L-truth. And second, it can be taken as indicating that a list of all state-descriptions amounts to no more or less than a truth-table for a statement formed by connecting all the atomic statements of the language system.

That a list of all state-descriptions cannot be taken simply as a truth table can be seen in at least two ways. First, because there are non-logical L-truths, e.g., '(x) (Hx=RAX)';⁸ and second, because state-descriptions are descriptions of possible states of the universe of individuals with respect to the properties of the system.⁹ As a description of a possible state of a universe, a state-description differs from a truth-value assignment in a truth-table by being semantic or interpreted rather than merely logical or syntactical. The possibility it states is not simply logical, but interpreted. And this creates a problem, for the construction of a list of all state-descriptions for a language system such as S1 is not entirely mechanical.

⁸Ibid., p.15.

⁹Ibid., p.9.

Since ' $(x) (Hx=RAx)$ ' is an L-truth, no state-description can contain 'Hs' if it does not contain 'RAs', and vice-versa. But there is a problem as to whether any state-description need contain 'Hw'. To include 'Hw' in a state-description is to say it is possible for the book Waverly to be a human being. And this seems peculiar.

The definition of a state-description requires that every atomic sentence or its denial be present. There is no corresponding definition for 'every state-description' requiring that all atomic sentences and all denials of atomic sentences be present somewhere in the list of all state-descriptions. So, there appears to be no need to include troublesome sentences such as 'Hw' in some state-descriptions. However, it seems preferable to have a rule stating explicitly the exclusion of 'Hw' from all state-descriptions. Such a rule is: ' $\neg Hw$ is an L-truth (in S1)'. For reasons similar to those advanced for 'Hw', it is well to consider ' $\neg Bw$ ', ' $\neg Aws$ ', ' $\neg Aww$ ', and ' $\neg Ass$ ' as L-truths.

A list of such rules is not supplied by Carnap, but the addition of such a list does not seem inconsistent with Carnap's intentions. In fact, such a list of non-logical L-truths, as a list of statements holding in every state-description (in S1), seems to be merely an addition to the rules of ranges. Such a list is a very convenient place in which to state that ' $(x) (Hx=RAx)$ ' is an L-truth.

There is another problem in the construction of a list of all state-descriptions. Stipulations of L-truths, both logical (by the rules of ranges) and non-logical (by the rules of designation or as above), serve to exclude certain sentences or sentence pairs from occurring in any state-description. But there is no rule giving the number of occurrences of, e.g., 'Fs'. There are rules such as ' $(x) (Hx=RAx)$ ' is L-true' which specify that the number of occurrences of 'Hs' is the same, and occur in exactly the same state-descriptions, as those of 'RAs'. But the statement could be L-true with no occurrences of either 'RAs' or 'Hs' anywhere in the list of state-descriptions. Of course, as noted above, excluding 'RAs' and 'Hs' from every state-description makes their negations L-truths, and this cannot be done without having or making a rule to that effect. Yet, given such a rule, ' $(x) (Hx=RAx)$ ' would still be L-true, since its substitution instances, e.g., 'Hs=RAs' would all hold in all state-descriptions.

This problem does not appear to create any difficulties for Carnap's analysis, but it is a problem for those who wish to consider the list of state-descriptions as a truth-table. The construction of a list of state-descriptions is not only not mechanical, as is the usual procedure for truth-tables, but is, with ~~certain~~ exceptions, not possible. The ~~certain~~ exceptions to this are the positional languages, languages whose individuals are numbers or ordered N-tuples of numbers and whose predicates are mathematical. Such

a language contains only L-true and L-false sentences.¹⁰

On the basis of the definition of L-truth, Carnap introduces L-falsity, L-implication, L-determinacy and L-equivalence. A sentence is L-false if its denial is L-true. A sentence L-implies another, if their material conditional is L-true. Two sentences are L-equivalent if their material biconditional is L-true. And any sentence is L-determinate if it is either L-true or L-false.¹¹

Sentences which are true but not L-true are F-true. Hence, there is F-falsity, F-implication, and F-equivalence. Any F-true or F-false sentence is L-indeterminate.

Equivalence and L-equivalence are extended to designators other than sentences: to predicators (predicate expressions) and individual expressions. Any two predicators of the same degree are equivalent if their full sentences (with the same sequence of individual expressions) are equivalent. If the sentences are L-equivalent, then the predicators are L-equivalent; if not, then F-equivalent.

In a language system with predicator variables a similar definition could be made for the equivalence (L-and F-) of individual expressions. Since S1 lacks these, the definition for equivalence of individual expressions is simply that they are equivalent if they name the same thing. Carnap does not say that individual expressions can be L- or F-equivalent, but it would seem that they can be either. For example, 'the author of Waverly' is

¹⁰ Ibid., p. 87.

¹¹ Ibid., p.11.

F-equivalent to 'Walter Scott', and, with some modification of S1, 'Scott' could be taken as L-equivalent to 'Sir Walter Scott'. These modifications could simply be stipulations in the rules of designation to the effect that 'Scott' names the same thing as 'Sir Walter Scott'.

Carnap next introduces the intensions and extensions. For predicators, the extensions are the corresponding properties.¹² These are choices or decisions, not conclusions. But Carnap does offer a few appeals to make the choices seem familiar. And these appeals, rather than making the choice of properties as intensions seem familiar, make them appear arbitrary.

Carnap notes that it is customary in analysing the meaning of an adjective in a name language or of a predicator in a symbolic language to speak of two entities--classes and properties. He remarks that talk of the two kinds differs essentially in that the identity conditions for classes and property are not the same. Classes are identical if they have established identity conditions. Carnap decides to rectify this situation by regarding two properties as identical if it can be shown by purely logical means (including semantical rules) that whatever has one property also has the other.

He takes properties as identical if predicators for them are L-equivalent. Since he has stipulated that L-equivalent predicators have the same intension, he chooses

¹²Ibid., p. 19.

properties as the intensions of predicators.

Properties are not the only possible choices for intensions of predicators recognized by Carnap. The class of predicators L-equivalent to the given predicator is another. This he considers less natural, since it is a linguistic entity rather than an extra-linguistic one.¹³

There is another choice for intensions of predicators which seems not only more natural in view of the choice of classes as extensions of predicators but also preferable for several other reasons. This is classes give by rule, e.g., the class of primes greater than one and less than seven. That is, it is preferable if the choice of extensions for predicators is classes given by enumeration, e.g., the class containing 2,3,5. If Carnap's choice of extensions for predicators is classes given by rule, then this other choice of intensions is not preferable. For Carnap insists that in general extensions and intensions are different.

It is not clear that Carnap has chosen classes given by enumeration as extensions of predicators. There are certain statements that make this seem very likely, and none that explicitly deny it.

Perhaps the clearest statement that the extensions of predicators are classes given by enumeration occurs in Carnap's assertion of the impossibility of the elimination of properties or intensions for predicators. He writes:

¹³Ibid.

the question might be raised. . . whether it could not happen in some system that a predicator has only an extension; . . . that it refers to a class without referring to any of those properties which have that class as extension. I think. . . this is not possible in a semantical system. . . . It is not possible to refer to a class without referring to at least one of the corresponding properties. This holds, even if the class is specified by an enumeration of its members, e.g., by a phrase like 'the class of the individuals a,b, and c' . . . This predicator does not lack an extension; it is the property of being (identical with) either a or b or c.¹⁴

Since this seems to make the required point better than any other of Carnap's statements, there is little to gain by quoting further. The epistemological characterization of extensions as concerning the location of application of designators and as determined only by factual knowledge also appears to support the notion that the extensions of predicators are classes given by enumeration, but the support is not explicit. It seems that what Carnap has in mind is some kind of empirical investigation, but what kind is not clear from his remarks.

At any rate, supposing that this quoted statement is taken as establishing that the extensions of predicators are classes given by enumeration rather than by rule, the advantages of taking classes given by rule as intensions of predicators can be stated.

