MODALITIES AND FORMAL SYSTEMS

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PREFACE

This thesis covers a relatively wide range of aspects of what is basically one problem - that of developing a tool which will enable us to symbolize 'modal' utterances as penetratingly as the now well-developed truth-functional calculus allows us to symbolize 'descriptive' utterances. It is suggested that the reader treat the Abstract as also being an Introduction; having read it, he will perhaps be less prey to distracting questions about where the argument is leading, or about how the individual sections fit together to form a whole.

My study of this topic began in Oxford in 1965 and has been completed during the last two years while working in Rotterdam. My supervisor for all but one term of this time has been Mr. H. R. Harré, and I have benefited much from his helpful comments and continuing enthusiasm. Mr. M. A. E. Dummett, who served as supervisor while Mr. Harré was abroad, and Mr. A. N. Prior, who read and commented upon the preliminary version of Chapters I and II, have both helped me to recognise passages which required amplification for those new to my arguments and general approach.

I further wish to express my gratitude for financial assistance from the Department of Education and Science, Unilever N.V., and the Committee for Advanced Studies.

P. H. M.

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ABSTRACT

General

This work is in three parts. The first considers the most popular symbolic approach to the modalities and finds crucial shortcomings in it. The second explores the nature of symbolization and of logical form, and the third, drawing on the conclusions of the first two, sketches a symbolic treatment appropriate for certain types of talk about "necessity".

The systems considered in Part 1 are those of C. I. Lewis and his followers. It is argued that they are unsuitable for use in symbolizing everyday modal talk, (i) because they do not have constants such as we should need for representing particular modalized utterances, and (ii) because they represent the valuation applied to those utterances which can be symbolized as being dependent upon features of symbolic systems rather than upon features of the symbolizanda.

In Part 2 attention is focussed on various levels of symbolization, ranging from the symbolic use of a knot in one's handkerchief to the construction of a fully-fledged logical calculus complete with a truth-table formulation. Further the nature of logical form is explored, and it is concluded from these two investigations that the first step in symbolizing any new subject matter should be to seek out some range of valuations applicable to, and interesting for, that subject matter.

The final part considers what is involved in claiming that something is necessary - whether in an everyday or a scientific context - and what it is for such a claim to be "acceptable". It is argued that many - and probably the most important - uses of the term "necessary" are in utterances which should be symbolized as ad hoc postulates prefixed to normal truth-functional calculus symbolizations This application of the approach argued for in Part 2 is complemented by some notes on other approaches to necessity and possibility, and the discussion is rounded off with the suggestion that further application of the "valuational" approach offers the best prospect for substantial advances in modal logic.

Part 1

In Symbolic Logic Lewis sets out to show that his connective ' $\dashrightarrow$ ' is superior to the ' $\supset$ ' of the truth-functional propositional calculus ('KC'). The superiority lies in the fact that for a "particular p and q" '$p \dashrightarrow q$' holds if and only if q is deducible from p; that is, Lewis claims, if '$p \supset q$' is a tautology.

It is unclear whether 'p' and 'q' are constants or variables. They are spoken of as "particular" and equated with individual utterances, yet they are, as arguments of a tautologous formula, presumably variables of KC. Further, the aspects of arguments of ' $\dashrightarrow$ ' which determine whether the relationship holds are the particular structures of these arguments and not the particular valuations assignable to simple variables of Lewis's system.

The attempt to specify the conditions under which a formula consisting of two constant expressions bound by ' $\multimap$ ' should be assigned a true utterance as interpretation leads to a bizarre conclusion even when allowance is made for Lewis's personal view of tautologousness and for various readings of his argument. The matter is to be decided, it seems, only by taking utterances such as would be symbolized by the two arguments of ' $\multimap$ ', symbolizing these utterances in some other calculus (presumably KC) and deciding whether the generalization of the resulting formula is a tautology.

Lewis's aims and methods are the root of the troubles. Although he condemns KC as unable to represent deducibility properly, he wishes his system to contain KC. This he hopes to achieve by retaining the existing apparatus of KC, adding a new operator and, inevitably, accepting the lack of any finite general method for determining the value of a formula.

Were this new operator to have had any influence upon what can be proved, then the individual symbols of the resulting system would have ceased to be comparable to KC symbols; for to choose a KC symbol as a means of representing an utterance is to accept that all and only its truth-functional relationships are worth symbolizing, but to choose the other sort of symbol would be to take more or different relationships as important-with the result that a quite different structure might turn out to give the most suitable representation of the utterance concerned.

In KC there is a simple general rule laying down what value is assigned to the formula composed of a material implication sign and two constants of given value. It is our knowledge of such rules which allows us to decide that KC is a suitable calculus for a given application. We are able to see that when we use designated constants to symbolize (all and only) true utterances, the fact that a given material implication is a designated formula has a certain significance.

The lack of such a general rule makes it a step in the dark if we decide to apply the Lewis systems and take designated constants to symbolize the true utterances we are considering. Furthermore it is clear that in order to assure ourselves of a strict implication holding for a given pair of expressions, we must complicate the structure of our symbolizations of them until the corresponding material implication, regarded as a KC formula, instantiates some theorem; or else we must introduce the desired strict implication directly as a designated (constant) formula. Since one is forced to introduce those strict implications which one wishes to employ later, as a deliberate part of the choice of symbolization for the individual utterances to which the system is to be applied, the symbol '$\dashv\!\!\supset$' may be seen as little more than a deviantly manipulated predicate, or '$P \dashv\!\!\supset Q$' may be seen as a single constant whose use is restricted to cases where 'P' and 'Q' occur separately in such a way that '$P \supset Q$' is designated.

One may thus agree with Lewis's comment that "the only case in which any truth-implication is likely to have any value in application, as the basis of inference, is the case in which it coincides with strict implication." The most satisfactory view of his system is as a meta-logic designed to deal with deducibility-relations within KC, and itself behaving as a truth-functional predicate calculus but lacking any distinction between variables of the meta-logic and variables of the object-logic, (that is, the 'p's and 'q's making up "particular" KC formulae dealt with).

A consideration of several writers who have attempted to extend and interpret Lewis systems reveals neither a solution to the problem of the nature of their constants, nor recognition of the philosophical significance of the absence of any finite general truth-table treatment for ' $\dashv$ '. This in no way depreciates the mathematical and logical ingenuity with which models for Lewis systems have been devised, but does leave the question of how the modalities should be symbolized a much more open one than might have been supposed. The question is best approached by considering what is involved in symbolizing a particular subject matter and what one should expect from a successful symbolization.

Part 2

In examining various levels of symbolic apparatus and the criteria for preferring one system to another at each level, we are led

to the conclusion that logical symbolization should always be seen as relative to some "valuation" of the symbolizanda, and that when given the task of constructing a logic for something one should begin by seeking the most interesting and important valuations applicable to it. It is further argued that logical form can only be determined in the light of knowing in which calculus and to which depth it is intended to conduct the symbolization. Given that information the problem becomes "What aspects of this symbolizandum could, by being otherwise than they are, force me to alter my valuation of it?" - where the valuation referred to is that upon which the calculus concerned is based.

The lowest level of symbolization considered is that of signs: these are no more than asystematically selected objects used as ad hoc reminders or representatives of other things. When there is some system in the way the representing objects are chosen, we may speak of a sign-system; the system need be no more than there being some aspect of the represented things by reference to which we can generate (the idea of) the representing things.

Where in a sign-system the representing things are in fact symbols for which an ordering is laid down in other contexts, it is a symbolism. The ordering concerned may or may not be parallel to a discerned ordering in that aspect of the symbolizanda by reference to which we have chosen the signs (symbols); such a parallellism is, however, preferable.

To the signs of a symbolism may be added other, complex, signs. Where this occurs and these complex signs are always equivalent to some simple signs, we talk of a sub-logic. The reducibility of complex signs to simple ones is pointful only when the symbolizanda can also be observed in groups or structures to which the same descriptions can be applied as apply to individual symbolizanda. A sub-logic is most useful where the descriptions satisfied by complexes of symbolizanda are functions of the descriptions satisfied by their members, where satisfaction of such descriptions is the aspect of the symbolizanda on the basis of which the signs can be generated, and where the ordering of the signs parallels that ordering which we find it natural to impose upon satisfaction of the various descriptions.

In a calculus, the highest level of symbolic system considered, the signs are supplemented by variables, and formulae involving variables and, possibly, signs occur as a part of the system. The series of complex signs resulting from the application to a formula of all possible sets of substitutions of signs for the distinct variables it contains, is called the set of "realisations" of that formula.

A calculus is endowed with an assertion sign which is applicable to any formula all of whose realisations are equivalent to some given ('designated') sign or signs. The selection of a designated sign or signs, however, is only pointful when we have symbolized the subject matter with respect to a range of descriptions certain of

which enjoy a special status. Such a range of descriptions results from what we describe as a "valuation" of that subject matter. It is such a valuation which should be sought when setting out to construct a logic for modal utterances.

Part 3

In the final part "necessity" is taken as an example of the type of subject matter we would like to symbolize. Leaving aside other attempts to devise logics of this notion, consideration is given to various cases of "saying that something is necessary" and to the extent and manner in which the utterances involved can be represented in a logical system.

The most common type of utterance of this sort is that by means of which someone, in order to spare himself the need to provide factual evidence for an assertion, or in order to enforce some convention or definition, invokes a necessity. The valuations 'true' and 'false' are inappropriate in this case, but we might well use 'acceptable' and 'unacceptable'.

To decide upon the applicability of such a valuation, however, we must consider the utterance in relation to its particular context; it is by variation in the context not in the utterance itself or in any factual matters that we might be forced to alter our valuation, for the acceptability depends upon whether any such convention, definition or assumption as is invoked does apply in that particular context.

If we were to proceed with symbolization taking this source of variability in the valuation used, as our starting point, then we should have to take contexts as individuals and produce a representation of the internal relations within such a context. If however we take a more traditional approach to logic, we shall prefer a system in which the individual utterance is the basic unit, and one in which the rules are context-independent. One way of achieving this and catering for the symbolization of invocations of necessity, is to add to the axioms present in the symbolization of some context, a set of ad hoc postulates - an 'assumption-set'; an invocation of necessity is then an utterance which purports to be a reiteration of some member of the assumption-set or of some consequence of it.

Contrasted with invocations f necessity are assertions of necessity. These are straightforward - 'true' or 'false' - claims that certain things are accepted as being necessary. They are to be seen as referring to particular groups of persons and particular contexts, subject to the usual practice of dropping such references where it is obvious that everyone or every context is intended.

Confusion between the truth of an assertion of necessity and the acceptability of an invocation of necessity, can account for the feeling that it makes sense to ask someone for his evidence that something is necessary, while it is quite clear that if a given thing is necessary no evidence should have any influence upon our acceptance of it. Again the attraction of the rule that what is necessary is

true, can be traced to the obvious oddity in the behaviour of anyone who, after assenting to a universally-framed assertion of necessity (which implicitly claims that he too accepts the necessity in question in all contexts) displays the falsity of what he has assented to by himself refusing to accept the corresponding invocation of necessity or factual claim.

An analogous approach to natural necessity is possible. The assumptions made in a given context - the theories and laws, in this case - determine which natural necessities are acceptable; these can then be invoked and made the basis for predictions. Assertions of natural necessity deal with the tenets of various theories, and the laws which are assumed to hold in various contexts - or disciplines.

It is not paradoxical that the most empirical type of knowledge is hereby mixed up with 'linguistic' matters and ad hoc conventions. To establish a law we must consider it first in a context in which it is not assumed; thereafter, however, it need not constantly be re-established every time we wish to apply it - it is in practice taken as a premiss which in conjunction with observations of a particular situation allows us to make deductions about that situation. It seems farther that 'lawlike' statements - those from which subjunctive conditionals may be derived - can be explained as being those statements which make up the assumption-set for a given argument.

The present treatment of necessity is not intended to be definitive; it is rather to be seen as a demonstration of the fruitfulness of the approach to symbolization which has been argued for, and as an indication of the direction in which logical study of the modalities might go. Two related topics relevant to such future work are considered in brief and without the presupposition of the results of the rest of Part 3.

The evidential interpretation of the modalities can be seen as an attempt to range the modalities along a scale of "strength" which would parallel that of the strength of evidence. Although it is difficult to carry through this programme for all modalities, it does seem to contain the seeds of some valuation which might yield a system for symbolizing the interrelations amongst the modalities.

Consideration of the various paraphrases of "necessary" in terms of possibility, and in particular the notion of "only possibility" suggests that a symbolization for necessities might emerge as a by-product of a system for enumerating possibilities. "Possibility", however, like "alternative", seems to be a word which is used for categorizing rather than for describing or characterising things; recognition of this removes much of the temptation to think of "possibilities" as concrete things which can be counted, or as attributes which can be spoken of <u>along with</u> the other things discussed in modal utterances.

In conclusion it is suggested that much more work on the modalities is called for, and that the intuitive semantic approach - especially when it employs the 'valuational' method - offers the most promise for worthwhile advances in modal logic.

PART 1

CHAPTER I

INTUITIVE SEMANTIC RULES FOR INTERPRETING LEWIS SYSTEMS*

In Chapter VIII of Symbolic Logic (520P) it is maintained that the "relation of strict implication expresses precisely that relation which holds when valid deduction is possible", (p.247). The argument leading to this conclusion, appearing as it does in the work containing the major exposition of Lewis's systems, may be taken as the locus classicus** for his views on their interpretation and value. It seems to us, however, that the desiderata which he there states, and which he claims his systems meet, cannot be met by anything which provides us with the normal facilities for symbolizing the logical behaviour of everyday utterances.

It is not our intention to argue that axiomatic formulations of the systems contain contradictions such as might make it possible to prove any well-formed formula. Nor do we deny the very real mathematical elegance and interest of the systems considered in the abstract. What is at issue is interpretation, and specifically interpretation in terms of ordinary language utterances; since it

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*We distinguish the type of semantic interpretation conducted here from that of such writers as Kripke and Hintikka by calling my discussions "intuitive semantics" and theirs "formal semantics".

**In adding Appendix III to the work in 1959, Lewis did not see fit either to qualify or to amplify the arguments discussed here; at that time he seems to have felt that the only - or the major - interpretative difficulty lay in the so-called "paradoxes of strict implication".

is dissatisfaction with such interpretations of the classical truth-functional calculus which Lewis says led him to propose his own systems, such doubts may fairly be raised against him in turn.

In this first chapter we shall concentrate on a close analysis of the text of Lewis's sections of Symbolic Logic. The purpose of this is to assure ourselves that the difficulties discussed later are basic to a system conceived as Lewis conceives his, and that he has either not foreseen the problems or has ignored them in view of the apparent intuitive correctness of the postulates chosen for his axiomatic formulation.

To help in systematizing our discussion of Lewis's argument we begin by summarizing the relevant parts of the crucial passage from page 235 to page 247.

Summary of Lewis's Main Argument

1. "There must be a truth about deducibility which is not relative to this variety and difference of systems and their relations".

2. Without denigrating other systems it is necessary to find "the one system which alone is true when pIq is translated "q is deducible from p"" - if such a system does exist.

3. The "I" concerned will have to be a relation which will account for the validity of the inferences which are possible in other systems (e.g. KC)*.

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*We abbreviate our references to the truth-functional calculus of propositions by using the letters "KC"; the calculus concerned is any equivalent to that referred to in Symbolic Logic as the "Two-valued Algebra of propositions" cf. Chapters IV and VII.

4. The desired relation must not hold when its antecedent is true and its consequent is false.

5. Truth-implication relations satisfy the condition above (in 4) but have the additional property of holding whenever both antecedent and consequent are true; this produces absurdity when we use the translation suggested (in 2). Thus although the relation desired must allow for the validity of truth-implication inferences, it cannot actually be a form of truth-implication.

6. The validity of truth-implication inference arises because the inference from p to q "is made on the ground that pIq is a tautology". (p. 240).

7. "A clue to the relation of deducibility may be discovered by attention to the nature of truth-implications when they have the character of tautologies".

8. But three points must be borne in mind when following up this clue:

(a) there exist other types of tautology apart from truth-value ones;

(b) if the laws of logic must be tautologies then "any law of deductive inference .... must be a tautology" and hence we would "require to discover a relation which when it holds represents a tautology" - yet does not always hold;

(c) the really important point is that the relation must hold

when and only when, for particular arguments, it expresses a tautology.

9. No truth-value relation can meet "this requirement" (presumably 8c).

10. In truth-value systems inference is "a special case of the kind of inference to which such a relation can give rise". Even when neither p itself nor pIq is tautologous the inference to q is valid because it is a feature of such systems that (p and pIq) I q be tautologous. This is despite the possibility that p and q are totally independent propositions.

11. Thus despite the "queerness" of the relation of truth-value implication, "it will be found that from the two premises (1) "p is true" and (2) "pIq (read in English) holds," the truth of q can validly be deduced, by the commonly accepted principles of deduction. And yet if one takes pIq to express "q is deducible from p" the relation may not hold at all:".

12. To elicit the desired relation we must be able to distinguish between "pIq is true" and "pIq is a tautology": "When pIq is a tautology, q can be deduced from p".

13. (a) The relation $p \rightarrow q$ meets precisely the condition that it should be true when pIq expresses a tautology.

(b) This is shown more strikingly by the fact that the laws of all truth-implication systems are laws in the strict implication system, and that those with a truth-

implication as a principal connective remain theorems if that connective is replaced by the ' $\dashv$ ' of strict implication.

14. Strict implication allows us to express what happens in the special case when a truth-implication is made the basis for the deduction of theorems in a truth-functional system: the formula 18.53 expresses this case as '$\underline{p} \dashv \underline{q} . \sim \Diamond \sim \underline{p} : \dashv . \sim \Diamond \sim \underline{q}$'.

15. In the light of these facts strict implication has been shown to express precisely the desired relation.

First of all we shall attempt to clarify the logical structure of the argument; it is doubtful whether the desired result could be conclusively established by the type of argument presented, but quite apart from the validity of the argument we shall be interested in finding clues as to how Lewis expects the strict implication systems to be interpreted.

If Lewis is to establish the value of his systems he has to show that (on interpretation) they cater better for certain of our real needs than do other systems. This involves him in discussion of (1) his own systems, (2) alternative systems, (3) interpretations of such systems, and (4) uses which we might have for interpreted systems.

He starts with the uses. It is in fact assumed that we are familiar with these: we are simply referred to the facts about deducibility and - throughout the book - invited to consider the

inadequacy of interpretations of KC formulae involving material implication, as paraphrases for statements that certain deducibilities hold. The argument is that in order to meet our needs deducibility must be symbolized in certain specified ways, that KC and other "truth-value" systems cannot provide such symbolizations, but that the systems of strict implication can and do.

In effect, Lewis's procedure is to outline a desired interpretation - on the basis of the "important" facts about deducibility, and to work backwards from the interpretation to the calculus. But the calculus to which he works in the first place is not one of the specific ones that have been introduced in the book: instead it is an arbitrary idealisation of some calculus. The form of argument demands that it be arbitrarily chosen so that one can then say "Any calculus having the desired interpretation will have these characteristics we have found". The final move is to identify the systems of strict implication with systems having the desired characteristics. In discussing the arguments we shall refer to the systems of strict implication as "SI"* and to the arbitrary ideal calculus as "SID".

The actual argument to establish what the characteristics of the desired interpretation have to be, is equally devious. Even given the structure described, Lewis might straightforwardly have said: "We know the following facts about deducibility, ...; of

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*The divergencies amongst the systems S1 to S5 (and S6-S8, etc.) are not relevant to our present concerns; we therefore follow Lewis in ignoring their differences for the moment.

these, the first three are ones in which we are interested and which we would like to symbolise. Therefore our symbolism must have an interpretation which we are able to describe as displaying all these three features of deducibility". Instead his method is to explicate the notion of "implication relation" from a point at which it covers any relation which will not hold if it has a true antecedent and false consequent, up to the stage where he calls it the implication relation "in the required meaning" (p. 241). The required meaning embodies all those aspects which he considers really important and he proceeds to discuss these briefly on the tacit understanding that the symbolic relation we are looking for will have to have an interpretation which displays these features. It is finally stated (rather than argued) that strict implication has an interpretation of the desired sort, and is therefore to be identified as what we were looking for, (it coincides with SID).

The particular section of the text which is of greatest importance lies between pages 241 and 243; on page 241 is a passage which seems crucial and is worth quoting in full (it corresponds to 8c in the summary).

"Third - and this is the important point, the other two being set down to guard it from misunderstanding - if "p implies q" is to be synonymous with "q is deducible from p"; and if q is deducible from p when and only when "p implies q" expresses a tautology; then "p implies q," in this required meaning, must be true when,

for a particular p and q, it expresses a tautology; and it must be false when, for a particular p and q, it does not express a tautology".

This embodies the final stage of what we have described as an explication of "implication relation". Its significance is that it states truth-conditions for such an implication relation's holding. To obtain a precise statement of the semantic rule for interpreting the ' $\rightarrow$ ' of Lewis's systems we shall have to examine these truth-conditions in detail, for they are those for the idealized implication relation in terms of which Lewis feels his systems can be interpreted. To be precise, when in the ideal system the implication symbol occurs with as its arguments two constants which may be interpreted in terms of the "p" and "q" to which Lewis here refers*, the resulting formula of the ideal system will be designated or not according to whether the truth-conditions for "p implies q" are fulfilled or not, (we assume the ideal system to be complete and consistent). The structure of his argument dictates that Lewis hold that when the constants 'A' and 'B' of his strict implication system are interpreted in terms of such 'p' and 'q' then 'A $\rightarrow$ B' will be designated when and only when the stated truth-conditions for "p implies q" are fulfilled.

It is therefore to be expected that a set of explicit semantic rules for interpreting formulae of SID (and hence of SI) can be evolved

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*Cf. page 10 where Lewis's use of "p" and "q" and his general lack of distinctness about constants and variables, is expanded upon.

on the basis of a consideration of this passage. Even should the reader not feel entirely confident about the detail of our account of the structure of Lewis's argument, it seems incontestable that this passage provides a suitable starting point for the search for an interpretation of the Lewis systems which will show them to have met their author's expectations.

