PROBLEMS OF FORMAL SYSTEMS AND THEIR INTERPRETATION

by

John R. Cameron,

being a dissertation submitted to the University of St Andrews in accordance with the Ordinances and Regulations governing study to be pursued by candidates for the degree of Bachelor of Philosophy of that University.

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being a dissertation submitted to the University of St. Andrews in accordance with the [illegible] [illegible] is [illegible] study to be pursued by candidates for the [illegible] of Philosophy of [illegible].

Introduction.

The candidate was admitted as a Research Student in the University of St Andrews on 1st October, 1958, with Mr. G. P. Henderson, Senior Lecturer in the Department of Logic and Metaphysics at that time, as his Adviser, and 'Problems of Formal Systems and their Interpretation' as his topic for study. He pursued study of this topic for three terms at the University of St Andrews, and has since continued his study at the University of California and at Cornell University.

I certify that JOHN R. CAMERON has spent nine terms in research work as a matriculated student of the University of St. Andrews, that he has fulfilled the conditions of Ordinance 61 (General No. 23) and University Court Ordinance 277 (St. Andrews No. 50), and that he is qualified to submit the accompanying dissertation in application for the Degree of Bachelor of Philosophy.

Supervisor.

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Chapter I: Introduction.

The topic of this dissertation is the interpretation of formal systems. It is an important topic for the following reasons. First, it has been argued that the procedure of 'formalisation' can be very valuable in constructing scientific theories, logical and mathematical systems, and 'conceptual frameworks'; and to formalise is to construct a formal system, together with a particular interpretation for it, precisely or loosely specified. Second, the application of logic and of mathematics in everyday life is very commonly explained on the analogy of employing interpreted formal systems.

The claims made for the usefulness of formalising deserve close strutiny; so also does the attempt to explain practical applications of logic and mathematics on the model of the use of interpreted formal systems. Enquiries into these two varieties of claims form the programme for this dissertation.

1. Formal Systems.

Throughout this discussion of formal systems, and their interpretation, the work of Rudolph Carnap will be taken as the principal source; he is the chief expositor of the construction of formal systems, and has considered their interpretation in more detail than anyone else. Further, what he has to say about formalising, and about formal systems, fits the procedure, and the products, of those who construct such systems, and agrees with their use of them, in so far as that use is clear.

A formal system, or syntactical system, or calculus, is a structure consisting of[1]

(a) a set of characters[2], classified into various groups (e.g., constants of various kinds, variables of various kinds, operators, logical signs, auxiliary signs).

(b) a set of rules of formation, governing the construction of 'formulas' by concatenation of characters of the system; these rules may be regarded (and formulated) as a definition of the term 'formula', or of the class of formulas as a subclass of the class of all possible strings of characters. The rules are generally required to provide an 'effective' definition of 'formula'; that is, a definition the satisfaction of which by any given candidate can be tested by a mechanical procedure[3]. Usually the rules form a recursive definition.

The rules of formation generally define 'atomic formula' first, and then 'formula' in terms of 'atomic formula': formulas are either atomic formulas or constructed in certain ways from atomic formulas, while atomic formulas cannot be analysed as constructed from more 'basic' formulas.

The set of rules of formation may also distinguish between 'open' and 'closed' formulas.

(c) a set of <u>rules of transformation</u>, governing the manipulation or transformation of formulas (or sets of formulas) into other formulas (or sets of formulas). These rules may also be regarded as constituting a definition, in this case of the relational term 'immediately derivable from' (meaning 'obtainable by a single transformation from'); this definition leads to a definition of the term 'derivable from' (meaning 'obtainable by a sequence of transformations from'). (Carnap uses the terms 'direct C-implicate of' and 'C-implicate of', respectively.)

The rules are often presented by setting out 'axioms' or 'primitive sentences', formulas into which any set of formulas (including the empty set) can be transformed, together with some rules of transformation; the list of axioms may be specific, consisting of individual closed formulas, or may be open, specifying, by means of 'schemata', whole classes of formulas as axioms. The rules are not required to be effective; where they are, the system is 'decidable'.

Some language has to be employed in setting out the structure of a formal system. It must be possible, using this language, to make reference to the characters of the system, and to strings of these characters. The language used is called the 'metalanguage' (in contrast with the system, thought of as a potential language, and called the 'object language'). Reference to individual characters may be effected by using special names for each character, or by using the characters themselves, either within quotation marks (e.g., using "'0'" to denote a zero), or without quotation marks, 'autonymously', as names of themselves (e.g., using "0" to denote a zero). Strings of characters may be referred to in corresponding fashion, or by using a sign of concatenation ("⌢") with any kind of names of individual characters. (Thus a zero in parentheses might be denoted by "'(0)'", or by "(0)", or by "'('⌢'0'⌢')'", or by "(⌢0⌢)", or by "left parenthesis⌢zero⌢right parenthesis".) Reference to strings of characters in general is usually effected by use of variables--capital Roman or Greek letters--having the class of all possible strings as their range. The various methods of forming names of strings may be extended to the case of variables as well (e.g., "(A)" could be used as a variable having the class of all strings of characters beginning with a left parenthesis and ending with a right one as its range).

The technical details of formulation of formal systems are not of vital importance for the discussion that follows. However, it may be useful to construct a simple formal system, by the methods typically used, to illustrate what has been said so far. A formal system incorporating[4] the propositional calculus could be constructed as follows. The metalanguage used is (a portion of) English, characters of the system are used autonymously in the metalanguage, 'A', 'B', and 'C' are used as variables having the class of all possible strings of characters of the system as their range, and names of strings are formed by concatenation of names of components.

(a) Characters:

(i) Propositional[5] constants: p_1 , p_2 , p_3 , ...;

(ii) Propositional[5] variables: π_1 , π_2 , π_3 , ...;

(iii) Logical[5] characters: $-$, $\supset$;

(iv) Auxiliary characters: (,) .

(b) Rules of Formation:

(1) If A is a propositional constant or variable, then A is a formula.

(2) If A is a formula, then $(-A)$ is a formula.

(3) If A and B are formulas, then $(A \supset B)$ is a formula.

(c) Rules of Transformation:

Let 'Λ' denote the null class of formulas.

(1) If A, B, and C are formulas, then $\wedge$ may be transformed into the formula $(A\supset(B\supset A))$,

or the formula $((A\supset B)\supset((B\supset C)\supset(A\supset C)))$,

or the formula $(((-B)\supset(-A))\supset(A\supset B))$.

(2) If A and B are formulas, then the class of formulas $\{A, (A\supset B)\}$ may be transformed into the formula B.

(3) If A and B are formulas, and C is the formula obtained from A by replacement of some propositional variable π_1 at each of its occurrences in A by B, then A may be transformed into C.

In (c), (1) might commonly be replaced by a list of axiom schemata, leaving (2) and (3) as the rules of transformation proper.

2. Semantical Systems.

A semantical system consists of a classified set of signs, from which sentences can be constructed, following rules of the system; these rules are generally required to provide an effective definition of the term 'sentence', and are called 'rules of formation'. In addition, the system must contain some set of rules which supplies meaning for each of the sentences constructible in the system.

The forms which these latter rules, the 'semantical rules', may take, are many. Carnap usually gives them in the form of 'truth conditions', or 'rules of designation' together with 'truth conditions'[6]. Truth conditions supply complete conditions for the truth of each of the sentences of the system, and thereby, Carnap claims, give meaning, since "to know the truth conditions of a sentence is to know what is asserted by it, ...--in usual terms, its 'meaning'"[7]. The rules of designation supply designata for individual and predicate constants, in systems in which such signs occur; they are supplemented by some such truth condition as:

> A sentence of the form '---is...' (or '---...'), where '---' is a name, and '...' is a predicate, is true if and only if the thing designated by '---' has the property designated by '...'[8].

Further truth conditions are usually supplied for negations, conjunctions, disjunctions, and possibly other molecular complexes of subject-predicate sentences.

Giving truth conditions for universal and existential sentences is more complicated, and involves introducing the subordinate concept of 'satisfaction' of a formula by an object, or an ordered sequence of objects. However, there is no need to consider the details of these more complicated rules.

Although this formulation of semantical rules is the most generally accepted one, it seems clear that semantical rules could be given in other forms. A system of rules which is based on a classification of signs, and gives meanings of various kinds to signs of the various classes, and thereby, indirectly, and possibly in conjunction with some truth condition, gives meaning to formulas, could be called a system of 'dictionary rules'. Carnap's semantical systems involve dictionary rules; presumably other kinds of dictionary rules could be used. But semantical rules need not take the form of dictionary rules at all. One could give truth conditions for sentences directly; or one could dispense with truth conditions altogether. Since the semantical rules, in whatever form, have to be given in some metalanguage, one obvious way to give semantical rules would be in such a form as 'The sentence "-------" is to mean ".......", where "-------" is a sentence of the semantical system, and "......." is a sentence of the metalanguage[9]. A language with rules of either of these latter kinds would be like the code of flags used at sea, which employs individual signs as sentences.

There are problems about the forms in which semantical rules should be given, problems lying in the province of the philosophy of language. For example, Alonzo Church[10], following Frege, has objected to Carnap's formulation of semantical rules, on the ground that a descriptive sign has not only a designation, but a sense; and that sentences should be considered as designating a truth value, as well as having a sense. Another criticism of Carnap's account is that of Gilbert Ryle[11], to the effect that it assimilates the semantical properties of all descriptive words to those of names, treating predicates, for example, as 'designating' properties. This general application of what he calls the 'Fido'-Fido principle, he shows, leads to absurd consequences, such as that some sentences are lists of names.

A further and yet more fundamental problem that Carnap's approach presents stems from his assumption that 'meaning' can properly be attached directly to sentences, rather than to sentences in context of use, or to statements made, or propositions asserted, by use of sentences.

I do not propose to consider problems of either of these kinds, serious as they may be for Carnap's account. I wish to assume (i) that it is possible, in talking about language, and linguistic systems such as Carnap's semantical systems, to frame anything one may have to say as a statement (or question, hypothesis, or etc.) about sentences, and, specifically, that it is possible to talk about sentences' having meaning; (ii) that it is possible to formulate rules in virtue of which sentences of a semantical system have meaning, more or less as sentences of natural languages can be said to have meaning.

3. Interpretation of a Formal System.

It is quite possible that there may be an exact correspondence between the set of characters of a formal or syntactical system, with its classification, and the set of signs of a semantical system, with its classification, so that, for example, the same design which is a character, and specifically a propositional constant, in the syntactical system, is a sign, and specifically a propositional constant, in the semantical system. And further, the rules of formation of formulas (of the syntactical system) and of sentences (of the semantical system) may correspond exactly, so that what is, as a string of characters of the syntactical system, a (closed) formula, is also, as a string of signs of the semantical system, a sentence, and conversely. This correspondence of rules of formation of the two systems may, or may not, spring from a correspondence between the structures of the two sets of rules.

Indeed there may be a correspondence between characters and signs, and between formulas and sentences, without there being identity of design between individual characters and individual signs. However, there is nothing essential about one design for a sign or character, as against another, and so, where there is a correspondence between a syntactical and a semantical system, without there being a shared set of designs, the closer correspondence can be obtained by replacing one or other of the systems by a system that is in practical respects indistinguishable from it, but has characters or signs (as the case may be) of a different design.

A syntactical system may be in correspondence in the way described, not with a whole semantical system, but with a 'proper part' of one; then all the designs which are characters in it are signs in the semantical system, and all its closed formulas are, as strings of signs, sentences in the semantical system; but the converse is not true.

In either case, Carnap says[12] that the semantical system is an _interpretation_ of the syntactical system. Specifically, the requirement of a semantical system as an interpretation of a syntactical system is that it should furnish a truth condition for each closed formula of the syntactical system.

The concept of 'interpretation' is not so important as that of _true interpretation_. A semantical system which is an interpretation of a syntactical system is in addition a true interpretation if it never, by the truth conditions it assigns, interprets as a false sentence a formula which is derivable, in the syntactical system, from sentences which are interpreted as true sentences. To put it more succinctly: a true interpretation of a calculus is an interpretation relative to which the transformations permitted in the syntactical system all 'preserve the truth value True', or yield only true formulas from true formulas.

In the case of interpretations, as elsewhere, the most detailed account is due to Carnap; but in this case what he says corresponds particularly well with the work of formal logicians; On the basis of concepts introduced by Tarski[13], there has recently been developed a branch of mathematical logic called 'model theory', which occupies the position of a complement to the traditional 'proof theory'; the notions of consistency and completeness basic to most of metamathematics are the result of a fusion of proof-theoretic and model-theoretic notions. And a <u>model</u> of a formal system is, roughly, a formal correlate of a true interpretation of it. Specifically, a model of a formal system consists of an <u>assignment</u>, which is a function correlating individual characters with members of some domain, and predicate characters with functions from that domain into the set $\{T, F\}$, and a <u>valuation</u> relative to that assignment, which is a function correlating either T or F with each formula (open or closed) of the system, on the basis of the 'values' the assignment gives to the characters occurring in it; an assignment together with a valuation relative to it constitute a model if they correlate with the element T all the axioms, and all the formulas derivable from them (or: all the formulas derivable from the empty set of formulas). A model of a system having only propositional constants consists of an assignment only[14].

Except that a model does not distinguish between variables in free occurrences and constants, and between open and closed formulas, it is the equivalent of a true interpretation, constructed formally and extensionally, and without any semantical connotations: it does not give the characters and formulas of the formal system meaning. Because Carnap deals with rules, and not functions, he is able (in <u>Meaning and Necessity</u>) to give some account of the intensional aspects of meaning, by considering the meaning of semantical rules--how they supply designata and truth conditions--as well as their effects--what designata and truth-values they supply; model-theory takes no cognisance of such matters.

The combined structure of classification of signs, rules of formation, rules of transformation, and semantical rules, which is obtained when a syntactical system and a <u>true</u> interpretation of it are conflated, might be called a 'formalised language'[15]. It is with formalised languages that this dissertation is concerned.

4. Use of the Concept of 'Formalised Language'.

As was suggested at the start, the concept of formalised language is brought into play in two kinds of context. First, the use of formalised languages is advocated in science, logic, mathematics, and conceptual enquiry[16]. Second, it is suggested that logical and mathematical procedures should be, or must be, described and/or explained and/or understood in terms of use of formalised languages.

These contexts are not completely separate. The thesis that application of logic or mathematics is to be thought of as use of a formalised language is clearly connected with the thesis that logical or mathematical theories are best formulated by constructing formalised languages. However, in the first kind of context, it is the construction and use of formalised languages that is being advocated, while in the second kind of context, the thesis put forward is that applying the concept of using a formalised language is fruitful, or essential.

It will prove convenient to proceed as follows: first, the general status of formalised languages will be considered, in its bearing on their utility, in Chapter II; I will then turn to the attempt to explain application of logic and application of mathematics on the model of use of formalised languages, in Chapters III and IV-VI respectively; in Chapter VII it will be possible to return to the question of the utility of formalised languages, and give a more adequate answer.

As was said earlier, I wish to assume that anything that can be said about language can be expressed in terms of sentences. I wish to assume, further, that logical principles can properly be framed in terms of sentences[17] (but I do not wish to prejudge the question whether logical principles can also properly be framed in other terms). Carnap, and all formal logicians, and almost all philosophers of mathematics, take this possibility so completely for granted that it is hardly possible to appraise what they have to say in any useful manner, unless one adopts their viewpoint, tries to argue in their own terms, and in particular uses the 'sentential' terminology they use in discussing language and logic. For the sake of understanding what Carnap says, and what former logicians assume, I will try to 'speak their language', even if a great deal of what I say is implicitly a denial of the appropriateness of that language.

Chapter II: The Preciseness of Formalised Languages.

The question I wish to discuss first is this: What is the difference between a formalised language and the technical language found, for example, in scientific textbooks, or systematic philosophical treatises? Is this difference a radical one, or is it merely one of degrees of preciseness?

Carnap and others write as if ordinary languages and formalised languages were absolutely different in certain respects, the former being 'inexact' and 'logically imperfect', while the latter are precise and free of logical imperfections[1]. Further, they seem to include among 'ordinary' or 'natural' languages the technical language which is a refinement of ordinary language, and is used in scientific, philosophic, and other special contexts; for in general they speak as if the only choice of languages open to us is that between natural languages and formalised languages, constructed more or less on the pattern outlined in the previous chapter, and where they do actually talk about scientific discourse, for example, they suggest that it is an inadequate vehicle of expression, and ought to be replaced by formalised language[2]. I want to show that this attitude is mistaken.

1. Meaning in Formalised Languages is 'Parasitic'.

The cornerstone of what I have to say is the following contention: the meanings and logical properties of, or connected with, the signs of a formalised language are parasitic upon the meanings and logical properties of, or connected with, the signs or words of the metalanguage used in formulating the semantical rules of the formalised language. This contention may be called, for short, the thesis of 'parasitic meaning'.

Suppose that we have a formal or syntactical system, and wish to supply an interpretation for it. As has been remarked, there seem to be various ways of doing this; the only requirement of any method used is that it give a meaning to every (closed) formula of the system.

Suppose that the method used is like Carnap's: the (syntactical) classification of the signs is brought into play, the various kinds of signs given different kinds of meaning (individual constants, e.g., being given substantival meaning, predicate constants adjectival meaning), and the set of semantical rules completed by truth conditions for atomic and molecular (i.e., complex) formulas. Typical examples of the two kinds of rules involved are:

Rules of designation:

(i) "The individual constant 'a' designates the city of Paris";

(ii) "The predicate constant 'F' designates the property of being in France";

Truth conditions:

(i) "If A is an individual constant, and B is a predicate constant, then the formula AB [or: $A^\frown B$, or etc.] is a true formula if and only if the object designated by A has the property designated by B";

(ii) "If A and B are formulas, then AvB [or: $A^\frown v^\frown B$, or etc.] is a true formula if and only if either A or B is true".

These examples may seem unduly simple to be typical; yet they are comparable with most of the examples of semantical rules offered by Carnap[3].

These rules give meaning to signs, or to groups of formulas, of a formal system; that is, they make it possible to consider, and use, the appropriate signs as words, and the appropriate groups of formulas as types of sentence. It is clear that they do this by 'trading on' the meaning that the words and phrases 'Paris', '(the property of being) in France', 'the object designated by ..... has the property designated by .....', and 'either ..... or ..... is true', already have, as words and phrases of the English language.