The first advantage concerns the epistemological distinction between intensions as actually conveyed by the designator and extensions as concerning the location of

¹⁴Ibid., p. 111.

application of the designator. Taking properties as the intensions of predicators, is not at all clear that their application, i.e., application of predicators understood as property expressions, would result in a class rather than simply a bunch. Classes, as is well-known, have definite characteristics of identity and inclusion. But there is no reason why these characteristics should be ascribed to the group of things considered as the extension of a property-predicator. That is, there is no reason to regard classes as resulting from the application of property expressions.

On the other hand, if the intensions of predicators are classes given by rule, it is quite natural to take their application as having the characteristics of classes.

Another advantage of taking classes given by rule as the intensions of predicators is that the class calculus allows definite decisions as to the inclusion, exclusion, or identity of one intension and another. This is not the case with properties. For example, where it is possible to, at best, argue that the property Featherless Biped somehow includes both the properties Featherless and Biped, it is possible to say that the class of all Featherless Bipeds is the intersection of the classes of Feathered Things and Bipeds. This allows talk of intensions not only to be more specific, but also more fruitful.

This notion of greater fruitfulness is not vacuous. It is a kind of economy in that the same notions have more useful consequences. Properties are identical, according

to Carnap, if predicators for them are L-equivalent; and the L-equivalence of predicators is known only by knowing the rules of designation. So, apparently the only way in which anyone can know that two predicators have the same property is by knowing the rules of designation. This is to say that the identity conditions of properties are semantic. Taking intensions as classes given by rule, the situation is somewhat different. Certainly, the identity of the intensions of 'rational animal' and 'human' does not follow from class theory, but given this as a rule of designation, the identity of 'brown-skinned rational animal' and 'brown-skinned human' follows without any additional rule of designation except that introducing the predicate 'brown-skinned' into the system.

Carnap does little to remove the obscurity of properties or any of the other choices of intensions. After saying that properties are not to be taken as either linguistic or as mental in the manner of images or sense-data, but as something physical that things have¹⁵, Carnap offers a speculative account of the knowledge of the properties expressed by compound predicators. Compound predicators are those formed by connecting the basic predicators with the connectives of the language system, e.g., '-H' is a compound predicator in S1, as is 'H&-F'. These require an extending of the use of such connectives from sentences alone to predicators. Since some of these predicators express

¹⁵Ibid., p.20.

properties not occurring in nature, e.g., 'H & F', knowledge of compound predicators need not be derived from experience of things exemplifying them. According to Carnap, such compounds are understood by understanding the components.¹⁶ This is not to say that a compound predicator can only be understood in this manner; some., e.g., 'F & B' can be understood by experience. It is to say that no compound need be understood directly, according to Carnap.

There are several difficulties with this speculative epistemology. First, as noted above, the compounding of properties is indefinite. Carnap has not shown how any compounding of the property Human and the property Ten Feet Tall produces the compound property Human and Ten Feet Tall. Common sense can complain that this is obvious, but common sense is either using, illegitimately, class notions, or is simply speaking vaguely.

The second major difficulty is that compounded properties are not always something that things have. They, in some cases, e.g., 'H & -RA' are impossible (according to the semantical rules of S1). Carnap calls unexemplified properties 'empty' and impossible properties 'L-empty', but does not explain how they are physical characters of any of the individuals (including a*) of the system i.e., S1.¹⁷

This inadequate treatment of properties is not the only, or the most objectionable, example of Carnap's neglect of epistemology. The most objectionable example occurs in

¹⁶Ibid., p.21.

¹⁷Ibid., pp.20-21.

his discussion of propositions. This is discussed below.

Carnap extends the notions of intensions and extensions to designators generally. Equivalent designators have the same extension; and L-equivalent designators have the same intension.¹⁸ He then goes about the business of choosing extensions and intensions for designators other than predicates.

As the intensions of sentences, Carnap chooses the propositions they express; as extensions, their truth-values. There is something strange about regarding anything as the extension of a sentence, since sentences, unlike predicates, do not seem to have a location of application. However, Carnap argues that since sentences can be taken as predicates of degree zero (i.e., predicates having no occurrences of free variables), and since extensions are what predicates have in common if equivalent, the choice of truth-values as the extensions of sentences is proper.¹⁹

Carnap does not explain whether truth-values are or are not linguistic or mental. But propositions as intensions of sentences are supposed to be, according to Carnap, non-linguistic and non-mental. Like properties, propositions are supposed to be "something objective that may or may not be exemplified in nature."²⁰

Again, while this vague characterization might be accepted for true sentences, the propositions expressed

¹⁸Ibid., p.23. ¹⁹Ibid., p.26. ²⁰Ibid.,p.27.

by false and L-false sentences seem no more objective or non-mental than do empty or L-empty properties.

The reason for this parallel between the difficulties of propositions and properties is that Carnap regards propositions as complex entities consisting of the intension of the individual expression or expressions and the intension of the intensions of the predicator or predicators.²¹ Thus the argument that propositions need not be experienced in order to be understood is the same as that for compound properties: knowledge of the components suffices.²²

What is important in this discussion of propositions is Carnap's closing comment, for it amounts to nothing more or less than abandoning epistemology so far as propositions are concerned.

It has been the purpose of the preceding remarks to facilitate the understanding of our conception of propositions. If . . . a reader should find these . . . more puzzling than clarifying, or even unacceptable, he may simply disregard them. They are not a necessary basis for further discussions in this book. . . . It will be sufficient for nearly all our discussions involving propositions to assume that they are entities of any kind fulfilling the following two conditions: (1) to every sentence in a semantical system S, exactly one entity of this kind is assigned by the rules of S; (2) the same entity is assigned to two sentences in S if and only if these sentences are L-equivalent.²³

But it does not seem that Carnap can abandon the epistemology of propositions and still maintain that propositions are intensions of any sort, since the intension-extension distinction is, as noted above, an

²¹Ibid., pp.30-31. ²²Ibid., p.31. ²³Ibid., pp.31-32.

epistemological distinction. So long as this is so, there seems to be a third condition which propositions fulfill: the epistemological condition of being something that a listener understands in understanding a sentence.

Apparently, Carnap does not regard this epistemological condition as necessary. In fact, he regards the entire discussion of the nature of propositions as extra-systematic and as "marginal notes with the purpose of supplying to the reader helpful hints or convenient pictorial associations which may make his learning of the use of the expressions easier than the bare system of rules would do."²⁴ He compares this discussion to "the ether picture or other visualizations of physical theories. . . ." ²⁵

Now, it is very difficult to accept the notion of this discussion as either extra-systematic or comparable to a picture of the theory. Certainly, since the bounds of the system have not been explicitly drawn, Carnap's assertion that this discussion is extra-systematic is not a conclusion. And again, since the theory is not one that appears in need of a picture, the analogy he provides seems improper.

Carnap's notion that the discussion is extra-systematic appears to be a stipulation of the boundary of the system. That is, Carnap appears to be simply registering his desire to avoid discussing the issue. However, his characterization of intensions as what is understood in understanding a designator appears to rest upon an unstated epistemology

²⁴Ibid., p.211.

²⁵Ibid.

of language. For an intension, and especially a proposition, is non-linguistic. And hence, Carnap appears to be asserting that designators, e.g., sentences, are understood by understanding some non-linguistic entity of some sort. Certainly, since this is an assertion of the existence of a non-linguistic intermediary in the process of understanding a designator, it is an epistemological position of a sort. It would seem that some further specification of this position is required if the characterization of intensions is not to be gratuitous.

It is interesting to note that while Carnap has failed to explain how intensions are the vehicles or intermediaries in the understanding of designators, he has provided several definitions of 'understand' which have remarkable consequences for his theory.

Carnap does not define 'understand' or 'know the meaning of' for designators generally, but he has defined them for sentences in at least three of his works.

To know the meaning of a sentence is to know in which of the possible cases it would be true and in which not. . . .²⁶

To understand a sentence, to know what is asserted by it, is the same as to know under what conditions it would be true.²⁷

The semantical system. . . contains rules which yield for each sentence Zi. . . a truth-condition pi such

²⁶ Ibid., p. 10.