Metalanguage and Semantic Rules

Lewis's inexplicit and apparently variable conventions about the metalinguistic representation of formal and informal object languages and their interpretations leave many openings for misunderstanding, and it will be necessary on several occasions to speculate on his intentions. If this is to be done satisfactorily the conventions employed here must be explicit and their relationship to such of Lewis's conventions as are constant should be stated. It is also worthwhile to expand briefly on the nature of interpretations and semantic rules.

As we have noted above, Lewis's concerns with the inadequacies of KC, with the interpretation of theorems of SI, and with the characteristics of ideal satisfactory symbolic system (SID) demand that he refer to these three systems ('(1)' and '(2)' above) and to their interpretations '(3)' above). He uses an informal metalanguage in which formulae of KC and SI may occur autonymously. He also employs expressions such as; "pRq", "pIq", "(p and pIq) I q", ""p implies q"", ""p is true"", and "p", "q". There is a fair measure

of consistency about the reading of symbolic expressions which in his text occur within double quotes (shown here with two sets of quotation marks as we are quoting Lewis): these are, as is indicated in what we quote from Lewis in paragraph 11 of our summary, to be taken as abbreviations for 'propositions' (Lewis's term*) which may be expanded and "read in English". When used by Lewis within double quotes "p" is a variable which has been instantiated and then interpreted to yield some arbitrary English sentence.

Where "p" and "q" occur as individual symbols in completely informal contexts it does not seem plausible to suppose that they are being used as autonymous representations of symbols of some calculus (were they being so used it would be hard to decide which calculus was intended, for "p" and "q" occur in both KC and SI and the context by no means always indicates which is being discussed). In many such cases, however, these symbols can be understood as stylistic variants of phrases such as "some one proposition" and "some other proposition" followed later by "that first proposition" and "that other proposition". Little more can be said about Lewis's symbols with any certainty; we shall devote considerable space to discussing the exact significance of various uses of "p", "I" and "q" in semi-formal contexts outside double quotes.

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*We ourselves use the term "proposition" in Part 1 to indicate an utterance or other linguistically based unit of meaning; this usage we accept from Lewis and other writers being discussed. In the later Parts where our own views are considered we try to avoid the term (cf. p.307 where our use of the term "utterance" is discussed).

Our own discussion takes place in an informal metalanguage with a semi-formal part whose individual symbols will be the two sets of constants; "a_1", "a_2", ... and "b_1", "b_2", ... and the corresponding variables "a_i", "a_j", "b_m", and "b_n", ... "b_{n+k}". As two-place functional constants we employ "R" "L", "K", and "C"; of these "R" is used for the strict implication operator of SID and "C" for the material implication operator of KC. The individual constants of the "a" series are used for constant expressions of SID and those of the "b" series for constants of KC. At the end of this chapter we shall introduce the n-place functional constants "F_1", "F_2", ... and the corresponding variables "F_s" and "F_t".

A semantic rule is a sentence in the metalanguage. It refers the reader to a formula (or a range of formulae) in some object language, to an interpretation, and to a condition. The method of referring to an interpretation will be, quite simply, to <u>specify in the metalanguage</u> that with which a formula is to be matched, i.e. that which "interprets" the formula. The interpretation will thus be described or otherwise referred to in the metalanguage, but is not itself <u>a part of</u> the metalanguage. It will assist in stressing this point if we adopt the phrase "interpreting realm" to apply to the universe of particular things with which formulae may be matched in some one case of a calculus being interpreted.

The condition which has to be met if a certain matching is to take place is also stated in the metalanguage. In the interpretation of classical truth-functional logic, the things which have to

satisfy certain conditions if a given matching is to be made, are things in the interpreting realm; that realm being in this example one of utterances and the conditions being stated in terms of the truth or falsity of those utterances. It is however conceivable that the condition should be one concerning things not in the interpreting realm and, particularly in relation to SID and SI we shall leave that point open.

It is also important to note that although for most logicians the interpreting realms chosen are linguistic or in some way closely connected with language, this is in no way essential to the notion of an interpretation. Interpretation in terms of (i.e. with interpreting realms drawn from the fields of) mathematical models or electronic circuitry is equally valid interpretation. It is merely different and attracts less or different philosophic attention. Our concern here is from the outset specifically restricted to linguistic interpretations, as was Lewis's when writing and revising his part of Symbolic Logic, but in contrast to the concerns of some more recent students of his systems.

The simplest way of referring to linguistic entities is to quote them if they are sentences or to quote paradigm ways of expressing them if they are things said. It will be obvious that when an interpreting realm consists of sentences or things said by sentences, it is wise to be scrupulous about the use of quotation. Lewis chooses "propositions" as the interpretation for his constants

(p. 123), and he frequently "translates" symbols into quoted expressions. Such a quoted expression must not be confused with the quotation of a metalinguistic remark about some formula (the formula will in Lewis's convention actually appear, autonymously). For example, if a certain formula is matched with the proposition "It is possible that p", then the quotation marks are picking out for our inspection some element of an interpreting realm, whereas if we conclude that the negation of a formula is not provable in some system, we in the metalanguage may say that that formula is possible (possibly provable), but to quote ourselves saying this is a very different thing from referring to a proposition like the above one which we take as being in the interpreting realm. This point seems obvious when the quotation marks are in place and the context clear but to every quoted expression there corresponds an indirect speech formulation, and in such formulations matters become less clear.

Semantic rules stated here will undertake the minimal amount of committment compatible with the reading of the text being adopted. We shall therefore follow Lewis in taking the interpreting realm to be populated with propositions and complexes of propositions only so far as is essential. Where possible we shall leave the specific matching of the constants open and shall seek a semantic rule which matches formulae containing implication symbols with either "a truth of the interpretation" or "a falsehood of the interpretation". This

procedure can be seen to be adequate inasmuch as the passage being considered deals with necessary and sufficient conditions of truth; there is no reference to other properties* which it might be relevant for us to ascribe to "p implies q", and in any case they cannot affect the matching in terms of truth and falsehood, since the conditions that we do have are both necessary and sufficient. The formulation adopted avoids appearing to reify the "Truth" and the "Falsehood" into a pair of individual entities, yet confines the interpretation within the bounds of the true-false dichotomy as Lewis appears to wish.

Finally it should be noted that the semantic rules deal only with the case of constants; they may be extended to deal with variables by the techniques of pp. 73-4 of Church's Introduction to Mathematical Logic (230H).

It will be obvious from the foregoing that the form in which we wish to state semantic rules is:

For any i and j, $R(a_i, a_j)$ is matched with a truth of the interpretation if and only if .....

As a comparison for reference two examples may be given; in the first the metalanguage constants are used to designate constants of KC, and in the second they designate individual constants of a logic for spatial relationships.

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*Such as, for example, "meaning ...." or "being of the form ....".

(i) For any i and j, K (b_m, b_n) is matched with a truth of the interpretation if and only if both b_m and b_n are matched with truths of the interpretation.

(ii) For any i and j, L (a_i, a_j) is matched with a truth of the interpretation if and only if that of the interpretation with which a_i is matched is to the right of that of the interpretation with which a_j is matched.

As may be obvious the two functional constants are intended to designate the truth-functional "&" of KC, and the symbol symbolizing being to the right of in the spatial logic.

Lewis's Notion of Tautology

It seems at first sight easy enough to obtain a semantic rule in this form from the passage quoted. A set of if and only if conditions for truth are stated there and we have already commented on the relationship between these and conditions for matching some formula with a true proposition (i.e. the relationship between the truth-conditions for some assertion about the holding of an implication relation, and the conditions laid down in a semantic rule for matching a formula symbolizing that implication with a truth of the interpretation) and indeed it is possible to derive the following formulation from the passage concerned:

a) For any i and j, R (a_i, a_j) is matched with a truth of the interpretation if and only if the particular p and q which correspond to a_i and a_j are such that ""p implies

q" expresses a tautology".

The first thing to decide is the exact nature of the "correspondence" or relationship between the particular a_i and a_j and what Lewis calls "a particular p and q". The example on p. 242 identifies the "p" and "q" with quotations of particular sentences (standing for "propositions" in Lewis's terms). It would thus seem that Lewis is thinking of an interpreting realm populated with propositions and that the "p" and "q" serve as metalinguistic variables for these; his use of "a particular" serves to select an arbitrary instantiation of the variables, and such instantiations can be equally well expressed by using the metalanguage constants which consist of quotations of the propositions designated.

Continuing on this understanding, one should take ""p implies q"" as being Lewis's metalanguage constant designating a complex proposition of the interpreting realm. Substituting for "p" and "q" the constants suggested on page 242, we find that ""p implies q"" is equally well expressed by the metalanguage constant ""That vinegar tastes sour implies that some men have beards""; or the same thing might be expressed as """Vinegar tastes sour" implies "Some men have beards"""", thus making explicit the internal complexity of the constant expression. (The latter formulation seems to attract difficulties about how the interpreting realm has facilities for referring to elements of itself within other elements of itself; for the moment we shall take the former formulation as standard).

It can be taken that for any "particular p and q" we are in a position, by the use of appropriately subscripted "a_j"s to refer to the constants of SID which take these propositions as their interpretations: accordingly reference to particular "p"s and "q"s may be abandoned in favour of reference to those things with which "a_i"s and "a_j"s are matched in the interpretation. Further, a proposition of the sort quoted in the last paragraph, and referred to by Lewis in such terms as ""p implies q"", will be referred to as "an implication-proposition" or as "an implication" with - in most cases - the addition of an indication of its components. Thus our semantic rule a) may be expanded to read:

a') For any i and j, R (a_i, a_j) is matched with a truth of the interpretation if and only if those things of the interpretation with which a_i and a_j are respectively matched are such that the implication from the former to the latter expresses a tautology.

Having reached this point it is necessary to consider the unusual view which Lewis seems to take of the notion of a tautology. Although there is no divergence between him and others about which formulae of systems with determinate value-matrices are to be reckoned tautologous, he requires a concept of tautology which will apply equally well in the case of systems for which no finite value-matrices are available. No whole articulate doctrine is contained in Symbolic Logic but what there is must be considered in the

discussion of this case.

Lewis gives a brief statement of the main points of his view when, in the Introduction, he says:

> "...logical truth ... is "tautological" - such that any law of logic is equivalent to some statement which exhausts the possibilities; whatever is affirmed in logic is a truth to which no alternative is conceivable". (page 24, Symbolic Logic).

All Lewis's discussions, like this one, seem to consider the notion of tautology in the context of an interpreted system. This despite the fact that the notion is very basic to his whole conception of logic, since he holds that any logic is based upon a principle of division, and that the essence of a tautology is to exhaust the alternatives presented by a principle of division. Nowhere does he talk of formulae in pure uninterpreted systems as being tautologies, and he may well not think that such formulae are so in the strict sense.

At at least one point he seems to make the notion of tautology relative to that of interpretation:

> "... if there be one designated truth-value equated with assertability, the laws of any truth-value system will be truth-value tautologies for any valid interpretation of the system". (p. 232, op cit.).

Yet an earlier passage suggests that it is the necessary truth of

the theorems of logic, rather than their tautologousness, which is to be seen as depending upon the interpretation being adopted:

> "... the tautological laws of any truth-value system are necessarily true; but (that) the symbolic system itself does not tell us what it is true of. It is true for whatever interpretation of its truth-values will make them exhaustive of the possibilities, ..." (p. 222, op cit.).

Further passages could be cited but without, we feel, giving clear grounds for deciding in favour of one reading rather than the other. In such circumstances the best guide one can take is that of Lewis's actual practice. Thus it is necessary to examine those passages where he addresses himself to the question of whether a specific expression is or expresses a tautology. In Chapter VIII itself there are two parts to consider: first, on pages 242-3 the question of whether an expression giving the form of truth-value inference is a tautology is considered both in general and in the case of the KC formula said to correspond to the general expression; again on pages 249-250 Lewis gives some examples of cases where theorems of SI coincide with the result of prefixing '$\sim\Diamond\sim$' to formulae which he says express tautologies.

Pages 242-3 are the more directly relevant in that the main object there is to show that a given expression is in fact tautologous. The expression concerned is given first as "(p and p I q) I q", and then as the KC-formula '$p.p \supset q: \supset .q$', which is said to

be a specific case of the same expression.

There seems to be room for doubt over the nature of the first expression. Our view is that it has to be read as an abbreviation for some English-language utterance, and that that utterance is best understood as being the utterance of some proposition from the interpreting realm for the calculus whose implication-relation we are considering. In support of this is the fact that the footnote talks of this expression as being "expressed symbolically" in different ways according to the system chosen; we take it that one finds out which symbolic form is appropriate by reading the whole expression out in full as an English utterance and then deciding how that utterance can best be expressed in the chosen calculus. The formula of KC which is discussed can thus be seen as the result of applying just this process. Further support comes from the fact that Lewis explicitly proposes on page 242 that as an example we should try making "p" and "q" equal to certain "propositions" expressed as English utterances; that is, we may simply substitute these expressions in the place of the "p" and "q" as outlined on page 16 above*.

The structure of the argument on page 242 can be shown by quoting selected parts of it:

".. '(p and pIq) I q' will be a tautology. This is the case because:

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*Cf. also page 65, and the comments on paragraph 11 of the summary on pages 2-3.

(1)

(2)

That is ..... "....." is a tautology".

This argument, though central, is far from straightforward and we shall discuss it at some length. Accordingly it is worth setting out a paraphrase which gives most of the points which seem to be involved. The text and presentation have been freely adapted and we use our own metalinguistic apparatus, but keep to Lewis's symbols. The argument must run something as follows:

Given: Let "p" and "q" be interpretations assigned to formulae of some truth-value system T; "pIq" is the interpretation assigned to a well-formed formula involving some "implication" symbol and the formulae whose interpretations are "p" and "q" (which it takes as its respective arguments). "pIq" is thus the implication-proposition from "p" to "q", and we refer to the implication symbol of T as that symbol corresponding to "I".

Assumption: Assume that the interpretation assigns true utterances to all and only assertible formulae of T.

Proof: (i) If an inference in T is to be based on the formal relation the symbol for which corresponds to "I", then there must be in T two premisses which are both assertible, and whose interpretations will be "p" and "pIq"

(for some completely arbitrary "p" and "q"). Since "p" and "pIq" interpret assertible formulae they are, by the assumption, true utterances.

(ii) Since the symbol corresponding to "I" is a truth-implication symbol it is impossible that the formula interpreted by "pIq" should be assertible if its first argument were assertible and its second argument were not; but "pIq" is true and interprets an assertible formula, hence the formulae interpreted by "p" and "q" are not respectively assertible and not-assertible; hence by the assumption "p" and "q" are not true and untrue respectively.

(iii) From the premisses ""p" is true" and "It is false that both "p" is true and "q" is not true" one may validly deduce ""q" is true".

(iv)a. "That is (....) "It is false that (p is true, and it is not the case that p is true and q false, but q is not true)" is a tautology".*

(v) Hence "(p and pIq) I q" is a tautology. Q.E.D.

At the outset this merits two comments as to its validity. First, it is not shown that "(v)" follows from "(iv)a". One might well doubt whether it does. On our reading "(p and pIq) I q" is an

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*This is the final sentence of Lewis's argument. His conventions for the use of "p" and "q" have been left unaltered.

abbreviation for some element of the interpreting realm, whereas the sentence quoted in "(iv)a" is a sentence of the metalanguage which is concerned with the truth or falsity of elements of the interpreting realm. Without some additional argument we should be no more justified in making the inference from "(iv)a" to "(v)" than we should be in making the inference from "It is a tautology that "p" is either in English or not in English" to ""p" is a tautology".

Secondly this argument appears to rely upon an understood relationship between deducibility and tautologousness, when in fact that is what the argument is meant to establish. This second point does not depend, like the first, upon the interpretation of Lewis's symbols we adopt. In the actual text the part marked "(2)" states that a certain conclusion is deducible from certain premisses; the argument then proceeds by saying "That is ...." .." is a tautology". Such a step can only be made if we are given some rule relating tautologousness to deducibility so that we can argue from the latter to the former. Yet the whole purpose of the argument is to show that a deducibility is based upon a corresponding tautologousness; to use such a rule in this context is to assume what it is desired to prove, and we might just as well argue directly from the validity of actual truth-value inferences to the tautologousness of "(p and pIq) I q".

Leaving these comments aside it is at least clear that this argument is relevant to the problem we face with the semantic rule.

This argument is designed to show that a certain expression in an interpreting realm is tautologous, and that is precisely what we desire to establish in the case of the applications of the semantic rule.

This argument purports to establish that the expression of the interpreting realm is tautologous, by first establishing that a metalanguage statement about the truth or falsity of the components of the interpreting realm expression is a tautology in the metalanguage, and then working from that fact back to the tautologousness of the interpreting realm element itself. If arguments can be found to justify the second step of this procedure we shall be somewhat farther forward. However as a preliminary, and to complete the investigation of Lewis's practice in detecting tautologies, let us consider the other passages in the Chapter, and see that they do not conflict with the present one.

The discussion on page 243 concerning the KC formula '$\underline{p}.\underline{p} \supset \underline{q}: \supset .\underline{q}$' is allegedly parallel to that on page 242. In fact it seems very different, and much more closely related to the section on pages 249-50. The discussion proceeds by applying to the formula a series of transformations each consisting either of definitionally based rewriting or of manipulation according to the rules of KC. It is thereby shown that the formula is equivalent in KC to the formula '$\sim (p \sim q. \sim (p \sim q))$'. Lewis then concludes immediately "Thus '$\underline{p}.\underline{p} \supset \underline{q}: \supset .\underline{q}$' is equivalent to "It is false that $\underline{p} \sim \underline{q}$ is both true

and false"; and that statement is an obvious tautology".

Such procedure seems to be validated, if at all, by some principle such as "if a formula of a system S is equivalent by the rules of S to another formula whose interpretation is an obvious tautology, then the original formula is a tautology". There seem to be certain rather obvious dangers in such a principle; first there is the possibility that in the system certain things to which we wish to give distinct interpretations, are treated equivalently* and hence through a series of transformations one might easily obtain the equivalence of a formula whose interpretation was a well-established necessary truth with a formula whose interpretation was by no means obviously a necessary truth at all; such a circumstance would certainly count against the system concerned being suitable for the symbolization of such necessary truths, but there is no hint in the use made of the principle that Lewis recognises any need to verify that the interpretation concerned is not so defective that a formula matched with some tautology in the interpreting realm is equivalent to formulae with non-tautologous interpretations.

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*A prima facie means of escape from this objection might be sought by insisting that an interpretation which distinguishes between formulae which are treated as equivalent within the system (whether by definition or through the application of rules and axioms) is not a valid interpretation. Such a route, however, is not open to Lewis because it would involve him condemning his own treatment of KC; he is quite content with the interpretation of "$\sim(p \sim q)$" and the burden of his complaint against KC is that the intended interpretation of "$p \supset q$" is not synonymous with this although the two formulae are treated as identical. Were he to apply the condition that equivalent formulae be given synonymous interpretations, then his only point would be that the interpretation of "$\supset$" in terms of implication is not possible unless one is prepared also to interpret "$\sim(p \sim q)$" in terms of implication.

The second objection is the very obvious one that systems are susceptible of a multiplicity of interpretations, and the notion of tautology is normally applied only in the case of linguistic contexts.* It is difficult to see how the application of the notion can satisfactorily be extended to all possible interpretations; the temptation would be to stand Lewis's approach on its head and say that a tautology was to be defined as anything in a valid interpretation which interpreted a theorem of a consistent calculus. Possibly one could use the "principles of division" here and say that anything which exhausts the possibilities admitted by the appropriate principle of division is a tautology. However it seems likely that this approach will collapse into the traditional view, because Lewis defines his systems as being based on the various principles of division. It would seem that a logic based upon a certain principle of division must have a range of constants which on the standard interpretation are matched with the complete range of possibilities admitted by the principle concerned, and this in turn is scarcely distinguishable from saying that the possibilities admitted by the principle of division behind a system are just those corresponding to the values which may be assigned to variables of the system. It then only requires that "exhaustion" be defined in terms of a formula's taking a designated value on all possible assignments to its variables, for the account to coincide with the normal one.

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*Apart, of course, from symbolic ones.

On pages 249 and 250 there are discussions of three examples which are said to show the way in which every theorem of SI states the tautologousness of some expression. In two cases Lewis cites alleged tautologies and then produces theorems of SI which 'express' the tautologousness of the formulae concerned; in the final example theorem 12.61 is shown to be of the form '$\underline{p} \vee \sim \underline{p}$' preceded by '$\sim \Diamond \sim$'. These examples do not purport to argue that given expressions are tautologies - they take it as having been earlier established that "Every tautology is expressible as some proposition of the general form $\underline{p} \vee \sim \underline{p}$.".* What is, however, of interest here is the recurrence of the two features which were so crucial on page 243 - viz. a series of transformations within the calculus from which an expression is taken, and the heavy reliance on interpretations when explaining what the force of the derived equivalent is. In these examples there seems to be no attempt whatever to discriminate between formulae themselves and their interpretations - either in respect of tautologousness or in respect of meaning.

The claim that indications have been given earlier to suggest that any tautology can be expressed in the form '$\underline{p} \vee \sim \underline{p}$' seems to refer to pages 209-210. In that passage a disjunctive form is produced and it is stated that each of the disjuncts represents an

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*This is a classic case where one would like to know from which calculus a formula is drawn, but Lewis does not say. Probably he only means to say that any tautology can be represented as the disjunction of some affirmation and the negation of that affirmation; if so he should refer to saying things, and the things said, instead of to a symbolic calculus whose only relation to things said arises through its interpretation, no hint of which we are given.

assignment of values to the variables occurring in it, while the whole disjunction states that at least one of the alternative sets of truth assignments must be made. The process used to obtain the disjuncts (each of which is a conjunction) is not explicitly stated, but appears to be the formation of a conjunction consisting of each variable which is assigned truth and the negation of each variable which is not assigned truth. This of course relates only to KC*, but one could imagine its being extended to systems with greater numbers of truth-values while retaining its independence of interpretation. However when it comes to SI for which there is no finite characteristic matrix we seem to meet a problem. How are we to know that 'p v ~p' is the universal function for SI and states the truth assignments that may be made? It would seem that our only guarantee of this is reference to the linguistic interpretation and argument back from the fact that this interpretation allows only for truth or falsity as values.** This is perhaps additional circumstantial evidence for the involvement of Lewis's notion of tautology

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*It seems that this method gives the same results as the "conventional" method of tautology testing by consideration of the value of an expression taking into account all possible assignments of values to its variables; at least that is reasonably sure in the case of KC, but being unable to see any reason why it must be the case in other calculi, particularly ones for which finite matrices are not available, we are forced to take the method at face value when discussing the testing of symbolisations in calculi other than KC.