The ways in which semantical rules of this kind 'trade on' the meanings of words, phrases, and sentences of the metalanguage they are formulated in may be roughly classified under three heads. First, these rules make essential use of such words as 'designate', and other semantical terms[4]. The semantical structure of a formalised language is supplied through the semantical terms used in its semantical rules, and this structure is derived, <u>via</u> these terms, from some extant semantical 'theory', which is the natural 'habitat' of these terms, and forms part of their collective connotation. (Similarly, the grammatical structure of a formalised language is derived, _via_ grammatical terms of the metalanguage, from some existing grammatical 'theory'.)

Indeed, the semantical structure of a formalised language is not independent of the semantical structure of the metalanguage itself. For example, it would be hard, if not impossible, to supply designata for individual names in a metalanguage which had nothing like nouns or noun phrases. In a similar way, a truth condition making a character a sign of conjunction would be very hard to express in a language in which it is not possible to conjoin sentences.

This latter example perhaps also illustrates a dependence in meaning of another kind, namely, dependence in the 'logical' component of meaning. A clearer example of this, the second way in which meaning is derived from the metalanguage, is to be found in _Meaning and Necessity_. Here Carnap, in setting up a semantical system S_1, gives as two of the 'rules of designation for predicates' the following:

> "'H_x_'--'_x_ is human (a human being)'";
> "'RA_x_'--'_x_ is a rational animal.'"[5].

He adds that "the English words here used are supposed to be understood in such a way that 'human being' and 'rational animal' mean the same"[6]. He later deduces that the sentence '$(x)(H\underline{x} \equiv RA\underline{x})$' is L-true (i.e., logically true), since its truth "can be established without referring to facts by merely using the semantical rules of S_1, especially [the rule of designation for predicates] (see the remark following this rule) and the truth rules for the universal quantifier and for '$\equiv$'"[7]; from this he further deduces that the predicators 'H' and 'RA' are L-equivalent (i.e., logically equivalent) in S_1. Obviously, the logical properties of the sentence, and of the two predicators, are derived from the (claimed) synonymity of 'human being' and 'rational animal', that is, from a feature of English words which is independent of (extra-linguistic) facts, and so 'logical'.

The third way in which semantical rules are dependent on significance in the metalanguage is in supplying designata (or whatever they do supply to give meaning) for the descriptive signs (in a Carnapian system, the individual, predicate, and propositional constants). This parasitism of 'descriptive content' is the most obvious form of parasitism of meaning. In effect, the rule "'a' designates Paris" provides 'Paris' as a synonym, or literal translation into English, of 'a'; "'F' designates the property of being in France" in effect provides 'in France' as a synonym or literal translation for 'F' (or rather, this is intended, but is not achieved, because adjectives are misconstrued as names). Semantical rules supply parasitic meaning in this way by functioning as translation rules, or dictionary entries.

It is worth considering in detail the case of semantical rules which take the form of rules of designation together with truth conditions for sentences, because this is the form semantical rules generally take. And what has been said about this case could, I think, be applied to any other case of semantical rules which are in the form of what I have called 'dictionary rules'.

But it is possible to give truth conditions for any finite collection of formulas directly, without intervening dictionary rules. Carnap himself gives an example of a directly formulated truth condition in <u>Introduction to Semantics</u>, pp. 22-23: "We know that the sentence 'Mon crayon est noir', uttered by Pierre is true if and only if a certain object, Pierre's pencil, has a certain color, black". Here again, it is clear that the semantical rule supplies meaning by 'trading on' meanings already 'given' in English. In this case the third kind of borrowing (of descriptive content) is obviously present; however, the first two kinds of borrowing (of semantical structure, and of logical properties) are not so clearly in evidence as they are in the case of interpretation by dictionary rules.

Nevertheless, these other kinds of borrowing are also present here. If a set of semantical rules is to supply meaning for a formula, it must supply <u>some</u> semantical structure and transmit <u>some</u> logical properties or relations. If the rules consistently give, as truth conditions for formulas of a certain structure, the possession of particular properties by particular objects, they in effect confer a subject-predicate semantical (and hence grammatical) form on sentences of that kind; if they regularly give disjunctive truth conditions for formulas with some other structure, they in effect make that kind of formula, _qua_ type of sentence, a disjunctive type. If they do not consistently give any one kind of truth condition for formulas which are recognisably of the same syntactical type, they in effect treat such formulas as singular unitary signs, making them like one-word sentences. And of course the semantical rules are supplemented by the grammar implicit in the syntactical classification of characters, which also helps to introduce semantical structure.

Similarly, if the truth condition given for one formula is logically implied by the truth condition for another, the conditions jointly confer the relation of logical implication on the pair of formulas in interpreting them. (The particular form of rules involved in the example taken from _Meaning and Necessity_ is inessential to the kind of parasitism of meaning it illustrates.)

It is perhaps unsafe to generalise dogmatically about this matter, but I cannot see how any set of semantical rules could supply meaning to a collection of formulas without making use of the 'given' meanings of the words, phrases and sentences of the metalanguage used in their formulation, and, specifically, of the semantical structure associated with, the logical properties and relations implied by, and the descriptive content of, these meanings.

Another way of arguing for the thesis of parasitic meaning, which sheds light on it from a different angle, is as follows. Suppose someone is being taught a formalised language; he is given the rules, including the semantical rules, constituting the system. These rules will be formulated in some metalanguage, which the learner must understand if he is to understand the rules and learn the language. The metalanguage may be his 'mother tongue', the first language he ever learned, or it may be a natural language, or conceivably an artificial or formalised language, which he has previously learned. If the metalanguage is not his mother tongue, then he has most probably learned it through rules--'translation rules' of one kind or another--in somewhat the way he is learning the new formalised language. These translation rules will have been formulated for him in some other language he already knew; it in turn may or may not be his mother tongue. This chain of dependence of the learning of one language on the previous learning of another may be considerably drawn out; but it must end somewhere (else he knows none of the languages), and the only way in which it can end is in his knowledge of his mother tongue, or of some language which he learned in the way he learned his mother tongue, that is, by a means not involving being given rules in any metalanguage. In the case of his mother tongue, there was no language he knew which could serve as metalanguage. One can be taught languages by being given rules only when one has learned <u>some</u> language in a different fashion; and the meanings of the words, phrases, and sentences of this language are in a sense 'basic' to the meanings of all words, phrases, and sentences of languages learned, directly or mediately, through translation rules linking them with this original language.

In this sense, then, meanings not only in formalised languages but also in acquired natural or artificial languages, so far as these are acquired by learning translation rules, are parasitic.

2. <u>Formalised Language is like Technical Diction</u>.

Because the meanings of symbols and sentences of formalised languages are parasitic, in the various ways outlined above, the effect obtained by formalising is exactly that achieved when ordinary language is refined and extended to form a technical language. For, the 'defining', in a metalanguage, of the meaning of a sign of a formalised language is just like the 'defining' of the special meaning of a 'technical term', in a natural language, for use in some scientific or other specialised context.

Of course, technical language is full of everyday words which are not given an explicit technical sense ('is', 'and', and 'all', for example), and it might be thought that their presence makes technical discourse that much less precise, and not comparable with a formalised language. But an examination of the semantical rules governing the use of the signs corresponding in formalised languages to these common words shows that the following is the case: if the vagueness and/or ambiguity of the everyday words cannot be overcome, then the formal signs which take their place must also be vague and ambiguous, and, contrapositively, if these latter signs are unambiguous and exact, there must be ways of using the everyday words unambiguously and precisely. Consider, for example, the rule of truth for the disjunctive symbol 'v' cited earlier:

> "If A and B are formulas, then AvB is a true formula if and only if either A or B is true."

As was noticed above, this formulation is typical of those given when formalised languages are constructed. Now either the phrase 'either ... or ... is true' is used unambiguously and exactly in the rule, in which case the meaning the rule gives to 'v' is precise, or the phrase is ambiguous, or vague, or both, in which case 'v' lacks clear and precise meaning.

Now all that is required for technical parlance to be (relatively) precise is that the words occurring in it, as they are used in these occurrences, are precise and unambiguous; it is not necessary that they be words which have no vague or imprecise uses, but only that their vague and imprecise uses, if any, do not occur in technical contexts. So if 'v' can be given precise meaning in a formalised language, 'either ... or ...' can be so restricted in use in technical discourse as to be precise, so that it in effect acquires a technical, precise, sense.

The same argument can be applied in the case of other everyday words; for in every case, the definition of the formal symbol involves the use of the word or phrase corresponding to it in ordinary language, or of an equivalent word or phrase.

The example considered is a very elementary one; but I think that it is not possible to attain any degree of preciseness in formalised languages, even employing much more complex rules than the proponents of formalising have considered, that cannot be attained by comparatively straightforward means in ordinary language, as refined for technical purposes.

However, it may be argued that what makes a formalised language radically different from ordinary and technical language is not anything connected with its interpretation, but its having explicit syntactical rules of transformation. Instead of going by logical principles, whose application is often far from clear, one can apply these exact rules of transformation to derive sentences from other sentences with ease, and with complete confidence in the propriety of one's procedure (since the interpretation one is employing is 'true'). It is this preciseness of logical procedure, it may be claimed, that sets the formalised language apart. This claim must now be considered.

3. Logical Rules are 'Meaning' Rules.

When once a person knows the meanings of words of a language, or, as we say, 'knows the language', he is already in a position to make inferences couched in that language. To put it in Carnapian terms, he is capable of making 'logical derivations of sentences'. Evidence of this is the fact that in everyday life people who know no logical principles engage in such practices as inferring, deducing, and arguing, and also accepting or rejecting the logical procedures of others, as valid, or invalid. Logical principles are (logically) posterior to logical practice; in fact they represent a 'codification' of logical practice (see Section 2 of the next Chapter).

This observation about logic and language applies equally to natural and to formalised languages. One can use the sentences of a semantical system in logical ways, for example, as premiss- and conclusion-sentences in inferences and arguments, irrespective of whether the system is a true interpretation of any syntactical system, or whether there are any explicit rules of transformation for the sentences.

As was pointed out earlier, semantical rules 'transfer' logical properties and relations as well as, or as part of, meanings, from metalanguage to formalised language. And perhaps even more than the semantical rules proper, the syntactical classification of signs, and the way in which the semantical rules are built upon it, help to create the 'logic' or 'grammar-cum-logic' of a formalised language. Calling a sign a predicate, and, more importantly, treating it as a predicate, gives it certain logical properties and relations, and is, along with other factors, what makes it possible to employ the formalised language it belongs to in logical procedures. And, as was argued earlier, even if only the sentences of a formalised language are interpreted, these sentences still acquire logical properties in being interpreted. It is part of knowing what sentences mean, in fact, that one can perform logical transformations of them correctly.

Logical rules are therefore an addition to semantical rules; they reduplicate explicitly 'implicit rules'[8] embodied in the semantical rules, and are in principle dispensible. Further, they must 'agree' with these implicit rules in their effects (i.e., the results of their application). In particular, if the rules of transformation of an interpreted formal system are to be regarded, under the interpretation, as logical rules, then they must accord, under the interpretation, with the semantical rules.

This requirement is in effect that given by Carnap for an interpretation of a formal system to be true. But it is not readily appreciated from his account that the semantical rules of a formalised language have a certain pragmatic priority. Carnap says merely that, for an interpretation of a formal system to be true, the truth conditions it assigns to formulas of the formal system must be such that transformations permitted by the formal system never yield formulas interpreted as false from formulas interpreted as true (cf. pp. 13-14).

He does not discuss the vitally important questions as to how an interpretation may be _shown_, and _known_, to be true.

If an interpretation of a formal system is to be shown true, and if the rules of transformation of the system have an unlimited number of possible applications (and this is the commonest case), there must be some _systematic_ means of showing that each transformation of the formal system always yields, from formulas interpreted as true, formulas also interpreted as true. By 'systematic' here I mean 'unlimitedly general', the contradictory of 'piecemeal'. This systematic means will have to rely on prior systematic knowledge that certain kinds of transformations of (meaningful) _sentences_ are legitimate ('preserve truth'); it must justify types of transformations, relative to the interpretation, by correlating them with types of non-formal, 'meaningful', sentence transformations which are already known to be justified. Hence there can be, in the general case described, no justification of the interpretation of the formal system unless logical principles of equivalent force are already known, with which the system's rules of transformation can be correlated.

Even in the special case of a formal system whose rules of transformation can be applied only finitely often, although it would be theoretically possible to show an interpretation 'true' by considering all applications of the rules to formulas of the system, the task would be so great as to make a systematic proof imperative, unless the number of possible transformations were very small; and if this condition were met, the system would be of extremely little interest or use. We may ignore this exceptional case.

Now a formal system with an interpretation which is true is of no use unless in addition it is _known_ that the interpretation is true; no one would be justified in taking advantage of the formalisation of logical procedure it provides, unless he knew that the formalisation was correct. Hence a pragmatic or practical requirement that any formalised language of any interest must meet, to be usable as a formalised language, is that its rules of transformation correspond, _via_ its interpretation, to known logical principles of (sentence-)transformation.

Carnap's criterion of a true interpretation of a formal system therefore gives rise to a pragmatic condition that the formal system match the interpretation, and match it very exactly, each rule of transformation corresponding to a rule of (sentence-)inference.

This argument, and the previous more general one, show, I think, that the logical rules which are the interpretation of rules of transformation in a formalised language are but an extension of the semantical rules, and at most serve to specify meaning more completely. To the extent that the semantical rules are determinate, admit of no ambiguity, and apply exhaustively, the logical rules, and indirectly the transformation rules, must match them; only in so far as the semantical rules do not cover every case, and the implicit logic that goes with them is incomplete (as in the case, e.g., of higher-order modal sentences), can the rules of transformation go 'beyond' the semantical rules. In this latter case the force of the interpreted transformation rules is merely to fill in 'gaps' left by the semantical rules as formulated--to complete these rules[9].

It should be mentioned that the semantical rules may not determine the rules of transformation uniquely; there may be different ways of formalising an 'implicit' logic', as we will see in the next chapter.

In conclusion, then, it appears that the rules of transformation of a formalised language merely make explicit something that is implicit in the semantical rules, and they represent an advantage which formalised languages have over other languages only in so far as it is an advantage to have rules of procedure explicit. They do not mark off formalised languages as better in principle than ordinary and technical languages; though of course in practice they may be very convenient to use.

4. Conclusion.

I cannot claim that the arguments of this chapter are very novel, or that the conclusions reaches are very surprising. However, I think that they do show something worth pointing out. The proponents of formalising seem to base their case rather less on the advantages undoubtedly possessed by formalised languages--explicitly formulated logic, the possibility of performing inferences mechanically, conciseness and simplicity of expression, and regularity of syntax--than on the claim noticed earlier, that formalised languages are (absolutely) superior to ordinary languages in their preciseness and logical perfection. And the preceding shows, I hope, that this claim has no basis in fact, preciseness and freedom from logical imperfections being only relatively present (or lacking) in any language.

Chapter III: The Formalisation of Logic.

The discussion of formalised languages in the previous chapter has implications for all possible uses of such languages. In this chapter I want to consider the idea that the formulation of a set of logical principles is, or is to be explained as, construction of a formalised language, or a skeleton or schematic form of such a language.

1. The Thesis that Logic is Part of Semiotic.

One of the theses developed and argued for by Carnap in The Logical Syntax of Language is that "as soon as logic is formulated in an exact manner, it turns out to be nothing other than the syntax either of a particular language, or of languages in general"[1]. When Carnap retracted his extreme claim that all philosophical and logical analysis is investigation of syntax, and admitted the need to consider the semantical aspects of language as well, he discussed the changes he felt necessary in some of the contentions he argued for in The Logical Syntax of Language; this discussion, which appears in an Appendix to Introduction to Semantics[2], makes no mention of logic. However, in the main part of this same book, Carnap does claim, in one section, that "logic, in the sense of the theory of logical deduction, will here be shown to be a part of semantics"[3].

The view of logic implied in this claim is what might be expected from Carnap after his 'conversion'. However, apart from that explicit remark, and some other slight evidence[4], Carnap shows in all his work subsequent to The Logical Syntax of Language an apparent reluctance to discuss 'what logic is'. He has taken rather to saying that certain semantical concepts may be taken as 'explications' of commonly used, but imprecise, logical concepts; the concept 'logically true', for example, he feels should be displaced by the concept 'L-true, relative to a semantical system' (i.e., 'true in virtue of semantical rules alone')[5].

It will be necessary to consider later what 'explication' consists of; for the present it is sufficient to notice that proffering semantical concepts as explications of logical concepts does not commit Carnap to saying that logic is part of semantics. An explicatory concept must be connected with its 'explicandum' in such a way that it can be used in its place; but it does not seem that the two concepts have to be identical in category, or logical type. At any rate, in his fullest discussion of explication[6], Carnap suggests, by way of illustration, that the concept of 'temperature' may be regarded as an explication of the concept of 'warmth'; and these two concepts are hardly of the same logical type.

There is sufficient fluidity in the notion of explication to make it possible to regard Carnap either as giving an account of what logic is, or as putting forward some concepts as useful to apply in logical enquiry. The approach of explication is somewhat pragmatic; and in stressing the explication of logical concepts, instead of claiming to 'explain' the nature of logic, Carnap is probably adopting the view that the question 'What is logic?' cannot be given a satisfactory answer--or is not worth trying to answer.

His view, then, is probably that logic is best and most profitably regarded as a part of semantics, rather than that it is essentially or by nature part of semantics. Clearly, these two views are related, the first being a weaker version of the second. Yet at the same time they are quite different in kind. I hope that the discussion in the next section will help to put these views into a more general perspective[7].

It is worth noticing that Carnap, who is so often the spokesman for practising formal logicians, is in this matter less dogmatic than they are. They, for the most part, write as if logic were quite definitely a part of semiotic, or 'the science of signs', and as if the only way to pursue logical enquiry were to construct formal systems with logical interpretations. Indeed, they seem to take the narrower view that the proper way to formulate logic is syntactically, standing by the claim made in _The Logical Syntax of Language_ that "'non-formal i.e., non-syntactical logic' is a _contradictio in adjecto_"[8] (whence their syntactical treatment of model-theoretic concepts). Whether in formalising syntactically, formal logicians think they are giving systems of logical principles in their proper formulation, or merely 'representing' such systems by 'structurally isomorphic' formal systems, is not completely clear; but certainly, even if, like Carnap, they regard calculi as merely formal representations of systems of logical principles (and I do not think that they do), they still do believe that logical principles really are linguistic principles of some kind.