²⁷ Rudolf Carnap, Introduction to Semantics (Cambridge: Harvard University Press, 1961), p. 22.

that Z_i is true if and only if p_i . Once this truth-condition p_i is obtained, we "understand" Z_i , we know what it "says" about the individuals of the domain in question, what its "meaning" is. Z_i says that p_i , i.e., . . . the individuals are of such a nature that the truth-condition is satisfied.²⁸

None of these definitions requires any notion of propositions or intensions of any sort. As Carnap says of the language system S_1 ;

The rules of truth together constitute a recursive definition for 'true in S_1 ', because they determine, in combination with the rules of designation, for every sentence in S_1 a sufficient and necessary condition of its truth. . . .²⁹

Hence, the introduction of propositions as vehicles for understanding sentences appears to be unnecessary.

It is difficult to see why Carnap, having such simple definitions of 'understand' for sentences, took up with propositions. To explain the understanding of sentences by the understanding of certain rules is to explain one kind of linguistic understanding by another. Such a procedure could avoid epistemology to the extent that the phrase 'understand the expression' could be left to some psychologist to define. This would not be as complete, nor therefore as satisfactory, as a semantics which included a psychological definition of this phrase, but it would be far more satisfactory than

²⁸Rudolf Carnap, Introduction to Symbolic Logic And Its Applications, Trans. William H. Meyer and John Wilkinson (New York: Dover Publications, Inc., 1956), p.101.

²⁹Carnap, Meaning and Necessity, p.5.

Carnap's awkward dismissal of the need for the epistemological characterization of epistemological entities.

It is unfortunate that Carnap has not offered definitions for 'understand' for individual and predicate expressions, for these might have shown that there is no need for intensions as vehicles for understanding of these kinds of expressions either.

Now, it seems possible to construct definitions for 'understand' consistent with the given definition for sentences for those non-sentential designators. One such definition might cover all non-sentential designators defining them as understood if the sentences in which they occur as components (i.e., subjects or predicates) are understood. Such a definition might appear to be too demanding a requirement if 'the sentences in which they occur as components' is taken as 'all the sentences in the language system in which they can appear as components', especially in a system which has a very large number of individual and predicate expressions. On the other hand, changing 'the sentences in which they occur as components' to 'at least one sentence in which they occur as components' might appear as not demanding enough.

This latter difficulty appears to be more apparent than real. It also appears to be a result of carrying over notions resulting from reflection upon ordinary language, which lacks explicit and unambiguous rules of designation, to formal language systems which do have explicit and

unambiguous rules. In ordinary English it is not unusual for one predicate, e.g., 'steal' to have different usages which alter the truth-value of the containing sentence, e.g., 'Jones steals home'. In a formal language this can be avoided by stipulating in the rules of designation for the predicate constant 'St' either 'steals (in baseball)' or 'steals (in law)'.

Hence it seems likely that a non-sentential designator can be defined as understood if at least one sentence in which it occurs as component is understood.

Taking 'Bs' in S1 as an example, the sentence is understood, according to Carnap's definition, if its truth-, or truth- and falsity-, conditions are known. 'Bs' is understood, then, since it is known to be true if Scott is a biped, and false if not. And since the non-sentential designators alone supply only the designata 'Biped' and 'Scott', understanding of them in such a case consists only in knowing the relevant rules of designation.

However, it does not seem wise to redefine, on the basis of this very simple example, the term 'understand' as applied to non-sentential designators generally. Such a definition, e.g., a non-sentential designator is understood if its rule of designation is known, might be acceptable for predicates, but it does not seem to be for individual expressions. For, among the individual expressions are not only constants such as 's' (in S1) but also descriptions

such as ' $(\exists x)(Axw)$ ' (in S1), and these individual descriptions can fail of uniqueness of reference. The example ' $(\exists x)(Axw)$ ' fails in just those cases where there is not exactly one author of Waverly, i.e., where there are none or several. In such cases of failure, Carnap, following Frege, assigns a^* as the referent of the description. The purpose of this is to ensure that every sentence in the system has a truth-value and that every designator designates. In a system such as S1, which is a thing language, a^* is called 'the null thing'. It is that thing which is part but not all of every thing. In a system whose individuals are numbers, a^* could be 0; in one whose individuals are, or include, classes, the null class.³⁰

Now, the relevance of this to the definition of 'understand' for individual expressions is that there are two cases where a sentence such as ' $B(\exists x)(Axw)$ ' is false: (1) where the author of Waverly is not a biped and (2) where there is not exactly one author of Waverly. This second case would be neither true nor false except for some rule such as the assignment of a^* as the referent of the failing description. Hence understanding of the sentence requires knowledge of this rule as well as the rules of designation. In addition, a rule may be needed stating that any atomic sentence with a^* as an individual constant is L-false.

There is another problem, resulting from Carnap's

³⁰Ibid., pp.36-37.

failure to give the rule of truth for sentences such as 'B(ix)(Axw)', as to the truth-conditions of sentences involving individual descriptions. The problem is whether the conditions under which such statements are true are adequately given by assertions such as 'the author of Waverly is a biped' or whether the individual (if there is one) must be specified by using its proper name. If the individual must be specified by using its proper name, then, aside from certain sentences, the truth-conditions of sentences containing individual descriptions (as components) cannot be given by the rules of designation and truth alone. In addition, the knowledge is needed that, e.g., the author of Waverly is Scott. Those sentences with individual descriptions as components which are exceptions to this are those with an identity connective, i.e., 'is(the same as)', e.g., 'the author of Waverly is (the same as) Scott' and 'the author of Waverly is the author of Waverly'.

It does not seem necessary to require specification of the individual by proper name, for, whatever is the descriptum (if there is one) of the individual description either is or is not, e.g., a biped. And in general, since most things do not have proper names, this demand for specification by proper name is undesirable; it would lead to truth-value gaps.

So, taking this definition of 'understand' for individual and predicate expressions, it can be seen that such under-

standing does not require intensions of any sort. As in the case of understanding sentences, knowledge of rules of truth and designation (taking the assignment of a^* as a rule of designation) suffices.

Of course, this is not Carnap's definition, but one constructed on the basis of his definition. As such, it appears to offer a way which he could have avoided introducing intensions of any kind.

If intensions are unnecessary as vehicles of understanding for designators, then, since this is apparently their basic function, they seem not to be needed at all.

However, as extensions are supposed to result from the application of intensions, the elimination of intensions seems to require the elimination of extensions as well. Hence, the elimination of intensions appears to create a gap in the semantical schema.

It might be asked why this lack of intensional or extensional characterization of the schema is a gap in any sense. The clearest answer to this is that, without some characterization of this sort, there is no way to state precisely under what conditions two terms, especially general terms, are coextensive. The need for intensions or extensions for expressions other than terms is less clear.

Reinterpreting the Carnapian schema wholly by classes, so as to avoid the difficulties with properties noted above, seems unobjectionable. Such a reinterpretation does not alter

the notions of state-description and L-truth; these are independent of intensions, extension, and properties.

On such a reinterpretation, any predicate, being a general term, refers to its corresponding class, e.g., 'human' refers to the class of humans. The problem of whether this should be taken as a class given by enumeration or by rule can be easily solved. Taking classes given by enumeration as the referents of predicates, there is the problem that any enumeration can contain at most a denumerably infinite number of names. Hence, the predicates could not include, e.g., 'number', or any other for which there is, or might be, a greater number of members in the reference class.

Taking the referents of predicates as classes given by rule, this problem does not arise. Since there is no enumeration there is no restriction of the number of members in the reference class.

It might be thought that taking classes given by rule as the referents of predicates entails the denial of the distinction between the merely happenstance coextension supposedly exemplified by the pair 'man' and 'featherless biped' and the formal coextension supposedly exemplified by the synonym pair 'man' and 'rational animal'. This need not be so. Synonyms can be distinguished from merely coextensive terms by occurring as the non-logical components in L-truths of the form 'All and only P are Q'. Such L-truths

need not be themselves stipulated in the semantical rules but may be the entailments of stipulated L-truths. For example, supposing 'All and only men are rational animals' and 'All and only rational animals are laughing animals' are both L-true, then 'man' and 'laughing animal' are synonyms. This is perhaps better seen in the case of mathematics. For it is commonly stated that '9 equals 3-squared' is logically true even though there is no special rule in mathematics stating this as an L-truth.