**H. S. Leonard's truth-tables for modal functions are discussed below (pages 145-159). Their existence does not seem to affect this point since they make essential use of the notion of "a null class of interpretations" and, as argued, this notion cannot be explained in purely formal terms.

It will be argued below (pages 104ff.) that only these two values are appropriate for the interpretations of the constants of Lewis's SI: the present discussion which foreshadows this conclusion is not dependent upon it.

with his intended interpretation of the system concerned; there is no clue in the earlier passage of a method for detecting tautologies which would apply universally to formulae and which might supplement that used on page 243.

Having dealt briefly with these cases where formulae rather than linguistic expressions are under discussion, it is time to sum up what conclusions may be taken from the passages, and then to go back to the case of "(p and pIq) I q". When we are presented with formulae of a given calculus and asked to test whether they are tautologous it is clear that Lewis would have us attempt to discover a set of transformations within that calculus by which we could discover another formula, equivalent in the calculus to the first, which is of the form '~(p.~p)' or the form 'p v ~p'. If there is no such equivalent formula it would seem that the original formula is not a tautology. There is no explicit rule for recognising the analogues in other calculi of the symbols given here as "~", " . ", " v ", but it is probable that they would be those used to symbolize negation, conjunction and non-exclusive disjunction respectively and that in the case of unfamiliar notations we have to refer to standard linguistic interpretations of the calculus and to take as the analogues of the forms quoted above whatever formulae are interpreted respectively as the denial of an explicit contradiction, and as the assertion of the disjunction of an exhaustive set of alternatives.

An Emendation of Lewis's Argument

Returning now to the discussion of the argument on page 242 it will be recalled that the most serious allegation made against the argument as it stood was that it seemed to commit a simple petitio principii by basing its conclusion on a general principle which would have been enough to render the whole argument unnecessary had it been given at the outset. To avoid such a fault it would seem necessary to drop completely the part of the argument marked "(2)" (given as "iii)" in the paraphrase). However this would clearly do great violence to Lewis's intentions, and so any alternative emendation should be preferred if it could avoid our striking out a numbered step of Lewis's argument.

This alternative emendation consists in making the substitution in the paraphrased argument of the following in place of "(iv)a":

(iv)b. Hence by the assumption the formula which is interpreted by "q" is an assertible formula; thus the interpretation "(p and pIq) I q" interprets an assertible formula (viz. a truth-implication of which both the antecedent and the consequent have been shown to be assertible). So by the assumption one may conclude that "(p and pIq) I q" is itself a true expression. This conclusion is reached despite the complete arbitrariness of "p" and "q", hence for any truth-implication whatever of this form and for "p" and "q", "(p and pIq) I q" is true.

When one inserts this step it does seem that something special has been shown about the truth of "(p and pIq) I q", and one may fairly take it that Lewis had something of this sort in mind when designating it as a tautology.

As amended in this way the argument no longer depends upon any general principle allowing us to base tautologousness on the existence of a deducibility between components of the expression being tested. Instead, proceeding on the basis that the antecedent is true and considering the expression in relation to some calculus and the metalanguage for that calculus, it is argued that because the calculus is complete the symbolization in it of the antecedent is assertible; from this and the nature of the calculus there follows the assertibility of the formula symbolizing the consequent, and hence via the nature of the implication-relation in the calculus there follows the assertibility of the formula which has as its interpretation the expression being considered. Since the interpretation is valid, the expression is thus shown to be true, given only that its antecedent is true.

The sense in which this shows the tautologousness of the expression is a little mysterious, but if one takes it that an expression is tautologous if its truth can be guaranteed without reference to its content, then the expression considered is in that sense tautologous - but only on the assumption that its antecedent is true.

The criterion of tautologousness which Lewis seems to use here can thus be stated as being that an expression in the form of an implication is a tautology if its truth can be deduced merely from the truth of its antecedent. It will be necessary to discuss what exactly this criterion amounts to, but there is one immediate difficulty in applying it in the present circumstances which should be dealt with before any more time is spent on exploring the criterion.

When the semantic rule is rewritten to incorporate this criterion for tautologousness it will refer us, as the condition for a formula's being matched with a truth of the interpretation, to the question of whether the implication-proposition is such that its truth can be deduced solely from the truth of its antecedent. The difficulty which immediately springs to mind is that such a condition is out of place in the statement of a semantic rule which will interpret formulae using a symbol which we intend to use to symbolize deducibility. It would for instance be a strange sort of semantic rule in KC for material equivalence, if it said that the material equivalence formula should be matched with a truth if and only if the interpretations of the arguments it took were materially equivalent. The oddness of such rules can be explored in more detail.

Abbreviative and Reductive Semantic Rules

Normally the semantic rules used in interpreting calculi tend in one or other of two directions; on the one hand are the rules for

the basic symbols - the primitives of the system, other than the logical primitives or 'operators'; and on the other are the rules for interpreting the operators and other symbols which are used to form functions in the calculus (excluding those which form functions of higher type than their arguments).*

The difference between these types of rule can be brought out by considering specific examples from KC as it is usually interpreted. The rules for interpreting propositional constants match them, let us say, with one or other of the two truth-values "true" and "false", and in a system which contains particular designated constants the semantic rule must lay down explicitly that they be matched with the value "true". Now whether we say that the constants "symbolize truth or falsity" or that they "symbolize propositions with respect to truth or falsity" it is clear that the rule merely lays down the precise inverse of the symbolization function which we wish the symbol to serve.

The case is otherwise when it comes to the rules for interpreting formulae which involve logical constants, i.e. operators. We may consider the case of the rule for K, which is stated on page 15 above. Knowing the purposes which the calculus is to serve we would say that K was the conjunction-relation or that it symbolized

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*This is not the place to involve oneself in type theory, but the predicate symbols of first-order calculus may be taken as being ruled out by this restriction, as may the class-inclusion symbol. The former are basic symbols and to be included in the first category, while the latter are of uncertain position relative to this dichotomy (which is by no means intended as an absolute one).

conjunction. Yet it does not do so in the same way that the constants symbolize truth-values. The expression which is matched with the formula involving K, is nowhere stated by the semantic rule to be a conjunction. Part of the reason for this is that KC is only a truth-functional calculus and a semantic rule which undertakes the minimum of commitment will say no more than that the interpretation of an assertible formula is "a truth of the interpretation" or "has the truth-value 'true'". But the more important aspect of the matter is that the rule does not explicitly refer to conjunction at all even when describing how one is to decide which matching to make. The decision on how the formula is to be matched is made on the basis of the previous decisions about the matching of the components of the formula - it is made, one might say, as "a function of the interpretations of 'a_i' and 'a_j'.".

Things could have been otherwise. Even while "a_i" and "a_j" appear in the system and are interpreted in the normal way, a rule for K (a_i, a_j) in the case of particular "i" and "j", could be given which had the effect of making K more like the basic constants "a_i" and "a_j", and which by referring explicitly to the conjunction of the interpretations of "a_i" and "a_j" made the interpretation of K (a_i, a_j) a function of this conjunction rather than of the elements in it. Such a rule, stated for the conjunction symbol "K'", might be: For any i and j, K'(a_i, a_j) is matched with a truth of the interpretation if and only if the conjunction of that with which a_i

is matched and that with which a_j is matched is a truth.

If one considers the application of this rule, it is clear that the decision on what interpretation is given to $K'(a_i, a_j)$ is not a function of the interpretations given to a_i and a_j, in quite the same way as is the decision how to interpret $K(a_i, a_j)$. To be specific, if we are interpreting $K(a_i, a_j)$ we have to know only whether the interpretations of a_i and a_j are truths and to know what it is for two things "both" to be the case; whereas in interpreting $K'(a_i, a_j)$ we need to know (i) what the interpretations of a_i and a_j are, (ii) how to form/recognise a conjunction, and (iii) what the truth or falsity of the conjunction in question is. Of these three things which one needs to know when interpreting K' it is the second and third which are important.

Even if we do know how to construct or to recognise a conjunction, we are presented with the problem of assessing its truth separately from the assessments we have given of the truth of its components; thus it is in principle possible that we should decide to say the conjunction of two sentences was false even though we had previously decided that the two sentences were themselves each true.* This decision would not in itself cause any inconsistency to arise in our system.** This we might sum up by saying that the question of

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*This would indeed be quite reasonable in cases where we are counting the use of "and" as a case of "conjunction" even though it is used to signify the serial occurrence of the matters mentioned in the conjuncts.

**Although if we were to have axioms which laid down the various normal properties of conjunction (say, detachability of components and constructibility out of independently assertible formulae) then a contradiction could well result.

the truth of the conjunction is no longer settled by the answers to the questions about the truth of its components; or again saying that the question of the truth of the conjunction is no longer "reduced" to that of the truth of its components.* One may well feel that proceeding in this way deprives the formal operator K' of its title to being called a conjunction operator; it remains none the less true that it is an operator which is interpreted by reference to conjunctions.

The reason for drawing attention to the fact that this type of rule requires us to construct or at least be able to recognise conjunctions, is that this fact taken in company with our hopes of using the system to symbolize the very conjunctions which we are referred to by the semantic rules, produces an odd triviality about the system - at least so far as it symbolizes conjunctions. Take any linguistic context which one might desire to symbolize in this calculus; as soon as one comes across an expression which is recognised as a conjunction, one will make the attempt to symbolize this by using the conjunctive symbolism of the calculus - i.e. by using K'. One finds however that the symbolization employing K'

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*K' is in fact introduced just as a predicate symbol would be introduced, and indeed we can see no practical difference between predicate constants taking propositional arguments and pseudo-operators like K'. Predicate constants are basic symbols for which the rules of interpretation are abbreviative.

is interpreted merely by reference to* this very same expression which is here to be symbolized. So far as concerns those aspects of the discourse in terms of which the calculus symbolizes it, there can be no difference between what we say of the conjunction which we hope to symbolize and of the interpretation of the symbolism by which we perform the symbolization. It is to be noted that in this case there cannot be any difference, whereas in the case of a symbolization using ordinary KC it is among the metatheorems of the system that there would not be any difference. The metatheorem concerned for KC is that which establishes its completeness with respect to the symbolization of two values - which we understand as

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*It is important not to confuse interpretation conducted by reference to an expression, as in this case, with interpretation in terms of that expression. In the latter case the expression is that of the interpreting realm with which the set of symbols is matched, and anything which may be said of it must ipso facto be equally predicable of anything which that set of symbols is used to symbolize (given that one takes as interpreting realm for the system just the same universe as one has used the system to symbolize). On the other hand it does not follow that anything which is predicable of a sentence symbolized by formula, F, can be said equally well of the interpreting realm element which is specified through the semantic rule for F (i.e. in terms of which F is interpreted). For instance one may use KC to symbolize meaningful propositions, but it would be out of place to predicate meanings of the interpreting realm elements for the constants of KC, since these elements have such familiar characteristics as being indistinguishable in case they are of the same truth-value (or are the same truth-value). One may say that the set of symbols in the calculus symbolizes with respect to those aspects of that symbolized which can be predicated of the elements of the interpreting realm; that which is symbolized may naturally enough have other aspects as well, though.

being truth and falsity values applicable to propositions. It is among the objects of our seeking such completeness proofs to assure ourselves that the reductions embodied in the calculus concerned do not result in the matching of a formula which, being designated, might have been used to symbolize a true proposition with an element of the interpreting realm which we should say was false.

Rules such as that for K', which in this way make it follow trivially that the symbolism has an interpretation with the exact characteristics we are symbolizing when we make use of the logic on that interpreting realm as subject matter, can fairly be called "abbreviative". By calling them this we bring out the fact that the symbolism concerned is not much more than a means of abbreviating that which is symbolized (or more precisely the aspect symbolized, and in the case of non-linguistic subject matter abbreviating the description of the aspects symbolized). The term is appropriate too in relation to the normal use of such rules in the interpretation of basic constants of a system, for insofar as each constant is given a specified matching its actual use in symbolizing some subject matter merely abbreviates the occurrence in the subject matter of some thing which has just the characteristics of that with which the matching is laid down. In Church's system, for example, the constant "f" is matched with the truth-value "f", and it is clear that to symbolize a proposition by using "f" is nothing but to abbreviate our assessment of its truth-value as being "false".

From the point of view of the Applied Logician then, there are two directions in which semantic rules may tend; either toward being reductive or toward being abbreviative*.

It seems unlikely that Lewis would have wanted it to be taken as abbreviative; the material implication connective C which he thought R would supersede had at least the virtue of being reductively interpreted. Further, it has been noted that the use of abbreviative rules results in a trivialisation of the application of the calculus in specific contexts. Lewis shows on page 122 that he is well-enough aware of this point, and yet he seems to have thought that SI would be illuminating with respect to the deducibility relation in a way which he himself shows that KC is not illuminating towards the truth of propositions such as those which set out valid

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*We would expect that any calculus worthy of the name to have both types of rule, since one lacking reductive rules would amount to little more than an elaborate set of abbreviations plus the rule that whatever was a theorem was matched with truth; however if all symbols were reductively interpreted then no such things as basic constants would exist in the system.

The distinction between logical and non-logical constants might be stated in terms of the fact that all logical constants have reductive semantic rules - even including "$\sim$" in KC. Such a means of making the distinction would not of course be purely formal. Its value is probably confined to the consideration of systems which have simple formulae of the same length as complex ones.

syllogisms. The result of a semantic rule for R which turned out to be abbreviative, would be to turn R into little more than a primitive predicate constant in a calculus of first order with the propositional symbols as individual symbols.

The account so far given of Lewis's method for the testing of informal expressions for tautologousness does not make the semantic rule unambiguously abbreviative. It can be stated at this stage as:

> For all i and j, R (a_i, a_j) is matched with a truth of the interpretation if and only if those things of the interpretation with which a_i and a_j are respectively matched are such that that truth of the implication proposition from the former to the latter can be deduced solely from the premiss that its antecedent is true.

A more explicitly abbreviative rule for R, might, for comparison, be:

> For all i and j, R (a_i, a_j) is matched with a truth of the interpretation if and only if that of the interpretation with which a_i is matched and that with which a_j is matched are such that the implication proposition from the former to the latter expresses a true deducibility.

Yet even if the presumed semantic rule is not in the most abbreviative form possible, it does seem to come very close to that form. What, after all, is the content of the implication proposition, if not that the interpretation of a_j is deducible from the

interpretation of a_i? It is just this proposition which we are required to prove to be true (under certain restrictions).

In addition to this the situation envisaged - i.e. its being possible to deduce the truth of the implication-proposition given only the truth of its antecedent - is rather a strange one. If an implication proposition is genuinely of this sort, it would appear to be such that no matter what the consequent of it may be, that consequence may be truly said to be deducible from the antecedent if that is true; yet what sort of antecedent can there be of which we might say that any consequence whatever can be deduced from it if only it is true? This condition sounds very much like that which lays down what it is to be self-contradictory - viz. being such that if true any proposition whatever might be inferred.

One may see the exact relation between the deducibility Lewis seems to need as his criterion for tautologousness, and the deducibility of the consequent from the antecedent, by considering what happens in cases where a deducibility of the former sort can be detected - given that one has only the amount of information presently embodied in the semantic rule, about the structure of the implication-proposition.

Suppose that we have some weak implication-relation to deal with; perhaps the informal analogue of material implication, such that the implication-proposition is false only if the antecedent is true and the consequent false. There are only two cases in which the

implication holds; (i) where the antecedent is false, or (ii) where the consequent is true. Of these the first is ruled out by the assumption in the testing procedure that the antecedent is true. Hence the whole implication - proposition can be true only if the consequent is true, and thus if we can deduce the truth of the whole proposition from the truth of the antecedent we can equally deduce the truth of the consequent from that of the antecedent. Not only is the deducibility of the truth of the whole proposition from that of its antecedent a sufficient condition of the deducibility of the truth of the consequent from that of the antecedent, but it is also a necessary condition, for in the case of material implication the truth of both the antecedent and the consequent is sufficient for the truth of the whole proposition, and in the present case the truth of the antecedent is assumed and that of the consequent is now presumed deducible.

Suppose on the other hand that the implication-proposition expresses a deducibility relation in some much stronger sense rather like Lewis's own. That is to say it will be true only if the consequent is 'genuinely' deducible from the antecedent. In such circumstances, given the truth of the antecedent, and asserting the truth of the whole (which is deducible from the antecedent, by supposition) one is in a position validly to deduce the truth of the consequent. Furthermore when it is shown that from the truth of the antecedent the truth of the consequent may validly be deduced, it seems to follow that the proposition asserting that the consequent may be

deduced from the antecedent has been established to be true.

To the extent that these considerations are compelling Lewis's criterion seems to reduce to one demanding that the truth of the consequent be deducible from the truth of the antecedent*. Is there, then, any difference between the condition that B is deducible from A, and the condition that the truth of B is deducible from the truth of A?

For the present purposes it does not seem to matter. When we talk about the propositional calculus, it does not matter very much whether we say that the constants symbolize truth-values, or that they symbolize propositions in respect of their truth values; in any case we know very well that no other aspects of propositions are treated but their truth or falsity. It is similar in the present case if we assume that the semantic rule is going to match R (a_i, a_j) with a truth just in case the truth of the interpretation of a_j is deducible from that of the interpretation of a_i. For, when we refer to this relation as the "truth-deducibility" of "a_j"s interpretation from "a_i"s, it is, despite the appearance that we are "reducing"

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*One might suggest the drawing of a distinction between "validly deducible" and "inferable", but Lewis himself makes no such distinction in this passage. (cf part(iii) of the paraphrase). The distinction would, we imagine, be such as to define inference as the process which utilises the existence of a valid deducibility, and would so enjoin us to say that when the truth of B is inferable from the truth of A, thenB is validly deducible from A in the sense that "A implies B" states a valid deducibility. Such a distinction as applied to Lewis's criterion for tautologousness would result in a simple proof that the semantic rule was wholly abbreviative and in fact coincided with that given on page 40 as the most explicitly abbreviative form of rule for R. In that circumstance it seems best to adopt the alternative reading.

deducibility to truth-deducibility, an insignificant move of the same sort as the apparent reduction of propositions to their truth-values, when KC symbolisation is employed. Instead of saying that the rule as at present contemplated reduced deducibility to truth-deducibility, or that it interpreted a relation which symbolized deducibility in respect of truth-deducibility, one might equally say that it interpreted a relation which symbolized truth-deducibility. And in terms of the symbolization of truth-deducibility the semantic rule would be outright abbreviative.

So long as there is no alternative symbolization in SID, in terms of which truth-deducibility can be symbolized, there is no relevant reduction. The reductiveness of K (a_i, a_j) is guaranteed by the fact that the conjuncts of the conjunction which it symbolizes can be themselves symbolized <u>in all respects relevant</u> to the value of K (a_i, a_j) by the separate syntactically well-formed constants a_i and a_j. Similarly in Church's system the semantic rule for '$\supset$' is a reductive one because (a) there is available in the system the possibility of writing other expressions which, for any complex involving '$\supset$' will completely determine the value of that complex. (In the case of '$A \supset B$' these expressions are such ones as '$\vdash B$' or '$\vdash \sim A$' or both '$\vdash A$' and '$\vdash B$'), (b) in any case an appropriate set of such expressions can be interpreted by the semantic rules in such a way as not to explicitly or implicitly refer to that which the '$\supset$' symbol is taken to symbolise, (i.e. the (informal) material

implication relation). The case of the reduction of deducibility to truth-deducibility is not like this because there is neither an alternative set of formulae in SID whose values would determine that of R (a_i, a_j) nor is there a way to state the interpretation without reference to that which might be taken to be being symbolized. So far as any reduction is involved it must be of the same sort as that involved in the use of truth-values to represent propositions, rather than that involved in the use of K to symbolize conjunction*.

However so far in the criticism of this view of the criterion for tautologousness, we have been ignoring an explicit aspect of Lewis's criterion, which does have a substantial part to play. This aspect is the structure of the expression which is being tested for tautologousness. There are in fact two related questions which should arise from the criterion as it had been expounded by page 32 above. First, one should ask what metalanguage one is to use and what metalanguage one is working with, when one shows that the truth of the implication-proposition (or of its consequent) is deducible

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*In explaining the reductiveness of the rule for K it may have appeared that the interpretations of the arguments had to be linguistic; this is not in fact so. The distinction between a reductive and an abbreviative symbolization for "conjunction" can be made merely in terms of talk about truth-values, so long as the notion of a conjunction of a pair of truth-values is understood. In those circumstances the characteristic of the abbreviative rule would be that it demanded our assessing the value of the relevant conjunction of truth values, whereas the reductive rule demands only that we discover the values of the two separately and evaluate for ourselves the algebraic product of them.

from the truth of its antecedent. And secondly, one should ask what general method we are to employ in order to arrive at a characterisation of the structure of our informal expression, such as the structure which is materially used in the argument we paraphrased.

Logical Structures in the Metalanguage

The relatedness of the two questions can be seen in the way Lewis actually conducts his argument; he is working with a metalanguage whose object language is some (arbitrary) truth-value calculus, and also with a meta-metalanguage in which he shows that certain statements of the metalanguage are deducible from others (the truth-ascriptions made to the interpretations assigned to formulae of the calculus). The informal expression he is assessing is given in the form "(p and pIq) Iq" which in fact illustrates the structure of the truth-value symbolization which is contemplated for it. Thus in the concrete example we have, the structure which is ascribed to the informal expression is that reflecting its symbolization in a truth-value calculus, and the metalanguage in which the deducibility of the truth of the implication proposition from the truth of the antecedent, is to be shown to hold, is in fact the semantic metalanguage for that truth-value system.