It may be helpful to conclude by attempting to distinguish the various ways in which the thesis that logic is part of semiotic may be formulated. There are three ways of interpreting the question 'What is logic?', and three ways of answering it are relevant to the present discussion (counting only versions of the thesis in terms of semantics, not ones in terms of syntax). Thus there are nine variant answers to be considered:

1. Answers to 'What is the nature of logic?':

$$\text{Logic} \left\{ \begin{array}{l} \text{(a) is} \\ \text{(b) is best regarded as} \\ \text{(c) may be regarded as} \end{array} \right\} \text{part of semantics.}$$

2. Answers to 'What is logical enquiry?':

$$\text{Logical enquiry} \left\{ \begin{array}{l} \text{(a) is} \\ \text{(b) is best regarded as} \\ \text{(c) may be regarded as} \end{array} \right\} \text{the con-}$$

structing of (skeletonic) formalised languages.

3. Answers to 'What are (are the uses of) logical principles?':

(Uses of) logical principles
- (a) are
- (b) are best regarded as
- (c) may be regarded as

(applications of) semantical rules.

The types of answers (a), (b), and (c) might be labelled 'realist', 'explicationist', and 'empirical', respectively. The truth of the empirical claims has already been assumed for the purposes of the present discussion. Both these and the explicationist claims are, of course, connected with claims on a lower level as to the utility of constructing formal systems in the course of logical enquiry. I will concentrate for the most part on the realist claims. The answer 2(a) is, I think, most nearly the formal logician's working credo; but the 'cash-value' of both it and the abstract answer 1(a) must be regarded as given in the answer 3(a).

2. <u>Codifying Practices</u>.

By a 'procedure' or 'practice' I mean a pattern of action commonly exhibited in the behaviour of some group of people. The people who go through a procedure may be following rules of some kind, rules which when followed always produce the given pattern of behaviour; or there may be no such rules, and the procedure may be ascribable only to a habit of some kind that is common among the particular group of people in question.

It is frequently the case that there is a need to 'regularise', or make explicit, to have laid down 'in black and white', the details of a given practice. In particular, it is often necessary, and usually helpful, to make a practice explicit in this way when it is to be taught to someone. One way of characterising a procedure is by constructing an explicit system of rules, or 'code', of such a kind that the behaviour resulting from following it matches the pattern of action which constitutes the procedure. 'Codifying' a procedure, as we might call this sort of characterising, is quite common. Grammarians codify; so do collectors of folk-songs; writers of 'How to' books attempt to codify success-producing procedures; codification of 'courteous behaviour' results in etiquette; and there are many other examples of codification.

The following observations on codifying have a bearing on the present topic of discussion.

I. Codification is not a mechanical procedure; in general, several different codes may serve equally well as codifications of one procedure[9]. Though the rules of two codes may differ, their 'effects' may be indistinguishable. This fact is obvious; but what is not so obvious is that, according to certain standards of 'identity of effect', two codes of different <u>kinds</u> may sometimes serve equally well as codifications of one and the same practice. By 'codes of different kinds' I mean codes whose rules are, in an obvious sense, 'about' different kinds of actions, or actions connected with different kinds of things.

It is a prime rule in golf always to keep one's eye on the ball; there is nothing to be gained 'directly' from having the ball in sight all the time (except that one does not lose one's ball so often), but if one so moves as to keep the ball in sight at all times in one's swing and immediately thereafter, one will, other things being equal, have one's body in the correct position throughout the swing, and will achieve the best possible result. The intended effect of the rule is that one will assume and retain the correct stance and swing the club in the correct fashion, although it is 'about' watching the ball. Now it should be possible, in principle at least, to formulate some other set of rules which could take the place of the rule 'Keep your eye on the ball', a set of rules about positioning of the body, movement of the arms, shifting of one's weight from one foot to the other, and so on. Such a set of rules would take the place of the other rule by producing, when followed, the same pattern of action as it produced when followed. It would constitute an alternative code, substitutable for the other; and yet the two are different, not just in detail, but in kind.

Consider another, more complicated example: a manual for sergeant-majors might give rules governing the issuing of commands on the parade-ground. The rules might be of the form 'In circumstances X, issue the command Y'; or they might be of the form 'In circumstances X, utter the words "Z"'--"Z" being an imperative form of words, e.g., "Right wheel!". Now either form of rule would if followed have the same results, because when a sergeant-major utters imperative forms of words of the appropriate kind on a parade-ground, he will be taken to be issuing commands, whether or not he intends to be so taken. The one kind of rule is about issuing commands, the other is about uttering words; the two are different in kind, yet they produce the same effects, and may be considered as alternative codifications of the same procedure.

II. This second example brings out the relativity of the phrase 'identity of effect of a set of rules'. If, as I have assumed, the actions of the platoon of men under the sergeant-major's command is the 'effect' of his following the rule, then the two kinds of rule mentioned have identical effects; but if the intentional issuance of commands, or absence thereof, counts as an 'effect', then the two kinds of rules are not identical in effect. The same observation can be made about the illustration from golf: if the procedure to be codified is no more than positioning and movement of the body, the two kinds of rules discussed are identical in effect; if knowing where the ball goes, for example, counts as part of the procedure, and following the proposed new set of rules does not ensure this, then this set of rules is not identical in effect to the rule 'Keep your eye on the ball'.

Clearly, therefore, it is necessary to ascertain exactly what kinds of actions <u>count</u> as part of any given practice before considering two alternative codifications of it. And the same is true, of course, in the more general case where one has to judge whether a given set of rules does or does not codify a particular practice correctly: before one can decide, one must have a thorough understanding of what the practice consists of.

III. It may be possible, then, to codify a procedure by sets of rules of different kinds. The procedure itself may consist of actions of 'different kinds': it may, for example, consist of some physical act or acts together with some 'mental' act or acts (e.g., an intention). But in general there will be one kind of code which is related in some particular way to a given procedure; a code of this kind is 'about' actions of the same (simple or complex) kind as the actions which constitute the procedure. The rule 'Keep your eye on the ball', as a rule intended to produce certain body positions and movements, does not constitute a code of this kind; the suggested substitute code would be such a code. In the case of the sergeant-major's manual, if the procedure, or what counts as the effect of following the rules, is 'getting the platoon to do certain things', then neither of the kinds of rule mentioned would form part of a code of this special kind; such a code would have rules of the form 'Get the platoon to do X'.

There is some basis for saying that a codification of a procedure by a set of rules having this particular 'same-kind' relation to the procedure is a more 'correct' codification, or at any rate, is correct in ways in which codifications not having this relation are not: it is 'literally' correct. 'Keep your eye on the ball', for example, happens to produce the desired pattern of action; it is in effect a codification of the pattern of action. But the hypothetical alternative codification would be a more 'proper' codification: it would be a codification 'by design' in a way in which the other rule (though intended to produce the same results) is not. In the case of the sergeant-major's manual, the effectiveness of the rule 'In circumstances X, utter the words "Z"' is even more obviously contingent. In both cases, some contingent fact is used, deliberately taken advantage of.

I will call a codification of a practice having this special 'same-kind' relation to that practice a <u>direct</u> codification, and a codification not of this kind an <u>indirect</u> codification.

Another way in which the difference between direct and indirect codifications appears is as follows. Codification is usually undertaken where there is no existing code. In such a case, the newly formulated code does not, initially, constitute a criterion or standard for judging whether people are going through the procedure correctly; although judgments of this kind can be and are made prior to and during this stage (a point not grasped by Carnap, I think[10]), they are made on the basis of some kind of knowledge of 'implicit rules'[11]. It is also by measurement against this intuitive knowledge that the codification itself is judged 'correct' or not, and wins, or fails to win, acceptance. As it wins acceptance, and, for example, comes to be used to teach people the procedure, it gradually comes to have a normative role or status, and to displace people's 'intuition' of what is correct, and what incorrect, practice. Only then does it become in any sense 'constitutive'[12] of the practice.

Now suppose that an attempted codification of some practice is, in the sense indicated, 'indirect'. In this case, it will never be possible to describe the procedure codified by saying that it consists of doing the things required by the rules of the code; that is, the codification will never become (in any sense) constitutive of the practice. For the practice preceded the code, and was describable before the introduction of the code only as consisting of actions of a certain kind, different from the kind of actions called for by the rules of the code (since the code is indirect); and it must be describable only in these same terms after the introduction of the code, or else it is not the same practice. (Of course the introduction and acceptance of the code may change the practice, and may even result in its displacement by a practice of a different kind--as when, for example, a formalised etiquette displaces ordinary polite behaviour--but that is another matter.)

Thus an indirect codification of a practice can never become constitutive of the practice; while a correct direct codification obviously can.

IV. It was noticed in II above that we must have detailed knowledge of some kind as to what a practice consists in before we can decide whether a purported codification of the practice is correct or not. The corresponding fact about judging as to directness of codifications is even more obvious: we cannot decide whether a given codification of a practice is direct except on the basis of knowledge we already have of the nature of the practice.

3. Logical Systems as Codifications of Logical Practice.

I wish now to apply these observations to the particular case of sets of linguistic or other rules as codes for logical procedures.

But before proceeding to this discussion, it may be thought appropriate to offer some grounds for assuming that logic is a codification of logical practice, and that consideration of the relation of logic to logical practice is relevant to discussion of the nature of logic.

It is hard to see what logic can be if it is not a codification of logical practice. However, from the writings of Carnap and most formal logicians, one gets the impression that they take a different view of logic. It may therefore be worth while to show why Carnap, despite his preference for considering abstract entities (e.g., sentences), rather than practices and activities (e.g., using sentences to make statements), must agree that the ultimate concern of logic is certain practices.

In introducing the concepts of semantics and syntax, Carnap first characterises a language as a 'system of activities, or rather, of habits, i.e., dispositions to certain activities'[13]); he then goes on to characterise semantics as the theory or investigation of language in which 'we abstract from the user of the language'[14], and (logical) syntax as that enquiry in which 'we abstract from the designata also'[15]. That is, semantics and syntax are theories about the appropriate aspects of language, i.e., about the appropriate aspects of certain systems of habits, or, as we might say, about semantical practice, and syntactical practice, respectively (these being parts or aspects of linguistic practice). One might put this another way by saying that the theoretical concepts of semantics and syntax, which are applied to sentences, have a 'cash-value' in terms of (conventional) uses of sentences.

Since Carnap constantly relates logic to either syntax or semantics, he is surely committed to saying that logic is, like them, a theory about some aspect of (the use of) language; and this aspect must presumably be 'logical practice'--the use of language in inferring, deducing, and arguing, and the connected activities of appraising inferences, deductions, and arguments, refuting them, and so on. To put it another way: the theoretical concepts of logic, which according to Carnap's account are applicable to sentences, have a 'cash-value' in terms of the use of sentences.

Turn now to consider the question 'What is logic?', or 'What is the nature of logic?'. Formal logic consists of something rather like codes, which contain such rules as Modus ponens, Modus tollens, the Law of Contradiction, and the Law of Excluded Middle. The problem which gives rise to the question 'What is logic?' is that these rules may be formulated as rules about 'making judgments', or about 'making statements', or about 'uttering declarative sentences', or about 'inferring propositions', and it is not clear which of these several sorts of practice (if any) the rules actually codify; that is, it is not clear what the nature of logical practice is.

What do the findings of the previous section suggest about the way in which this problem can be solved? First, it is clear that the question 'What is the nature of logical practice?' may itself be rephrased as 'What form would a direct codification of logical practice take?'. Second, it is apparent that certain ways of trying to show that logic _is_ this, or _is_ that, are inappropriate.

In particular, the thesis of _The Logical Syntax of Language_, that logic, precisely formulated, is an extension of syntax, seems to be argued for in that book merely by formulating a codification of logic in syntactical terms; Carnap apparently assumed that the achievement of such a codification proved that logic is syntactical in nature. But, as we saw, it is quite possible that more than one _kind_ of codification of logical practice is feasible; and so, even if Carnap's codification had been entirely successful, he would still only have shown that logic _may_ be syntactical.

Presumably it is possible to codify logical practice directly; but nothing about Carnap's codification suggests that it is direct. The same may be said of his claimed semantical codification of logical practice in <u>Introduction to Semantics</u>; in so far as he goes beyond explicating vague logical concepts, and makes a serious attempt to show that logic is a part of semantics, he tries to prove his case simply by constructing a semantical codification of logical practice, without making any attempt to show that this codification should be considered 'direct', in the sense indicated.

In general it is not possible to show what the nature of logic is merely by producing a codification of logical procedures. Indeed, it may be that <u>in practice</u> a direct codification of logical procedures is not feasible (just as it may be impracticable to give a direct code to replace the indirect rule 'Keep your eye on the ball'); and if this happened to be the case, then the more one concentrated on actual codifications of logical practice, the less likelihood there would be of one's gaining an understanding of the nature of logic.

Indeed, a more definite conclusion than this can be reached. It was observed in the previous section that, in order to tell what a direct codification of a given practice would be like, one must first know exactly what the given practice is, and how it may be properly described. It follows from this that before one can decide whether a particular codification of logical practice is 'direct', one must first determine what logical practice is; that is, one must in effect answer the question 'What is logic concerned with?'. Hence it is useless to try to prove anything about the nature of logic _via_ construction of codifications.

Thus the question 'What is logic?', though it is equivalent to 'What form would a direct codification of logical practice take?', is not to be answered on the basis of any codification of logical practice; it may be that Carnap has given up trying to answer the question because he recognizes this fact. I do not propose to consider the question itself here, except to say the following. First, it is clear that it can be answered only on the basis of a detailed examination of logical practice. Second, as mentioned earlier, I wish to appraise Carnap's account in his own terms, and so I have been talking of logical properties and relations as properties of, and relations between, sentences. But in fact this terminology, and the 'sentential' view of logic it embodies, are hard to justify, because of philosophical difficulties connected with the theory (e.g., the contextual and indexical ambiguity which is possibly an essential feature of ordinary sentences). Further, this theory is misleading, and hampers understanding of logical practice, and the important relation between it and logical theory.

This last point touches on the value of the theory qua explication, about which a little more should perhaps be said. In so far as Carnap's explication of logical concepts in terms of semantical ones is intended to constitute a literal description of the nature of logic, the comments made above, about the 'realist' account of logic, are applicable. In so far as the explication is intended to yield insight or understanding, or to explain what logic is, the criticism at the end of the previous paragraph is applicable. There certainly seems to be some truth expressed in the dictum that logical truths are truths which hold 'in virtue of meanings alone', but Carnap, who takes this conception of logical truth as his 'explicandum'[16], does not make it clear what this truth is: he merely gives a precise, artificial sense to the dictum, a sense which has no bearing on logical practice.

4. Alternative Logics.

It has often been asked 'Are there alternative logics?'. A fairly brief, and I think enlightening, answer can be given to this question, on the basis of the discussion in Section 2.

First, it should be noticed that although there can be only one kind of codification of logical practice that is direct, there may be several different direct codifications, all correct. Suppose the sentential mode of codification were the direct one; then the several forms of the propositional or sentential calculus, which take different logical constants as primitive, and have different sets of axiom schemata, would, if taken in the customary sentential interpretation, be alternative direct codifications of logical practice with respect to sentences. Thus, quite apart from the possibility of codifications of logic of different kinds, direct and indirect, there is the possibility of alternative direct codifications.

But all correct alternative logics will have to be equivalent <u>in effect</u>; for their effects will have to match logical practice. Only to the extent that logical practice is ill-defined and incompletely determined will there be scope for genuinely alternative logics, distinguishable one from another in the practice they call for; as was suggested earlier, one possible locus of such divergence is in higher-order modalities[17,18].

Chapter IV: The Carnapian Account of Mathematics.

In the previous chapter, the topic of discussion was the thesis that logic is part of semantics. Various forms of the thesis were mentioned, the 'realist', the 'explicationist', and the purely empirical. In this chapter I wish to consider a corresponding thesis that may be advanced about mathematics. In particular, I will consider the thesis in relation to the application of mathematics, or 'mathematical practice', somewhat as the thesis about logic was considered in relation to 'logical practice' (though the two cases are not entirely similar; cf. p. 83); and I will use Carnap's formulation of the thesis, in terms of interpreted formal systems, as a starting point. Again, there are nine variants of the thesis that could be considered, but only the stronger versions, the realist and explicationist ones, are of real interest. According to the realist version, mathematical theories <u>are</u> formal systems, and are applied by employing true interpretations of them; according to the explicationist version, mathematical theories are best understood as formal systems, and applications of them as use of such systems in true interpretations. The relative importance of these two versions will be considered in Chapter VI.

Carnap does not have much to say about mathematics; what he does say falls into two categories. First, what he says about logic in various places is extended, explicitly or by implication, to mathematics; and second, in _The Foundations of Logic and Mathematics_, he gives what must be regarded as an explication of how mathematics is applied. In fact the application of mathematics is not, as Carnap assumes, like the application of logic. Comparison of what Carnap has to say, in _The Foundations of Logic and Mathematics_, with the facts of how mathematics is applied (in Chapter VI) will prove very instructive.

1. Carnap's Account of Mathematics.

The title of Carnap's monograph, _The Foundations of Logic and Mathematics_, suggests that in it, if anywhere, one will find Carnap's view of the nature of both logic and mathematics. Also, it comes in point of time after his 'conversion' from the view that syntax is all-important, and what he says in it about the application of logic and mathematics has not been rendered out-of-date by any more recent discussion. Thus it may be taken as representing his current views about logic, and more particularly mathematics, and their application.

In fact Carnap here, as in almost all his writing since The Logical Syntax of Language, avoids talking directly about 'the nature of logic', and 'the nature of mathematics'. What he does talk about is the application of logical and mathematical formal systems, or, as he calls them, 'calculi'. A logical, or mathematical formal system is one which is constructed with a logical, or mathematical interpretation, in mind[1]. Since he nowhere speaks of logic or mathematics as such, it must be presumed that he is discussing 'the foundations of logic and mathematics' by giving an account of logical and mathematical formal systems, and their application. This presumption is vindicated by consideration of what he actually says.

(1) It should be remarked that at the very start of the monograph he characterises calculation as 'a special form of deduction applied to numerical expressions'[2].

And throughout the monograph[3], as well as in other writings, he treats logical and mathematical interpretations of formal systems as essentially similar, lumping them together as 'logical' interpretations, in contrast with 'descriptive ones.

(2) Under the heading 'Application of Mathematical Calculi', Carnap offers[4] a very simple example of a logico-mathematical deduction in which 'we apply a certain part of the higher functional calculus and an arithmetical calculus'[5]. He presents the example by setting out a sequence of sentences, which must presumably be regarded as interpreted formulas of the combined calculus. The details of this sequence are not of particular interest, except that two of them are as follows:

> "For every F, G, H, m, n [if m and n are finite cardinal numbers and G is an m and H is a n and for every x] x is an F if and only if, x is a G or x is an H] and for every y [if y is a G then, not y is an H] then F is an m + n]."
>
> "3 + 6 = 9."