The referents of other terms, individual constants and descriptions, can be chosen easily. For individual constants, the individual. For descriptions, the individual or, in case of failure, the null class.

This last choice ensures not only that every term has a reference, and hence that every sentence has a truth-value, but it also ensures, if certain rules are observed, that every full sentence with a failing individual designator is false. These measures are, if anything, clearer here than in the original Carnapian interpretation, for they consist simply in understanding the copula of the sentences of the language as 'is an element of', and in denying that the null class is an element of any class for which there is a descriptive predicate. Hence, e.g., 'The sole author of Principia Mathematica is a horse' is false on set-theoretical grounds, since the null class is not an element of the class of horses. The gain in clarity of this over Carnap's choice of a^* as the null thing is that whereas the

null thing might be considered to have the empty or L-empty properties, the null class definitely has no properties.

The characterization of these terms seems sufficient interpretation for the purposes of this section. Since statements are not terms, it seems an error to say that they have referents.³¹

To use this referentially interpreted schema to explain synonymy and analyticity behaviorally, it is necessary to present and semantically re-describe certain anthropological, archeological, and psychological data, and to interpret the schema by them.

The situation here is very similar to that of the relation of an explication to what it explicates. That is, the semantical schema is not a literal reproduction of this data but is a formal specification of the logico-semantical aspects of the data. It is important to recognize that the data could be used for a different theory, even a theory using an intensional semantics. This is not a failing of the present theory, but is to be expected, since no data will serve for only one theory. However, here the earlier comments on the gain in economy and clarity resulting both from the abandoning of properties and of intensions generally are relevant. The present theory may not be the only one imaginable but it seems to be one of the more clear and economical.

³¹Quine, Word and Object, p.201.

CHAPTER III

This chapter contains an informal interpretation of a semantical model. The model is, of course, the extensional or referential revision of Carnap's schema which was discussed in chapter two.

There is, basically, one reason why this interpretation is called 'informal': the semantical notion that a predicate stands for a class is loosened. Strictly speaking, this makes the work of this chapter something other than an interpretation of this particular schema. However, as the predicates are considered to stand for collections of a sort which Vygotsky calls 'complexes'¹ and as these collections are class-like, it does not seem wholly improper to consider the present effort an informal interpretation.

This study, unlike Quine's study of applied semantics in Word and Object² does not consider language behavior. Rather, it is study of conceptualization, i.e., of primitive artifacts and culture and of the preschool child's organizational behavior.

This approach expresses the consideration that conceptualization is possible apart from language. Quine

¹L.S.Vygotsky, Thought and Language, ed. and trans. Eugenia Hanfmann and Gertrude Vaker (New York: John Wiley and Sons, 1962), pp. 61ff.

²Quine, Word and Object, chap. ii.

appears to disagree with this, writing "Conceptualization on any considerable scale is inseparable from language. . . ."³

It is likely that this disagreement is only apparent. Quine's dictum appears to be relevant only to contemporary scientific conceptualization, not to the conceptualizations of children or of primitive man.

The data for this study are drawn from the writings of L.S. Vygotsky and V. Gordon Childe.⁴ It suggests not only that the analytic could have arisen out of such conceptualizations, but that it is likely that it did.

The experimental study of concept formation whose results are summarized by Vygotsky proceeds by a method he terms 'double stimulation.' That is to say: "Two sets of stimuli are presented to the subject, one set as objects of his activity, the other as signs which can serve to organize that activity."⁵ The first set of stimuli, the objects, are:

22 wooden blocks varying in color, shape, height, and size. There are 5 different colors, 6 different shapes, 2 heights (the tall blocks and the flat blocks), and 2 sizes of the horizontal surface (large and small). On the underside of each figure, which is not seen by the subject, is written one of the four nonsense words: lag, bik, mur, cev. Regardless of color or shape, lag is written on all tall large figures, bik on all flat large figures, mur on the tall small ones, and cev on the flat small ones. At the beginning of the

³Ibid., p.3.

⁴V. Gordon Childe, "The Prehistory of Science: Archaeological Documents," The Evolution of Science, ed. Guy S. Metraux and Francois Crozet (New York: New American Library, 1963).

⁵Ibid., p.56.

experiment all blocks, well mixed as to color, size and shape, are scattered on a table in front of the subject. . . .⁶

The other set of stimuli, the signs which can serve to organize the subject's activity, are given by the examiner.

At the beginning of the experiment. . . . the examiner turns up one of the blocks (the "sample"), shows and reads its name to the subject, and asks him to pick out all the blocks which he thinks might belong to the same kind. After the subject has done so. . . the examiner turns up one of the "wrongly" selected blocks, shows that this is a block of a different kind, and encourages the subject to continue trying. After each new attempt another of the wrongly placed blocks is turned up.⁷

The point of these stimuli is given in the following:

As the number of the turned blocks increases, the subject by degrees obtains a basis for discovering to which characteristics of the blocks the nonsense words refer. As soon as he makes this discovery the . . . words. . . come to stand for definite kinds of objects. . . and new concepts for which the language provides no names are thus built up. . . . the use of concepts has a definite functional value for the performance required by the test. Whether the subject actually uses conceptual thinking in trying to solve the problem. . . can be inferred from the nature of the groups he builds and from his procedure in building them: Nearly every step in his reasoning is reflected in his manipulations of the blocks. The first attack on the problem; the handling of the sample; the response to correction; the finding of the solution--all these. . . provide data that can serve as indicators of the subject's level of thinking.⁸

In addition to the application of these newly developed concepts to the block problem, Vygotsky indicates that the subject is led to (1) use the concepts in talking of other

⁶Ibid., pp. 56-57.

⁷Ibid., p.57.

⁸Ibid., p.57.

objects, and (2) define them generally.⁹

Vygotsky asserts that his study has shown that the formation of genuine concepts begins only at puberty and that, until then, other intellectual formulations or notions more or less perform the function of concepts. There are, says Vygotsky, three phases in this development, each of which is itself divided into stages.

The first phase is generally one of unorganized heaps or congeries of disparate elements. The word, i.e., 'cev', 'bik', 'mur', or 'lag', "denotes nothing more than a vague syncretic conglomeration of individual objects that have somehow coalesced into an image in his mind."¹⁰ Because it is eclectic, says Vygotsky, this image, and the corresponding concept, is unstable, but it can be used for communication with adults especially if the conversation is about, or refers to, the concrete objects of the child's habitual environment.¹¹

The three stages of this phase differ only in the kind of syncretic heap formed. In the first stage, the heap is created at random, each object added as a guess. In the experiment, this was evidenced by the child's replacement of only the one block that had been shown to be wrongly chosen.¹²

Spatial position largely determines the formation of the second stage's heaps. Vygotsky says these heaps are formed by "a purely syncretic organization of the child's

⁹Ibid.

¹¹Ibid., p. 60.

¹⁰Ibid., pp. 59-60.

¹²Ibid.

visual field."¹³

The third stage is a combination of the first two. The heaps formed are formed from elements of the different heaps that have already been formed. This is a two-step operation, but remains at the purely syncretic level. Although Vygotsky does not explicitly say so, it seems likely that the formation of the groups of the third stage proceeds merely by trial and error selection from the groups present to the child's attention.

The second phase Vygotsky calls 'thinking in complexes'. It differs from the first phase in that the groups are formed mainly on the basis of bonds or relations observed to hold between the objects. These bonds, e.g., color or shape similarities, are objective, i.e., recognizable by the examiner. In this they differ from the associations of phase one.

Vygotsky notes that adult language contains some thinking of this sort. Family names, he says, express the factual and concrete relationships existing in the child's complexes. As Vygotsky puts it, "The child at that stage of development thinks in family names, . . . the universe of individual objects becomes organized for him by being grouped into separate, mutually related 'families'."¹⁴

The point of calling these families is, as noted above, that the relations in complexes are concrete and factual rather than abstract. Perhaps the word 'family' is too

¹³Ibid.