The problem which one faces in applying the method to the context of the semantic rule is that there is no given structure for the implication proposition which has to be assessed. In view of

this one must seek some way of determining the structure, and the question then arises of what system one should base the structure on.* The system chosen will be the object language of the metalanguage in which the truth-deducibility is to be shown to exist, and that truth-deducibility will be between elements of a realm which, according to the metalanguage, interprets the system chosen. The situation may be depicted as follows:

meta meta language
(example)

the truth of pIq is deducible from that of p

informal metalanguage (example)	formal metalanguage (example)
If it is not the case that 'p' is true and 'pIq' is false, then 'A ⇁ B' is matched with a truth	(T'p' & F'pIq') M 'A ⇁ B'

Lewis's reference	symbols	interpretation (containing truths and falsehoods)
(p & pIq)Iq	p.p ⊃ q: ⊃ q A ⇁ B	

*See note on page 48.

*Note: "Structure" in this sense is logical structure, which, as we explain in more detail later (Chapter V), we conceive of as making implicit reference to some symbolization which could be given for that which is said to possess the structure. Alternative structures may be discerned in the same thing; for instance the same proposition may be seen in terms of the single propositional constant of KC, the predicate expression of first, second or n-th order predicate calculus, a relational form in a predicate calculus, the form of a constituent of an Aristotelian syllogism, or in manifold other ways. It does not follow from the fact that the implication-proposition with which one must deal here can be described in terms of its being the result of joining by "implies" two propositions (or whatever else they may be) which are matched with a_i and a_j by the interpretation, that it must be so described or that this description has any particular relevance to the structure which should be investigated in order to test for tautologousness. As is argued below, it is above all unsuitable to assume that the structure which should be tested is precisely that of the formula which we are at present in the course of interpreting.

If we succeed in finding a formal system relative to which to define the structure of the metalinguistic implication-proposition a new option is open to us. This is to apply Lewis's criterion for the tautologousness of a symbolic formula, to the formula in that system which would be used to symbolize the implication-proposition. We shall not attempt to prove that this formal tautology test will yield the same conclusion as the informal one would have; it seems obvious that its failure to produce the same results would count strongly against using Lewis's criteria for tautologousness of informal expressions at all. Furthermore the criterion for the tautologousness of a symbolic formula is much less vague than that for an informal expression, and to that extent should be preferred. Finally there is the fact of Lewis's own willingness to "put the matter in more completely symbolic form"

when on page 243 he gives a specific example of the supposedly general informal argument on page 242, by using the tautology criterion for formulae. If in the case of the paradigm on page 242, it is allowable to argue equally in terms of an abstract formula, then it will presumably also be so in the present case. Accepting these arguments we therefore treat the question of the selection of the system required by the informal test, as coinciding with the question of how to select a calculus in which we might re-symbolize the implication-proposition in order to apply to the resulting formula Lewis's test for the tautologousness of symbolic formulae.

The superficially attractive step of selecting SID or SI as the calculus for this purpose (which I shall refer to as the "reformalisation" of the implication-proposition) cannot be taken. Were we to do this an infinite regress would arise because in order to assure ourselves that formula A rather than formula B was the correct symbolization of the implication-proposition we should have to employ semantic rules for the calculus concerned - yet it is still to be shown that such semantic rules can be given, and how they operate, particularly as regards the symbolizations of implication-propositions.* The reformalisation has to be conducted in a system which we are at present able to interpret.

If the use of SID or SI for the reformalisation is not open, the next most natural system to choose is KC. Lewis asserts that any theorem of KC is a theorem of SI, and indeed that any theorem of

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*See note on page 50.

*Note: In particular one notes that if the symbolization of the implication-proposition is in fact R (a_i, a_j) itself, then one matches that formula with truth if and only if it is a tautology. This, although in almost the very words used by Lewis on page 241, would seem to make an unreasonable demand of the ideal calculus. Lewis says of KC that it works only because whenever material implication is made the basis of an inference it is in fact tautologous material implication; by contrast one would expect that inference in his ideal calculus could be based on an implication relation if it were merely true. Yet rather than that we should produce the result by this sort of rule, that the implication relation never held except when it was tautologous (we shall return in Chapter II to what exactly might be meant by this). It is tautologousness in the system SID which is at point here, and so far as it counts against KC that inference can be based on implication only when the implication is tautologous, it would seem to count against such a form of SID that it possesses no formulae which might allow inference when merely true and not tautologous.

KC with the material implication symbol as its main connective is a theorem of SI when the material implication symbol is replaced by his strict implication symbol; and we know that a formula of KC is a theorem if and only if it is a tautology by the normal truth-table method. Further, as has been noted earlier, Lewis's method of testing for tautologousness and the normal method produce the same results in the case of KC. It is also clear that in SI no pair of arguments between which a material implication (in the SI sense of that symbol) does not hold, can be such that the strict implication does hold.* Lewis thinks it at least plausible that every law of

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*cf. Symbolic Logic page 199, where this is exhibited in a truth-table like tabulation. It is intuitively clear since the only case where material implication does not hold is that where the antecedent is true and the consequent false, and in this case the strict implication is also ruled out.

logic is a tautology*, and so one should not be going far wrong in using the tautology test on the reformalisations in KC, when we know that the formulae which pass the test will be all and only those which, when the strict implication sign replaces the material, will turn out tautologies (or at least theorems) of SI.

To conduct the discussion we introduce some further formal apparatus into this metalanguage. The reason for this is that it does not seem to us valid to assume, as Lewis does, that any constant of SI (or SID) coincides exactly with some constant of KC for which we may use the same typographical symbol/. This is not plausible even in the weaker sense that SI and KC constants might be related as symbols having identical linguistic interpretations.** Symbolization of the same utterance in two different calculi is always liable to produce different results in each - even if the two have identical lists of basic symbols; if this were not liable to happen it is difficult to see a sense in which the two could be

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*Page 241 of Symbolic Logic.

/We deal with point more extensively in Chapter II.

**Except of course if SI were extended into a system which reduced to KC.

called different systems.* In the particular case which will arise it is hard to accept that the KC formula 'P ⊃ Q' which is taken to symbolize utterance U, is genuinely the same and indistinguishable from the SI formula 'P ⊃ Q' symbolizing the same utterance, when Lewis is at pains to argue that for many such U the SI symbolization 'P ⥽ Q' better represents the meaning than the rival KC formula 'P ⊃ Q'. Lewis's simple identification of the typographically identical symbols is in fact a matter which requires justification; we shall refer to Lewis's procedure as "transcription" and the results of it as the "transcripts" of the formulae from which they originate. In this terminology one would express the last point by saying that it is very odd indeed if 'P' and 'Q' may both be transcribed from KC to SI, as well as ' ⊃ ', when one is asked to believe that the proper SI symbolization of what 'P ⊃ Q' symbolizes in KC, is 'P ⥽ Q' rather than the simple transcript 'P ⊃ Q'.

To avoid following Lewis's metalinguistic convention which fails to distinguish KC formulae and their SI transcripts, we use the constants "b_1", "b_2", ... to designate individual constants of KC, and shall take as the corresponding variables, "b_m" and "b_n", "b_{n+1}", "b_{n+2}", ... "b_{n+k}" (where m, n, and k are finite). The

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*One might suppose that two genuinely different systems could symbolize the basic "unanalysed" utterances identically but differ in the rules and/or axioms they contained. But if this were so we should surely be unwilling to use the typographically identical logical constants as if it did not matter from which system they came, when symbolizing complex utterances.

two-place functional constant "C" will designate the material-implication function of the designata of its arguments.

The problem is now to decide whether the KC reformalisation of the implication-proposition is or is not a tautology. Representing its form as "C (b_m, b_n)"* the putative semantic rule may be restated as:

> For any i and j, R (a_i, a_j) is matched with a truth of the interpretation if and only if those things of the interpretation with which a_i and a_j are respectively matched are such that when the KC reformalisation of the implication proposition from the former to the latter is C (b_m, b_n), C (b_m, b_n) is a tautology, for that particular m and n.

In the conventional sense of tautology C (b_m, b_n) is quite clearly not a tautology. This is the case merely because it is composed entirely of constants and the conventional test for tautologousness relies upon the results of substitutions of constants for variables which occur in the formula being tested; there being no variables for which constants could be substituted, the test is inapplicable.

Nor would it be satisfactory to extend the conventional test so that it admitted of application to constant formulae and still gave the result that a formula was a tautology if it was assigned no

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*For particular values of m and n

undesignated truth-value as a result of the (vacuous) substitutions. For in that case every formula which was merely true (matched with truth) would be counted as a tautology and every other would count as a self-contradiction. Such a result would be highly counter-intuitive and would in any case result in the degeneration of SID into a notational variant of KC, because whenever C (b_m, b_n) was designated, R (a_i, a_j) would in turn be matched with truth (and for a valid interpretation, designated). (This assumes only the possibility of the reformalisation as C (b_m, b_n)).

Lewis's testing procedure for formulae does not refer to the presence of variables, so one might hope that it would provide a means for showing whether C (b_m, b_n) is a tautology, without bringing on such undesirable consequences. However the absence of a requirement that variables must be used in the formula to be tested, is in fact a fatal defect in the statement of Lewis's procedure. To see this one need only consider the following case:

1. b_1 and b_2 are designated constants and hence C (b_1, b_2) is also designated.
2. b_1 and b_2 being different and non-complex, C (b_1, b_2) though designated is not a substitution instance of any tautology, and is not itself a tautology.
3. 'p v —p' is a formula of KC whose interpretation is "an obvious tautology" in the sense of Lewis's test.
4. Any designated formula is assertible in the course of a KC proof.

5. The following is a proof in KC:

(i) $\vdash$ Q — by 4 above

(ii) $\vdash$ Q v $\sim$(p v $\sim$p)

(iii) $\vdash$ $\sim$ (p v $\sim$p) v Q

(iv) $\vdash$ (p v $\sim$ p) $\supset$ Q

(v) $\vdash$ p v $\sim$p — theorem of KC

(vi) $\vdash$ (p v $\sim$ p) v $\sim$ Q

(vii) $\vdash$ $\sim$ Q v (p v $\sim$p)

(viii) $\vdash$ Q $\supset$ (p v $\sim$p)

(ix) $\vdash$ Q $\equiv$ (p v $\sim$ p)

6. Let "Q" in the above proof be C (b_1, b_2).

7. By the argument of 1, 3-6 it has been shown that C (b_1, b_2) being equivalent in KC to a formula whose interpretation is an obvious tautology, is a tautology.

2. and 7. constitute an explicit contradiction, showing the procedure to fail.

Cases like that just shown must be ruled out in some way if Lewis's method is to be applied successfully. Two ways of ruling out such difficulties present themselves; on the one hand we might require that Q be asserted in the system only if it is a theorem, while on the other we could require - like the conventional method - that it contain no propositional constants. The former restriction seems unsatisfactory, for when it is used as the basis for testing tautologies it becomes trivial that the tautologies of a

system are all theorems, and there seems to be no alternative way presented in which one might reinstate the notion of the completeness of the system as being something more than a trivial consequence of the definition of tautology. Furthermore the former alternative involves very much the same as the latter - at least in normal calculi - for where the rule of substitution for well-formed parts of theorems is allowed (whether directly or through the device of axiom-schemata) it follows that the Q in each case will be tantamount to an expression containing no non-logical constants.

To take the second alternative without demanding explicitly that the Q which is to be proved equivalent to some formula whose interpretation is an obvious tautology should be a theorem, seems the best course. The restriction may be phrased as the requirement that Q be "maximally general". A formula is maximally general when the only constants it contains are logical ones. To every constant expression of KC there corresponds some maximally general formula (to which we shall refer as the "c.m.g.f." or "corresponding maximally general formula" of that constant formula) which contains at least one variable. (This follows from the fact that no number of logical constants, standing alone, is well formed).

Whether one were to choose the first alternative or the second, it turns out in practice that the result of applying Lewis's test coincides with the result of applying the conventional test. For, if one demands that the formula tested be a theorem, then only

theorems are entered for the test and all these pass the test on the conventional method, while they all pass it by Lewis's method because all are equivalent in KC to the formula 'p v ~p' which has an interpretation which is an obvious tautology. While if one demands only that maximally general formulae be tested then the set which will pass the conventional test are precisely those which are theorems of KC, and that set is in turn the set which will pass Lewis's test because all and only those formulae which are theorems of KC are both maximally general and equivalent to 'p v ~p' or some other formula whose interpretation is an obvious tautology in his sense. Thus in either case, all and only those formulae which are tautologies in the conventional sense will pass Lewis's test for being a tautology.

But although we have saved Lewis's test, and have shown that in the case of maximally general formulae of KC it in fact produces the results which the conventional test gives, we have still not succeeded in finding a way to decide whether a constant formula of KC "is a tautology". The best solution to this problem seems to be to extend the definition of "tautology" in such a way that if a maximally general formula is a tautology then all other formulae resulting from the substitution throughout it of one or more constants or constant expressions in the place of the one or more variables it contains, are also to be described as tautologies. Thus if any constant formula has a corresponding maximally general

formula which is a tautology* then it too is called a tautology. In the normal terminology of such matters one might say that all the substitution instances of a given tautology are also called tautologies; it is clear that if a certain constant formula is a substitution-instance of a tautology, then the c.m.g.f. of that constant formula will be that tautology**.

The Semantic Rule for '$\rightarrow$'

Having now reached some understanding of what it is for a constant expression to be a tautology, one may return to the development of the semantic rule for R. We read that R (a_i, a_j) is matched with truth if and only if C (b_m, b_n) is a tautology. However it is at once apparent that C (b_m, b_n) is not a tautology unless b_m and b_n are one and the same constant. For if they differ then the only c.m.g.f. which C (b_m, b_n) can have is a formula like '$p \supset q$' which is clearly not a tautology. Thus, alas it would seem that only if R (a_i, a_j) is such that its reformalisation in KC is C (b_m, b_m) for some m, is there any possibility of matching R (a_i, a_j) with a truth of the interpretation.

Lewis presumably did not intend this. It would seem odd that no two linguistic expressions could properly be said to have a

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*From here onwards unless explicitly stated otherwise, we use the term 'tautology' and the related terms without qualification and to indicate at least the conventional sense of the notion of tautology, and wherever possible also Lewis's sense.

**Strictly speaking it might also be a tautology with fewer distinct variables, as for instance when the tautology "$p \supset (q \supset p)$" is first instantiated as "$P \supset (P \supset P)$" and then the corresponding maximally general formula taken as "$p \supset (p \supset p)$".

deducibility relation of the sort symbolized by R, unless they were such that in the respects symbolized by SID they would have to be symbolized in KC as equivalent; conversely it is odd if the matching of R (a_i, a_j) with truth is possible when and only when the interpretations of a_i and a_j are bound to have one and the same truth-value. To deal with such difficulties it is necessary to return to the example on page 242 and to pay particular attention to the structure which is displayed when the expression is given as "(p and pIq) Iq".

Continuing to take "(p and pIq) I q" as an expression built up by the use of "implies" from the interpreting realm elements matched with constants of an object language (in this case some truth-value system) one can see that no progress is possible if one takes "(p and pIq)" as being the interpretation of some single constant of the object language concerned. That would be precisely the situation envisaged so far, in that one would use "(p and pIq)" as the matching for b_m and "q" as the matching for b_n, and would reach the conclusion that the whole is not a tautology. In order to reach the conclusion that the expression is a tautology one requires, as in the paraphrased argument, to analyse "(p and pIq)". But so long as it is taken to interpret a single constant no such analysis is possible.

Although one may have to translate the abbreviation "(p and pIq) Iq" into English in order to know how to symbolize it in a

particular calculus (cf pp19-20), still the structure displayed in the presentation given to the expression is assumed to match the structure of the logic which would symbolize it, and insofar as "(p and pIq)" is complex it has to be seen as the interpretation of something complex in the calculus. Not only is it complex, but it is not independent of the "q" which constitutes the second argument, for if "r" is substituted for that "q" the tautologousness disappears. Thus one may say that the arguments of the principle truth-value implication symbol in the formalisation of such an expression are being assumed to be both complex and related.

The symbolization of "(p and pIq) I q" need only have a complex first argument, and since the arguments are not independent, that first argument is a function of the second; thus we might symbolize it as C $(f(b_n), b_n)$ in the case of its KC symbolization, where we take 'f' as a constant designating some logical function of the designatum of its argument. However one must work more generally, and it is clear that either or both of the arguments of the principal connective may be complex, or that at least one individual symbol must occur on both sides of that connective. To represent this, we shall express the two arguments each as functions of the same list of arguments, each of these arguments being a constant designating one of the KC constants in the KC-reformalisation of the implication-proposition. Thus it will be understood that the functions concerned may take certain of their arguments vacuously, in

the sense that no alteration of those arguments need vary the value of the function in any case. Adopting this procedure we use "b_1" to designate "P", "b_2" to designate "Q" and "b_3" to designate "R", and then represent the formula

$$(P \supset Q) \supset ((\sim Q \supset \sim P) \vee R)$$

in the metalanguage as

$$C\ (F_1\ (b_1, b_2, b_3), F_2\ (b_1, b_2, b_3))$$

Finally we generalize this by explicitly introducing the range of functional constants "F_1", "F_2", ... which have as the corresponding variables "F_s" and "F_t".

When this apparatus is applied to the problem of how to represent the symbolization in KC of the implication-proposition referred to in the semantic rule, this is given as:

$$C\ (F_s\ (b_{n+1}, \ldots b_{n+k}),\quad F_t(b_{n+1}, \ldots b_{n+k}))$$

for some k, n, s, and t. And thus the semantic rule comes to be stated as

> For any i and j, R (a_i, a_j) is matched with a truth of the interpretation if and only if those things of the interpretation with which a_i and a_j respectively are matched are such that when the implication proposition from the former to the latter is symbolized in KC as $C\ (F_s\ (b_{n+1}, \ldots b_{n+k}), F_t\ (b_{n+1}, \ldots b_{n+k}))$ then that formula of KC has a c.m.g.f. which is a tautology.

CHAPTER II

THE AIMS AND APPLICABILITY OF LEWIS'S SYSTEMS

In order to reach this statement of the presumed semantic rule for interpreting R, Lewis's ideal symbolization of the implication relation, we have pursued a somewhat tortuous course. What remains to be done is to examine the rule as it has now been formulated and to see whether it can be used in practice, what assumptions are implicit in it, and what implications it carries for the symbolization of the notion of necessity. Before launching into that discussion it is worthwhile recapitulating what has gone before, and attempting to show that the rule which has been reached does not differ very substantially from those which might have arisen if some other choice had been made at one of the points along the way where we have been forced to choose between alternative readings of Lewis's text.

Review of Chapter I

Taking Chapter VIII of Symbolic Logic as the primary source of Lewis's views on how his systems are to be interpreted, we set out to formalize his discussion by deriving from it a semantic rule for interpreting the strict implication relation symbol. It was noted that Lewis in fact makes a general claim about the characteristics of any symbolism which will satisfactorily symbolize the deducibility relation, and that he then identifies SI as one system possessing the necessary characteristics.

The characteristics are stated on pages 240 to 242, and what we took as the crucial point was that Lewis wishes to symbolize a relation "p implies q" which "is true when, for a particular p and q, it expresses a tautology;". In attempting to build this feature into the semantic rule we were faced with Choice 1 which was to decide whether the "p implies q" under discussion is an expression of ordinary language, given in an abbreviated form, or is instead, the result of substituting the word "implies" for the implication-relation symbol in a formula of the ideal calculus Lewis is characterising.

Having decided on the former answer to Choice 1, it was necessary to expand on the notion of a "tautology" as it then occurred in the semantic rule. The question of whether this notion is to be taken in the conventional sense or in some sense peculiar to Lewis himself, may be called Choice 2. In fairness to Lewis it seemed best to try to work with his notion of a tautology and so we set about finding out in detail how one should test for tautologousness, in his view.

In examining the relevant passages we made Choice 3 which was to separate the discussion on page 242 as being unlike those on page 243 and on pages 249-250 in that it related to an expression of informal discourse while they related to symbolic expressions - formulae of the calculi concerned. Taking "(p and pIq) I q" as an abbreviation for some English expression, forced us to adopt a reading of the argument on page 242 which made it different in

character from those relating to symbolic formulae; the argument appeared to be invalid, on any reading, and a patch which accorded with our reading was supplied to ensure some semblance of validity.

However it appeared that the criterion for tautologousness implicit in the revised argument of page 242 had undesirable results when incorporated into the semantic rule, because it either made the symbol for the implication-relation a basic non-logical constant rather like a predicate symbol, or else made it both that and a symbol which in fact symbolized another relation for which the term "truth-deducibility" was coined. In either case the calculus concerned could not have been the one that Lewis imagined he was constructing.

Turning then to another aspect of the application of the criterion of tautologousness in the semantic rule, it was pointed out that the argument of page 242, even taken as we interpreted it, has an implicit dependence on our insight into the structure of the expression being tested - and that it is the logical structure that is involved. While the logical structure of the expression in the semantic rule which is to be tested could not be explained by reference to its symbolization in SI, we proposed to consider its symbolization in KC as giving the relevant structure.

Having been forced to consider the symbolization of the informal expression that had had to be tested for tautologousness, we made Choice 4 and instead of continuing to discuss the matter in terms of

the test derived from the example on page 242, chose to apply the type of test used on page 243 for symbolic formulae, and to apply it directly to the KC symbolization of the informal expression.

It was observed that both the conventional notion and Lewis's notion of tautology would give the same results in the case of KC formulae. We examined the conditions under which the informal expression could pass this test, and noted that the appropriate form in which its KC symbolization should be represented was one in which the principal connective was a material implication sign and the two arguments were either complex or identical. Thus the final version of the semantic rule was produced.

The "Choices" indicated mark the four points where there seems to be a possibility of honest disagreement with our reading of Lewis's intentions. While we should not feel any such disagreement to be entirely reasonable, it is at least worthwhile to discuss the consequences of taking the other choice in each of these cases, because it turns out that the other choice either leads to insuperable difficulties or to an interpretation which can easily be discussed on the same lines as will be that which is given according to the semantic rule now developed.