From the way in which he sets out this example, Carnap seems to be equating the application of mathematics with the use of mathematical formal systems in (true) interpretations.

(3) Support for construing his account in this way is to be found in what he says in a previous section about the relation of ordinary deductions to the use of interpreted logical systems[6]. As in the case just mentioned, he gives an example of the application of a logical formal system, by setting out a sequence of sentences, which are to be considered as interpreted formulas of the system, or rather of a filled-out version of the system[7]. He remarks that while the example is of a very short deduction, it is typical of the form of longer ones as well; and he continues:

> "In practice a deduction in science is usually made by a few jumps instead of many steps. It would, of course, be practically impossible to give each deduction which occurs the form of a complete derivation in the logical calculus, i.e., to dissolve it into single steps of such a kind that each step is the application of one of the rules of transformation

of the calculus, including the definitions. An ordinary reasoning of a few seconds would then take days. But it is essential that this dissolution is theoretically possible and practically possible for any small part of the process. Any critical point can thus be put under the logical microscope and enlarged to the degree desired."[8]

Since Carnap regards mathematics and logic, and their respective applications, as only superficially different, it seems fair to apply the import of this remark to the case of application of mathematics[9], and to conclude that Carnap thinks that a mathematical deduction of the kind he has outlined is the archetype or ideal of proper 'mathematical practice', and that other kinds of procedure may diverge from this ideal only in having, in place of 'steps', 'jumps' which are dissoluble into 'steps' on demand.

We may conclude, I think, that Carnap holds that an application of a part of mathematics must, in principle, be analysable in terms of the employment of a (mathematically) interpreted formal system or formalised language (the interpretation must presumably be true). Consider a typical example of application of mathematics, the performing of an arithmetical sum in the course of solving a practical problem. According to Carnap, the sum is to be regarded as consisting of a sequence of formulas, obtained by transformations performed in accordance with the rules of a formal system, each formula being interpreted in the practical situation as a sentence, and rules of transformation corresponding, *via* the interpretation, to known principles of inference (cf. Section 3 of Chapter II). More precisely, the sum either is such a sequence, or can be expanded into such a sequence.

2. The Presupposition of this Account.

In this discussion, as everywhere else, Carnap is satisfied to talk about sentences; he nowhere gives an account of how sentences are applied or acted on in practical situations, where there is something to be done done, or a decision to be made, or plans to be drawn up. Now the application of mathematics does not end in the writing down or affirming of sentences; we apply mathematics to the world, and the practical problems that crop up in everyday life. So Carnap's account as it stands is incomplete: it only gets to the level of sentences, and does not reach the level of practical action.

However, Carnap could defend his procedure quite easily, by pointing out that the problems of applying or acting on sentences are problems related to language as a whole, and not peculiarly associated with what he is discussing, namely, mathematical sentences, or formulas in mathematical interpretations. He might therefore claim to be justified in presupposing some explanation of what 'acting on a sentence' consists of, or leaving others to provide such an explanation, and restricting his attention to the 'gap' between arithmetical sums, for example, and sentences.

This defence would be in order, if the assumption it is based on were correct; this assumption is that the 'gap' between manipulation of figures and action in a practical situation is composed of two smaller 'gaps', one between manipulation of figures and sentences, and another between sentences and actions. Or, put another way, the assumption is that sentences 'mediate' between calculations and the actions based on them. This assumption is questionable; and it will have to be examined presently. If it is a mistaken assumption, then Carnap's method of escape fails: if sentences do not mediate in the application of calculations, or in the application of mathematics in general, then one certainly cannot explain how this application is effected in terms of interpretation of formulas as sentences. For the present, it is sufficient to notice that this assumption is necessary for Carnap's account.

3. <u>The 'Linguistic Model'</u>.

Carnap's assumption that mathematics is applied through the medium of sentences is but one version of an assumption that has been almost universally made by writers on the philosophy of mathematics. This assumption is that the way in which mathematics is put to practical use is by treating mathematical characters (numerals or figures, variables, and the rest) as symbols of some kind, and, correspondingly, mathematical formulas as (significant) sentences. For convenience, the picture of the application of mathematics involved in this assumption may be called the 'linguistic model'. It will be helpful to consider some of the forms in which this assumption crops up. It should be remarked that Carnap, in talking about the interpretation of formulas of mathematical calculi as sentences, makes the assumption much more explicitly than do most writers, who seem to make it quite unconsciously.

The assumption may take the form that the formulas of mathematics are, _per se_, significant. This view is probably not commonly held today, when it is generally recognised that mathematical theories can usually be 'interpreted' in different ways, or given two or more distinct models. It is interesting to notice, however, that a view somewhat of this kind can be ascribed to Hilbert. Hilbert was the chief spokesman of the Formalists, who hold that questions about the 'meaning' and the use of mathematical formulas lie outside the province of enquiry into mathematics proper, and in general ignore such questions. But Kleene remarks[10] that Hilbert at one time drew a distinction between 'real' and 'ideal' statements in classical mathematics; the former are statements 'being used as having an intuitive meaning'[11], and the latter are statements which are not being used in this way. Hilbert appears to have been talking about the use of mathematical formulas, not 'in interpretation', but in formal mathematical procedures, e.g., in proofs, and so to have been maintaining that at least some formulas (the 'real' ones) are, in themselves, meaningful sentences.

Most commonly, however, the linguistic model is applied, not to formal mathematics, but to its applications. This is how Carnap applies it.

Versions of the assumption may differ in other ways. It may be applied only to formulas as a whole, or it may be extended to particular kinds of characters, leading to the further assumption that characters of these kinds have, or can be treated as having, a specific role (e.g., as names, or predicates).

The most obvious case in which the latter version of the assumption is involved is that of theories of 'number'. Application of the linguistic model (consciously or unconsciously) to arithmetical procedures prompts the question "What kind of symbol is a numeral?"

(or "What kind of symbol is a numeral in the customary interpretation?"). People have assumed for a long time that this is a proper question to ask, and have tried to explain what kind of thing a 'number' (i.e., the designation of a numeral) is.

The account commonly accepted today, and used by Carnap in his account of the interpretation of arithmetical formal systems, is that due to Frege[12] and given currency through the work of Russell. According to this account, a numeral in arithmetic denotes a cardinal number, which is the class containing as its members all the classes of a certain numerical size; there is one cardinal number for each size, and the cardinal number three, for example, is the class of all collections of three things. Numerals thus designate classes of equinumerous classes, and expressions built up out of numerals by means of addition- and multiplication-signs designate classes related in certain ways to the classes designated by the component numerals. Equations express identities between classes of numbers (or identity of reference of numerals). Thus, for example, the formula '3 + 6 = 9' quoted on p. 67 is interpreted to mean something like 'the class 3 + 6 is [or: is identical with] the class 9'; while 'the class 3 + 6' is understood as 'the class of those classes which are the unions of pairs of disjoint classes which are members of the classes 3 and 6 respectively'.

Commonly accepted accounts of many other parts of mathematics are based on this 'theory' of number, and so are based on the assumption that application of mathematics involves something analogous to the use of language. The application of geometry (e.g., in optics, and in other parts of science) is explained independently, but in a similar fashion[13].

It is useful to consider what are the influences which have led to the widespread assumption that mathematics is like language, or, at any rate, is applied through some kind of linguistic medium. The following list may not exhaust the totality of relevant factors, but it may make it easier to understand the readiness with which this assumption has been made, and still is made. The first factors are of a very general sort.

(a) There is a very obvious apparent analogy between calculating and using language, an analogy which has no rivals in its obviousness.

(b) More specifically, (i) mathematical practice involves use of a set of characters, and (ii) this use is governed by a convention; in both these respects there is an analogy with language.

(c) It may be that the fact that there is such a thing as 'mathematical discourse' (e.g., what is to be found in textbooks and mathematical journals) is mistakenly taken as showing that mathematical theorems and mathematical notation are meaningful (whereas in fact 'mathematical discourse' is carried on in a metalanguage, and is concerned with formulas, proofs, and the rest, as <u>objects</u> of discussion).

(d) The apparent analogy between calculating, and mathematical practice in general, on the one hand, and deducing, on the other, suggests that the former, like the latter, involves language.

(e) Philosophers of mathematics have concentrated very heavily on 'pure' mathematics, and have considered only the application of mathematics to 'theoretical' problems. For example, they have considered the application of mathematics to arithmetical problems, such as that of 'finding the sum of two numbers', rather than its application to problems of the 'bath-tub' variety, which are the stock-in-trade of arithmetic and calculus textbooks (problems about times of emptying of bath-tubs, amounts of wallpaper needed to decorate a room, dimensions of containers having minimum surface area and given volume, and so on).

To concentrate on the 'theoretical' problems is in effect to apply the linguistic model; it is to regard application of mathematics from the outset as a means of providing answers, framed in sentential form, to questions similarly framed, rather than as a practical activity.

(f) The gradualness of the growth of distinctively mathematical, as opposed to logical, practice, and the relative effectiveness of linguistic analysis of logical practice, have made it less likely that applicability of the linguistic model to mathematical practice would ever be questioned.

These are the 'general' factors. The other factors, which may be labelled 'linguistic', have been perhaps even more influential.

(a) In the particular case of numerical mathematics, the link between number-words of ordinary language (e.g., quantitative adjectives) and figures or numerals is a very interesting one. The <u>written</u> number-word 'five' and the corresponding numeral '5', for example, are not used interchangeably, in general; but the <u>spoken</u> word 'five' is used as the vocal correlate of either. (Or perhaps it would be more correct to say that the vocable 'five' is used both as spoken number-word and as spoken numeral.) Hence the apparent distinction that is made between the written 'five' and '5' can be explained away as merely a part of literary etiquette, or otherwise purely conventional. As a result, there is a temptation to confuse the use of figures or numerals and the use of number-words, and to assume that the former must, like the latter, have a meaning.

(b) Mathematical notation has grown out of ordimary language by gradual stages of extension, and is still, despite the recent trend to formalisation, full of everyday words; words like 'homomorphism','ring', and so on, are recognisably linked with ordinary language. And the notational innovations that have been made in mathematics can be construed as useful, but (in principle, at least) dispensible conveniences; variables, for example, take the place of such locutions as 'all ... ', 'some ... ', 'one ... another ..... the one ... the other ... ', and so on[14]. Hence it is possible to assume that mathematical notation is just another technical language, similar to that of physics, for example.

(c) The terminology adopted by writers on the nature of mathematics, and of its application, has, in many cases, 'built into' itself the linguistic model. The characters of a mathematical notation, for example, are generally called 'signs', except by Formalists; and the term 'symbolism' is very commonly used instead of the neutral term 'notation'[15]. The problems of mathematics tend to be discussed in terms of what 'mathematical propositions' are, or what they mean. In each of these cases, the terminology used makes it virtually impossible to consider the application of mathematics on any model save the linguistic one.

Similarly the commonly used term 'numeral' (as opposed to the much less commonly used 'figure') has an implicit connection-and-contrast with the term 'number', and employing it involves presupposing that figures are names of objects called 'numbers'.

Further, some terms which were not originally 'loaded' in this way have acquired overtones of a similar kind. Nominally, the term 'formula' does not carry a connotation of having meaning, or being significant; it should be applicable to anything written down 'in a piece' in the course of a (written) piece of calculating, for example. But it would seem odd if one were to call a figure written down as the quotient of a division, say, a 'formula'; and this oddness is due to the fact that we feel that a formula should have a recognisable sentential form, and be 'interpretable' in some way as a sentence. Similarly, an equation is in practice thought of as a potential identity-sentence, with its equals-sign as copula, and the strings of characters on either side of it as substantival expressions.

In these and other cases, development of the linguistic model, and its general acceptance, have led to shifts in the meanings of commonly used terms, which render them unusable for criticism of the linguistic model; and no substitutes for them have been introduced.

This 'loaded' terminology has had great influence in moulding thought about mathematics; it is so universal that it has been used even by those who, I think, hold views that involve implicitly the rejection of the linguistic model as inappropriate. I have in mind the Intuitionists and (the later) Wittgenstein. The conflict between the accounts they wish to put forward, and the presuppositions they are committed to in using the terms they do use, may be a source of much of the apparent paradox, and of the great obscurity, in what they say. (Cf. the Appendix.)

Chapter V: Mathematical Practices.

This chapter is devoted to an examination of how mathematics actually is applied. In Chapter VI, the conclusions of this and the previous chapter will be brought together.

It is important to consider calculating and other mathematical practices, because they are to mathematical theories _somewhat_ as logical practice is to logical systems (although of course the mathematical theory can precede its applications, while the logical system is always derivative upon logical practice). More specifically, though the work of mathematics is not mere codifying, it is in terms of calculating and other mathematical practices that mathematics has 'cash value'; as long as a mathematical theory has no known application, it is just a game, of the 'paper and pencil' variety, though much more sophisticated than the ones to which we usually apply that term[1].

1. Terminology.

Before embarking on a discussion of what does happen when mathematics is put to practical use, some remarks on terminology are called for, especially in view of what was said at the end of the previous chapter.

First, I intend to use 'formula' in such a way that all calculating can be described as making transformations from one or more formulas to some other formula. This sense of 'formula' is very broad.

I will use the word 'character', as hitherto, for any uninterpreted design, in place of the commonly used term 'sign' and 'symbol', and the word 'notation' for 'set of characters'[2]. More specifically, I will use 'numeral' and 'figure' indifferently, to denote a string of 'digits'.

Finally, by 'mathematical practice' I mean any practical procedure which would ordinarily be described as involving the application of mathematics, but not any procedure which is straightforward deduction (even if it involves, e.g., reference to numbers). Ordinary usage fairly well justifies distinguishing calculating from deducing in this way. Examples of mathematical practices are the various sorts of calculating--doing arithmetical sums, differentiating, integrating, and the other procedures of what is commonly called 'calculus', solving equations and the other procedures of classical algebra--and performing geometrical constructions. The term 'calculus' will be applied to the system of rules and notation (if any) used in any particular calculation (this use should not be confused with Carnap's use of the term to mean 'formal system').

2. Practical Calculating.

The term 'calculation', as ordinarily used, may be applied to either of two procedures. First, it may be used to denote actual manipulation of figures or other notation; or again, it may be used to refer to the process of solving some problem by a means that involves some 'calculating' in the first, narrower, sense. An example of the latter kind of use would be someone saying 'I have calculated that we need five rolls of wallpaper to decorate this room'; an example of the former kind of use would be someone saying 'I made a mistake in my calculations--I copied down a figure wrongly'. I will use the term 'practical calculating' for the second kind of calculating, and reserve the term 'calculating' for calculating in the first, more restricted, sense. Practical calculating, then, consists of solving practical problems by calculating. And 'practical', in this context, is applicable to almost anything that is 'beyond' the calculation itself; thus a 'theoretical' problem of arithmetic, 'finding the square root of 5', could be regarded as a practical problem of a special kind.

It will be useful to have an example of practical calculating, as a peg to hang discussion on. Consider the following situation, in which calculation might typically occur.

A foreman has to get a trench dug in four hours. He must assign sufficient men to get it done, and all he knows is that one man can be expected to dig two yards of trench of the appropriate breadth and depth in an hour. He may proceed by guesswork, or trial and error, or he may find out the length of the trench, and try to make a better founded decision. In the latter case, he may make a deduction, or he may calculate.

Consider now the actual details of the various procedures he may go through. Let us suppose that the length of the trench is two hundred yards.

(a) He might, to start with an extreme possibility, take a lot of pebbles, and lay them out in a pattern of successive rows, with each pebble 'representing' one foot of trench dug, say, and each row 'representing' the total amount dug by one man in four hours, until he has laid out two hundred pebbles; then he may count the number of rows, and put a like number of men to work on the trench.

(b) He might do something similar, but shorter, using an abacus.

(c) What is most likely is that he will write down something like

$$8\overline{)200}\ ,$$

and then add in succession a '2 and a '5' to produce

$$8\underline{)200}\atop 25\ .$$

He will then assign twenty-five men to digging the trench.

(d) He might quite probably perform this (or strictly, a corresponding) calculation 'in his head', or aloud.

(e) He might conceivably write down a sequence of sentences, perhaps as follows:

One man will dig two yards of trench in an hour;

So, one man will dig eight yards of trench in four hours;

So, 200/8 men will dig two hundred yards of trench in four hours;

200/8 = 25;

So, twenty-five men will dig two hundred yards of trench in four hours;

He may stop here, or continue:

This trench is two hundred yards long;

So, twenty-five men will dig this trench in four hours;

He may stop here, or he may go further:

This trench is to be dug in four hours;

So, twenty-five men should be assigned to digging this trench;

We would consider him very odd if he went further still:

So, assign twenty-five men to digging this trench.

(f) He might go through the reasoning corresponding to the procedure of (e), in any of its forms, without writing down sentences.

Which of these procedures is calculating? If the foreman proceeded as in (a), for example, he would be representing his problem, or, strictly, its 'pattern', to himself in a form which, in contrast with the form in which the pattern appears in the actual problem, he can manipulate in such a way that he is able to 'see' the answer to his problem. His procedure really just makes obvious to him what he should do, in a case where, if the quantities involved were sufficiently small--if, for example, the trench were eight yards long--it would be obvious from the start what he should do.

Another way of describing case (a) is to say that the foreman would be 'arguing by analogy'; he would be relying on the analogy of manipulable pebbles in a pattern with non-manipulable, abstract, 'units of work'. The propriety of the analogy is obvious (and it would be practically impossible to justify it except by 'intuition').

In the second procedure, with the abacus, there is again an element of straightforward representation; but elements of standardisation of procedure, and convention, have also crept in. The abacus constitutes a general, not an <u>ad hoc</u>, procedure for solving problems of a numerical nature. The way in which its beads represent is complex: some of its beads stand for units, while others stand for groups of ten, others for hundreds, and so on, according to certain conventions. On account of this fact, I think, we would probably say that one calculates when one uses an abacus, but not when one proceeds as in (a).