¹⁴Ibid., p. 62.

suggestive of a definite and unchanging rule of grouping. In fact, as ¹⁵, Vygotsky presents them, the various complexes are not formed by any one definite rule but are formed by various and varying kinds of associative rules.

There are five basic types of complexes, the first of which is characterized by having as a nucleus the sample block. The other blocks are added to this on the basis of any noticed bond or similarity or contrast. Proximity, which figured in the formation of the heaps of the first phase may also be used. The word here, e.g. 'cev' is not a proper name but a kind of family name in the sense of holding of a diversely related collection.¹⁵

The second type of complex has, apparently, two forms. The first of these two forms is formed by first taking some attribute of the sample, e.g., its color, as the basis, and then slipping to some other attribute, e.g., shape as the basis. It differs from the first in that, apparently, the resulting collection has fewer different relations.¹⁶

Vygotsky notes that this is a long and persistent stage in the child's development which is rooted in the child's practical experience. He points out that certain collections of this type, e.g., place settings, sets of clothes, are part of the child's habitual environment and do serve as models for building collections. Hence there is, apparently,

¹⁵Ibid., p. 62.

¹⁶Ibid., p. 63.

a grading off of collections of the first form of this type to the second form, which is formed on the basis of these practical models. Vygotsky suggest that these are groupings on the basis of participating in the same practical operation.¹⁷

The third stage in the phase of complex formation is the third type of complex, the chain complex. This is similar to the first form of the second type in that the rule of association is not constant throughout, or across, the collection. But there is a difference: there is no nucleus. That is to say, each link added to the chain is as important for the choosing of the next block as was the sample for choosing the second block. This means, of course, that the sample may be disregarded in choosing the third or any later block in the chain.

According to Vygotsky, the concrete nature of the formation of complexes leads them to have a vague and floating quality. This phrase 'vague and floating' is meant to apply not only to the varying nature of the relations between elements, but also to the variance between subjective relations (those due to "a dim impression that they have something in common"¹⁸) and genuine relations holding between the objects.

This vague and floating quality, according to Vygotsky,

¹⁷Ibid.

¹⁸Ibid., p. 65.

leads to the fourth type of complex, the diffuse complex. This kind of complex is, apparently, a variation on the chain complex. Here a yellow triangle may be followed by trapezoids "because they make him think of triangles with their tops cut off."¹⁹ To quote Vygotsky,

Trapezoids would lead to squares, squares to hexagons, hexagons to semicircles, and finally to circles. Color as the basis of selection is equally floating and changeable. Yellow objects are apt to be followed by green ones; then green may change to blue, and blue to black.²⁰

Vygotsky relates this kind of complex to "the child's generalizations in the nonpractical and nonperceptual areas of his thinking, which cannot be easily verified through perception or practical action. . . ." ²¹These, he says, are "the real-life parallels of the diffuse complexes observed in the experiments."²²

The fifth and last kind of complex Vygotsky calls 'the pseudo-concept' because although the behavior, i.e., the block-assembling, appears to be directed by a concept, experimental analysis reveals that only an associative bond has been formed. That is to say, when the examiner turns over a wrongly chosen block, the subject takes, or allows to be taken, away only that particular block. This, although Vygotsky does not use these terms, shows that the subject has not been assembling the blocks according to the rule

¹⁹ Ibid.

²⁰ Ibid.

²¹ Ibid.

²² Ibid.

'All murs (or cevs etc.) are tall red (or short blue etc.) blocks.' Under the direction of a concept, knowledge of an exception would result in discarding all but the sample block and beginning anew.²³

Vygotsky asserts that these psuedo-concepts are the bridge between complexes and adult concepts²⁴ and that . . . "if it were not for the prevalence of psuedo-concepts the child's complexes would develop along different lines from adult concepts. . ." ²⁵ This suggests that the attainment of adult concepts is a socialization of thinking,²⁶ and this suggestion is developed by Vygotsky in his extending of his notion of thinking in complexes to anthropological and historical examples.

After noting that some ancient languages, Hebrew, Chinese and Latin, contain single words for opposities, e.g., the Romans had one word, 'altus', for both high and deep, thus indicating thinking in complexes, Vygotsky asserts,

Primitive peoples also think in complexes, and consequently the word in their languages does not function as the carrier of a concept but as a "family name" for groups of concrete objects belonging together, not logically, but factually.²⁷

The child, primitive man, and the insane, much as their thought processes may differ in other important respects, all manifest. . . complex thinking and. . . the function of words as family names.²⁸

²³Ibid., p.67.

²⁴Ibid., p.66.

²⁵Ibid., p. 68

²⁶Ibid., pp.85-86.

²⁷Ibid., p. 72.

²⁸Ibid.

The history of language clearly shows that complex thinking with all its peculiarities is the very foundation of linguistic development.²⁹

To support this contention, Vygotsky cites examples from the Russian language. The term 'sutki' in the Russian language

"originally. . . meant a seam, the junction of two pieces of cloth, something woven together; then it was used for any junction, e.g. of two walls of a house, and hence a corner; then it began to be used metaphorically for twilight, "where day and night meet"; then it came to mean the time from one twilight to the next, i.e., the 24-hour sutki of the present."³⁰

Other Russian words, e.g., those for cow, mouse, and ink, also show the purely associative thinking of complexes. For, according to Vygotsky, the Russian words for cow, mouse and ink originally meant 'horned', 'thief', and 'blacking' respectively. These, he notes, are both too broad in the sense that not all horned animals are cows and not all thieving animals are mice and not all ink is black, and too narrow in the sense that there is more to cows, mice, and ink than horns, pilferage, and black color.

Vygotsky explains such words as resulting from a transference of name via the varied associations of complex thinking from the primary picture-name or complex-expression. This same sort of transference, he claims, is present in common adult usage, e.g., 'neck of a bottle', 'bottleneck'.³¹

²⁹Ibid.

³⁰Ibid., p. 73.

³¹Ibid., pp. 74-75.

The third phase in concept development is not, as Vygotsky presents it, a phase like the first two. Rather, it is the traditional complementary of association or synthesis: analysis or abstraction. Hence, the point of separating analysis into a third phase is only to emphasize that it is a different, independent root of concept formation.³²

Analysis, as Vygotsky sees it, consists, at the child's level at least, in the preferring of one attribute or trait of the object to another. This was seen in the chain complexes where similarity of color or shape was chosen as the basis for the addition of the next block. Vygotsky notes that even in the first stage of complexes, those with a nucleus, certain traits of the blocks have been given a preferred status to the others. And, as noted above, this preferential treatment appears to become more exclusive as the complexes lose their nucleus and become chainlike. That is to say, the number and diversity of the relations of the families appears to decrease.³³

At some point in this development of abstraction, and of concept-formation, the child begins to form groups on the basis of a single attribute. Perhaps the first occurrence of this is within the diffuse complex, for although the complex as a whole is marked by variation of associations, there are patches or segments of the complex containing only

³²Ibid., p.76.

³³Ibid., pp.76-77.

triangles or blue blocks etc.³⁴

Vygotsky mentions something that can be taken as indicating the development of abstraction toward the formation of groups built on a single attribute in his discussion of a different experiment. He writes,

Simple experiments show that preschool children "explain" the names of objects by their attributes. According to them, an animal is called "cow" because it has horns, "calf" because its horns are still small, "dog" because it is small and has no horns; an object is called "car" because it is not an animal. When asked whether one could interchange the names of objects, for instance call a cow "ink" and ink "cow," children will now answer no, "because ink is used for writing, and the cow gives milk."³⁵

Vygotsky concludes from this that the connection between word and object is so inseparable in the child's mind that an exchange of names would mean an exchange of characteristic features.³⁶

What the children seem to have here is a notion of the proper application of a word to objects. That is, to take these examples, the refusal to call ink 'cow' and cow 'ink' because ink does not yield milk and cows cannot be used for writing seems to indicate that the children consider certain features as essential for the proper application of each word.

This is not to suggest that the child has an elaborate semantics which includes a technical notion of the propriety of applying a word. But, as the block experiments show, the

³⁴Ibid., p.129.

³⁶Ibid., p.129.