Choice 1

The alternative to our reading seems to be that of treating "p implies q" as a semi-formal expression. It might be, for instance, the result of substituting for the implication-sign of

some formula in a calculus the actual word "implies" in such a way as to show the scope and arguments of the original formula but also making the interpretation quite determinate. Or again the expression might be one of the sort which Quine frequently uses, and should therefore be taken as the result of partially translating into a calculus, some sentence of "regimented" English (cf. Word and Object (69OT) pp 157-161 ff).

Whatever one decides about the origin of such a piece of semi-formal language, its status is different from that of "p implies q" when that is read as an abbreviation for some ordinary English utterance. The difference seems to demand that we start to ask the questions that would ordinarily be asked of a formula. Thus the question of whether it is tautologous or not, should be settled by the use of some form of the tautology testing procedure for formulae of symbolic calculi. If one is choosing this alternative it will be natural to say that the argument on page 242 is of such a sort and is designed to show that no matter what the truth-value system used may be, it must be possible to deduce the expression given in semi-formal terms as the quoted part of the last sentence of the paragraph discussed*.

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*A propos of this we may consider yet another way to understand the paragraph on page 242. Taking it at face value as exactly paralleling the argument on page 243, one expects it to contain reasons to conclude that '(p and pIq) I q' is equivalent to an obvious tautology. The derivation of this conclusion ostensibly occurs in two stages; (i) that '(p and pIq) Iq' entails "It is false that (p is true, and it is not the case that p is true and q false, but q is not .....

In the case of the semantic rule however one is not necessarily concerned only with truth-value systems in Lewis's sense and must take the question of whether "p implies q" (in this sense) expresses a tautology, at its most general. The only way in which one can do this seems to be by the explicit consideration of the calculi to which the expression approximates in each case where it may be taken as a semi-formal expression of a calculus. The considerations which dictated our choice of KC as the calculus for the reformalisation of the implication-proposition, will in this case determine the

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..... true)" (this is the "obvious tautology") and (ii) the reverse entailment.

The conclusion (i) is obtained by successively "reading the meaning of" the "I"s as truth-implications. (ii) is established by taking "(2)" of Lewis's argument in conjunction with the similar assertion about the truth of "pIq" given that "It is not the case that p is true and q false".

Apart from the difficulty of identifying the parts of such an argument in the text, one can despair of its providing any help in the assessment of the tautologousness of an expression which is not of the given truth-functional sort. For if the expression to be tested is a genuine implication relation (in the "desired sense") then we shall not be able simply to read the implication into the form shown by the quoted expression in the last line of Lewis's argument, since this will not be an accurate rendering or reading of the meaning. Even if one takes the truth of this form as being entailed by the truth of the implication proposition (certainly in accord with Lewis's views, for a strict implication relation being stricter that a material implication relation, will certainly hold only when the truth values of its parts are as stated by the readings), it will not be possible to establish the reverse entailment, because by supposition the genuine deducibility does not hold merely in virtue of the existence of the stated relationships between the truth and falsity of the parts.

Yet by taking an implication relationship which is implied by the expression like that quoted, one ensures that it is a truth-value type of tautologousness which is detected, and this is equally .....

same approach.

Having adopted this choice one comes immediately to the use of the tautology-test for formulae on whatever formula results in a given case from the replacement of the "implies" by the implication-sign appropriate to the calculus concerned. This approach will differ in its results from the approach of Chapter I only in that the formula tested will be obtained directly from the already semi-formalized "p implies q" and it may therefore not be necessary to suppose that the formula tested is a reformalization of the SID formula being interpreted. We shall take account of this possibility when discussing the notion of transcription and the implications of the rule actually developed in Chapter I.

..... well or better detected by applying the conventional test to the straightforward KC symbolization; while by seeking to find an "obvious tautology" of the informal type which does entail the expression being tested, one loses the easy reading of the implication-signs in the tested expression into truth-and-falsity conditions (unless the systems SID and KC coincide).

The main point of this note is to point out that when one abandons the restriction of the example to truth-value systems it is no longer possible to show the equivalence of the "obvious tautology" and the tested expression; on page 242 Lewis may appear to discuss explicitly only the entailment in one direction, but equivalence seems necessary to his test. (For instance a self-contradiction entails a tautology to his mind, but since they are not equivalent this does not mean that the self-contradiction is a tautology itself).

In leaving this choice, it should be noted that taking the alternative discussed here, tends to bring very sharply into focus the question of whether "p" and "q" are variables or constants; but nothing seems to follow from this choice which might alter the treatment of that problem which we put forward when discussing the situation with respect to the resymbolization of the implication-proposition. It seems that the only sense which can be attached to the notion of a constant expression's being a tautology is that it be a substitution instance of a maximally general formula which is, properly speaking, a tautology; this may be taken to hold in SID as well, but since no explicit discussion of the application of the notion to SID or SI is given to us, it can only be presumed that this is so.

A particularly straightforward approach which could be entertained at this stage would be that of taking "p" and "q" as constants - in view of the reference to their being "particular" - but taking them to refer to symbolic expressions composed of variables. Thus one would understand the constants (of SID, presumably) "A_1" and "A_2" to refer to (or "designate") the formulae (in some way contained in SID), say, 'p. $\sim$ p' and 'q' respectively. There would then be a sense in saying that these particular constants, "A_1" and "A_2" were such that the formula which would be described as "A_1 implies A_2", and would actually be '(p. $\sim$ p) $\supset$ q', was a tautology. Thus "A_1 implies A_2" expresses a tautology, and

the symbolization of it (i.e. the completion of the half-performed process of formalizing it), viz. R (A_1, A_2), is to be matched with a truth of the interpretation.

In practice this approach leads to almost exactly the result which the semantic rule we have developed gives. To adopt this straightforward approach at the outset, however, requires one to argue that "p implies q" is definitely at least semi-formal, that the "p" and "q" must be taken as constants while the tautology test must be applied to formula with no non-logical constants, and that there is some subtle differentiation being made between expressions which "are" tautologies and those which "express" tautologies*. We did not feel able, in view of Lewis's extreme vagueness, to give unequivocal answers such as these, and have therefore reached substantially the same conclusion by a path which tries to avoid this conclusion at every turn. (The reason why the conclusion is to be avoided will presently be shown).

Choice 2

Was settled by giving Lewis the benefit of the doubt as to whether there was a difference between his tautology tests and the

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*On page 244 Lewis says at one point "When pIq is a tautology q can be deduced from p." and a little later tries to show that ' $\dashv$ ' symbolizes deducibility, saying "Whenever any truth-implication, pIq, expresses a tautology (is necessarily true) the relation p $\dashv$ q holds:" There seem to be many examples where the distinction is not observed and no place where it is explicitly introduced.

conventional ones. If one does not allow Lewis this latitude, then the result reached, comes all the quicker and no less surely.

We have suggested that those who differ from us on Choice 1 will wish to choose differently in the case of Choice 3 as well. They are not forced to, but if they follow our reading of page 242, then the only tools that can be open to them for testing the semi-formal expression that Choice 1 leaves them with are those of page 243 and pages 249-50. The use made of these tools in Chapter I is not dependent upon the outcome of Choice 1, and with us on Choice 3 to reach the same ultimate conclusion. Finally, those who choose with us on Choice 1 and against on Choice 3 are in a difficult position; there is then no example of the testing of an informal expression for tautologousness, which they could follow. It immediately becomes necessary to test a formalisation of that informal expression, instead of it itself; thus one is led immediately into the discussion of the resymbolization of the implication-proposition, and the arguments given apply directly.

In the case of Choice 4 it does seem open to those with enough determination, to insist upon trying to apply the tautology-test for informal expressions, to the implication-proposition, given the explicit assumption of some structure in it. As suggested (page 48) it is damaging to the status of this tautology test if it should be found to give results inconsistent with those obtained by applying the tautology test for formulae to the formula obtained by

symbolising in the calculus by reference to which the "structure" is defined. Still, it might be of residual interest if even this castrated form of tautologousness could be used.

To see how such a test would operate let us consider another example from the same mould as that of page 242. It should be pointed out that our reading of page 242 is adopted at this point since those who have not chosen with us on all of the three previous choices will not have needed to face Choice 4 at all. Accordingly the case to be considered may be:

$$(p \text{ and not-}p)\ \underline{I}\ q$$

(mimicking Lewis's symbolism), which is, we shall suppose, an abbreviation for "That this (p) should be true and should not be true, implies that this other (q) be true."; and it abbreviates that assertion, or one very like it (perhaps spelling out the two things supposed as being true and/or false), in such a way as to indicate that it would be symbolized in KC as;

$$'p.\sim p: \supset q'$$

and Lewis's view of SI, would have us symbolize it in SI as

$$'p.\sim p: \dashrightarrow q'$$

The tautology-test for informal expressions will be satisfied if, in the light of our knowledge that the expression would be properly symbolized in either of these ways, we can either (a) validly deduce the truth of the whole given only that of the antecedent, or

(b) validly deduce the truth of the consequent given only that of the antecedent.

Even were we able to do either we should not satisfy Lewis, it seems, for both of the formulae cited are regarded by him as paradoxes, which is to say that he finds the informal expression interpreting them to be counterintuitive. It is true that he considers the possibility of saying that a deducibility does exist, while an inferability does not (page 514, Appendix III (1959)) but he explicitly refrains from advocating that one makes this distinction. (loc. cit.). Indeed the deducibility of quite specific assertions from contradictory premises - in an ordinary sense of "deducibility" such as one must be using here if one opts not to take the formalisation as equally good - is hard to accept.

A dilemma may be proposed to those who adopt the alternative offered by the fourth choice. Either one so interprets "deducibility" in the context of this test that one does actually assign truths to the instances of theorems of SI; in which case one swallows the paradoxes of strict implication, and is using a notion of "deducibility" which is in effect defined only by the system one is supposedly interpreting: or else one holds loyally to the ordinary sense of "deducibility" but risks matching the instances of Lewis's paradoxical theorems with falsehoods of the interpretation. In the light of this dilemma, and in the hope that our approach, although unsympathetic to Lewis's proposed linguistic

interpretations, will throw more light on the systems based on SI, we take the alternative offered by Choice 4 as unacceptable.

Thus the prospects for arriving at a satisfactory alternative to the semantic rule we have developed, are negligible even if one adopts some of the alternative choices outlined. The nature of a system with a rule of the type given, or with one of the similar types to which the alternative readings might lead, should now be discussed in more detail.

The Meaning and Use of Strict Implication

Having reached this point by way of a close examination of the views put forward by Lewis in a single exposition of the system of strict implication, we shall now broaden the discussion and deal with the interpretation of the whole range of systems related to those which Lewis originally put forward. The semantic rule which has been developed, and those which arise from slightly different readings of Lewis, will provide a starting point for the discussion of the inadequacies which these systems have as vehicles for the symbolization of ordinary language utterances. Although these inadequacies are reasonably evident when one considers the semantic rule as it has been formulated, they are far less evident when one is dealing with systems of strict implication which are formulated in other ways, and without so much interpretative comment as Lewis gives.

Chapter III will be devoted to a consideration of presentations of Lewis's systems by other writers; the remainder of the present Chapter deals with the philosophical problems arising from the semantic rule and with the possibility of any logic of a familiar sort being adequate for Lewis's stated purposes.

Transcription

The first difficulty to arise is one which is basic to Lewis's programme of substituting for KC a more adequate type of logic. He holds it to be a virtue of SI that it is such as to have as theorems "the laws of (any) relation of truth-implication introduced into the system of Strict Implication, by suitable definition". He cites KC as an example and says "... we introduced the relation of material implication into the system of Strict Implication ..., by the definition of $\underline{p} \supset \underline{q}$. From this definition, it was possible (using the propositions of Strict Implication as principles of deduction) to deduce every theorem of the system of Material Implication. This was demonstrated by deducing the postulates for Material Implication, as given in Principia Mathematica". (loc. cit.)

The definition referred to is numbered 14.01:

$$14.01 \qquad \underline{p} \supset \underline{q}. = . \sim(\underline{p} \sim\underline{q})$$

One may accept that this introduces into the system SI a symbol which is typographically identical to that used in Principia Mathematica and known as the material implication symbol, yet not

accept that this does in fact introduce that relation into SI. The only ground for claiming that the ' ⊃ ' of SI is a symbolization of the same relation of "material implication" which the ' ⊃ ' of KC symbolizes, is the belief that the conjunction, negation, and definitional equivalence relations of KC and SI in some sense coincide. The belief in such a coincidence is a belief in the justifiability of what we have earlier called "transcription".

Without the legitimisation of the device of transcription the only claims which are justifiable are such ones as that the typographical symbol ' ⊃ ' occurs in SI and KC in such a way that those formulae of KC which are theorems of KC, are typographically identical with formulae of SI which are theorems of SI. Similarly one could describe SI as being a calculus which might be constructed by adding to the set of formulae (typographically defined) which appear in the abstract calculus KC, a certain set of formulae ...., and so forth. But both of these claims immediately cease to hold when one substitutes for the Principia Mathematica formulation of KC, the formulation in terms of the same axioms written in Polish notation.

It seems evident that Lewis does not wish to claim only that the Principia Mathematica notation for KC is such as to make all theorems of KC typographically identical with theorems of SI as it is expressed in his notation. He renders 14.01 into English by saying "That is "p materially implies q" means "It is not the case

that p is true and q false."" The coincidence of the rendering "it is not the case that p is true and q false" with Lewis's reading of the meaning in English of 'pIq' when 'I' is a truth-implication, is not fortuitous. He is taking '$p \supset q$' to symbolize 'It is not the case that p is true and q false.' in both KC and SI, and the reason for this seems to be the belief that, whether it occurs in KC or in SI, '$\sim(p \sim q)$' symbolizes exactly the same proposition in exactly the same respects. The sense in which Lewis believes that 14.01 introduces material implication into SI is thus a strong one, such that one could say that 14.01 had introduced into SI the relation which occurs in 'Cpq' (as it conventionally occurs in the Polish notation formulation of KC). This strong sense of introducing the relation '$\supset$' involves the belief that the interpretation of the '$\supset$' now occurring in SI, and the '$\supset$' which occurs in KC, will coincide, and it is this semantic relationship which we denied to hold, when we attacked the device of transcription on pages

The argument against the validity of transcription given already seems quite ample. It is worthwhile, however, to make it clear that the invalidity of transcription between different calculi, does not entail that talk about one system's being "contained" in another is necessarily illegitimate. So long as "containment" can be defined in purely formal terms, it escapes any taint of the defects which attach to claims like Lewis's that a certain relation of one calculus can be introduced into another.

The containment of one calculus in another could be defined inoffensively as being the condition where all the theorems of the first could be mapped onto a subset of the theorems of the second. In practice one usually shows this by establishing a mapping of the axioms of the first onto a set of theorems of the second, and showing in addition that the rules of the first lead only to theorems which can similarly be mapped onto theorems of the second, and often finally that the theorems of the second onto which the axioms of the first were mapped are themselves a complete set of axioms for some part of the second calculus. The coincidence of two calculi can be defined as their mutual containment.

In the special case of two systems which coincide it is possible to base transcription upon containment; for, since the theorems of the two systems can be mapped in a one-to-one correspondence it is possible to select an interpretation which will satisfy both, (so long as it is possible to select an interpretation which will satisfy either); in terms of this interpretation we may say that both calculi provide symbolizations for the same things in the same respects, and that transcription from one to the other can be valid.

One might say that it is a consequence of one calculus's being contained in another but not vice versa that there can be an interpretation of the second which also provides an interpretation for the first, such that the first calculus cannot symbolize as

theorems certain truths about that interpretation; that is a case can be found for which the containing calculus would only coincide with the contained calculus in respect of the interpretation of the 'common' theorems if that containing calculus were deductively incomplete or otherwise inadequate for symbolizing the subject matter in question.

Transcription in the semantic rule

Lewis's reliance on transcription when he is extolling the merits of SI relative to KC, might well be forgiven and forgotten. But its use in less rhetorical contexts is a more serious matter, and in particular we must take care that the semantic rules for SI do not become infected by it.

In Chapter I, when developing our version of the semantic rule, we explicitly rejected any reliance on transcription; as a result it was necessary to leave open the question of the actual structure of $C\ (F_s\ (b_{n+1}, \ldots\ b_{n+k}),\ F_t\ (b_{n+1}, \ldots\ b_{n+k}))$ which, it will be remembered was the symbolization in KC of the implication from the interpretation of the first argument of R to the interpretation of the second argument of R.

When it comes to actually applying the semantic rule, this openness is fatal. There is no particular reason to assume that any particular structure must be employed in the KC symbolization of a given case. For example, suppose that it is desired to interpret '$p \dashv (q \supset q)$' which is a theorem of SI. One of the

instances of this, to which we shall have to apply the semantic rule, will be 'P $\rightarrow$ (Q $\supset$ Q)'; the two arguments of R in this case are thus 'P' and 'Q $\supset$ Q'. Suppose for the moment that the rule covers the case of complex constant formulae as arguments, as well as that of simple constants as arguments. The rule requires us to form the implication proposition from that with which 'P' is matched to that with which 'Q $\supset$ Q' is matched, then to symbolize that proposition in KC and to apply the tautology test to the c.m.g.f. of the KC formula we obtain. The elements of the interpreting realm which are matched with 'P' and 'Q $\supset$ Q' may be supposed to be propositions* and for simplicity one might even suppose that the element matched with 'Q $\supset$ Q' is such as to be distinctively an element matched with some formula containing ' $\supset$ ' and not merely some true element. But even with these assumptions there is still not sufficient information about the implication-proposition for us to conclude that its KC symbolization should be 'R $\supset$ (S $\supset$ S)'. For this conclusion to follow we should have to be assured that any element which is matched with a formula of the form 'Q $\supset$ Q' (in SI) is such that its KC symbolization is of the form 'S $\supset$ S'; but that element, if it is truly matched with 'Q $\supset$ Q' in an interpretation which satisfies SI, may be symbolized in SI by 'Q $\supset$ Q', and

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*See our comments on page 10.

further it may be used to serve as the element of an interpretation of KC which is matched with 'S ⊃ S': thus, in virtue of the existence of an element such as this - i.e. one for which we are sure the KC symbolization is systematically related to the SI formula which it matches - we are assured of the validity of transcription between the calculi SI and KC. If one understands transcription broadly - not merely as the possibility of using typographically identical formulae in two different calculi to symbolize exactly the same things, but as using formulae which are of systematically related typographical form, for this purpose in different calculi, then it follows from the considerations just adduced, that any assumption which is strong enough to determine any predictable structure in the KC symbolization of the implication proposition, is an assumption which is tantamount to that of the validity of transcription as between KC and SI.

Hence without the assumption of the validity of transcription one cannot make any prediction of the result of the tautology test which is called for by the semantic rule. Every case will have to be judged on its own. But this conclusion is most odd. Suppose that there are two sets of semantic rules which are sufficient to interpret all formulae of SI other than those involving '⥽', '◇', '○' or in later versions '□'; call these sets 'A' and 'B'. To obtain a complete set of semantic rules for SI we have to add to either A or B a rule for interpreting '⥽'

(since the other symbols may be defined in terms of ' $\dashv$ '), so let us try to add this rule as it is formulated at the end of the previous chapter. Now suppose that the difference between A and B includes some difference in the interpretation they give to the constants of SI*; from this it will follow that in any given case a constant formula of SI involving ' $\dashv$ ' runs the risk of being differently matched as regards truth or falsity according to whether A or B is the set to which the semantic rule is being added. This situation arises simply because the arguments of ' $\dashv$ ' in this expression have to be interpreted in terms of the set of rules concerned, and if there are two different sets of rules, the resulting interpretations for these arguments may differ substantially; in particular they may well differ in the KC symbolization which would be given to them and to the implication from one to the other, and there is nothing to prevent this difference leading in the one case to the matching of the SI constant formula with a truth, and in the other to its being matched with a falsehood of the interpretation. Now in normal situations the possibility of such a difference presents no problem, for one says immediately that interpretation which does not match assertible formulae with truths is not a valid interpretation (even though it may be valid for the part of the calculus

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*If one thinks of interpretation as the inverse of symbolization, as Lewis presumably does, then this need mean no more than that a given designated constant is matched with one true proposition in A and a different true proposition in B.

which does not include ' $\rightarrow$ '). But in the present case this conclusion cannot be drawn; for even when A and B are identical it is possible that the elements of the interpreting realm which they match with the arguments of ' $\rightarrow$ ' may be differently symbolized in KC simply according to our whim. For instance the sentence "That some men have beards implies that it is implied by its being sunny today that it is sunny today." can be symbolized in KC equally well as 'P $\supset$ Q' and as 'R $\supset$ (S $\supset$ S)'. In the actual practice of symbolization one chooses the simplest formula which has parts interpretable in terms of the relevant* parts of what one desires to symbolize. Thus the KC symbolization is apt to depend upon many extraneous and subjective factors which cannot be determined merely by the SI formula which is being interpreted. The lesson which must be drawn in the case of SI when a set of rules including the present one yields an interpretation which does not match an assertible formula with a truth, is that the same interpretation has to be tried again, rather than that it is not a valid interpretation. Such an attitude to interpretations robs them of any value, for it was as reliable sets of rules for matching all and only the theorems of symbolic systems with truths, that one originally sought to find interpretations; now the reliability has been destroyed.

Transcription then is necessary to the use of the semantic rule in giving a reliable interpretation to SI: but it is invalid

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*e.g. those parts which recur as important parts of other 'bits' of what is being symbolized, or appear alone as bits to be symbolized.

unless SI and KC in fact coincide. This last conclusion is undoubtedly the last one to which we should allow ourselves to be driven, so let us return to the question of whether nothing can be done to assess the tautologousness of SI formulae by means of direct application of tests to SI variables.

Tautology-testing of SI formulae

On page 49 of Chapter I, we dismissed the idea of reformalising the implication-proposition formed during the application of the semantic rule, either in SI or SID. The reason for this was that these calculi were taken to be as yet uninterpreted and hence it would not be possible to verify any putative symbolization in them by means of an interpretation of the formula suggested; in other words, interpretation and symbolization are so closely interrelated that it is not possible to conduct the process of symbolization except in a calculus which is seen as being interpreted.