If the foreman acted as in (f), we would say that he went through a logical, not a mathematical, procedure, and that he worked out by inference, or deduced, that twenty-five men will dig two hundred yards of trench, _or_ two hundred yards of _this_ trench, in four hours, _or_ that twenty-five men should be assigned to digging this trench, _or_ that he should assign twenty-five men to digging this trench, as the case may be. I think that even the step or steps from one man's output being eight yards in four hours to twenty-five men's output being two hundred yards in that time may be regarded as an inference, or inferences; it is certainly not a calculation. We might describe it as a 'mathematical' inference, but by this we would mean only that it has somehow to do with numbers. Passing _immediately_ from any sentence or collection of sentences to any other sentence is never 'calculating' (though one can make such a passage mediately, on the basis of calculation: this is 'practical calculating').

If the foreman followed the pattern of (e), he might or might not be reasoning or inferring, depending on whether he made the transitions between sentences by 'inference' or 'formal transformation'. Even if what he was doing was transforming sentences mechanically, he was doing something which is to be regarded either as not calculating at all, or as a 'limiting case' of calculating, as I hope to show later (pp. 104-105 and 138).

The case of (c) or (d) is the one that concerns us most here; it is the commonest one, but it has not been considered in detail heretofore, and has not been satisfactorily described and explained. This is calculating in a typical form, and it is quite different from deduction or inference, as exemplified by (f), and possibly (e).

Suppose that the foreman does calculate, either by using an abacus, or by performing a piece of written or mental arithmetic. How is his behaviour to be described? Several descriptions are possible.

The foreman has to take a certain action which will bring about a certain goal, and he doesn't know which particular action from a given range is the appropriate one. So his problem may be formulated in the question 'How many men should I assign to digging this ditch to get it dug in four hours?'. His problem could also be regarded as a more general one, which would be formulated in the question 'How many men, digging two yards each per hour, will dig two hundred yards of trench in four hours?'; this formulation is that of 'textbook problems'. Again, as a problem in arithmetic, the foreman's problem is to divide 2 x 4 into 200.

According as one or other descriptions of the foreman's problem is adopted, the rest of his actions will be described, with some interpretation, in one or another of various ways. It is not possible to eliminate 'theory' entirely in describing what happens when someone performs a practical calculation.

However, one can say the following without begging questions of interpretation. The foreman makes some kind of transition from the problem facing him, in whatever terms _he_ sees it, to the starting-point of his calculation (in the narrow sense). This starting-point consists of what we might call an 'initial pattern', or 'initial configuration' of characters, or whatever it is he is going to manipulate; that is, he starts with a set of objects of some kind arranged in a certain way. He then calculates, applying the rules of the calculus he is using, manipulating the objects of the initial pattern into new patterns (creating a new pattern of 'types', with fresh 'tokens', if the objects are characters). In his manipulations he follows what may be called (drawing on the analogy with computing machines) a 'programme', which is a pattern or sequence of manipulations. Following this programme, and starting from an initial pattern of a certain kind, always results in a final pattern of a particular kind (e.g., a quotient, or other figure, or an equation). The foreman, having performed the last manipulation and obtained the final pattern, makes a transition back to the problem (as he sees it): he applies the final pattern to the arithmetical problem or to the general problem about digging trenches, or to the problem about <u>this</u> ditch, as the case may be. And then he proceeds to act. Indeed, it may be that it is improper to impute any particular mental activity of 'transition to the problem'; he may merely obtain the final pattern of the calculation, and 'act accordingly'--if the final pattern is '25', he may immediately assign twenty-five men to the job.

3. Varieties of Mathematical Practice.

Consider now the different forms mathematical practice may take. First, consider cases where a notation is involved, i.e., cases of calculating of the ordinary kind.

(i) A calculation may take the form of a sequence of transitions from one or more numerical equations to some other equation.

(ii) A particular sub-case of (i) is that in which the initial equations and the final equations, or what we might call the 'end-equations', of the calculation involve only numerals of a certain kind, while in the 'intermediate equations' numerals of a different, and classically derivative, kind occur. Specifically, the end-equations may contain only numerals which classically denote natural numbers, while numerals which denote negative integers occur in the intermediate equations; or the end-equations may contain only numerals classically denoting integers, or rational numbers, or real numbers, while intermediate equations contain numerals which classically denote rational numbers, or irrational numbers, or complex numbers, respectively. In each of these cases, it is possible to interpret the end-formulas of the calculation in a way which is not available in the case of the intermediate formulas.

Suppose a calculation of this kind occurs in a situation in which there is an obvious 'interpretation' of the end-formulas, one which cannot be applied to the intermediate formulas. Here is where Hilbert's notion of 'ideal statements', or 'ideal elements', can be used. The calculator proceeds from meaningful ('real') sentences, or rather sentences used meaningfully, by way of 'ideal' sentences, which cannot be taken as having meaning, to other sentences which can again be treated as meaningful, and can be acted on. This case is a clearer case of calculating than one in which the same interpretation can be placed on all the formulas of the calculation, for the latter can be regarded as a case of deduction, like the example (e) above: the procedure involved is an exact correlate, on a formal, formula-manipulating level, of a process of inference.

(iii) A central case of calculating is the performing of arithmetical sums. The case of long division is very instructive, and it will be enlightening to describe it in the appropriate terms. A terminology for describing it may be constructed out of the standard terminology of arithmetic, by prefixing a 'C' throughout; thus 'C-quotient' means 'the numeral of the number obtained as a result of division', 'C-division' is the procedure of manipulating figures corresponding to ordinary division of 'numbers', and so on.

The calculus of C-long division includes or presupposes other calculi, for C-multiplication and C-subtraction. The calculation can be broken down into similar stages, each of which ends with the writing down of a new C-sub-remainder; each stage consists of (a) the formation of a C-sub-dividend, (b) a 'guess-work' C-short division of this C-sub-dividend, (c) C-multiplication of the C-divisor by the C-quotient of the guess-work C-short division, and finally (d) C-subtraction of the resultant

C-product from the C-sub-dividend to yield the C-sub-remainder. Call this sequence of steps a 'subroutine' (borrowing again from computer jargon); and call a numeral a 'sub-numeral' of another if it has no more digits than the other, and each of its digits is the same as the digit in the corresponding position, reading from the left, in the other. Then instructions for C-long division of a numeral A by a numeral B are: repeat the subroutine, taking as first C-sub-dividend the shortest sub-numeral of A C-divisible by B (or: C-greater than B), and forming each successive C-sub-dividend by copying down, on the right of the C-sub-remainder of the previous stage, the first digit from the left in A which has not been used in a previous stage; stop when all the digits of A have been used; then the successive C-sub-quotients, in sequence from left to right, form the C-quotient, and the final C-sub-remainder is the C-remainder.

This description is rather complicated; but it is much less exceptionable as a factual description of the process of long division as people actually go through it than a description in terms of manipulation of numbers, or inferences about numbers. People calculate <u>mechanically</u>. To put it another way, using the terminology introduced in Chapter III, the above description has a very much better claim to be considered a 'direct' codification of actual arithmetical practical than codifications in terms of numbers.

(iv) Some cases of calculating depend on what may be called 'notational algorithms'. For example, a commonly used short-cut for multiplying by ten (in the case of positive integers) consists of what would usually be called 'adding a zero to the end of the number to be multiplied'. This description is confused, of course; the short-cut is a short-cut because it consists merely of adding a zero to the end of something, but that something can only be the numeral of, or denoting, the number to be multiplied (a number has no end!).

This short-cut takes advantage of the structure of the Arabic numeral notation (or calculus), with its decimal base. It would not exist if a duodecimal-based notation were standard, though a corresponding short-cut would then exist for multiplication by twelve; and if an irregular notation, with any base (e.g., Roman numeral notation) or none, were standard, there would be virtually no short-cut algorithms for multiplying.

Another example of a notational algorithm is the one for testing divisibility by three: 'add the digits of the dividend, and see if the sum is divisible by three'. Again, this formulation is confused; properly expressed, the rule is 'add the numbers whose numerals are the digits of the numeral of the dividend, and test the sum for divisibility by 3' (or: 'C-add the digits of the C-dividend, and test the C-sum for C-divisibility by "3"'). As it stands, this test is only partially notation- or calculus-dependent: it reduces testing of the divisibility of one number to testing of the divisibility of another number, which is smaller (since its numeral is shorter[3]). It could be made a complete test, fully notation-dependent, as follows: 'C-add digits of the C-dividend, repeat with the digits of successive C-sums until a single-digit numeral is obtained; if this numeral is '3', '6', or '9', the number is divisible by 3, otherwise it is not'.

While this test can be given a fully 'notational' form, it cannot be given a form which is fully notation-independent. If a duodecimal-based notation were standard, there would have to be a completely different rule[4].

There is reason for saying that all calculi, and not just notational algorithms, are notation-dependent; the standard 'programme' for addition (or C-addition) reduces addition of multiple-digit numbers[5] to addition of single-digit numbers in a systematic fashion; this would be very difficult in, say, the Roman numeral notation[5].

Consider now mathematical practices in which no notation is used.

(i) In the case of calculating by use of an abacus, the beads are treated somewhat in the way characters of mathematical notations are treated; but they are not reproducible at will. Nevertheless, we would certainly say that using an abacus is calculating; and so we see that calculating need not involve the use of any notation.

(ii) The case of geometrical constructions is quite different from any so far discussed. In performing a geometrical construction, one manipulates physical objects, which do not seem in any sense to 'represent' anything. Yet such a construction is still a mathematical procedure.

The continuity between cases of calculating, and those of performing geometrical constructions, lies in this: people who go through either kind of procedure follow a 'programme', and the programme has a 'theoretical' justification. In the case of the foreman working out his problem by laying out pebbles, there is a programme, but its propriety is obvious, and intuitable; it can hardly be given a theoretical justification. All the cases of calculation considered are cases where the procedure followed does not lead to an 'intuition' of the answer; they are procedures which require justification, in terms of the convention of representation used--whether it is one of notation or of beads.

Similarly, the procedure of a person in constructing a right angle by forming a triangle with sides in the ratio 3:4:5 is not obviously bound to produce the desired end; it must be proved that it will do so, and the major part of the proof consists of an application of the Converse of Pythagoras' Theorem. (The proof should also show that there is a correlation between pure and physical geometry, and specifically between the constructed triangle and an 'ideal' Euclidean triangle; but in practice, this part of the proof is taken for granted.)

It is an interesting fact that ancient Egyptian surveyors used the 3:4:5 triangle construction to obtain right angles, presumably without knowing any 'theoretical' justification of the construction, or knowing that there could be such a justification of it. They cannot be said to have been performing a geometrical construction, in our sense of that term; the rule they knew was a piece of practical technology, based on an empirical knowledge of physical geometry.

4. Some Conclusions.

On the basis of the observations of this chapter, a few preliminary conclusions may be drawn.

(a) Most commonly, nothing we could call 'interpreting of formulas as sentences' occurs when a calculus (in my sense) is applied in a practical situation; in particular, people do not seem to apply the results of arithmetical sums _via_ sentences, but act on these results in some other fashion.

(b) It might be suggested that numerical calculating can be regarded as a standardisation of the procedure of laying out pebbles; there is a continuity between the latter case, through the case of reckoning by means of an abacus (where the representation is partially conventionalised), to the case of numerical reckoning[7]. At any rate, calculation can be applied to situations because there is a relation between numerals and quantities connected with these situations, a relation something like conventionalised representation.

(c) The general characteristic of any mathematical procedure is that it consists of applying rules from a set, in accordance with some 'programme', and that it can be 'theoretically' justified, either by consideration of the calculus used, or the convention of representation employed, or else on the basis of certain assumptions about the objects being manipulated (physical lines, points, arcs, and the rest), together with theorems of 'pure' geometry, which develop these assumptions.

(d) The difference between calculating and reasoning lies in the use of some convention to shorten the deductive procedure, and in the fact that the application of a calculus has to be <u>justified</u> (because the representation involved is not direct and obvious, as in the analogical argument with pebbles), by a proof which is based on consideration of (a) the nature of the convention of representation, and (b) the 'practical calculus' governing the transition from problem to calculation, and back again. The convention is 'contingent'--it might be other than it is--and adventitious, and the proof has to show how the calculus takes advantage of the possibilities for simpler and shorter procedure provided by the convention. This is most obvious, of course, in the case of notational algorithms.

(e) Finally, a procedure of transforming formulas, and then applying the result by 'interpreting' the whole sequence of formulas, is not a mathematical procedure, or if it is, it is a limiting case of such a procedure. For the justification of such a procedure must consist of correlating it in detail with a logical procedure of sentence-inference (cf. pp. 36-37); that is, it must bring out the self-evident correctness of the procedure.

This case is comparable with that of reckoning with pebbles: there is a certain absurdity about providing a 'justification'.

These last points will have to be considered further and amplified in the next chapter.

Chapter VI: Calculating and 'Applying Mathematical Calculi'

I want now to compare calculating, as described in Chapter V, with the account of application of mathematics in terms of use of formalised language, as considered in Chapter IV. But first it will be necessary to consider how accounts of the application of mathematics, and Carnap's in particular, are to be taken. For convenience, I will use the term 'calculation' and related terms with an extended sense, to cover <u>all</u> applications of mathematics, throughout this chapter.

1. Criteria for Appraising Accounts of Calculating.

A. As Descriptions:

One obvious way in which to appraise an account of anything is to find out whether it 'fits the facts'. We may enquire, of a given account of calculating, whether the pattern it postulates is exhibited by typical examples of calculating, and, further, whether every example of what we agree to call calculating exhibits the pattern. We may also ask whether each of the distinctions embodied in the account corresponds to some recognisable feature of calculating, and, conversely, whether the account recognises each of the stages, and categories of action, involved in calculating.

Unfortunately, this method of appraisal is not completely effective in the present case, because there is no unique and agreed set of 'facts' to compare accounts with. As was remarked earlier, according to how one approaches an instance of calculating, one will give a different description of the procedure the calculator goes through. In fact the account adopted colours the description which is supposed to be the standard against which it is to be measured. A Carnapian would see the calculator manipulating formulas, which are interpreted at the beginning and the end as sentences, and proceeding from practical problem to calculation and back again _via_ these sentences. Others, however, might see the calculator's behaviour quite differently. Only a strict 'behaviourist' description would win agreement from everyone; and everyone would agree that it was an incomplete description, and in fact missed the essential features of the calculator's actions.

It is clear, therefore, that comparison with the 'facts' of calculating will not get us very far in making appraisal of divergent accounts. However, the extent to which such 'facts' as there are have to be 'interpreted' in order to be reconciled with an account will obviously tell against it in any appraisal.

B. As Explications:

Carnap has talked frequently about the introduction of new concepts as 'explications' of old ones, whose vagueness and ambiguity detract from their usefulness. He has for the most part been concerned with explication as an aid to enquiry into logical matters[1], and to the construction of scientific theories[2]; in particular his 'definitive' discussion of explication[3] treats of explication as a tool of scientific investigation, and is somewhat specialised on that account. However, the criteria he gives for an 'adequate' explication in the course of that discussion are general, or may be generalised, as follows[4]: the explicating concept

(1) should 'correspond' as far as possible with the old concept;

(2) should be precise, exactly defined in terms of

other precise concepts--preferably, it should be defined in the context of a formalised language;

(3) should be 'fruitful', i.e., should lead to new grasp of the aspect of the world with which the old concept is connected[5];

(4) should, so far as the other requirements permit, be a simple concept.

This notion of 'explication' may, I think, be extended to accounts of calculating, regarded as groups of concepts, or 'conceptual frameworks', intended to replace groups of interconnected empirical concepts. Carnap's own account of the application of 'mathematical calculi', as outlined in Chapter IV, can be regarded as an explication of the rather inexact and unclear empirical concept of 'calculating'[6].

Treating accounts of calculating as explications of descriptive concepts will, I think, prove more fruitful than treating them as rival and conflicting descriptions. The notion of 'explication' may itself be regarded as a precisification or 'explication' of the notion of 'theory'; and, as we have seen, the attempt to describe what goes on in calculating involves 'theoretical' concepts, such as 'acting on a sentence', from the start. To give an account of what goes on in calculating involves some interpretation and explanation, and is in effect to construct some kind of theory, or something resembling an explication.

If this is the proper way to regard accounts of calculating, then such accounts cannot be judged 'correct' or 'incorrect' as straightforward descriptions can. They must rather be judged on complex criteria, such as those suggested by Carnap.

For the present purpose, I wish to add one further criterion to Carnap's list. This criterion is that of general applicability to all instances of the concept being explicated. (It may be regarded as one implication of the requirement that the explicating concept should correspond as closely as possible with the explicated concept.) An account of calculating which applies only to some, not all, varieties of what we call 'calculating' is surely on that account less acceptable.

2. Carnap's Account of Calculating.

To consider Carnap's account as a description is to apply the first criterion of 'adequacy' mentioned above, and is therefore a suitable way of starting.

1. As was remarked earlier, it is possible to distinguish, following Hilbert, 'real' and 'ideal' elements or formulas in mathematics, and, correspondingly, it is possible to envisage applications of mathematics in which 'real' problems give rise to 'real' or meaningful interpreted formulas, which are transformed, _via_ intermediate formulas which are 'ideal', not interpretable in the way in which the original formulas are, to yield formulas which are again 'real', and interpretable, and can be applied to the problem, or 'acted on' in the practical situation.

Not only is it possible to envisage such applications, but they occur. Real solutions to equations with real coefficients are sometimes obtainable more simply, or perhaps only, by application of methods of complex-variable analysis. Another example is that of the predicate calculus in its customary interpretation: sometimes only the closed formulas (those in which no variables occur free) are interpreted, while there are rules of transformation which allow passage from closed to open formulas, and in the reverse direction[7]; and it is quite possible that one closed formula might be derivable from another by a derivation involving open, uninterpretable, formulas.

Accordingly, if Carnap's account is to be considered an account of application of mathematics in general, it will have to be modified to allow in some way for more 'selective' interpretation of individual formulas, or of calculations as a whole, involving interpretation only of end-formulas.

2. A more telling criticism of Carnap's exposition is that considerable imagination is called for in applying it to the case of arithmetical sums.

When the foreman of the example used earlier does a sum, all he does is write down figures (and draw lines) in a pattern; he never writes down an equals-sign, for example. According to the customary interpretation of arithmetical calculi, a figure is the name of a cardinal number; if the formulas of the foreman's calculation consist only of figures, then they lack anything that may be interpreted as verb or predicate, and so cannot be understood as sentences, as is required by

Carnap's account. In this case, not only the intermediate formulas, but the end-formulas also, are uninterpretable; and this is true no matter how it is decided what is to count as a formula.