³⁵Ibid.

child has the ability to rule out certain noticed features as irrelevant to the formation of groups. And, of course, this ruling out of features is an excluding of blocks having these features. In the construction of a diffuse complex the child excludes all non-blue blocks from the blue section or all non-triangular blocks from the triangle section.³⁷ This same kind of exclusion applied to the things of ordinary experience might result in the excluding from the group labelled 'cow' all things lacking horns, milk-giving ability etc., and from the group labelled 'ink' all things lacking wetness or mark-making ability.

This can be seen as a refusal to allow the basic image or group of features to change as, e.g., those of the Russian word 'sutki' were allowed to do. That is, it might be taken as a refusal to apply the term heretofore only applicable to the seat of one's trousers to the corner of his house etc.

Interpreting the refusal to apply 'ink' to cows and 'cow' to ink as an extension of the abstraction ability shown in block experiments might seem to miss a difference in complexity between blocks and cows. That is, a block is tall, short, big, little, red, blue, yellow etc.; but a cow seems to be a good deal more complex.

This difference in complexity is obvious to an educated adult but perhaps not to a child or an ignorant peasant. As a matter of fact, Vygotsky himself

³⁷Ibid., p. 78.

suggests something of this when he notes that the original meaning of the Russian word for cow was 'horned', and when he reports the child's conception of the notion of reasonability:

"Reasonable means when I am hot and don't stand in a draft."³⁸ Taking this as a model, the content of the refusal itself might be interpreted as saying that, to the child, 'cow' means 'large horned milk-giving animal' and 'ink' means 'something to write with.'

The importance of taking the refusal as an extension of abstraction ability is that it shows that the child has some notion of essential features. And while this notion seems to be limited only to properties essential for the proper application of a word, it can also be taken as the notion of the features essential for anything to be considered, e.g., a cow.

This perhaps more significant notion of essentiality seems to be expressed by the child in the following conversation.

In one experiment, the children were told that in a game a dog would be called "cow". Here is a typical sample of questions and answers:

"Does a cow have horns?"

"Yes."

"But don't you remember that the cow is really a dog? Come now, does a dog have horns?"

"Sure, if it is a cow, if it's called cow, it has horns. That kind of dog has got to have little horns."³⁹

The sentence showing that the child considers horns an essential feature of cows is: 'If it is a cow, it has

³⁸Ibid., p.78.

³⁹Ibid., p.129.

horns'. Actually this should not be too surprising, since the child's refusal to call anything other than a cow 'cow' is evident not only from the earlier refusal, but from his interpreting a dog called 'cow' as a special kind of dog.

The importance of this for the present study is that the child and primitive man thus seem to have working notions of essence. And given this, it seems that they have the basis for statements of essential predication, i.e., analytic statements.

In fact, to re-examine the quoted series of questions and answers, the child's replies seem to be nothing more or less than an explanation to the examiner that the statement 'all cows have horns' is true because cows must ("got to") have horns, i.e., because horns are an essential feature of cows.

If this analysis is correct, then at least one of the behavioral aspects or bases of the analytic has been uncovered, or partially uncovered. That is to say, the analytic is a result of the notion of essence which seems to be a result of the dogmatic preference for certain features of the object. Some of the reasons for this dogmatism are given in the introduction.

Admittedly, this account is vague, especially insofar as the establishing and preferring of associations is concerned. Part of this seems unavoidable, since there is apparently no standard or predictable set of associations or preferences. The situation is, in fact, very much the opposite of

standard. The diversity of associations was glossed over in the discussion of the child's notion of cow, but re-examination of the quotes from the interrogation experiments shows that the children made different associations. The first took milk-giving as essential; the second, horns. Exactly why this is so is not clear.

V. Gordon Childe has laid the diversity of associations and of preferences not only to the trial and error initial investigations of primitive men but to their establishment of strict traditions of method as well.⁴⁰

Childe's claim that primitive men established traditions of method is supported by both the geographical diversity and the typological-historical continuity of discovered artifacts. Childe explains how geographical diversity supports this claim:

In fact in the same period and often in one and the same ecological province we often find quite different assemblages distinguished one from the other by apparently arbitrary divergences in types of artifact serving much the same purpose --- e.g., of ax, bracelet, or dwelling --- and often by similar divergences in the food eaten or the sites selected for habitation. Such observed arbitrary differences are attributed to divergences of tradition between distinct, though contemporary, communities.⁴¹

Similarly, after noting that there are "Notoriously . . . two ways of learning --- by trial and error and by example."⁴² Childe takes what he regards as a well-documented example, the manufacture of hand axes, and points out the

⁴⁰Childe, op. cit., p. 36.

⁴²Ibid., p. 37.

⁴¹Ibid., p. 36.

improbability of taking these as merely the result of trial and error individual work.

There are, says Childe, two obvious methods of making sharp-edged tools from flint or other micro-crystalline stone. The first is to chip off bits of the core until it has the desired edges and shape; the second is to detach flakes which themselves serve as tools. Both procedures require considerable familiarity not only with stone but with the human body as well. For, as Childe writes,

Just bashing two stones together is not likely to yield a usable flake or core tool. To produce either the blow must be struck with precisely the right force and at the correct angle on a flat surface --- the striking platform. The latter must in turn make an acute angle with the adjacent side of the lump and the blow must fall at the proper distance from the edge thus formed.⁴³

There are thousands of well-finished tools and some failures to show that Lower Paleolithic man had this technique well established. In fact, since the failures show the application of the same method to flawed cores, Childe considers that they are better evidence for this.⁴⁴

Both the geographical distribution and the typological sequential nature of the improvements of hand axes indicate that a tradition of toolmaking had been established. According to Childe, the tens of thousands of similarly shaped hand axes made from the same sort of crystalline stone using the same shaping methods make it

⁴³ Ibid., p. 43.

⁴⁴ Ibid., p. 39.

extremely unlikely that these were the result of anyone's trial and error experimentation.

And although Childe does not explicitly state this, it seems that the absolute predominance of hand axes in areas bordering on areas where hand axes are rare⁴⁵ indicates not only that there is a divergence of traditions of tool-making but also that there is, in each tradition, a rejection of nontraditional methods as well. After all, given a technique which can produce both cores and flakes literally at one blow, it seems odd that the core toolmakers did not also become flake toolmakers and vice versa. This seeming oddity seems to be accounted for by an orthodoxy of tradition, i.e., an exclusive tradition.

If this is the case, then there is a parallel between the child as stipulator of essences and the primitive man as stipulator of proper methods of toolmaking. Both appear to reject exceptions because they lack features considered essential.

Childe contends that hand axes are simply a good example of his thesis that tools are the products of a tradition. He sees no reason to restrict his thesis to hand axes alone.⁴⁶ He even goes further and argues that what holds of toolmaking and the related knowledge of geology should also hold good of hunting, agriculture, etc.⁴⁷

⁴⁵Ibid.

⁴⁷Ibid., p. 41.

⁴⁶Ibid., p. 40.

Supposing Childe's contention is correct, then notions of essentiality were widespread in prehistorical times. For such notions are embodied in every technical orthodoxy, and all primitive men hunted, fished, or farmed.

These notions of essentiality, whether of procedures of hunting or toolmaking or of simple naming and classifying, appear to be more properly taken as legislative than as merely inductive generalizations. This can be seen first, by considering the relation of such notions to exceptions. Where inductive generalizations are falsified by exceptions, these rules or notions count the exceptions as bogus. This is shown in the child's contention that any dog called 'cow' must have the essential properties of a cow, i.e., horns. It may also be considered to be expressed by the absence of flake tools in the core tool areas, and vice-versa. For example, it is not too speculative to imagine a primitive tool maker of one school refusing to call a tool made by another method 'tool.' After all, such comments exist into the present day, e.g., in the field of musical criticism.

The second consideration illustrating the legislative or stipulative character of such notions rather than their character as inductive generalizations is their obvious material falsity. Core tools were not the only kinds of tools made at that time. Hence, it does not seem likely that these notions are simply inductive generalizations. Rather, they appear to be seizures upon certain useful techniques discovered by trial and error and perpetuated by

an apprentice system of a sort.