On the other hand it might be argued that the reformalisation was bound to yield R (a_i, a_j) itself, since R is by supposition the symbol which should be used to symbolize "implies" and the things which appear as arguments for 'implies' in this case are known to be interpretations of (and hence symbolizable by) a_i and a_j respectively. Thus one might be persuaded that R (a_i, a_j) was to be matched with a truth if and only if R (a_i, a_j) itself was a tautology of SI (or SID). Further, had one made Choice 1 along the lines suggested at the beginning of this chapter, and wished simply

to look on 'p implies q' as a semi-formal expression of some calculus, then one might also have been faced with the question of how to test 'p implies q' for tautologousness in that calculus (which is presumed to be SID).

For the reasons given above (see Chapter I, pages 53-4), it is not possible to apply any tautology test directly to a constant formula such as R (a_i, a_j) (we shall include in the discussion of R (a_i, a_j) here the case of 'p implies q' when that phrase is regarded as semi-formal: it is to be regarded as a notational variant of "R (a_i, a_j)"). Thus it is to the c.m.g.f. of R (a_i, a_j) that one must attempt to apply the tautology test.

The incorporation into the semantic rule of an explicit statement of Lewis's tautology testing procedure*, gives it a most odd appearance. It turns out to enjoin us to match R (a_i, a_j) with a truth of the interpretation if and only if its c.m.g.f. is equivalent in SI to 'p v ~ p'. In SI however all theorems are equivalent, and since SI is consistent and 'p v ~ p' is a theorem all and only theorems are equivalent to the formula cited. Thus it turns out that the rule lays it down that R (a_i, a_j) be matched with a truth of the interpretation if and only if it is an instance of a theorem. Such a rule does not tell us how to interpret R (a_i, a_j); it in fact lays down only that we shall not interpret it in any way which might cause the interpretation not to satisfy SI. One perfectly

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*as amended in the discussion on pages 53ff.

well realises that if an interpretation were allowed to have all its rules of this form, then there would not be any possibility of arguing from the existence of an interpretation for a calculus to its being consistent, since even for an inconsistent system it is possible to lay down an interpretation such as "Every formula which is an instance of a theorem, is matched with a truth".

To avoid such complications one might try to apply the ordinary sort of tautology test in terms of truth-tables (or as we shall call them in the more general case "value-matrices"). Thus it would be that R (a_i, a_j) was matched with a truth if and only if every value assignment to the variables of the c.m.g.f. of R (a_i, a_j) was such that a designated value was assigned to the whole expression. Prima facie one might say that there is a close connection between knowing whether to match R (a_i, a_j) with a truth, and knowing whether the value to be assigned to a particular case of the assignment of values to the variables in the c.m.g.f. of R (a_i, a_j) is to be a designated one. However it is not easy to articulate the connection clearly, and since the topic of truth-tables for strict implication has several points of contact with our discussion of Lewis's systems, it is worthwhile to spend some time in making explicit our notion of value matrices.

Articulation of the semantics of value-matrices

First of all we set out the abstract features of a value-matrix, and then specify our own particular interpretation of the

significance of such structures. The specification of the abstract features owes something to H. S. Leonard (515A) but is more detailed than his presentation, while retaining his terminology as far as possible.

1. A value-matrix consists of a row of maximally general formulae of some calculus, and a finite number of rows of symbols such that each row is of the same finite length as the row of formulae and the rows can be linearly ordered.

2. The members of the list of formulae are known as "headings", and where 'E_i' is the i'th heading, the i'th symbol in each row of symbols is said to appear "below" E_i, and E_i to appear "above" that symbol; the set of symbols above which E_i appears are said, when ordered according to the linear ordering of the rows from which they come, to constitute the "column" below E_i, or to be the "i'th column" of the matrix. The symbols which appear in the rows and columns may be referred to as "value-signs" or "values"; the presence of a given value sign under a particular heading is referred to as the "assignment" of that value-sign (or value) to that heading "in" or "by" the row of the matrix in which the symbol appears.

3. The value-matrices considered here will all be such that:

(i) no row of value-signs is repeated,

(ii) no heading appears more than once,

(iii) no heading contains any well-formed part which does not appear as a separate heading previous to that heading in the row of headings.

(iv) there are no headings in the row of headings which are not well formed parts of the last heading in that row.

These criteria are satisfied by the truth-tables considered by Leonard, except that he does not explicitly state restriction 3(iv) without which there is the risk of unnecessary variables appearing in the row of headings and producing superfluous multiplications of the number of rows in a valid table. We shall employ Leonard's variables "r", "r_1" for rows, "V" and "V_i" ($1 \leqslant i \leqslant n$) for value-signs, "A", "A_1", etc., for value matrices and "E", "E_i" ($1 \leqslant i \leqslant n$) for headings. None of the other symbols introduced in §4 of his paper (515A;44) will be used.

Our particular interpretation of the semantics of value-matrices is given in the following stipulations, which should be added to 1 - 3 above.

4. Where W is the set of all those well-formed formulae of the calculus concerned which consist only of constants, each of the value-signs is matched with some subset of W in such a way that these subsets form a partition of W into a number of equivalence

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*3(iv) could not, at least in the form given here, be stated in the context of Leonard's exposition since it would rule out the building up of a table for '(p v q).(q v r)' by causing the deletion of one variable every time one of the conjuncts appeared as the last heading or one of the conjuncts every time the third variable was last heading; we are not, however, here concerned with the construction of matrices in this way).

classes, each of which may be referred to as "CV" where "V" is the value-sign matched with it.

5. The replacement of every occurrence of some variable in some formula by an occurrence of some one constant is known as "the application to that formula of a substitution for that variable." Every member of W may be obtained by the application to some maximally general formula of a finite number of substitutions; and every heading of a value matrix is such that a member of W results from the application to it of a set of substitutions which includes a substitution for every variable in the heading.

6. The replacement of every occurrence of some variable in some formula by some formula (possibly different in the case of different occurrences of the variable) of CV, is said to be the "application of an m-substitution for that variable to that formula." and the application may be said to be "with respect to V". Where the number of formulae in CV is n and the number of occurrences of the variable, E, in a given formula is s, the number of different m-substitutions which may be applied to that formula for that variable with respect to V, is clearly n^s.

7. The significance of the assignment to a heading E of a value-sign V by a row $\underline{r}$, is that when the variables in E are q_1, ... q_n, and the values assigned to these variables by row $\underline{r}$ are respectively V_1, ... V_n the set of formulae of W which results from the application to E of any set of m-substitutions for each q_i with respect

to V_i* is a subset of CV**. Where E_f is the heading in question, this set of constant formulae resulting from the application to E_f of all m-substitutions for the variables in E_f with respect to the value-signs which row r assigns to them, is referred to as the set ' $S^r(E_f)$ '.[/]

8. A value-matrix which is such that V_i, ... V_{i+j}, is a proper subset of its value-signs and $CV_i \cup CV_{i+1} \cup \ldots \cup CV_{i+j}$ is the set of all assertable constant formulae of the calculus concerned, is said to be an <u>assertability</u> - value-matrix; and the values V_i, ... V_{i+j} in such a case are said to be "designated" value-signs.

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*V_i is, of course, the value assigned to a_i in row r.

**If E is anything other than a single variable then, in a calculus which has more than one logical constant, this set of formulae will usually be a proper subset of CV: for instance, all m-substitutions for 'p' with respect to T in the heading '$\sim$ p' are constant formulae which commence with a '$\sim$' and thus are only a subset of CF, unless some other factor ensures that there are no single constants in CF.

[/]This notation is chosen because the set can more elegantly be defined by the recursive rules:

1. Where 'L' is a logical function of n arguments, the expression (L, CV_1, ... CV_n) is the set of constant formulae resulting from the application of L to all n-tuples of formulae drawn successively from CV_1, ... CV_n.

2. Where E_f is a single variable and row r assigns V to E_f, $S^r(E_f) = CV$. Otherwise where L is the principal connective of E_f and where L has the n arguments, q_1, ... q_n in E_f, $S^r(E_f) = (L, S^r(q_1), S^r(q_2), \ldots\ldots S^r(q_n))$.

An assertability-value-matrix is said to be "full" if in rows which assign identical values to all other single-variable headings, it assigns the values-signs V_1, ... V_n to every given variable, when $CV_1 \cup .... \cup CV_n = W$. A full assertability-value-matrix is said to "verify" a heading if each row of it assigns a designated value-sign to that heading.

9. A value-reference table for a logical constant, L, is the specification of an algorithm whereby, given the value-signs assigned to the arguments of L, the value-sign to be assigned to the binding of those arguments by L, is uniquely determined. (For two-place logical functions, the form of the value-reference table is normally a square array, indexed by the specification of the value-signs of the arguments, and containing as elements the value-signs to be assigned to the binding of arguments of the respective value-signs by the function concerned). A value-matrix is said to "implement" such value-reference tables as determine the value-signs which it in fact assigns to its headings, (a value-matrix trivially implements any reference table for a logical constant which does not appear in any of the matrix's headings). A value-reference-table is said to be "finite" if the algorithm it specifies has a finite termination in all cases.

10. A set of value-reference-tables is said to be "characteristic" for a calculus C, if and only if:

(i) it includes a value-reference-table for every logical function allowed by the formation rules of C.

(ii) all and only those formulae of C which are theorems (including axioms) are verified by value-matrices which implement all members of the set.

11. A value-matrix for a formula of a calculus C is said to be "valid" if and only if it:

(i) is a full assertability-value-matrix.

(ii) implements a set of value-reference-tables which is characteristic for C.

Returning now to the question of whether it is possible to lay down as the semantic rule for R (a_i, a_j) that it be matched with a truth if and only if its c.m.g.f. is a tautology, one is able to clarify the connection between a formula's being a tautology and its instances' being matched with truths on interpretation. Since any valid interpretation will match all assertible formulae with truths, it must follow from the tautologousness of a maximally general formula that its instances are to be matched with truths.

In particular we see that R (a_i, a_j) is a constant expression having R as its principal logical connective; the a_i and a_j which occur in the formula are themselves constant formulae; thus a_i, a_j, and R (a_i, a_j) are all constant formulae in the class W for SI, and as such must belong to certain equivalence classes within W. Now in virtue of the matching between value-signs and equivalence classes

of W, it is possible to infer from the fact that the value sign assigned to the c.m.g.f of R (a_i, a_j) is always that matched with the class (or some sub-class) of assertable formulae, no matter what value-signs are assigned to its arguments, that no matter what equivalence classes a_i and a_j may belong to, R (a_i, a_j) is assertable.

It thus seems to follow quite simply from the fact that the c.m.g.f. of R (a_i, a_j) is a tautology, that R (a_i, a_j) is to be matched with a truth on any valid interpretation. But this follows whatever R may be, and in no way uniquely characterises deducibility or implication. If one were to give a semantic rule which matched R (a_i, a_j) with a truth of the interpretation when its c.m.g.f. was a tautology, one would thereby trivially ensure that the calculus was complete relative to this interpretation (so far as these with R as main connective are concerned, at any rate) but would give no means of distinguishing R from any other connective (given that the interpretation was valid).

It is however the converse condition in the semantic rule which causes most trouble - i.e that a formula R (a_i, a_j) is matched with a truth of the interpretation only if its c.m.g.f. is a tautology. Now since in a full value-matrix there must be a line which assigns to the arguments of the c.m.g.f. in question values such that a_i and a_j are in the corresponding equivalence classes of W, it will follow from the fact that this formula is a tautology, that there

is a row of its truth table which can be read as indicating that when a_i and a_j are substituted for the arguments of R in this formula, the resulting formula will be assertable.

Now in order for us to be able to employ the value-matrix to obtain the result that the c.m.g.f. is a tautology we have to be sure that the value-matrix is a valid one. Thus it must implement a set of value-reference tables which is characteristic for SI. This requirement, although, formulated in our terminology, is one which must be laid down on any account of value-matrices or truth tables. Now by 10.(ii) a set of value-reference tables which is characteristic for SI will result in only those formulae which are theorems being verified by value-matrices which implement it; we therefore may conclude that if the value-matrix concerned implements characteristic set of tables and assigns a designated value to the c.m.g.f. of R (a_i, a_j) in every row, then the c.m.g.f. of R (a_i, a_j) is a theorem of SI. Hence only if its c.m.f.g. is a theorem is our formula matched with a truth of the interpretation.

Yet it is a necessary condition of a formula being a theorem, probably in general and certainly in SI*, that any instance of it be assertable. Hence R (a_i, a_j) is assertable and as such must be matched with a truth. Thus it is a necessary condition of

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*Substitution (b) on page 125 (520P:125) ensures that this holds for all theorems of SI.

R (a_i, a_j) being matched with a truth, that its c.m.g.f. be a theorem, and a necessary condition of that, that R (a_i, a_j) be matched with a truth. The regress is apparent.

One interpretation of this regress could well be that any formula with R as main connective may be postulated in SI without any risk of the resulting system requiring a different interpretation in any detail. On that basis one would expect that the interpretation here dealt with would be quite satisfactory if one added to SI the two postulates

$$p \dashv q$$

$$p \dashv \sim q$$

yet by substitution of any theorem for 'p' one may detach successively 'q' and " $\sim$ q" thus showing the resulting system inconsistent. Thus it would appear that the interpretation here contemplated, while not necessitating that SI be inconsistent, would at least be quite compatible with that.

" $\Box$ p" is true if "p" is a tautology

The previous case is worthy of discussion more because it completes the picture and articulates the odd impression one receives from taking 8.(c) of the summary at face value. By contrast a view which actually seems to have been held is that " $\Box$ p" is to be regarded as assertable if "p" is a tautology. In the specific case of R this amounts to saying that R (a_i, a_j) is assertable if C'(a_i, a_j) is a tautology, where C' is the SI symbol

" $\supset$ ". This case should not be confused with that of the semantic rule on page 61 or the case discussed on pages 79-91 concerning the testing of the transcript of the SI formula resulting when " $\rightarrow$ " is replaced by " $\supset$ ". Here we are concerned with testing by SI value-matrix, not KC value-matrix.

A convenient reference for this sort of approach is the article by E. J. Lemmon, "New Foundations for Lewis Modal Systems". (512K). Lemmon does not attempt to give any account of methods for the construction of a set of characteristic reference-tables for any system of the SI range or for his own system S0.5 at least in the case of SI systems proper it is known that no finite reference-table set can be given*. However he does produce a set of reference-tables for his own system which is not characteristic but does verify all theorems of that system when they are tested by value-matrices. He describes the tables as follows:

> "All tautologies take throughout the value 1 on this matrix interpretation: thus, if α is any tautology, $\underline{L}\alpha$ takes the (designated) value 2 throughout". (512K; 181)

The view which we wish to discuss here is that something on these lines - even if it turns out to need an infinite reference-table for some logical symbols, should be tried on the assumption that it will enable us to carry through the interpretation.

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*This is Dugundji's result (294A). It does not of course show that any particular finite formulae is not decidable (by means of methods such as those discussed in chapter III, for instance).

Lemmon himself makes comments about the interpretation of his system S0.5, saying "We interpret L as a statement-forming operator on statements with the meaning 'it is tauologous by truth-table that' ..... S0.5 contains, for each tautology, the statement that it is a tautology". This may be taken as the background against which one should discuss the interpretation of R (a_i, a_j) in the present case.

For terminological convenience we introduce the inverse of the term "escalation" in the forms "de-escalation" "to de-escalate" and "the de-escalate of", to indicate the process the action and the result of changing an occurrence of " $\rightarrow$ " as principal connective in an SI formula to an occurrence of " $\supset$ " as principal connective in an SI formula. In view of the rules of SI, the term will equally apply to the eliminating of an occurrence of "$\sim\Diamond\sim$" or "$\Box$" at the head of a formula*. In SI, if a formula is assertable, then its de-escalate is assertable.

First of all there is a clear intuitive objection to Lemmon's account of how S0.5 may be interpreted. He says that the system can be seen as making "for each tautology, the statement that it is a tautology". However the notion of tautology which is involved here is that of value-matrix tautology ('truth-table tautology' in Lemmon's terminology), and as has been pointed out above one meets troubles such as the pseudo-proof on page 55 unless we restrict the

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*It may be applied in an extended sense to the elimination of the first of a series of modal operators prefixed to a formula, if that first is "$\sim\Diamond\sim$" or "$\Box$".

description "value-matrix tautology" to formulae which are maximally general. Thus whatever constant formula of S0.5 " β " may be, we know that "Lβ" is not assertable, because if it were there would be an assertable formula of S0.5 whose interpretation was not a truth. Nonetheless S0.5 contains some tautologies and also contains explicitly the rule '(PCb)' which licenses substitution for propositional variables in theorems; thus there is in S0.5 some formula " α " which is a tautology, and for which it will be an assertable formula of S0.5 that "Lα", and further by PCb it is possible to substitute for all variables in "Lα" constants of S0.5, thus giving a formula of the form "Lβ" for a constant formula " β ", which is assertible contrary to our argument above. The only ways out of this dilemma seem to be (a) to deny that assertions to the effect that constant formula are tautologies are false, or (b) to qualify the application of PCb in some way which Lemmon does not contemplate, or (c) to deny that S0.5 contains any constants. All of these courses involve radical alterations in the system Lemmon proposes and purports to interpret and it seems clear that his interpretation is not intuitively adequate.

A reason for these difficulties, and an articulation of the features of Lemmon's interpretation which might well be thought to recur in other proposed interpretations for Lewis systems, can be sought in a more detailed examination of the significance to be given to the assigning of designated values to formulae of the

form."Lα".

The set of stipulations for value-matrices given above lays down one reading of the rows in the matrices; no doubt other readings are possible, but the reading we have given is one which will work for KC and many other logics but will not work for strict implication systems if their matrices are constructed according to the rules Lemmon seems to contemplate. The reading can therefore be made the basis for a differentiation between types of system; having differentiated the types of system, one may then attack the use of matrices such as Lemmon seems to wish, without thereby calling in question the normal procedures of matrix-construction. It is worth noting in support of our interpretation of the rows of a matrix that it chimes in well with the informal comments made in standard introductions of the notion of truth-assignments and truth-tables (e.g. Copi, Symbolic Logic (254A;Ch.2)).

Assuming that a valid value-matrix can be constructed for any formula of S0.5, and that in it the assignment of a designated value to any heading "LE_i" is made in every row if and only if some designated value is assigned to the heading "E_i" in every row, we may consider the specific case where E_i is the formula "α" which contains no modal symbols (i.e. is a transcript of some KC formula). As Lemmon wishes "Lα" is then assigned a designated value in any (and every) row if and only if "α" is a transcript of a KC-tautology (assuming the reference tables for all transcripts of KC symbols

into S0.5 are the same as for the original KC symbols).

According to stipulation 7 the significance of a row of value-assignments is that where V_i is assigned to heading E_i the members of $S^r(E_i)$ are all members of CV_i In the case of "α" we see thus that any m-substitution for the variables in "α" with respect to the values assigned to these variables when they appear as headings in the row concerned, yields a formula which is assertable. One of the important points about this reading is that it is a sufficient condition for a member of $S^r(E_i)$ to be a member of CV_i, that the substitutions made in E_i be m-substitutions with respect to the values assigned to the appropriate variables <u>in that row</u>. Thus in the matrix:

p	q	$q \supset p$	$p \supset (q \supset p)$
1	1	1	1
1	0	1	1
0	1	0	1
0	0	1	1

it is sufficient to determine that the formulae resulting from sets of m-substitutions for "p" and "q" in "$p \supset (q \supset p)$" will be assertable that these m-substitutions be with respect to a pair of values for "p" and "q" which are assigned in some row which assigns the designated value to "$p \supset (q \supset p)$".

This reading of the rows of value-matrices does not work when we introduce modal operators like Lemmon's "L". It is a necessary condition of "Lα " being assigned a designated value that " α " be assigned only designated values in any row of the matrix; it is no longer sufficient to the inclusion of $S^r(E_i)$ in CV_i that the substitutions in E_i which $S^r(E_i)$ deals with should merely be m-substitutions with respect to the values assigned by r to the variables concerned. For instance the matrix may be extended so that it includes a heading " $\Box (p \supset (q \supset p))$" and the column under that heading contains the values V_1, ... V_4 all of which are designated (and which from Lemmon's examples would seem to be likely to be identical). But if we do this it will not be possible to read along row 1 and conclude that any formula resulting from a set of m-substitutions for "p" and "q" in " $\Box (p \supset (q \supset p))$" with respect to the values 1 and 1, will be a member of the class of formulae CV_1.

This is all in a sense profoundly unsurprising. It is presumably the conclusion which Lewis meant to illustrate by his tabulation on page 199. The non-existence of a finite general method for deciding formulae of the Lewis modal systems has been known for long enough to have become almost a defining characteristic of them. But it seems to us that one cannot simply accept this fact without comment; by reworking it in a variety of different contexts, we hope to make it clear that a logic

possessing such a feature is an unsatisfactory logic. We do not thereby wish to rule out a priori any logic which lacks a finite general method for deciding whether certain formulae are assertable, but only ones which in principle lack a finite general method for deciding whether the application of a so-called logical operator to an argument results in a formula which is assertable. Plenty of logics contain predicates, and these are not normally such that it can be decided within the system whether or not the application of a particular predicate to a given argument will yield a formula which is assertable or one which is not; predicate logics are not the object of our criticism. We wish to criticise the notion that a logical operator or constant can be such that (a) the value of the formula resulting from the binding of an expression of known value by that operator is not representable as a function of the value of the bound expression, and (b) no finite general method for determining the value of formulae including the symbol can be formulated. Of these two conditions it seems to us that the former is the more objectionable.