There is one way out of this difficulty for a proponent of a Carnapian view. He might say that a predicate is 'understood' for each figure or group of figures of a certain kind that is written down; that is, that the situation is somewhat analogous to that in which a person in effect makes a statement by merely saying 'Yes', or 'Probably', or saying, in a triumphant tone, 'Victory!'. In such cases, a word serves as a substitute for a whole sentence. And similarly, it might be argued, a man writing down figures is, as it were, making statements in an abbreviated form, or 'mathematical shorthand', which omits predicates in a systematic fashion, leaving them to be understood.

Now there is no obvious direct way of showing that this view is wrong; the facts seem to be interpretable in this way, with a certain amount of ingenuity; but the view can be shown to be very implausible. Consider how it would work out in detail, in connection with a typical example. Suppose the 'abbreviated' form of a long division sum,

$$\begin{array}{l} 13)2793(214 \\ \underline{26} \\ 19 \\ \underline{13} \\ 63 \\ \underline{52} \\ 11 \end{array}$$

is to be 'expanded' into a sequence of sentences. The working of the sum may be regarded as the successive filling in of the gaps in the equation

$$2793 = 13 \times 100 \times .. + 13 \times 10 \times .. + 13 \times 1 \times .. + .. ,$$

and each stage of the division could be regarded as leading to the affirming of an intermediate equation, first

$$2793 = 13 \times 100 \times 2 + 193,$$

then

$$2793 = 13 \times 100 \times 2 + 13 \times 10 \times 1 + 63,$$

and finally

$$2793 = 13 \times 100 \times 2 + 13 \times 10 \times 1 + 13 \times 1 \times 4 + 11.$$

But it is surely absurd to say that anyone who is performing a long division sum is supplying or 'understanding' equals- and plus-signs all the time, or even that this ever happens, or that calculation can 'in principle' be performed in this manner.

To give such a roundabout and involved explanation of what goes on when a calculation is performed and applied is perverse, unless there are good reasons for saying that it is the only consistent description possible. And there do not appear to be any such reasons.

There is no need for the foreman of our illustration, for example, to 'interpret' any of the figures or groups of figures he has written down as sentences, when he applies his calculation to the problem facing him. Further, his procedure is proper and legitimate in its own right, even if he does not interpret anything in this way: it is not even necessary that he should be able 'in principle' to apply his calculation by interpreting formulas as sentences.

3. The really crucial case for the Carnapian account of calculating is that of notational algorithms. Ordinary sums can be construed, albeit with difficulty, as applications of arithmetical formal systems, involving interpretation of formulas as sentences; 'short-cut' calculating surely cannot be treated in this way.

A notational algorithm can be consistently formulated only as a rule about manipulating notation; and it does not correspond to any rule about manipulating numbers, for example, at all. Suppose I have to pay ten men two shillings each, and I work out how much money I will need to do this; I write down a '2', add a zero on the right to get '20', and conclude that I will need twenty shillings. It does not seem possible to regard my procedure as writing sentences, or doing anything which is a formal equivalent of writing sentences. This may be seen as follows. If what I did was to write sentences, then what justifies the transition I made, from '2' to '20', must be a logical principle of sentence-inference (cf. pp. 36-37); correspondingly, if what I did was a formal equivalent of writing sentences (i.e., writing interpretable formulas), then what justifies the transition is a rule of transformation which is a formal equivalent of such a logical principle. But there is no logical rule, valid or invalid, which corresponds to the rule I followed in my calculation, and which could (if true) justify the application of the particular 'calculus' I employed in the situation in question. The rule followed was a rule about notation, and the justification of the procedure must of necessity be 'meta-notational'; it will be based on consideration of the adventitious fact that one can obtain the C-product of '2' and '10' by adding a zero on the right of the '2'. To label this fact 'logical' would surely be to do violence to the meaning of that term[8].

What a person following a notational algorithm writes down does not seem to admit of being 'expanded' into an interpretable formula. Despite the looseness of connection, mentioned earlier, between calculating and accounts of calculating, it does not seem possible to reconcile the fact of the existence and use of notational algorithms with a Carnapian treatment of calculation.

Further, the case of the notational algorithm is not exceptional or odd, to be allowed for by minor modifications of the Carnapian account. Rather, all the standard calculi of arithmetic (those employed in doing sums) are in an essential measure tied to a particular notation, as was remarked earlier, and take advantage of conventions of representation.

4. The basic weakness in the Carnapian account of the application of mathematics is the two-stage distinction it implies. It was suggested at the end of the previous

chapter that this distinction seems to have no basis in fact; and it was clear that the foreman might well not have recourse to sentences at any point in his practical calculation.

There is a great deal that might be said about how people apply sentences, and how they apply formulas of calculations, and how the two cases are related. But the following observation must suffice here.

Many questions call for an answer not in the form of a sentence, but in a word or phrase. 'How many?', 'When?', 'Where?', 'Who?', 'Which', 'What?', and similar questions call on the persons addressed to <u>specify</u> something. To specify, in answer to such a question, is to 'point', using words, to one alternative from a range of alternatives implicitly or explicitly suggested by the question. And calculations are undertaken in order to answer questions of just this kind. The foreman's problem, for example, is to specify a number of men, as an answer to the question 'How many men, digging two yards of trench per hour, will dig two hundred yards in four hours?' (or a corresponding question on another level, according to how he sees the problem).

Now to regard a specification (as I will call it), in response to a question of the kind described, as an abbreviation for a sentence, is to make a mistake. While, for example, the statement 'Twenty-five men will dig two hundred yards of trench in four hours' contains the answer to the question 'How many men will dig two hundred yards of trench in four hours?', it cannot be said to be the answer. For it is as much the answer to 'How many yards of trench will twenty-five men dig in four hours?', and also to 'How long will it take twenty-five men to dig two hundred yards of trench?'. These three questions are distinct, and call for specifications of different kinds of things; the specification can in each case be based on the given statement or sentence, but goes 'beyond' it, picking out or emphasising a particular 'aspect'[9] of what the sentence conveys. The answer 'Twenty-five men' goes 'beyond' the sentence, and represents a move along the way from the sentence to acting upon it; and it is this kind of answer that the 'How many?' question calls for.

Another example may make this clearer. Suppose a teacher asks a pupil 'How many notebooks could you buy for eighteen shillings, if they cost three shillings each?', and the pupil answers 'You can buy six notebooks costing three shillings each for eighteen shillings'. The teacher might quite possibly follow this up by saying 'You haven't answered the question; the answer is "Six notebooks"'. And the reason he might do this is that it is possible that the pupil gave the sentential answer without knowing how to utilise the information contained in it, how to pick out what it has to say about different situations; and this would be a serious gap in the child's capacities. For example, if the child 'knew' in some sense that six notebooks cost eighteen shillings, but could not pick out the component 'Six notebooks' from it on demand, he would not know how many books to ask for if he wanted to buy eighteen shillings' worth, nor would he know how many he should receive if he asks for eighteen shillings' worth. Now specification is merely a verbal expression of the result of such a mental act of picking out; and so inability to specify is symptomatic of lack of this capacity to pick out information from facts.

If these arguments are correct, and a specification is something distinct in kind from a statement, then three stages can be distinguished in the application of a Carnapian calculus. First, the formula is interpreted as a sentence; then the 'aspect' of what the sentence expresses or conveys which is appropriate to the given situation is picked out, in the form of a specification of some kind; and then the specification is acted on. But, I wish to maintain, practical calculations commonly bypass the 'sentence' stage, yielding specifications directly.

It has been argued that calculations are undertaken in order to answer questions calling for a specification of some kind; and that making a statement is inappropriate as an answer to such a question. If this is correct, then it is possible to show that it is not merely implausible but incorrect to suggest that a man performing an arithmetical sum is writing down abbreviated sentences, with predicates 'understood' throughout. For an arithmetical sum is performed in order to specify a quantity of some kind, and the figure obtained as a result of the calculation will be interpreted as 'standing for' a number of units of the appropriate kind. To interpret the figure as a shorthand sentence would be to travel <u>away</u> from the specific problem towards a general statement, to go from a position nearer to solving the problem to one farther removed from solving it.

The remarks of this sub-section have implications of great interest; in particular they point to the existence of a strictly 'extra-sentential' use of referring words, a use which yet most closely resembles the use of words to make statements. It may be that an account of language could be developed in which the criterion of the meaning of a word having a designating 'dimension' (as distinct from the word's having a reference, in the way 'New York', e.g., does, but 'The present King of France' does not) would be whether it could possibly be used in answering some question that calls for specification.

However, I do not propose to develop this matter further here, except to say that I am sure that such 'meaning' as numerals have is very largely derived from their use in specification.

3. <u>Carnap's Account as Explanation</u>.

I want now to consider the Carnapian account of the application of mathematical formal systems, as an explanation of how calculation is possible, and how it is justified, and of how people know how to proceed when they calculate. To do this is to apply the third of the criteria of an adequate explication, namely, the criterion of fruitfulness in yielding insight or understanding.

In this respect the account is open to severe attack. Carnap explains in detail how one employs a formal calculus, and how one interprets it, and what the structure of the rules governing transformation and interpretation is like. But he does not consider more basic questions, namely, (1) How is the use of a calculus in a given interpretation justified? (2) How do people know which calculus to apply when faced with a given problem, and how to apply it, that is, what programme of calculation to follow in order to get an answer? and (3) How do they make the transition from problem to initial sentences, and from final sentence back to the problem?

The first question was considered earlier, in Chapter III, when the following conclusion was reached:

because an interpretation of a calculus must, in order to be usable, not only be true, but be known to be true, the only calculi of practical utility, apart from very trivial ones, are those which are exact formal correlates of systems of logical principles known to be valid.

When one considers the second question, it does not take one long to realise that on the Carnapian account a calculator must either go mechanically through a rigid procedure that he has been taught by drilling, with no understanding of what he is doing, or else he must constantly be 'interpreting' the formulas he is manipulating in order to see what move he should make next. Now of course much calculating is done mechanically: people are taught to solve problems of particular kinds by application of particular calculi (in our sense), following specified programmes; however, a lot of mathematical working is done, not 'by rote', but in an <u>ad hoc</u> fashion, and such working does not involve constant 'interpreting'. In fact in numerical calculating the programme of moves is obvious because the relation between the given situation and the calculation is a kind of representation, conventionalised but intuitively comprehensible; and in applying geometry, people proceed largely by intuition, rather than formally.

The third question, as we saw at the end of the previous section, is not easily answerable on the basis of a Carnapian approach.

The overall impression that consideration of these questions gives is that the application of an interpreted formal system, in the manner envisaged by Carnap, would be far more complicated and difficult than proceeding in an intuitive, or semi-intuitive fashion. One is indeed less likely to make mistakes, and unconscious assumptions, in manipulating strings of characters according to explicit rules, but the groundwork of showing a given interpretation of a formal system to be true, of determining which problems may be solved by using it, of setting up the various programmes of moves to be made in the various situations, offers scope for many mistakes and unconscious assumptions. Indeed, one of the commonest mistakes of philosophers who try to explicate by constructing formal or axiomatic systems is to make incorrect or questionable assumptions in correlating formal systems with fields of experience; in general they spend much time elaborating their systems, but not enough time in showing that the interpretations they have in mind are in fact true, and true in detail. Correspondingly, the Carnapian calculator is liable to err in using a formal system in a practical situation with an interpretation which is not true.

4. Appraisal of the Linguistic Model in General.

We will have to return to Carnap again; but first it may be instructive to consider how far the criticisms of the previous sections apply generally to all accounts of calculating based on what I have called the 'linguistic model'.

Consider first the Frege-Russell theory of number. The discussion in part (4) of Section 2 suggests another way of explaining how numerals signify, namely, in specification. This discussion does not suggest that numerals refer directly to anything, however, but implies that they are used with nouns to specify quantities of various kinds of units (e.g., seven men, ten dollars, twenty-two miles, an hour and seventeen minutes); that is, that they are used as adjectives of quantity are used, and not substantively[10].

The Frege-Russell account of number is the best account there can be, it seems: Frege effectively disposed of all the alternative accounts extant in his day, and no new ones have come forward since. Even if one agrees that the question 'What do numerals denote?' is a real question, however, one will not find the account very persuasive. We may admit that a collection of objects is a collection of five objects because it has the property of 'fiveness', or quintuplicity; but it is a big leap from saying that fiveness is the property of all five-member classes to saying that '5' denotes the class with fiveness as its defining property. This identification seems to be justifiable only on the principle known as Occam's Razor; but this principle seems to apply more appropriately to the hypostatisation of numbers, that is, against the prior assumption that there are objects, denoted by numerals, whose nature requires to be characterised.

A more down-to-earth objection to the Frege-Russell theory is that people used 'numbers' for so long prior to the 'discovery' that numbers are classes of classes, and that many people still ignorant of the theory can calculate correctly. Perhaps still more significant, people who are aware of the theory and accept it do not, I am sure, calculate differently in consequence, nor do they judge the correctness of a calculation by its dissolubility into a sequence of formulas interpretable as sentences in accordance with the Frege-Russell theory, a sequence that constitutes a logically valid derivation.

Consider now the applicability of the linguistic model to mathematical practices in general. I think the arguments put forward in parts (2), (3), and (4) of Section 2 tell strongly against any account of calculating that might be constructed on the basis of the linguistic model.

The discussion in part (4) of that section suggests that one stage in the application of mathematics may be explained on the model of a particular use of language, namely, the use of words in specification. Indeed, the relation of this use of language to application of calculations is closer than that of a model or analogy: calculations may lead to a specification, formulated in words, as an answer to a practical question.

But it is also quite possible that the calculator may proceed directly from writing figures to action, without any intermediate specification in words being involved; the figures may act as a direct trigger to action.

There is an account rather like a weak version of the linguistic model, which is not so exceptionable as the versions so far considered. According to this account, a calculator 'interprets' the formulas resulting from his calculations somewhat in the way an experimental scientist interprets the results of his experiments, and in doing this he treats the formulas as 'meaningful' or 'significant'.

This is nearly correct; but in so far as it is correct it does not represent an application of the linguistic model, while in so far as it does represent an application of the linguistic model it is not correct. It will be worth exploring this point in detail.

In the first place, it is not a formula that the calculator 'interprets' as meaning that he should do thus and so; just as the scientist interprets, not figures or events, but <u>results</u> of experiments he has performed, or <u>data</u> obtained by systematic observation, so what the calculator interprets is, not a figure, for example, but the appearance of the figure as a C-sum or C-product or C-quotient of a calculation he has performed.

In this respect what the calculator interprets (and equally what the scientist interprets) is rather like a 'natural sign'[11]. A lightening of the sky during the night may be taken as a sign of the approach of dawn; a darkening of the sky during the day may lead people to say 'That means it's going to rain'; a patch of green in a dry landscape may 'mean' that there is water in some form at that place. Each of these is an example of a thing which is, or may be taken as, a 'natural' sign: people take it as a sign that something other than itself is present, or is the case. It is possible to take these phenomena as signs in the respective ways mentioned, because there is in fact a connection between their presence or appearance (as the case may be) and the presence or appearance of that which they are taken as signs of; and further, it is proper and legitimate for people to take them as signs, because they know of this connection.

Natural signs are contrasted with 'conventional' signs, which are connected with what they signify not by a natural regularity of concomitance, but by an artificial convention of some kind. People who know the convention may use a conventional sign to communicate with others. Signals and sentences are examples of conventional signs.

Natural and conventional signs differ also in that the former are characteristically 'phenomena'--things happening, or appearing, while the latter are more properly 'objects'. It is the sky's becoming lighter, not its being of a certain lightness, that is the natural sign; it is the appearance of the green in a particular place, not its existence _per se_, that is the sign. More specifically, the significance of a natural sign is essentially a function of the place and/or time at which it appears or occurs: the lightening of the sky at place $\underline{x}$ and time $\underline{t}$ is a sign that, at place $\underline{x}$ and time $\underline{t}$, dawn is near. Further, such a sign does not have even this meaning except in an appropriate context (the sky having previously been dark for some time, or a desert landscape).

Of course, conventional signs are also context-dependent; they are objects used or treated as signs, not signs in any absolute sense. But at the same time they are independent of context of use or occurrence in a way in which natural signs are not (Wittgenstein *et al.* notwithstanding); if this were not so the compiling of dictionaries would be out of the question. The context-dependence of conventional signs can be either eliminated or reduced to a dependence of an indirect kind, by stipulating a 'universal' spatio-temporal frame of reference; this frame then becomes a conventional 'universal context'. There can be no such generalised context in the case of a natural sign, which by its very nature must 'mean something different' every time it appears.

To return to the calculator and his 'interpretation' of formulas: he treats an end-formula of a calculation in much the way people treat natural signs, and not in the way they treat conventional signs. It is, and must be, the appearance of the formula at the end of the particular calculation, in the context of the practical situation which led to the act of calculating, that is interpreted, and treated as significant; the interpretation is in terms of the situation. In the context of a problem about how long a bath will take to empty, a '5' appearing at the end of a calculation will be interpreted as a sign that the bath will empty in five minutes, or five hours, or five of some other units of time, according as numerals of the initial formula or formulas were treated as representing lengths of time in minutes, or hours, or some other units. And in the context of another problem a '5' would be interpreted quite differently. What the appearance of a numeral as a result of calculation signifies is in an essential way a function of the circumstances which gave rise to the calculation, and the way in which the calculator passed from these circumstances to his calculation. Thus the meaning of formulas in calculations is intrinsically context-dependent, like that of natural signs; like these signs, formulas 'never mean the same thing twice'.

Since language is a use of conventional signs, and not of natural ones, it cannot provide a proper model for the signifying use of mathematical notation; there are different kinds of context-dependence involved in the two cases. Further, when we say that the calculator treats the result of his calculation as a sign, we may mean no more than that he acts on it. We do not mean that he writes it down as a sign in the first place; only when he has written it down, and has finished his calculation, does he interpret the result. Nor, when once he has interpreted the formula, would he use it to communicate what he interprets it as meaning; '5', for example, would not normally be used to tell someone that a particular bath will empty in five minutes (though 'five minutes' might be)[12]. In both these respects, as well as that of context-dependence, the processes involved in applying a calculation, though they can be described as 'taking formulas to mean something', are dissimilar from the processes involved in the use of language.

Of course, mathematical notation is used in accordance with conventions; and it is 'reproducible'--the 'type'-'token' distinction, or a similar one, is applicable to it. In both these respects it resembles the vocabulary of a language, and differs from natural signs. But the conventions associated with a mathematical notation do not determine the 'meanings' of its character-types; they have to do with how the character-types are to be manipulated, and the ways in which the character-tokens are to be used to represent.

5. <u>Final Appraisal of Carnap's Account</u>.