That is to say, to follow Vygotsky, these notions would seem to be due primarily to one or several individuals. Socialization of them would be due to the apprentice system. Such a system would spread them across a society or a portion of it, e.g., the toolmakers of the society.

There are several reasons to consider an apprentice system as one of the more important ways in which such notions were socialized. First, it is a method of transmitting techniques and/or acquired knowledge. Some such method is necessary for any society if every generation or every individual is not to be forced to learn by his or its own trials and errors.

Second, as a method of transmission, the apprentice system has certain advantages over other imaginable methods. For, unlike the contemporary techniques of classroom education or their imaginable outdoor primitive precursors, the apprentice system is neither dependent upon the existence of a developed language or upon the existence of an economy which can support an individual or individuals who divert their activities from the production of basic necessities.

The apprentice system is one that can be carried on without disturbing the daily round of activities necessary to maintain life. The hunter can teach hunting while hunting, and the farmer can teach farming while farming. The apprentices themselves can be employed in tasks requir-

ing little or no specialized knowledge, and can be tested by observation of their performance of more demanding tasks.

Because the teaching is done during the performance of the activities, such a system would seem to require little more language than indicator words on the order of 'this' and 'that', or even appropriate grunts. It is interesting to note that such a teaching method might well serve to introduce new terms into the language. Certain expressions which were originally merely indicator words might well become, by simple association of word and object or situation, the name of the object or situation.

It is important to note that the introduction of new terms can of itself serve as a source of social conceptions of necessary states of affairs. For some of these new names might well be taken as the child took 'cow', i.e., as being applicable to only things having a feature regarded as necessary. In addition to this, the apprentice system can itself serve to introduce orthodoxies because of its teaching methods. For such a method, stressing mimicry, and most likely discouraging innovation (because innovation looks like error), could of itself harden even merely useful generalizations into authoritarian stipulations. Although the mechanism here seems complex, it is no doubt partly that of association via the technique of reward and punishment.

This emphasis upon the apprentice system may be misleading. Other factors may also have contributed greatly

toward the formation and socialization of notions of necessary states of affairs. The significance of the apprentice system is that it is a method for the transmission of acquired techniques which not only can function under primitive conditions but which can contribute to the formation of orthodoxies as well.

Now, supposing this entire account is basically correct, i.e., that there were socially recognized stipulative or legislative truths present at least implicitly in the technical orthodoxies of primitive culture, the problem remains to relate these to the extensional revision of Carnap's scheme.

The intended parallels in the formal scheme are the referential stipulations or L-truths. The legislative or stipulative nature of the primitive notions seems fairly clear both from the consideration of their falsity as material statements and of their relation to purported exceptions. What remains to be shown is that these are referential in nature.

It is clear that the demonstration of the referential nature of the assertions of these primitive notions of necessary states of affairs is not a matter of investigating primitive language. This would appear to be impossible in any case, since there are apparently no artifacts bearing samples of these languages. In addition, as has been noted previously, it seems that any sentence can be interpreted intensionally or extensionally. This is familiar case of

the same data serving for alternative theories. In such cases consideration of the relative simplicity of the alternative theories is necessary.

On this score the considerations of chapter two are relevant. The extensional revision of Carnap's scheme appears to be not only simpler because of the elimination of intensions but clearer and more familiar as well. The gain in clarity is a by-product of the elimination of intensions, since the role of intensions was somewhat puzzling. That is, as noted in chapter two, intensions were characterized by Carnap not only as the vehicles by which designators are understood but as unnecessary for this purpose as well. The gain in familiarity concerns mainly the substitution of the established class calculus for Carnap's own attempted formalization of the relations of properties.

However, these considerations concern only the formal portion of the theory. What is perhaps more to the point of the interpreted theory is that the view implicit in Vygotsky's study is closer to the referential than to the intensional view of language. That is to say, the word-complex relation in Vygotsky's study is closer to the extensional term-class relation than to the intensional designator-intension-extension relation in several important ways.

First, the relation of a term to a class or of a word to a complex is basically a one-one relation. Allowing for several words to name the same class or complex, the

relation is at most many-one. But the relation of a designator to its intension and its extension is one-two.

Second, classes and complexes are collections. In this, both differ from properties which, as Carnap sees them, are physical characters of things.

The third point concerns the number of different kinds of designators. Vygotsky mentions only one kind of designator--the word which is associated with a complex.

Vygotsky's view thus differs from both the intensional view and its extensional revision. The intensional view considers sentences, individual expressions and predicate expressions as designators. The referential revision eliminates sentences as designators and retains the individual and predicate expressions. These become singular and general terms, i.e., expressions referring to individuals and classes respectively.

While both the intensional and the extensional views appear to be quite different from Vygotsky's view, the extensional revision of Carnap's scheme can be modified to bring it closer to Vygotsky's. This modification consists simply in taking each individual as a class whose sole member is that individual. This modification is not without precedent,⁴⁸ and it does serve to make the relation of reference generally that of a term to a class.

⁴⁸Willard Van Orman Quine, Mathematical Logic(rev.; Cambridge: Harvard University Press, 1955) p. 135.

Such a modification necessitates other small modifications. The copula, having been 'is an element of', must be changed to 'is a subclass of'. Thus, 'John is tall' becomes 'The unit class of John is a subclass of the class of tall things'. This is effectively the same as 'The individual John is a member of the class of tall things'. Similarly, a^* , having been taken as the null class, is now taken as the class whose sole member is the null class. Here the former stipulation that ^{the} null class is not an element of any class for which there is a predicate expression is changed to the stipulation that the class of the null class is not a subclass of any class for which there is a predicate expression. Like the earlier stipulation, this ensures that every full sentence with a failing individual designator is false.

This modification differs from the view implicit in Vygotsky's study in two important ways. First, classes are not complexes. Both are collections, but Vygotsky gives no information as to the identity and membership conditions of complexes. From his remarks concerning the replacement of erroneously chosen blocks, it would seem that these conditions are not precise or explicit. Since classes differ from informal collections in having precise identity and membership conditions, this is a significant difference.

Second, this modification contains a distinction between two kinds of classes--those with exactly one member and those whose membership is numerically unspecified.

This distinction has no parallel in Vygotsky's study.

However, these differences do not seem sufficient to show that primitive language, at least as Vygotsky sees it, is non-referential. Rather, it seems possible to regard these differences as differences in precision of the structure of the basically referential language. To regard the differences in this manner is, of course, to treat the primitive language in the manner of an explicandum for the formal view as explicatum.

The point of so treating the relation of the primitive language to the later formalism is to emphasize the similarity of the two language structures.

It is clear that classes are an improvement in precision over complexes. However, it may not be as clear that the distinction between kinds of classes is a difference of precision. This can be seen by considering that this distinction provides the basis for the precise statement of the truth conditions of both singular and universal statements.

That is to say, on the basis of this distinction between the two kinds of classes, it is possible to regard singular statements as of the form 'The unit class of A is a subclass of the class of B's', and to regard universal statements as of the form 'The class of C's is a subclass of the class of D's'. Hence, it is possible to see clearly that, while singular statements assert the existence of some individual in some class, universal statements do not assert the existence of any individuals at all. Hence, a

statement such as 'All unicorns are blue-eyed' is true if there are no unicorns at all. The reason for this is that, under the usual set-theoretical interpretation, the null class is a subclass of every class.

Actually, since the distinction between universal and singular statements is essentially a distinction of truth conditions, it can be said that the introduction of a distinction between types of classes is the (precise) introduction of the different kinds of statements themselves. And this is an improvement in the precision of the language as a tool of expression.

Thus, taking primitive language as referential seems more appropriate than does taking it as intensional. Hence, the stipulations of the primitive language appear to be better taken as referential than intensional. And hence, it would appear that the assertions of primitive orthodoxies can be taken as the primitive behavioral parallels to the L-truths of the formal schema.

As this completes the account of this section, it may be thought that this account is not an interpretation of the formal scheme but an addition to it. It is certainly not a formal interpretation in the sense of the physical interpretation of a mathematical model. That is, there has been no attempt to find behavioral or cultural parallels for each of the several notions of the formal schema.