The problem about a matrix of the Lemmon sort is that there seems no reason why the values assigned to the formula "Lα" should appear in the same rows as the values assigned to "α". For one thing, the values assigned to "Lα" are all identical; secondly, they do not represent the value which the application of "L" to $S^r(\alpha)$ would have for that particular row taken singly, and thirdly

the notion of different "cases" of assignments of values to the variables in "Lα " now becomes very blurred when one knows that either all or no substitutions or sets of substitutions will result in assertible formulae. One might just as well write a separate row for the value-assignments to "Lα " and in it assign to the variables the pseudo-value 'U' which is matched with the class W of all constant formulae;

p	p	p.~p	~(p.~p)	□~(p.~p)
1	0	0	1	
0	1	0	1	
U	U			V_1

Rearranging this tabulation slightly one may now simply eliminate the heading "Lα " and write the value assigned to that heading under a line drawn at the foot of the column for " α ", as if that column were to be totalled. Of course, when there is just the one designated value ("1") and there are n rows in the matrix for " α " the value written in this position ("the "Lα " position") will be the integer equal to or immediately less than the result of dividing the sum of the values assigned to " α " by n. That is "Lα " is assigned the integer value equal to or less than $\frac{\Sigma\alpha}{n}$.

Analogously "Mα " might be assigned the integer equal to or immediately greater than $\frac{\Sigma\alpha}{n}$. Writing the matrix in this fashion makes the problem of how to read rows much simpler, but of course brings to the fore the questions which may be posed about

iterated modalities. For when " α " is of the form "p $\dashv$ q" the value assigned to " α " is the L-type value obtained from the column for the de-escalate "p $\supset$ q"; but when " α " is of the form " $\Box$ (p $\dashv$ q)" or "q $\dashv$ (p $\dashv$ q)" it ceases to be very clear where one should place the value assigned to "Lα" or how one should evaluate it. In the light of this it is notable that the area of variation amongst the different systems SI to S5 lies entirely within the field of formulae with iterated modalities, and formulae involving multiple occurrences of modal symbols (such as the " $\dashv$ "s in T-principles).

Constants and Values

Having shown how a Lemmon type of matrix is incompatible with our reading of value-matrices, we go on to argue, as promised, that it is the matrix of Lemmon's type, and with it the system SI which should be rejected rather than our reading of value-matrices.

It will have become obvious that most of the shortcomings found in SI and S0.5 relate to the constants and/or values which may be assigned to variables. A final detailed look at Lewis is now called for in order to determine his view on two questions arising here; (1) What are the constants of SI? and (2) How many and of what significance are the values which may be assigned to SI variables?

The first thing to consider is what Lewis himself says; but this is not always as clear as it should be. On page 123 of

Symbolic Logic (520P) where he starts his description and development of SI, Lewis introduces the "p" "q" etc., in terms of which he formulates postulates and theorems, as symbols for "propositions".

It has always seemed to us that a "proposition" as distinct from a "propositional form" was a constant expression, and thus it would be expected that the symbols which were symbols for propositions would be constants of their system. But of course one does not normally state one's axioms in terms of constants; still, so far as concerns what is said in the succeeding few pages there is nothing to prevent this, and one may take it that Lewis is using "p" and "q" to stand for arbitrary propositions. His statement of the rule of substitution (520P; 125) neatly avoids reference to the status of the symbols in theorems.

However, when one comes to page 127, one meets the claim that 12.1 of Lewis's system ("$\underline{p} \dashv \underline{p}$") is the theorem ""$\underline{p}$ implies $\underline{p}$" or "Every proposition implies itself."" The two renderings which are thus casually put together are by no means identical; "$\underline{p}$ implies $\underline{p}$" is a rendering quite consistent with "$\underline{p}$"s being a constant, while "Every proposition implies itself" certainly is odd as a rendering of a constant formula (albeit composed of arbitrary constants) and is even somewhat strange as a rendering of a formula composed of variables - one would be tempted to suppose it most suitable for rendering "$(\forall p)(p \dashv p)$" (accepting for the moment the use of "$\dashv$"). Such a reading is in line with Lewis's practice in relation to

Existence postulates, for he says on page 181 that "the idea of universality, implicit in the assertion of the principles of previous sections, should be explicitly symbolized" and goes on in fact to prefix "(p)" etc. in the form of universal quantifiers, to the axioms and theorems of the system as so far developed.

The understanding of the axioms and the theorems developed from them, which seems most appropriate, then, is that they are stated in terms of universally quantified propositional variables (except of course for existence postulates), but that from time to time they may be regarded as if they were stated in terms of arbitrary constants. Lewis's English renderings of his theorems cannot really be put into any system; a quite typical example of his practice is the reading of 12.8 ("$\underline{p} \sim \underline{q} . \dashv . \sim (\underline{p} \dashv \underline{q})$") as "If $\underline{p}$ is true and $\underline{q}$ false, then $\underline{p}$ does not imply $\underline{q}$; a true proposition does not imply any which is false.": the last words are general as if to interpret variables, the earlier words specific as if constants were involved, and the "$\sim$" symbol is interpreted indifferently as either an attribution of falsehood to some expression, or the result of inserting 'not' into that expression.

That it is not merely carping to stress the inconsistency and imprecision here, can be seen when one turns to Chapter VIII and considers the use of "$\underline{p}$" and "$\underline{q}$" as metalanguage autonyms. On page 242, as we have noted already, it is suggested that we take "$\underline{p}$" as equal to "Vinegar tastes sour" and "$\underline{q}$" as equal to "Some men have

beards". Taking the symbols in this way must involve either their being abbreviations for the linguistic expressions concerned, or their being autonyms for a pair of particular constants in some system (such that these are the particular constants which would be used to symbolize the linguistic expressions). There is clearly no room for us to suppose that "p" and "q" should be thus and be autonyms for universally quantified variables or for arbitrary constants such as occur in theorems. For to suppose "p" equal to "Vinegar tastes sour" would involve reading 'p → p' into English as "Vinegar tastes sour implies vinegar tastes sour."; yet that English expression while it is perhaps a reading into English of the result of substituting some particular constant for the "p" in the theorem concerned, is certainly not the reading into English of the theorem itself.

In the light of this the doubts which arise over the status of "p" and "q" as they appear in "(p and pIq) I q" (520P; 242) and in "p implies q" (520P; 241) may be seen to be not at all forced or captious.

What seems to be important for the present discussion is (i) that Lewis does envisage SI as having both constants and variables, and that a constant (a particular constant, not an arbitrary one) could be interpreted in such terms as "Vinegar tastes sour", and (ii) that no very clear guidance is available from Lewis on what notation should be used for particular constants to

distinguish them from variables or arbitrary constants.

The difficulties which arise concerning constants of the system can be divided up into (a) those concerning the interpretation and semantic rule formulation, (b) those concerning the reading of value-matrices in terms of equivalence classes of constant formulae, and (c) those concerning the use of constants in symbolizing set subject-matter by means of SI.

Regarding (a) it need only be noted that this consideration of constants and variables as Lewis treats them, does not conflict in any way with the approach adopted earlier to the formulation of the semantic rule. We introduced the range of metalanguage constants "a_1", ... and the corresponding metalanguage variables "a_i" and "a_j" as designating the constants of SI; by this was meant particular constants and not arbitrary ones. Thus in interpreting "$\underline{p} \dashv \underline{p}$", (theorem 12.1) by means of any semantic rule so far developed it is necessary to consider the cases generated by substituting for the occurrences of 'p' actual particular constants of the system; the numerical values of "i" and "j" which appear as subscripts to "a" are thus correlated with particular "propositions" in Lewis's terms, and these will be such as he would render in English by utterances like "Vinegar tastes sour".

It seems to be implicit in this procedure that the various formulae which result from substitutions of particular constants for

the "p", "q" etc. in theorems, are assertable in the system. The assumption that this is so, has been made several times in the derivation of paradoxical results about SI, always in relation to substitutions in theorems (or putative theorems) involving modal symbols; there is in the present discussion nothing to indicate that we should drop this assumption, in the absence of which any special attention to provability in SI would become pointless from the point of view of application of the system.

This assumption is particularly relevant in the reading of value-matrices; but what Lewis says in the text is insufficiently explicit for us to do more than infer his view. He formulates the system in such a way that it is apparently able to deal with particular constants, and whether the question is framed in terms of the reading of value-matrices as we have framed it, or is put more straightforwardly, it is clearly in order to ask what uniform procedure there is for deciding the value to be assigned to a constant expression involving modal symbols. Lewis's disavowal of the intention of giving, in SI, a system which has a decision procedure in terms of value-matrices, cannot absolve him from such questions, because it is always possible to pose the question of how one is to determine in general whether a constant formula is a member of the class of assertable constant formulae.

The third difficulty which arises is one not so far discussed. None the less it is of considerable importance in relation to any

actual use of the Lewis systems since it concerns the concrete results which are to be expected of attempts to use these systems to symbolize actual pieces of argumentation.

For Lewis the unsatisfactoriness of KC lies in the way it symbolizes deducibility and implication. He rightly points out that there is a difference between a case where one assertion is deducible from another, and a case where the former is merely not false if the latter is true. Believing that KC attempts to symbolize both these cases and that it succeeds only by confusing them, he formulates SI and proposes it as a calculus which is immune from the attack he has framed.

To examine the claim that SI is superior to KC in this way, let us consider some specific argument which it is desired to symbolize, and let us compare the symbolization of it in KC with that in SI. What is in point is the difference between deducibility and the other relation which for brevity we shall call "material implication". Suppose this context to contain genuine examples of both relations, for example by the assertion J being deducible from the assertion H while the assertion K is only materially implied by H.

Now the first observation which should be made is that so long as the argument contains only the assertions H, J, K and no other assertions which are composites including one of these, there can be no difference between the two methods of symbolization. This is clear enough since these assertions are all either true or false,

and in the light of our judgement on this we choose whether to represent them by assertable or non-assertable constants when we produce the symbolization. Whether we are dealing with the KC symbolization or the SI symbolization, it is clear that when we use an assertable constant to symbolize H, then we must also (if we accept the outlined relationships amongst H, J, K) use assertable constants to symbolize both J and K.

The contrast between SI and KC only arises when we consider the use of complex formulae to symbolize assertions. Thus it is of interest to consider the symbolizations which would be given for the assertions which state that J is deducible from H and that H materially implies K; supposing that the SI symbolizations of H, J, K respectively are "X", "Y", "Z", these assertions will be symbolized as "$X \dashv Y$" and "$X \supset Z$". Lewis's complaint against KC is that the symbolization in KC would use the same connective for each of those assertions; thus (using the device of underlining a formula to give us the KC formula of which the formula underlined is the transcript)* the KC symbolizations would be "$\underline{X} \supseteq \underline{Y}$" and "$\underline{X} \supseteq \underline{Z}$".

Accepting for the moment that transcription is valid in so far as it is used in this example, the question which arises is what practical advantage there can be in using the SI symbolizations in preference to the KC symbolizations. Immediately we may rule out the most normal sort of difference that might exist between two symbolizations - viz. that one or other of them allows the deduction

*and using "$\supseteq$" as the KC connective usually written simply as "$\supset$".

(according to the rules of the system concerned) of a formula whose interpretation is not among the interpretations of formulae deducible in the other symbolization. This is ruled out because Lewis explicitly states that no deduction according to the rules of KC is invalid, and he therefore incorporates in SI the facility for making any deduction in KC (i.e. for deducing the transcript of any formula deducible in KC): on the other hand " $\dashrightarrow$ " in SI is allegedly more strict than " $\supset$ " in KC, so that wherever a formula having " $\dashrightarrow$ " as main connective is deducible in SI, the corresponding formula of the same structure with " $\underline{\supset}$ " as main connective is deducible in KC (i.e. that formula of which the de-escalate of the SI formula is the transcript, is provable in KC).

Bizarre as it may seem to postulate a "difference" which makes no difference with regard to what is derivable, this seems to be what Lewis wishes. The use of "X $\dashrightarrow$ Y" instead of "$\underline{X} \supset \underline{Y}$" as the symbolic representation of the assertion that $\underline{J}$ is deducible from $\underline{H}$ does not allow us to prove formulae whose interpretations are in any way different, except as regards the reading of " $\dashrightarrow$ " back into English. This one factor of how one reads the connectives back into English when interpreting the formulae of a symbolization, seems to be all that is involved in Lewis's complaint.

It is certainly true that we shall not get very good results if we symbolize the material implication from H to K by the use of " $\underline{\supset}$ " and then, in pursuance of a policy of interpreting " $\underline{\supset}$ " in

terms of the use of the word "deducible", interpret the resulting formula by the use of an assertion that K is deducible from H; yet it may well be that the use of "deducible" would not be undesirable when interpreting the formula used to symbolize the deducibility of J from H. But that presupposes some policy of using "deducible" in the assertions which we employ to interpret formulae which contain " $\supseteq$ "; and that is a policy which would have to be changed should it lead to such an undesirable consequence.

Lewis seems to assume that just because KC can be used for symbolizing relations which are in fact deducibility relations, it is - in those cases - being used to symbolize them as deducibility relations; but the very fact that one cannot always interpret " $\supseteq$ " in terms of "deducible" indicates that this is not so. The mistake is as basic as that of assuming that because the conjunction operator sometimes occurs in formulae which are used to symbolize assertions involving "but", it follows that the conjunction sign is used to symbolize "but" in the sense that it is used to symbolize the joining of two assertions by "but" rather than in any other way. The fact that such an assumption is not justified should not lead us to introduce into KC a new symbol which does symbolize the joining of two assertions by "but", but should lead us to revise the assumption.

But suppose we are specially interested in the joining of assertions in a "but"-like way. We may well have a legitimate wish to distinguish this sort of conjunction from others which in KC are

symbolized by the conjunction sign. If this is so then we shall certainly wish to devise a calculus with a separate "but"-conjunction-forming operator. However our task will not be complete when we have merely written this wish down on paper; for unless we can specify some difference in the way "but" behaves as compared with other conjunctions we shall be without sufficient reason for making the distinction - and if we make the distinction by introducing a new symbol, yet do not have as axioms any formulae in which the new symbol cannot be replaced by the old conjunction-forming operator, then we shall not have succeeded in introducing the difference concerned into the newly devised system. If the system is properly to symbolize "but" as opposed to the other conjunctions then it must be such that formulae derivable* from the (putative) "but"-symbolizing formulae are not in all cases identical with the formulae derivable from symbolizations of assertions which use other conjunctions in place of "but".

Thus SI seems bound to have inadequate resources for symbolizing deducibility as such, in that it does not differ enough from KC. If all formulae of SI (except Existence theorems) are de-escalated**, we have a system in which there are no provable formulae whatever which differ from theorems of KC (typographically). The formulae

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*whether as theorems, instances of theorems, or merely by means of the rules of the system as applied to some specific assumption.

**assuming SI to be written in terms of ' $\dashv$ ' and ' $\Box$ ' as sole modal symbols.

of SI which may be derived from the symbolization "X $\dashrightarrow$ Y" will in no case differ from the consequences derivable in KC from the symbolization "$\underline{X} \supseteq \underline{Y}$" in any respect other than the prefixing of " $\Box$ " or the appearance of " $\dashrightarrow$ " in place of " $\supset$ " in some formula which is the transcript of "$\underline{X} \supseteq \underline{Y}$" or one of its consequences. If the interpretation which we give to KC is such as to take account of the fact that in some cases the relation dealt with in a sentence or assertion symbolized by a formula involving " $\supseteq$ " is a deducibility relation, then that interpretation will in all cases provide a reading of the consequences of "$\underline{X} \supseteq \underline{Y}$" which covers the case of any reading of a consequence of "X $\dashrightarrow$ Y" provided by the interpretation of SI. In this sense one may say that the facilities which SI provides for symbolizing deducibility give no scope for us to contrast deducibility with mere material implication, or to draw out any consequence of deducibility which cannot be drawn out by the KC symbolization.

Particular "$\underline{p}$" and "$\underline{q}$"

Faced with this situation one is tempted to go over the ground again to see whether Lewis is in fact trying to get at something quite different. But it is certain that we have not misrepresented him by taking his own example of equating "$\underline{p}$" with "Vinegar tastes sour", and that example fits admirably into the discussion just set out - e.g. take $\underline{H}$ as "Vinegar tastes sour", $\underline{J}$ as "Vinegar does not taste sweet", and $\underline{K}$ as "Some men have beards". Whatever the

difficulties, Lewis is committed to the view that SI somehow helps us to deal with the symbolization of particular assertions; his claims on pages 261 and 262 seem particularly sweeping, although they come mainly in the form of attacks on KC, leaving it to implication that SI escapes such attack.

The attack which Lewis formulates on pages 261-2 is however of some interest. It is that "the only case in which any truth-implication is likely to have any value in application, as the basis of inference, is the case in which it coincides with strict implication". Here is an outright admission of what we have argued above - that the " $\dashv$ " symbol of SI adds nothing to our resources for symbolization, and that the time spent verifying that a given case is one for " $\dashv$ " rather than " $\supset$ " is time wasted since nothing extra can be derived in the way of consequences. Nor, of course, can anything less be derived since we have the rules of adjunction, detachment and the theorem 14.29 to allow us to prove anything (on Lewis's terms at any rate) that can be proved in KC. Lewis as good as admits that SI allows us to prove no more and no less than KC.

The basis of Lewis's claims for SI and of his claims against KC seems to lie in the truth-conditions for the assertions which he matches with assertable formulae of the calculi. Thus on pages 261-2 he is not at all concerned with what we can deduce in the axiomatised systems, but only with the "ways ... in which this

implication-relation can be known to hold". Throughout this passage he is considering the constituents which might go to make up some assertion of the existence of an implication-relation (i.e. the things one of which is said to imply the other); the basis of the argument is the consideration of the knowledge which will be sufficient for us to verify the assertion in the case of a given pair of constituents.

The allegation against KC is that it is a doctrine of that system that we can verify the assertion of an implication-relation's holding of two things, as soon as we know only that the former is false or the latter true. Such an allegation is of course entirely beside the point since KC is a logistic calculus and as such implies no doctrines about verification. Even when we come to interpret KC, the arguments which Lewis has brought forward need only warn us not to interpret formulae involving " $\supset$ " in terms of assertions about implication-relations.

What Lewis wishes is a calculus which will have an interpretation in terms of such assertions about implication relations. His strictures on KC lead him to identify this goal with the goal of laying down "stricter" conditions for saying that we have verified an assertion about implication-relations. The degree of strictness which he wishes in this set of truth-conditions is, not unnaturally, that the implication-relations alleged to hold, do in fact hold. We should note that the imposition of such truth-conditions merely

rules out the use of such assertions about implication-relations when interpreting KC; it does not carry any implications whatever about logistic calculi.

Yet the argument for a strict set of truth conditions takes place against the background of KC and its assignment of the designated value to "$p \supset q$" in case "p" is assigned the undesignated value or "q" the designated one. Lewis manages to confuse the truth-conditions for the (linguistic) assertion that one thing implies another, with the conditions for the assignment of a designated value to a formula of some calculus; hence he is led to seek some rules for the assignment to a formula in his idealised calculus, of some designated value, and to identify the rules he finds to lay down truth-conditions with rules for assignment of values to a formula.

At this point there enters a difficulty. The rules laying down the truth conditions for a given assertion are not absolute for all contexts (cf. Chapters V and VII). If we take the assertion that "Vinegar tastes sour" implies that vinegar does not taste sweet, then the truth-conditions for this will probably involve us with psychologistic and physiological theories about taste. The only general remark which can be made about such an assertion is that it will be verified if the alleged implication does in fact hold. But there is no general method for predicting - independent of knowing the subject matter of the things between which the

implication supposedly holds - whether or not the implication does hold.

Because Lewis is thinking in terms of conditions for the assignment of values to formulae and not in terms of truth-conditions for particular assertions, he passes unnoticing from talking about how we would know that a given assertion about implication relations (such as the assertion that H implies J, to recall the earlier example) is true, to talking about how we would know that "a proposition" (in general) did truly imply another, and thence to the enumeration or attempted enumeration of the different sorts of proposition which might be involved, and the question of whether an assertion of that form with those sorts of proposition involved, would - in general - be true. But this last problem is a matter which concerns propositional forms; just as it involves variables when framed in formal terms as the problem of what value to assign to a formula whose variables have been assigned such and such values, so in informal terms it is a matter of the form into which we arrange the constituents of our assertion - it is not a matter at all of the assessment of a particular assertion, but is rather a matter of theorising about the effects on classes of assertions of various operations on their members.

If an assertion of the existence of an implication is to be a true assertion when the implication does in fact exist, then Lewis is faced with the problem of finding an account of when an

implication (in general) does in fact exist. The theory of when certain, "formal" implications do in fact exist, is usually called "logic", and the logic which Lewis - perhaps surprisingly - chooses to enshrine in his new system is KC itself, for he remarks that it never leads to the derivation of a false conclusion from true premisses. It is strange that at this level he realises that the implications with which KC deals are those amongst the premisses and conclusions of proofs, yet at the initial level imagines that the implications dealt with are those which are sometimes (and mistakenly) used as interpretations for particular formulae of KC. It thus happens that Lewis constructs a calculus in which the criterion for the assignment of a designated value to some formula is that it be a theorem of KC, or the result of prefixing " $\Box$ " to a theorem of KC or else the result of joining together by " $\dashv$ " two formulae such that either the latter is a KC theorem or the former is the negation of a KC theorem.

Such a logic can be seen, as SI has often been seen, as a metalogic for KC, dealing with the interrelations of KC formulae (cf. our discussion of Lemmon (96ff)). Its defect in this respect is that the rules of substitution are not clear enough to lay down that the "p" in " $\Box$ p" is a KC formula rather than a formula of SI itself; in this respect one might call SI ambiguous in respect of the language to which the arguments of modal symbols belong - a property we might call "level-ambiguity".

Whether or not such a logic is of intrinsic interest, it involves difficulties which are particularly crucial when we consider the nature of the constants in the system. It has been suggested that Lewis in seeking for a systematic way of determining whether one "thing" implies another, and in effect treats different KC formulae as the values which may be assigned to the variables of the SI formula R (a_i, a_j), so that he can lay down as the criterion for the assignment to R (a_i, a_j) of a designated value, the condition that the formula corresponding to a_j be derivable in KC from that corresponding to a_i The derivability concerned is however of maximally general formulae and not of constant expressions of the system; were we to use the latter the system would immediately collapse into KC. Thus the R (a_i, a_j) is doubly general in that it itself has as instances the various particular cases corresponding to different i and j's, and these particular cases are themselves general formulae which in turn may have various cases to be considered.