Carnap's account of calculating is not a very good description; nor is it a very good explanation. As an explication it therefore shows up poorly also: it satisfies the criterion of exactness, but it fails to meet the other two main requirements, of correspondence with the empirical concept of calculating, and of fruitfulness. The fourth criterion, of simplicity, is a subordinate one; I do not know how Carnap's account would measure up to it.

The previous section was intended to place the failure of Carnap's account within a larger perspective, by showing how any presupposition of the appropriateness of the linguistic model must inevitably mislead.

Carnap's account is not so much wrong as very incomplete. He is correct in recognising that mathematical practice grows out of logical practice, but wrong in regarding it as a straightforward extension of logical practice. It is of course impossible to draw a sharp line between logical and mathematical procedures; but if there is any feature which is peculiar to and characteristic of mathematical procedure, it is the use of conventions of notation and/or representation in providing simpler and shorter methods of problem-solving. All mathematical theorems and proofs can, I think, be regarded, so far as they are really meaningful, as 'pseudo-object' sentences and sequences, to use Carnap's terminology[18]; that is, they can be regarded as being 'about' notation while appearing to be 'about' mathematical objects of some kind. Not only can they be regarded as 'meta-notational' theorems and proofs, but they must be so regarded.

This view sounds very strange, it must be admitted; but if it is conceded that the nature of mathematics must be explained in terms of mathematical practice, of the application of mathematical theorems, then I think it must be granted that this account is the only consistent one possible. Of course, there is no condemnation implicit in the contention that mathematical theorems and proofs are 'pseudo-object' sentences and sequences of sentences; if methods of calculation are to be developed prior to the discovery of ways in which to apply them (and this procedure has proved very fruitful hitherto), then it is simplest to frame them in the form of theories about some kind of abstract objects, and to frame proofs of the appropriateness of these methods as 'factual' proofs about these objects; these 'factual' proofs can then be applied to <u>justify</u> methods of reckoning and calculating when the theories come to be applied, just as other methods and procedures can be justified by citing facts[14].

Carnap's error, as I see it, lies in not appreciating the many ways in which a mathematical practice can be justified[15]. He assumes that it must be justified by correlating it, step by step, with a logical practice; at any rate, this is what is entailed in his notion of a true interpretation of a calculus, and his view that a mathematical procedure is an application of a calculus in a true interpretation. The only concession he allows, presumably, is the one he allows for logical practice: 'jumps' can be substituted for 'steps', provided the 'jumps' can, on demand, be dissolved into 'steps'. Even the common mathematical practices which led Hilbert to talk of 'real' and 'ideal' elements--using complex-variable theory to solve real-variable equations, and the rest--do not fit Carnap's pattern; and the other examples of calculating produced in Chapter V are still farther removed from the Carnapian conception. The justification of a notational algorithm involves showing that two ways of solving problems of a certain kind--a logical way, proceeding by sentence-transformation, and a 'notational' one, which involves representation-cum-specification--are equivalent in effect; it does not correlate the two methods directly at all, and could not do so, because they are radically different, the one being, like all logical procedures, independent of the particular mode of expression used, and the other essentially notation-dependent.

The justification envisaged by Carnap is the limiting case of justification; it consists of showing that two procedures, one logical, the other the exact formal correlate of it, are equivalent in effect. This involves merely pointing out the correlation, and drawing the obvious conclusion. The proof is really just a 'proof by inspection'. Hence, I think we must say, the method that is justified is not a mathematical one, but a logical one, or rather, a method of formal logic. If it is a mathematical method, it is only trivially so.

A mathematical practice is one with a theoretical justification, one eventually based on appeal to logical principles. All formal logic, and use of formalised language is in this sense mathematical practice; but it is so only as a limiting case, because its justification consists of a 'proof by inspection'. Hence, to regard the application of mathematical theories in general as the use of them in 'true interpretations' is to fail to do justice to the real power and rich variety of mathematical methods; it is to assimilate all these methods to one not very useful and far from typical method. The general aim in mathematics is to provide an alternative to a process consisting of a sequence of logical steps, an alternative that is provably equivalent in effect to that process, but not necessarily stepwise equivalent. This can be achieved in two ways. First, notational manipulations can be substituted for logical inferences or deductions; and second, the formulation of 'data' in sentences can be by-passed, by use of the method of representation-cum-specification. By these two means, a much shorter procedure can often be obtained; and this is the point of the whole endeavour. For if one is going to forsake intuitive logic, and proceed formally, one should try to gain the maximum compensation for the loss of intuitive understanding, by using the simplest possible formal procedure. The procedure involved when one uses a Carnapian interpreted formal system in a practical situation is of course just as long as the corresponding intuitive logical one, because of the stepwise correlation of formal and logical procedures; it makes use of neither kind of short-cut.

Chapter VII: The Utility of Formalising.

I would like to conclude by considering the question of the utility of constructing formalised languages in general. This will prove a useful way of drawing together the various threads of discussion.

First, consider how the findings of the previous chapters bear on this question. The implication of Chapter II relative to the question is obvious: one cannot gain anything in the way of linguistic preciseness or logically perfect expression by using a formalised language, that cannot be gained in other ways. In Chapter III, nothing was concluded about the possibility of formulating logic by constructing skeletonic formalised languages (this possibility was in fact explicitly assumed), but only about what the fact of a successful codification of logic of this kind would or would not prove. And the great stimulation given to the study of logic proper by the trend to formalising indicates without doubt that it is fruitful to codify logic in this way, irrespective of whether the codification is 'direct' or not. In Chapters IV-VI, the relation of the process of formalising to those of constructing mathematical theories and setting up mathematical techniques was considered; and it was concluded that using a formalised language could be considered as constituting only a limiting kind of mathematical procedure, in that, though it does enable one to proceed formally instead of logically, the substitute procedure is no shorter or simpler than the original logical one would be. This led to the conclusion that it is not true to say that mathematical theories are formal systems, and are applied by using them in true interpretations.

1. Formal Systems as Mathematical Systems.

The first point I wish to make can be made by reversing the approach of Chapters IV-VI, and comparing, not the applications of mathematics with use of formalised languages, but the use of formalised languages with applications of mathematics. In fact, formalised languages and applied mathematical systems are alike; but it is the latter that are historically and genetically prior to the former, which form a special species of applied mathematical theories. The advantages of formalising should therefore be exactly those of using mathematics; it is as a method of formulating mathematical procedures that formalising in general should be judged.

This judgment has already been made, in effect, at the end of the previous chapter, where it was argued that the products of formalising fail to offer either of the procedural advantages typical of mathematical procedure, namely, substitution of shorter mechanical procedures for intuitive logical ones, and bypassing of language by the use of representation-cum-specification. It is for this reason that the construction and use of formalised languages is of no great value, and has failed to become popular outside logical and logico-mathematical fields (and there they are used somewhat differently).

2. The Use of Formalised Languages in Metamathematics[1].

I would like to emphasize that throughout this dissertation I have been concerned with what might be called the 'direct' uses of formalised languages, that is, the uses of them as languages, and, correspondingly, with attempted explications of empirical concepts in terms of 'direct' uses of formalised languages. Now of course in formal logic the aim in formalising is an 'indirect' use of formalised languages, namely, as objects of study; here they are studied as precise systems, relative to which concepts can be defined exactly, and indeed formally, concepts such as those of completeness, consistency, and decidability which form the basis for metamathematical exploration. Formalising thus is what has made possible the monumental work of Gödel, and the various methods of investigation of deductive theories that have grown out of that work[2]. And it might be argued[3] that it is only in this way that formalised languages are useful; that they are indeed not intended for direct use, in setting out scientific theories, for example.

If these contentions were correct, then some of the previous discussion, and especially Chapter II and the preceding parts of this chapter, might perhaps seem pointless. But I do not think that this is the case. In reply to the second claim, it can only be repeated that Carnap and others have written about formalised languages as if they regarded them as intended for direct use, as vehicles of expression and communication[4].

And though the first claim is partly correct, it does not represent the whole truth, for it does not take into account the status of metamathematics; this may be seen as follows.

There is a certain dependence of metamathematics on the existence of mathematical and logical practices. The importance and value of metamathematical enquiries cannot be denied; but, so far as I can see, this importance consists entirely in the implications metamathematical findings have for ordinary logical and mathematical procedures. That is, if metamathematics is to be useful and valuable, its results, which are in terms of interpretations, or rather, of models, of formal systems, must be translated into terms of actual everyday logical and mathematical practice. The question thus arises as to what the relation is between formalised languages and logical and mathematical practice. And this question inevitably reduces to the following one: What is the relation between the direct use of a formalised language and ordinary logical and mathematical practice?

Here is where the discussion of the previous chapters is clearly relevant. It may have been noticed that throughout Chapters III-VI, I discussed formalised languages from just the point of view of this question. And the conclusions of these chapters are relevant to this problem as follows.

(1) Mathematical practice is not, in general, use of formalised languages. Hence the application of metamathematical results to mathematical practice must at best be indirect, being based on showing that the logical procedures to which mathematical procedures correspond can or cannot be replaced by decision procedures, do or do not lead to contradiction, will or will not always yield a proof or refutation, and so. Further, it is not very clear how such a result as Gödel's Incompleteness Theorem could be applied to mathematical techniques based on the method of representation-cum-specification rather than on that of formulation of data in sentences. And, more generally, the implications of metamathematical findings about deductive systems for genuinely mathematical practices are far from clear.

(2) In the case of logical practices, the connection with formalised languages seems more direct and obvious, and the application of the results of

metamathematics seems straightforward. (It should be noticed, however, that, even if this is so, there will inevitably be controversy over the significance of Gödel's results.)

Consider, however, the functions of logic. Logic is not basically metamathematics. The formal logician indeed deals with logical calculi; but his first and basic task is to decide whether particular calculi in particular interpretations codify logical practice correctly. And in order to be able to do this, he must first discover in detail what logical practice is (cf. II on pp. 50-51). And his second task is of a similar kind, namely, to decide whether given codifications of logic are direct codifications; here again, an enquiry into logical practice, in which codifying can play no part, is called for. Relative to these tasks, setting up formal systems is 'hack-work', or 'notational engineering'; like all pure mathematical labours, though it is complicated, it is only a means to an end. However, formal logicians have been too interested in logical calculi, neglecting the end in favour of the means, and forgetting that logic is basically neither pure nor applied mathematics, but is <u>applying</u> mathematics--testing particular codifications of logical practice for correctness and directness. As a result, they have tended to overlook these two basic tasks, and to focus attention on a third, but subsidiary task, that of investigation of logical procedures <u>via</u> metamathematical consideration of formal systems codifying them. And they have perhaps overlooked the fact that in any given branch of logic this task cannot properly be undertaken until the first two tasks have been completed.

So in fact the application of metamathematical results to logical practices is not so straightforward and obvious as it appears to be. When once logicians have settled the questions as to what are adequate codifications of logical practice, and what are direct codifications, the application of metamathematical results will be a mechanical business; but until it is clear what logical practice is, the way in which metamathematical results should be applied to logical inferences, deductions, and the rest, must inevitably be somewhat uncertain. These results are presently framed in terms of sentence-transformation; it is not very clear whether corresponding results could be obtained in every case if metamathematical enquiries were framed in terms, say, of statements (i.e., types whose <u>tokens</u> are <u>events</u>). And it is certainly not clear at present what the nature of logical practice is, and what terms metamathematical results will have to be translated into to be significant.

Appendix.

I would like here to try to justify the suggestion made on p. 82 about the Intuitionists and Wittgenstein, and relate their views to those put forward in Chapters V and VI. This will be only a brief outline, however.

1. Mathematical Intuitionism.

I will take as my source the most recent authoritative exposition of Intuitionism, A. Heyting's Intuitionism: An Introduction. The aspect of Intuitionism that concerns us here is its view of mathematical propositions.

Heyting says, for example, 'a mathematical assertion affirms the fact that a certain mathematical construction has been effected' (p. 3); '"2 + 2 = 3 + 1" must be read as an abbreviation for the statement "I have effected the mental constructions indicated by '2 + 2' and by '3 + 1' and have found that they lead to the same result"' (p. 8); and '"the proposition p is not true" or "the proposition p is false" means "if we suppose the truth of p, we are led to a contradiction"' (p. 18).

He also remarks, in discussing the Intuitionist Propositional Calculus, that 'our logic has only to do with mathematical propositions; the question whether it admits any applications outside mathematics does not concern us here' (p. 97).

These statements may leave something to be desired in the way in which they employ semantical terms; but it is clear what sort of account they embody. Summarily, they express the view that asserting a mathematical proposition _p_ is identical with asserting that one can prove _p_ by construction.

This view is interesting, though wrong, and much might be said about it. The following comments will suffice here. First, it seems clear that assertion of '2 + 2 = 4' and assertion of one's ability to prove that 2 + 2 = 4 are quite distinct; the latter assertion involves reference to a person, while the former does not. But, second, there is some connection between making an assertion and being able to back it up. Normally, people do not make assertions unless they can back them up; though it is possible to make an assertion without being able to back it up: there is nothing absurd or unintelligible or impossible about doing this. In the case of mathematical assertions, the backing called for is proof of some kind; and here again, it would seem that there is a conventional, but not a logical connection between asserting a mathematical proposition and being able to prove it. Mathematical statements do not 'presuppose' (to use P. F. Strawson's terminology[1]) statements about the speaker's ability to prove them. Further, even if they did, the Intuitionist view would not follow as a consequence: one statement may presuppose another without that other being <u>made</u> when the first one is made.

These remarks, I think, are sound, in that they follow from the nature of assertion of propositions in general, and in particular, assertion of mathematical propositions, as that is commonly conceived; and Heyting talks, as do all Intuitionists, about 'asserting mathematical propositions'. Heyting clearly accepts what I have called the 'linguistic model' as appropriately applicable to mathematics, and it is because he does this that his view seems so unacceptable.

However, it is possible to interpret the reasoning behind this view as based on a recognition, albeit perhaps confused, of some of the features of mathematical procedure that are incompatible with a linguistic account.

Heyting talks for the most part about 'mathematical assertions', rather than talking about propositions or sentences in the abstract; that is, he talks in terms of action, not of linguistic or conceptual entities. Now if we apply what he says, not to acts of asserting, but to acts of writing down mathematical formulas in the course of calculations or formal derivations, we will obtain an obvious truth. The doctrine under discussion, thus modified, becomes: a formula should not be written down unless the writing of it can be justified. And in fact the writing down of a formula is intelligible only when it occurs in the course of a calculation or formal derivation, where it can be justified by reference to the rules of procedure. While it is the case that, for example, the proposition 'Action and Reaction are equal and opposite' can be asserted meaningfully by someone ignorant of physics and unable to justify his assertion, it is not the case that a person's writing down, say

'127' is, taken by itself, an intelligible action. Such significance as does attach to an act of writing down a formula is built upon, and inextricable from, the rules governing the procedure in which it must properly occur; and these are the rules which justify the act. This kind of act thus 'makes no sense' apart from its justification, in contrast to an act of assertion, which does.

This, I think, is the kernel of truth in the Intuitionist view; and in terms of it, most of Intuitionist theory can be explained (though not Brouwer's epistemological doctrines). If Heyting had talked in terms of the writing of formulas, he could have expressed the view without distortion, instead of producing the palpably false account outlined above.

2. Wittgenstein's Discussion of Mathematics.

As is probably obvious, I am deeply indebted, in my treatment of mathematics, to Wittgenstein's approach, as it is embodied in the writings to be found in Remarks on the Foundations of Mathematics. This approach is one not used by any writer previous to the publication of that book, save perhaps Wittgenstein's pupil, R. L. Goodstein[2]. The basic idea I have borrowed, from which all the others flow, is that mathematics should be explained and understood in terms of its applications. Indeed, the general approach to formal systems employed throughout this dissertation is the Wittgensteinian one of looking, not at words and sentences in isolation, but at the use of them.

I would like to take this opportunity of dealing with a possible objection that might be made to my treatment of what I called the 'linguistic model' of applications of mathematics. Wittgenstein's later work, and especially the discussions to be found in the Philosophical Investigations, are directed toward showing that characters may be meaningful in very different ways, and that being meaningful is fundamentally a matter of having a use, of playing some rôle in a 'language game' that is actually played by people. It might be argued that on this criterion mathematical characters can be regarded as meaningful, in that they do have a use, do play a rôle in a language game. However, this contention is obviously incorrect, for while the characters of mathematics do play a rôle in a procedure or game, that game is not a language game. It is a game which may be, and usually is, engaged in by a single person, and this 'single-person' game is not parasitic upon any 'two-person' or 'many-person' game, as, say, 'talking to one's self'is. The game of calculating, and all other mathematical practices, are essentially non-linguistic. Wittgenstein himself recognised that the use of language is superfluous in mathematical activity[3].

This brings us to the main point to be considered here, the suggestion that much of the apparent paradox in Wittgenstein's remarks may be traced to the fact that he used terminology 'biased' in favour of a linguistic analysis of mathematical practice to point out facts which are incompatible with that analysis. For though he recognised that language could be dispensed with in mathematical practice, he continued to talk about 'mathematical propositions'.

To vindicate the suggestion in detail--supposing it could be done--would take much time and space. But I would like to illustrate it by examining one particular example from the <u>Remarks</u>.

Wittgenstein there makes several statements roughly to the effect that the meaning of a mathematical proposition is determined by its proof. For example, he says:

> "A psychological disadvantage of proofs that construct propositions is that they easily make us forget that the sense of the result is not to be read off from this by itself, but from the proof."[4]

The similarity between the case of this view and that of the Intuitionist doctrine discussed above is obvious; this view sounds nonsensical on first consideration[5], but, translated into other, more suitable, terms, it is quite intelligible and correct. What Wittgenstein had in mind, I think, was this: a mathematical proposition must have an application, and its meaning is dependent on this application[6]; this application will be in the justification of a method of calculating or some other mathematical procedure; as Wittgenstein puts it, the proposition will 'determine' us or make us decide to act in certain ways[7]. Now what determines us to calculate in certain ways is not an abstract mathematical proposition; such a proposition is used as a summary of a proof; and it is this proof, read as a sequence of 'pseudo-object' sentences[8] (cf. p. 136 above), that determines us to proceed as we do. And so the application of the mathematical proposition, and hence its meaning, is determined by the proof--what sort of procedure it can be used to justify.

If Wittgenstein had avoided using the term 'proposition' altogether, he could have said this much more clearly, and prevented misunderstandings. It is clear that he recognised that 'mathematical propositions' are hardly propositions at all, from his talk throughout the <u>Remarks</u> about such propositions as 'rules', or as 'commandments', or as 'determining us to use new concepts'; but, in the main, he continued to talk about 'mathematical propositions'.