Perhaps the present account is best called, following Quine, a sketching of factors relevant to analyticity into

the model language. For a sketching is, apparently, not a formal procedure, but a drawing of the main lines. Since the problem of explaining analyticity is a complex one, little more than this could be attempted here.

At any rate, it would seem that the basic lines have been drawn and that Carnap's semantical notions of state-description, semantical rules, explication, and L-truth (in an extensional revision) have been shown to be useful in explaining analyticity in natural languages.

CONCLUSIONS

The conclusions of this essay are the fulfillments of its purposes. The attacks upon the sharp analytic-synthetic distinction have been shown to be ill-conceived, and Carnap's basic semantic method of constructing artificial languages has been shown to be useful in explaining analyticity in natural languages.

This is not to say that there are no problems with this method or with any of its concepts. On the contrary, both explication and intension require further study. The relation of the explicandum and the explicatum needs further specification, and the need for intensions generally needs to be further investigated. Intensions may not be needed for the explanation of analyticity, but analyticity is not the only problem of logical semantics.

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ABSTRACT

The purpose of this essay is to reply to the attacks upon the sharp analytic-synthetic distinction made by Quine in "Two Dogmas of Empiricism" and by White in "The Analytic and the Synthetic: An Untenable Dualism". This essay attempts to show not only that these attacks are ill-conceived, but also that Carnap's semantic methods can be used to explain analyticity in natural languages.

The two attacks are, in effect, attacks upon the conception of the analytic as being definitely different from the synthetic. Quine's attack is directed primarily at three of Carnap's basic concepts--state-description, explication, and semantic rule. These he regards as separate attempts to explain analyticity. White attacks the claim that some natural language has the sharp analytic-synthetic distinction of an artificial language. He does this by considering primarily two imagined experiments.

Both attacks fail. Their failure is due not only to particular failings of argument, but to more general methodological misconstructions as well. In particular, not only are each of Quine's different attacks upon the several notions misconceived, but the very method of piecemeal attack is itself a misconstruction of Carnap's semantic methods. Similarly, White's imagined experiments

not only fail to demonstrate his points, but his method of attack appears a methodological misconception. The failures of experiments do not entail the rejection of the hypothesis.

Quine's attack upon the notion of the state-description, or rather, of the list of all state-descriptions (for a language system) consists in taking this list as a truth-table for some compound of all the atomic statements in the language system and in asserting that it cannot then serve as a tabular criterion of analyticity. Quine errs here on two specific counts. First, the list of all state-descriptions is not a truth table. Second, it is not a criterion, but a definition of analyticity. An analytic statement is defined as one which is true in every state-description.

Quine's next attack--upon explication-- is one he has come to reject himself. In this attack he contends that, as explication requires the (partial) synonymy of explicandum and explicatum (or explicans, as Quine has it), it rests on synonymy and cannot be used to explain it. However, Carnap never suggests that explication requires the (partial) synonymy of explicandum and explicatum. In fact, Carnap's own explication of analytic or necessary truth, which has as explicandum what Leibniz called necessary truth and what Kant called analytic truth, illustrates the allowed divergence between explicandum and explicatum. Carnap's is a semantical notion, not one couched in the framework of metaphysics or epistemology. In Word and Object, Quine comes to agree to

this: synonymy between explicandum and explicatum is not required.

Quine's last attack--upon semantical rules--is at once the most patently false and the most suggestive of his attacks. Quine's contention that semantical rules are distinguishable only by appearing upon a page marked 'Semantical Rules' is patently false. Semantical rules, like theorems, rules of inference, etc., are distinguished by function. To say, then, that a statement is true according to the semantical rules of the language system is not to say nothing. It is to say something comparable to the assertion that some statement is entailed by another according to the rules of inference of some logical system. That is, it is to place the statement within a formal system.

It is precisely this neglect of the total formal schema which is Quine's most serious error. In attacking these three notions separately, Quine has indicated that he has misconstrued Carnap's method. The three notions are not each ways of defining or explaining the analytic; they are components of a structure in which analytic statements have a proper place.

However, to return to this last attack, Quine does make a very useful suggestion in noting that an appeal to artificial languages could be helpful in explaining analyticity if the relevant behavioral and cultural factors were sketched into the simplified model. This suggestion is taken up and developed in the later part of this essay.

Quine himself does not do so.

An approach such as the one suggested by Quine is different from that taken by White, for White's approach involves neither the investigation of behavioral or cultural factors relevant to analyticity nor the interpretation of a semantical model. Rather, White approaches the problem with more direct methods. His two imagined experiments, which involve the interrogation of native subjects as to the synonymy of expressions, differ mainly in the kind of interrogation employed. The first experiment uses the method of ostension, i.e., pointing to objects, saying native expressions taken as 'man', and awaiting native assent or dissent. Since the natives assented to neither the calling of non-featherless bipeds nor non-rational animals by the native expression taken as 'man', White concludes that this method will not distinguish supposed coextensives from supposed synonyms. Oddly enough, having said this, White concludes that the criterion of term-withholding, which was supposedly being applied here, will not help in making the distinction between coextensive and synonymous terms. Yet, it seems obvious that it is not the criterion which has failed, but the method of application.

The second experiment allows White to converse with

the natives. The method of interrogation is verbal rather than ostensive. White asks the natives if they are not surer in withholding the term 'man' from non-rational animals than from non-featherless bipeds. And getting an affirmative reply, White concludes that he has the making of a criterion which makes the distinction between synonyms and coextensives merely a matter of degree. He asserts that rationality is merely a better sign of manhood than is featherless-bipedity.

These are obviously non-sequiturs. Not only are degrees of felt certainty compatible with as deep an analytic-synthetic distinction as is imaginable, but replies to questions such as this one do not produce criteria.

Turning to Carnap's semantic methods themselves, both to support the replies to Quine and to develop the semantical model with which to develop Quine's suggestion, the essay becomes somewhat more technical. Aside from the many details, the key point of this discussion is the status of intensions in Carnap's scheme. In particular, propositions seem an unnecessary complication. Not only are propositions supposedly the intermediaries or vehicles in the understanding of sentences, but Carnap defines 'understand' for sentences in such a way as to indicate that propositions are not necessary for the understanding of sentences.

On the basis of this definition of 'understand' for sentences, a definition of 'understand' for non-sentential designators is constructed. This has the effect of rendering

all intensions unnecessary from the epistemological point of view. Since the epistemological function appears to be their most basic function, this appears to be a method of eliminating intensions altogether. Following this, Carnap's schema is reinterpreted extensionally, i.e., wholly in terms of classes and elements or members of classes.

The resulting extensional scheme is then used as the model into which behavioral and cultural factors relevant to analyticity are sketched.

The behavioral factors relevant to analyticity are, first, the child's and, apparently, primitive man's natural propensity to think in complexes or association groups and secondly, his propensity to prefer, after a time, certain associations to the others. This leads the child to insist, e.g., that cows must have horns. Hence, the child is, in effect, asserting 'Necessarily, cows have horns' or, in other words that 'All cows have horns' is analytic or L-true.

The cultural factors relevant to analyticity are the orthodoxies which spread such preferences across a society or a portion of it. Such orthodoxies, according to V. Gordon Childe, were the rule in prehistorical times. They appear to be the result of the two ways of learning open to primitive man--by trial and error and by example. And since experimentation is suicidal for primitive peoples living at the level of economic subsistence, those that survived and aided in the formation of our culture established orthodoxies. Hence, analytic statements form a part of our

cultural heritage from prehistorical times.

Now, strictly speaking, classes are not the same as complexes. Classes have definite identity and membership conditions; complexes do not. But, complexes, being collections according to several rules, appear to be a rudimentary form of classes. Hence the interpretation of these factors into the revised Carnapian schema can be taken either as an informal interpretation of the schema or as an explication of the primitive conceptual scheme.

In sum, the attacks upon the sharp analytic-synthetic distinction are seen not only to be ill-conceived both in their particular details and in their more general and methodological aspects, but also to be erroneously directed at a useful contribution to the culture of man.