Footnotes.

Chapter I.

1. Cf. Carnap, The Logical Syntax of Language (hereafter referred to as 'LSL'), p. 4;
Foundations of Logic and Mathematics (hereafter referred to as 'FLM'), pp. 18-21;
Introduction to Semantics (hereafter referred to as 'IS'), pp. 156-158;
Introduction to Symbolic Logic and its Applications (hereafter referred to as 'ISL'), pp. 79-80;
A. Church, Introduction to Mathematical Logic, vol. I (hereafter referred to as 'IML'), pp. 48-50 (the 'primitive basis');
S. C. Kleene, Introduction to Metamathematics (hereafter referred to as 'IM'), ch. IV;
J. G. Kemeny, 'Models of Logical Systems' (hereafter referred to as 'MLS'), p. 17 (the definition of 'logical system').

2. I wish to use the term 'character' instead of the usual 'sign', because the latter is misleading in a way that will become clear in Chapter IV.

3. Cf. Church, IML, p. 50.

4. A 'logical' system contains no descriptive (designating) signs, such as individual, predicate, and propositional constants, and so it is, as Carnap says (ISL, p. 1) a schema or skeleton of a language, to be incorporated into other languages; correspondingly, the formal system constructed here incorporates the propositional calculus.

5. The use of such terms is not strictly justifiable; it is a reflection of the 'customary' interpretation of the system. Cf. Carnap, IS, Section 37: 'Calculi' and 'Variables'.

6. Cf. FLM, pp. 8-11;

   IS, pp. 22-25;

   ISL, pp. 4-15.

   In Meaning and Necessity (hereafter referred to as 'MN'), Carnap gives a somewhat different account, supplying 'intensions' and 'extensions' for all designating signs (including sentences); he also constructs the Language B in ISL somewhat similarly. However, this modification does not require special consideration here.

7. FLM, p. 10. Cf. ISL, p. 15: "A knowledge of the truth conditions of a sentence is identical with an understanding of its meaning."

8. Cf. FLM, p. 9.

9. Carnap envisages the possibility of semantical rules of this kind in a remark in FLM, p. 11.

10. Cf. his Review of Introduction to Semantics, and also IML, Sections 01 and 04.

11. In his Discussion of Meaning and Necessity. Carnap's reply, in his article, 'Empiricism, Semantics, and Ontology', seems to miss the point of Ryle's criticism. Cf. also W. Mays, 'Logique et Langage chez Carnap', Section 3.

12. FLM, p. 21 (but note also the use of 'interpretation' on p. 11, at the top);
IS, pp. 203-204;
ISL, pp. 80 and 101.

13. The locus classicus for these ideas is his Der Wahrheitsbegriff in den formalisierten Sprachen, translated as 'The Concept of Truth in Formalised Languages' (hereafter referred to as 'CTFL'), in the volume Logic, Semantics, Metamathematics.

14. Cf. Kemeny, MLS and Kleene, IM, Sections 28 and 36, for formal definitions of these concepts. Carnap adopts a like structure in the semantical system for Language B in ISL (cf. pp. 95-98).
15. This use of the term is more or less the same as that of Tarski in CTFL (cf. p. 166).
16. See especially Carnap, ISL, pp. 1-2, and the axiomatisations of logic, mathematics, scientific theories, and legal concepts in Part II. Cf. also Woodger, The Technique of Theory Construction (hereafter referred to as 'TTC'), pp. 1-2 and 71-72.
17. As, for example, in the first sentence of Section 1 of the next chapter.

Chapter II.

1. This view is implicit in their overall attitude and procedure, but is not often expressed explicitly. See however Carnap, ISL, p. 2, and Woodger, TTC, pp. 1-2. Cf. also Carnap, LSL, p. 2, Church, IML, p. 2, and Tarski, CTFL, pp. 164-165.
2. Cf. the references in the previous footnote to Carnap, ISL, and Woodger, TTC.

3. Cf., e.g., FLM, Section 5;

IS, Chapter B, the semantical systems S_1 to S_7;

MN, Section 1;

ISL, Sections 4 and 25.

4. In particular, the terms used in the classification of signs (which are often transferred to the syntactical classification of characters--cf. note 5 of Chapter I) are semantical terms ('predicate', 'constant', 'variable'); and they often play an important role in the interpretation. Cf. also p. 34 above.

5. MN, p. 4.

6. Ibid.

7. Ibid., p. 15.

8. Cf. M. Black, 'Notes on the Meaning of "Rule"' (hereafter referred to as 'NMR'), II, pp. 149-151, for a discussion of 'implicit rules' which are logical implications of explicit ones. The present case is similar, though not quite identical: the 'implied' implicit rules here are logical, the explicit ones semantical--there is a difference of kind not found in the cases Black considers.

9. Such cases will not occur often. Suppose, for example, a logician is debating whether a certain schema, interpreted in a certain way, is valid or not, and it happens that logical practice does not yield a direct answer; it may well happen that logical practice does yield an answer for the case of some schema logically derivable (in the interpreted system) from the problematic schema, and that this answer--or the answers for several such cases--gives an indirect answer for the original case (since the implications of a valid schema must be valid). So the logician must explore all the 'consequences' of the problematic schema, and their relation to logical practice, to see if he can thereby decide about that schema; only if he fails in this (possibly open-ended) task can he make an 'arbitrary' decision, on the basis of simplicity of the resultant system, say. Thus the logician's main task is exploration of logical practice; this may involve analysis of the logical relations built into metaphysical or other philosophical concepts that are commonly current. Cf. also pp. 135-136 below.

Chapter III.

1. LSL, p. 233 (italicised in the original). Cf. also Section 1 and pp. 7 and 259.
2. IS, Section 39.
3. Ibid., Section 14, p. 60.
4. E.g., ISL, p. 16: he decides to call a procedure 'logical' "when it is grounded only in the analysis of senses" of sentences;

   p. 102, top: he speaks of logical relations "holding between . . . sentences".

   He also generally restricts the use of more technical logical terms, such as 'proof' and 'derivation', to contexts in which transformation of formulas is being discussed (ISL, pp. 89-93), though not invariably (ISL, p. 33: he talks of a derivation as a sequence of sentential formulas).
5. Cf. MN, Section 2;

   ISL, p. 2.
6. Logical Foundations of Probability (hereafter referred to as 'LFP'), Chapter 1.

7. Specifically, what I have in mind is the following: corresponding to the possibility of describing in different ways, there is the possibility of codifying practices, and logical practices in particular, in different ways (cf. pp. 48-50); corresponding to the feeling that there should be some 'correct' description, a set of concepts that are appropriate on a literal, not an 'as-if' basis, there is the feeling that we should be able to give a 'direct' codification (cf. pp. 51-54) for any given practice. And, as any codification of a practice, whether direct or indirect, does determine that practice, and does yield a great deal of information about it, so an explication, whether it can be taken literally or only as 'a way of looking at things', has or lacks a certain kind of correctness, and so is worth constructing. Cf. also the discussion in Section 1 of Chapter VI.

8. LSL, p. 259.

9. As Carnap recognizes in the case of codifications of linguistic practice (FLM, p. 6: "These semantical rules are not unambiguously determined by the facts.").

10. He says (FLM, p. 7) that "A question of right or wrong must always refer to a system of rules", with the implication (cf. the foot of p. 6) that there is no right or wrong procedure until there is a codification. But surely this is not so: for example, people can (logically) make grammatical mistakes before any grammarians have got to work, and they can, and do, recognise such mistakes without knowing any explicit grammatical rules.
11. Cf. Black, NMR, II, pp. 152-154, for a discussion of whether the term 'implicit rule' can be applied in such a case.
12. Cf. again Black, NMR, II, pp. 146-148, for a discussion of this notion.
13. FLM, p. 3; cf. IS, p. 3.
14. IS, p. 9; cf. FLM, p. 4.
15. Ibid.; cf. again FLM, p. 4.
16. Cf. MN, p. 10, and a similar treatment in ISL, pp. 16-18, where Leibniz' notion of logical truths as truths which hold in all possible worlds is also 'explicated' by a precise concept, without being explained in any way.

17. But cf. again the footnote accompanying that suggestion (Chapter II, note 9).

18. The case of Intuitionist Logic is somewhat different; as a logic, it is wrong, but as a code of mathematical procedure it can be justified. Cf. the Appendix, Section 1.

Chapter IV.

1. Cf. FLM, pp. 29 and 38.
2. FLM, p. 1.
3. Ibid., pp. 22, 29, and 44. Cf. also note 9 below.
4. Ibid., Section 19.
5. Ibid., p. 44.
6. Ibid., Section 15.
7. Ibid., p. 36. Cf. also p. 32, and note 4 to Chapter I.
8. Ibid., pp. 36-37.
9. The passage quoted continues: "In consequence of this, a scientific controversy can be split up into two fundamentally different components, a factual and a logical (including here the mathematical)." (p. 37)
10. IM, pp. 55-56; Hilbert actually talks about 'real' and 'ideal' elements (entities), rather than statements.

11. IM. p. 55.

12. Cf. The Foundations of Arithmetic.

13. Cf., e.g., FLM, Section 21.

14. Cf. ISL, p. 2.

15. But it is interesting to notice that the term 'notation' is still used, and retains a connotation of 'meaninglessness'; this suggests that the linguistic model has not completely dominated thinking about mathematics.

Chapter V.

1. Cf. the various remarks to this effect by Wittgenstein in the Remarks on the Foundation of Mathematics (hereafter referred to as 'RFM'), especially IV, paragraphs 1-8 (pp. 133-138) and V, paragraph 25 (p. 180).

2. There seems to be no verb correlated with 'character': one 'writes' words, letters, and symbols in general; one 'draws' designs, and possibly pictographs; should we say that one 'inscribes' characters? I have used 'write' in the text.

3. E.g., the algorithm reduces the testing of any number whose numeral has eleven digits or less to the testing of a number whose numeral has two digits or less.

4. E.g., suppose that we had a duodecimal notation correlated as follows with the standard decimal one:

$$\begin{array}{cccccccccccccc} 1 & 2 & 3 & \ldots & 9 & 10 & 11 & 12 & 13 & \ldots & 23 & 24 & 25 & \ldots \\ \underline{1} & \underline{2} & \underline{3} & \ldots & \underline{9} & \underline{D} & \underline{U} & \underline{10} & \underline{11} & \ldots & \underline{1U} & \underline{20} & \underline{21} & \ldots; \end{array}$$

then '$\underline{U3}$', for example, would correspond to '135', which satisfies the test ($1 + 3 + 5 = 9$) and is divisible by 3; but $\underline{U} + \underline{3} = \underline{12}$ (i.e., 14), so the test fails. In this notation, the test for divisibility by $\underline{3}$ would be: is the final digit of the numeral of the number a '$\underline{3}$', '$\underline{6}$', '$\underline{9}$', or '$\underline{0}$'?

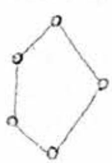
5. Abuse of terminology!

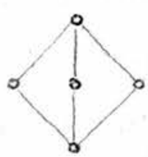
6. Further, this generalisation can be extended beyond the bounds of arithmetic, as the following examples show. (1) The algorithm for solving quadratic equations is essentially: substitute the appropriate numerals in place of 'a', 'b', and 'c' in the expression '$(-b \pm \sqrt{b^2 - 4ac})/2a$'; the corresponding task in terms of handling numbers is conceptually very complicated, and would be as hard to perform as solving the equation directly. (2) The method of working out the value of a determinant by 'pivotal condensation' (See A. C. Aitken, *Determinants and Matrices*,

p. 45-48) is essentially a procedure of manipulating notation; it depends for its practical usefulness on the standard representation of determinants by patterns of rows and columns of numerals. As an abstract procedure, it would be impossibly difficult to execute in the case of determinants of rank greater than 3.

(3) The method of representing a power of a quantity by writing the expression for that quantity with the appropriate numerical index (e.g., representing 5.5.5 by '5^3', and x.x.x.x.x by 'x^5') undoubtedly plays an important role in many contexts, but particularly in differential and integral calculus, where, for example, the derivative of a polynomial function is another polynomial function whose coefficients are obtainable from the coefficients and indices of the powers in the first function.

Still, it might be argued, these examples all come from 'numerical' mathematics. Modern algebra is non-numerical; does the same hold good for it? Modern algebra has been developed more abstractly and logically, and less with particular applications, or indeed any applications, in view. Yet there are examples which show that the 'cash-value' of abstract algebraic theories is in terms of something like notational algorithms. (1) The standard test for modularity in a lattice is: does it contain a 'five-sided' sub-lattice? (See Garrett Birkhoff, Lattice Theory, p. 66) And the standard test for non-distributivity in a modular lattice is whether or not it contains a sub-lattice of another form. (Ibid., p. 134) The value of these tests is related to the method of representing lattice by 'Hasse diagrams', which are lattices of lines and points; the diagrams of the sub-lattices specified are like this:

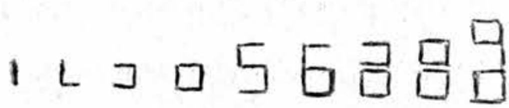




(Ibid., pp. 5-6)

It is the simplicity of these diagrams, and the ease with which it can be determined whether any Hasse diagram contains either of them that makes these tests useful; once again, the corresponding abstract tests would be as difficult and lengthy to perform as direct tests based on the definitions of modularity and distributivity respectively. (2) The method of testing for tautologies and contradictions in the propositional calculus is indubitably a mathematical technique, and can be regarded as an algebraic one (it can be formulated within the theory of Boolean Algebras). This method consists of writing out a 'truth-table' of 'T's' and 'F's' for the propositional schema to be tested, according to certain rules, and ascertaining whether a certain column of the table contains only T's (in which case the schema is a tautologous one) or only F's (in which case it is a contradictory one). (Cf. Quine, *Methods of Logic*, Sections 5 and 6.) Here again, it is the possibility of proceeding concretely that makes this 'decision procedure' (and, similarly, all other decision procedures) valuable.

7. R. L. Goodstein, in *Constructive Formalism*, has suggested that the Arabic numerals '1' up to '9' are modified forms of patterns directly representative of groups of one, two, three, and so on up to nine, respectively, as follows:



1 2 3 4 5 6 7 8 9 (*Op. cit.*, pp. 65-67)

Chapter VI.

1. Cf. MN and ISL, the passages cited in note 5 of Chapter III. Carnap's correlation of 'L-concepts' with 'Radical concepts' in IS (cf. Chapters B and C) foreshadows his explicationist approach to logic.
2. Cf. the discussion cited in the next note.
3. LFP, Chapter 1.
4. Cf. LFP, pp. 5-7.
5. In Carnap's formulation, the requirement given is that of fruitfulness for scientific investigation, and the development of scientific theory; I think this modification of it is fair.
6. It should be noticed that Carnap does not present his account in FLM as an explication, but appears to put it forward as a 'realist' descriptive account; today, however, he would probably want to put it forward as an explication.
7. Cf., e.g., the rules UI, EG, UG, and EI introduced by W. V. Quine in Sections 27 and 28 of his Methods of Logic. Formulas in which variables occur free can usually be interpreted as universal sentences, of course (cf. the Language I in LSL); but that does not affect the point made.

8. Logical truths, after all, are supposed to be 'above' language, or 'language-indifferent', formulable, without essential modification, relative to any language.

9. Cf. S. Halldén, <u>The Logic of Nonsense</u>, pp. 29-30, for discussion of a notion of 'propositional aspects'. It is not quite clear whether this notion and the one introduced here are the same or not; the particular kind of aspect of propositions I am concerned with here is the kind of aspect that is emphasised when someone says 'It was Cassius who engineered the assassination of Caesar', or 'It was Caesar whose assassination Cassius engineered', or 'Fifteen shillings was what it cost me'.

10. Cf. also note 12 below.

11. The distinction between 'natural signs' and 'conventional signs' is fairly common, and can be traced back at least to C. S. Peirce, who used the terms 'index' and 'symbol' respectively. Cf. <u>The Philosophy of Peirce</u>, Chapter 7.

12. Of course, 'five' would impart this information, if said in reply to the question 'How many minutes will it take for the bath to empty?'; but this is use of

the adjective of quantity, not the numeral: the substantive 'minutes' is understood.

13. LSL, 'Pseudo-Object Sentences', p. 284ff.

14. Black, in NMR, finds that 'rule' can be applied to four kinds of things, one of which consists of principles or 'general truths'; these are different from other kinds of rules in that they are formulated and behave very much like statements: they can be true or false, and evidence can be found for or against them. They serve as rules only indirectly, e.g., by providing mnemonics, and, Black concludes, constitute a degenerate case of rules. Mathematical theorems function in a somewhat similar fashion, I think: they are formulated, and generally treated, as statements of some kind, but their use is ultimately as (justified) rules of procedure.

15. Cf. Wittgenstein's comments in various places in RFM to the effect that mathematics is a motley of techniques. See, e.g., II, 46 (p. 84), 48 (p. 88), and IV, 46 (p. 155).

Chapter VII.

1. I am indebted to Professor Max Black for comments which gave rise to the writing of this section.
2. In formal semantics, too, formalised languages are constructed as objects for study; here they serve as rudimentary languages, whose simplicity makes them easier to handle than natural languages. However, it is not clear that this use of formalised languages has produced any great achievements, and so I have not felt it necessary to consider it in the discussion that follows.
3. Cf. A. R. Anderson, 'Mathematics and the "Language Game"', p. 449.
4. Cf. notes 1 and 2 to Chapter II.

Appendix.

1. Cf. Introduction to Logical Theory, p. 175.
2. In his book, Constructive Formalism.
3. Cf. RFM, I, 142-144 (p. 43), Appendix I, 4 (p. 49), and III, 15-19 (pp. 118-119).
4. Ibid., II, 25 (p. 76).
5. Miss Alice Ambrose, in her article 'Proof and the

Theorem Proved', concludes that it makes sense only when applied to arguments for notational conventions, mistakenly regarded as 'proofs'.

6. Cf. the passages in RFM cited in note 1 to Chapter V.
7. Cf. RFM, e.g., pp. 76-77, 122.
8. Cf. RFM, IV, 16 (p. 142):

   "The comparison with alchemy suggests itself. We might speak of a kind of alchemy in mathematics.

   "Is it the earmark of this mathematical alchemy that mathematical propositions are regarded as statements about mathematical objects,--and so mathematics as the exploration of these objects?"

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