It would be pleasant if matters ended there and if the cases of the general formulae which correspond to particular values of a_i and a_j were of the same sort as the cases of the "p" and "q" in the general formula of KC "$p \supset q$"; but one cannot be sure even of that because it remains to be settled whether or not the a_i and a_j may have as particular values formulae which themselves involve modal symbols - and thus are to be taken as formulae of SI itself. There is the possibility at least of an indefinite regress; the regress

is of course only indefinite since any R (a_i, a_j) must be of finite length in order to qualify as a well-formed formula, and hence only a finite number of levels need be involved; however the set of "cases" or values assigned to a_i and a_j as arguments of R at any given level need not be finite unless in some way the number of well-formed formulae in the KC calculus is restricted to a finite number.

Returning now finally to the nature of the constants which may occur in SI and to the way in which we might use them to symbolize particular assertions, we recall the interpretation given earlier to the assignment of values to KC formula by means of value-matrices. In the reading given there we pointed out that the values assigned could be seen as "implementing" a reference table which laid down for the binding of a pair of arguments by an operator, which equivalence class the resulting formulae would belong to given various equivalence classes from which the arguments might be drawn. In the case of " $\rightarrow$ " no such reference table in the normal form was available. The bearing of that reading of the value-matrices on the present case was that it gave a way of relating value-matrices to constant formulae of the system concerned, and that we may now be able to see the place of the constants in SI by considering the type of semantic rule developed and the corresponding rule for assigning a designated value to an SI formula.

There is in KC a class of assertable constant formulae; we have associated this class with the value-assignment "1" in the value-

matrices for KC. We may suppose that there is similarly for SI a class of assertable constant formulae (for without it we have no scope whatever for symbolizing particular assertions in SI). Now suppose that some of these assertable constant formulae contain the connective " $\rightarrow$ ". Let "F" be the corresponding maximally general formula for one such constant formula which contains an occurrence of " $\rightarrow$ ". By the analogy with KC there ought to be a row of the value matrix for "F" in which the designated value is assigned to "F" and the values assigned to the arguments of " $\rightarrow$ " in "F" are such as to indicate the equivalence classes to which those arguments of " $\rightarrow$ " in the particular constant formula under discussion fall. However, as has already been suggested, this cannot be so; for the assignments which can be made to the arguments of " $\rightarrow$ " in a maximally general formula are assignments of KC-formulae and not of values in the normal sense, thus the instances of "F" are not constant formulae at all but are themselves general formulae, thus contradicting the hypothesis that "F" is the c.m.g.f. of some constant formula containing " $\rightarrow$ ".

The reactions to this contradiction are varied. One may simply deny that there are any constant formulae containing occurrences of " $\rightarrow$ " which are ever assertable. One may lay down simply as a <u>fiat</u> and with disregard for the reading of matrices - or for any role in this context for the device of matrices - that the class of assertable constant formulae consists of all and only

those expressions which result from the replacement in theorems of SI of variables by members of the class "G" of constant atomic formulae and certain constructs out of them by means of the operators of the system. Thirdly one may attempt to form equivalence classes of the formulae of KC so that one may class together all assignments of KC formulae to the arguments of " $\dashrightarrow$ " under a certain finite number of heads. An example of this is the case cited by Lewis himself in Appendix III (1959 edition) where - in an almost unique admission of the sense of assigning values to SI formulae - he says:

> "Strict Implication escapes the paradoxes of the truth-value logic of propositions because, by the different interpretation of 0 (= p $\sim$p) and 1 (= p v $\sim$p) and by repudiating the restriction of propositions to these two values, it also escapes restriction to the four alternatives tabulated above for the relation of material implication" (sc. the assignments to "p $\supset$ q" for various values of "p" and "q")." (520P; 511).

In this way Lewis introduces a way of doing something like the assignment of values to the arguments of " $\dashrightarrow$ ", for in terms of these interpretations of "0" and "1" we can at least write down the following value reference table:

$\dashrightarrow$	1	0
1	1	0
0	1	1

(= q)

(= p)

However this value reference table is incomplete if we allow the assignment to the "p" and "q" of formulae which are not equivalent

either to "p $\sim$p" or "p v $\sim$ p". One can still see that SI could be used in this way as being a KC-type metalanguage applicable to the class of formulae which are either theorems of KC or the negations of these, (since the negation operator " $\sim$ " forms theorems from contradictions and vice-versa in KC).

At this point it is time to leave the consideration of Lewis's presentation of SI, and the interpretation of the system as he has constructed it. The remainder of this part will be devoted to the consideration of the later formulations of Lewis systems, so as to show the generality of the difficulties which arise in trying to decide how to interpret SI or how to use it in the symbolization of particular arguments.

CHAPTER III

LEWIS-LIKE SYSTEMS

Having considered both Lewis's presentation of his systems and the prospects for devising a logic answering to what he feels the needs to be, we turn to a brief survey of significant aspects of the work done by others to interpret systems of the type outlined in Symbolic Logic (520P). What is presented here is by no means intended as a complete account of the vast range of work done on Lewis systems in the last 25 years (much of this is to be found in the works referred to in our Bibliography); it is more an attempt to highlight developments and new approaches while giving comment on the philosophical implications of different formulations and models for SI systems.

It is the aim of this Chapter to show on the basis of a wide section of examples that the difficulties found in Lewis's own presentation recur in other forms in later presentations even though these are far more rigorous than Lewis's and despite the ingenuity with which they have been devised. The particular authors we shall consider are J. C. C. McKinsey, H. S. Leonard, S. A. Kripke and J. Hintikka, and E. W. Beth and J. J. F. Nieland. The discussion of E. J. Lemmon's system S0.5 which is given in Chapter II can however also be seen as part of the present argument, to which it provides a suitable prologue.

J. C. C. McKinsey's "Syntactical Construction"

In 1945 J. C. C. McKinsey published a development of a Lewis-type system of modal logic, which attempted to give a more precise account than Lewis had been able to do, of the truth conditions for the sentences or propositions which are used to interpret propositional constants of the system.[1] His presentation consists of a set of statements about the relationship of the notations of the system to a universe of "sentences" which is taken as a paradigm interpretation of the system. The containment of KC in the system is handled by first presenting KC in this manner, and then proceeding to use an extension* of the same universe of sentences when defining the notations of the modal system and doing so in such a way that the sentences interpreting the designated expressions of the KC system are a subset of those interpreting the designated expressions of the modal system. McKinsey begins by presenting a system in which only constants occur, and later defines a system employing variables on the basis of the modal system of constant expressions.

McKinsey's article is of particular significance because, although close to Lewis's own ideas on the interpretation of the systems, it avoids the obscurities of his presentation and therefore gives a statement of one type of interpretation for a Lewis-type system in such a way that we are able to discuss it without an

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*Precisely how the definition of modal terms is to be understood is one of our main points of discussion here; the impression is certainly given that some universe of sentences is extended.

[1](547L)

elaborate system of suppositions and counter-suppositions about the author's meaning.

We shall not concern ourselves in great detail with the technicalities of McKinsey's "construction" of a modal system in which he claims all theorems of S4 can be proved. It is however worthwhile establishing that his procedure is one of defining the notations of the system "in use" by means of statements about a paradigm interpretation. Further we shall discuss the significance of Tarski's reported criticism of the "syntactical construction", and the adequacy of any interpretation such as that implicitly proposed by McKinsey.

McKinsey sets out to deal with sentences among which are some which we can qualify as being "possible". This attribute he defines by saying that to say a sentence is possible means that there exists a true sentence of the same form. The notion of "same form" then requires explanation, but McKinsey says merely that two sentences are to be regarded as being of the same form when one can be obtained from the other by substitution for permitted terms; he admits frankly that there are philosophical problems in specifying the matter further, and proceeds on the assumption that the philosophical difficulties in defining the permitted terms and the rules of substitution can be solved.

The first step in the construction deals with formulae of the KC type. "L" and "T" are defined as two arbitrary sets of

sentences subject to:

P1: if α is a sentence of L, then $\sim\alpha$ is a sentence of L.

P2: if α and β are sentences of L, then $\alpha.\beta$ is a sentence of L.

P3: T is a subset of L.

P4: if α is any sentence of L, then $\sim\alpha$ is in T if and only if α is not in T.

P5: if α and β are in L, then $\alpha.\beta$ is in T if and only if both α and β are in T.

Although McKinsey never says that by "sentence" he means anything other than an utterance such as the logically unsophisticated layman utters in his "ordinary language" activities, we might be tempted to suspect that he is contemplating a set of sentences*, L, one of which might be for example "~A". As an alternative to this reading, one must take him as saying not that "~A" is a member of the set L, but that "~A" designates or denotes** a member of that set. Further evidence allowing us to decide between these alternatives is needed.

Before proceeding to discuss the set of sentences in terms of which the use of the modal symbol " ◊ " is explained, McKinsey familiarises us with the set, S, of "substitutions" by defining their application to the set L. The set, S, consists of the substitutions "s_1", ..., and if "s_n" is any element of S, then "$s_n(\alpha)$." is the

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*We shall here, somewhat arbitrarily opt for this term in view of its echo of the term "notation" which is used to talk about the symbolic formulas which McKinsey is trying to habilitate by means of this 'construction'.

**In the manner of Carnap : Meaning and Necessity (207K).

sentence of L which results from " α " by the application of "s_n". The following identities are stated:

A1: $s_n(\sim\alpha) = \sim s_n(\alpha)$

A2: $s_n(\alpha.\beta) = s_n(\alpha).s_n(\beta)$

A3: $s_1(\alpha) = \alpha$

A4: $s_t(\alpha) = s_n\, s_m(\alpha)$.

Presented with this, there is little difficulty in concluding in favour of the former of the two alternative views of McKinsey's intention (indeed the simpler view), viz. that the symbolic expressions which occur in statements such as P1 and A1 are not autonymous occurrences of members of the set L, but are various means of denoting the members of L (or variable expressions for such means of denoting members of L).

The construction proceeds farther by defining sets L_1 and T_1, where L_1 is the intersection of sets K such that:

B1: L is a subset of K.

B2: if α is in K, then $\sim\alpha$ and $\Diamond\alpha$ are in K.

B3: if α and β are in K, then $\alpha.\beta$ is in K.

It is thus clear that the sets K, and hence L_1 contain at least a subset of ordinary language sentences. In addition to the previously introduced notations we now meet the symbol' $\Diamond$ ' used as a unary operator on expressions which designate ordinary language sentences. The notation '$\Diamond\alpha$' occurs here just as '$\sim\alpha$' occurred previously in a characterisation of a set by means of references to

its members; the member referred to by '$\Diamond\alpha$' - or by '$\sim\alpha$' for that matter - is not described in any way, and it does not follow from what is here said that it should have any particular formal or semantic attributes such as involving the phrase 'It is possible that' or constituting a "modal assertion". Neither '$\Diamond\alpha$' nor '$\sim\alpha$' need in fact have any semantic relationship to 'α' itself; all that is determined by the postulates is class membership, and so long as we can identify the sentences referred to by these notations by other means there will be no reason to invoke such semantic notions as "negation of 'α'" or "assertion that 'α' is possible". However, let us complete the description of the system before drawing conclusions from this.

McKinsey next states rules corresponding to A1 and A2 above for the application of substitutions to the sentences of L_1 and a further similar rule for the substitutions on a sentence described as '$\Diamond\alpha$':

C1: $s_n(\Diamond\alpha) = \Diamond s_n(\alpha)$.

The set T_1 is then defined by the following postulates:

D1: T_1 is a subset of L_1.

D2: T is a subset of T_1.

D3: if α is a sentence of L_1, then $\sim\alpha$ is a sentence of T_1 if and only if α is not a member of T_1.

D4: if α and β are in L_1, then $\alpha.\beta$ is in T_1 if and only if both α and β are in T_1.

D5: if α is a sentence of L_1, then $\Diamond\alpha$ is in T_1 if and only if there is some s_n such that $s_n(\alpha)$ is in T_1.

With these postulates McKinsey's basic system is complete; it involves only constants according to his view, and this makes it difficult to compare with other systems, so he later proceeds to define a system employing logical variables. In this latter there is a set L_2 consisting of formulae and certain of these are members of the set T_2 in accordance with the provision:

> "if $\alpha(p_1, \ldots p_r)$ which involves only the variables $p_1, \ldots p_r$ is in L_2, then $\alpha(p_1, \ldots p_r)$ is in T_2 if and only if $\alpha(\alpha_1, \ldots \alpha_r)$ is in T_1 for all sequences of $\alpha_1, \ldots \alpha_r$".

This variable-possessing system is then taken to be such that every provable formula of it yields a formula of T_1 when constants are put in place of the sentential variables.

It is D5 which must receive our attention if we are to consider this presentation of the Lewis type of modal system. The analogy between '$\Diamond\alpha$' and '$\sim\alpha$' has already been remarked upon; if this analogy is to stand the two notations will also have to refer to the sentences of L_1 in an analogous way. This is however not the case. When we refer to a sentence by means of '$\sim\alpha$' we refer to it in such a way that whenever one knows the characteristics of 'α' one knows all the relevant characteristics of the sentence referred to: we refer to this sentence as a function of the sentence 'α'. But a sentence to which we refer as '$\Diamond\alpha$' cannot in all

cases be fully characterised (at least as far as those characteristics relevant for the system are concerned) simply on the basis of the information we can derive from knowing the sentence referred to as ' α '. If ' α ' is a member of T_1 then of course by virtue of A3 and D5 ' $\Diamond\alpha$ ' is also a member of T_1, but should ' α ' not be a member of T_1 we have no means of knowing whether ' $\Diamond\alpha$ ' is or is not a member.

This aspect of ' $\Diamond$ ' should not be taken as showing that it has the welcome property of giving us more freedom when we symbolize some sentences. In virtue of the rules for ' $\sim$ ' every sentence which we symbolize in this system must be determined as either being or not being a member of T_1; this choice cannot be evaded or delayed, and indeed since we are reading McKinsey as talking about the sets of ordinary language sentences L_1 and T_1 there is no question about this matter. What is involved here is not the symbolization of some known sentence in such a way as not to lay down ab initio whether it is true or false, but rather the description of "a sentence" in such a way that it is not clear whether it is true or not - in a system where the truth or falsity of the sentences is their primary differentiating characteristic.

What D5 in fact says is that the sentence S is a member of T_1 if and only if it itself or the sentence which results from applying any permitted substitution to it, is a member of T_1. If the sentence is itself a member of T_1 then the specification ' $\Diamond\alpha$ ' can apply to the same sentence as ' α ' itself applies to, and certainly as far as all

characteristics relevant to the system are concerned the sentence to which ' $\Diamond\alpha$ ' applies will be indistinguishable from that to which ' α ' applies. But otherwise the sentence to which ' $\Diamond\alpha$ ' applies will have characteristics the same as either ' $\sim\alpha$ ' or ' α ' and which of these two is the case can only be decided if we consider the effect of all possible substitutions on ' α '. It seems then that ' $\Diamond\alpha$ ' as introduced by McKinsey is equivalent to ' α v $s_2(\alpha)$ ... v ... $s_n(\alpha)$ ' where s_1 to s_n are all the substitutions defined on the sentences by the system and where 'v' is introduced in accordance with the usual rules. The question which naturally arises is whether there is any sort of sentence which it would be convenient to symbolize by this sort of expression.

This question, we shall return to later (p.138); in the meantime let us deal with one objection to our presentation of McKinsey's formal argument, and with the objection Tarski is reported to have made to the whole syntactic construction.

It may be felt that notwithstanding the clear reference of the talk about substitution at the beginning of the article to substitution in sentences of ordinary language, McKinsey, at least when he is talking of L_1 and T_1, is talking about sets of formulae. Such a view gains some support from the way in which the instances of formulae in T_2 are taken to <u>be</u> rather than to <u>designate</u> members of T_1. Such a view does not cause very much alteration to our problem about the system. We no longer have a problem because of a description or

designation, '$\Diamond\alpha$' which does not always refer to any adequately determinate sentence, but instead we have a formula '$\Diamond\alpha$' which is sometimes of a truth value known only as being equivalent to that of another formula. The admission into a formal system of an operator which is not value determinate is no easier to justify than the references to sentences by means of obscure symbolisms; the problem presented turns out to be exactly the same, since when we have a formula whose value must be either true or false but is not a simple function of its well-formed parts we are led to the problem of when we should ever have reason to use such a formula to symbolize a sentence.

The syntactic construction which McKinsey gives in his article can be regarded in two ways. One of these is as an attempt to reach a Lewis-type system from another starting point showing how the system complete with variables and theorems is related to constant expressions which can be directly mapped onto sentences of ordinary language. The second is as an attempt to make the abstract system more understandable by relating it to concepts of a more familiar type, which are suggested to be basic to the interpretation of the system. It is however futile to argue over these two descriptions of McKinsey's goal as if they were mutually exclusive; in giving a new construction for a system one starts from points which one expects to be interesting and illuminating - hoping that the reader will thereby find the constructed system intuitively more attractive; in

giving an interpretation to a system one focusses on a universe an abstraction from whose characteristics could provide a basis for constructing the system - and one shows the relationship between formulae and symbolizanda in accordance with some paradigm for the basing of construction of a logical system upon intuitively attractive atomic parts.

We regard McKinsey's purpose in writing as he does as being to pick upon one interesting way of generating a system with the same type of properties as the Lewis systems, and thus illuminate a possible interpretation of these systems. Our objection will be that the basis chosen is not interesting in the way that interests us, and that as a result the interpretation which is implicitly described is not the one we are seeking - namely one which shows the Lewis systems as peculiarly or even at all suited for symbolizing ordinary language talk about the necessity and possibility of things. However McKinsey has also been attacked by Tarski, and seems himself to feel that the attack is a telling one; how telling it is depends on just how high McKinsey's aims were, but it seems that if they were no higher than we have just outlined he need not find Tarski's objection damaging.

Tarski's point is reported as being that it is possible that the language being discussed should turn out to have insufficient substitutions. This could happen, for example, because there were insufficient "descriptive" terms which one could interchange in the

course of substitutions. The alleged objectionable consequence of this is that '$\Diamond \alpha$' would become false in a trivial way because no substitution for 'α' exists let alone being a member of T_1; and similarly '$\sim \Diamond \sim \alpha$' could become trivially true.

The argument of McKinsey's article, however, does not seem to be vulnerable to such an attack. It would only be a telling objection if there were no other means of showing the systems concerned to be suitable for use in symbolizing utterances of ordinary language.

Thus although it is argued here that the notion of substitutions is an inadequate basis for the construction of a general theory of, and ultimately a logic of, possibility, it is perfectly plain that there do exist certain sets of utterances which have interrelations such as those McKinsey is thinking of. Thus in a closed set of utterances about chemical elements which included only sentences of the form "x is a gas", where the name of a chemical element replaces "x", plus sentences used to assert members of the latter group to be logically possible, one might well find that the substitution of the names of chemical elements, one for any other, was a "permissible substitution" which made this set of utterances suitable for interpreting the logic of which McKinsey is speaking.

The inadequcy of an attack on McKinsey which points to the possibility of there being sets of sentences for which no suitable "permissible substitutions" can be found, is that McKinsey does not

claim that no such set exists. Although he uses the jargon of set theory, he is clearly referring to the whole corpus of ordinary language sentences. As we have pointed out, it is only on this assumption that he has a reasonable argument in the "syntactical construction", for unless he shows that a familiar set of sentences has properties which allow it to be used as an interpretation of S4 or other systems, and that this familiar set can be used to illustrate the hitherto suspect " ◊ " operator, then all he can hope to achieve is the formulation of a modal logic with the help of statements about a quite arbitrary abstract model for it. If what is said about sentences and substitutions is correct, and if we can recognise in the set of sentences talked of, some set of things which we judge it to be important to be able to symbolize, then a case for the modal logic has been made. This case cannot be destroyed by saying, in effect, that there may exists other sets of sentences (whether or not we should like to symbolize them) for which no substitutions could be specified which allowed us to interpret " ◊ " in terms of possibility*.

In order to show the inadequacy of S4 symbolizing our normal talk to the effect that various things are possible, we have to show that the type of substitution system which McKinsey supposes would

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*An analogous attack on McKinsey's construction of KC via the sets L and T could be made by supposing that there might happen not to be any false sentences; this would be just as ineffective an attack on KC as is that discussed.

in fact be unsuitable for a theory of possibility, and that the philosophical "solution" he thinks can be found, cannot in fact be found. Such an argument achieves out purpose here; it does not however finally show S4 to be useless, for there remain other fields in which an interpretation may be found, and the theoretical possibility of other sets of rules for interpreting it in terms of ordinary language can never be completely ruled out. What we are concerned to contest is that there is - and McKinsey has discovered - in ordinary language a set of foundations which are architect-designed for the construction of a logical superstructure such as S4.

The keystone of the syntactical construction is the doctrine that a sentence may be said to be possible if there is another sentence of the same form which is true. The particular explanation of "form" given by McKinsey, is considered here; the more general notion of "logical form" could also be substituted, but without making the argument more adequate*.

Two sentences are of the same form if one can be generated from the other by means of a permitted substitution; this is the doctrine we have to judge. Let us take some examples:

1. The Radcliffe Camera is a square cupola.

2. The Radcliffe Camera is a stone building.

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*The notion of logical form is considered in Chapter V. We take the view that the logical form of an utterance can only be decided relative to a particular prospective symbolization of that utterance in some system. Reliance on the notion of logical form in this case is thus begging the question.

3. The Radcliffe Camera is the Radcliffe Camera.

4. Ice on the road caused the accident.

5. Undetectable disturbances of the ether caused the accident.

6. Ice on the road caused ice on the road.

Of the sentences displayed as 1. to 6., number 1. is not possible, number 5. is perhaps possible in the sense of "logically possible" which is found in philosophical literature, but is not possible in the world as it is experienced by scientists, policemen, and insurance companies with their various interests in causation. Number 6. is not possible in the further sense that it is odd in a way resembling that in which a category mistake is odd; one cannot properly say that a thing caused itself, and one therefore does not wish to allow that the claim can be true. Numbers 2., 3. and 4. all either are or could be true, and 3. may by some be regarded as "necessarily" true, and therefore of a diametrically opposite character to 1.

The most straightforward type of substitution we might consider is that of one term for another in the standard formulations of sentences. Thus 1. and 2. would be of the same form since either could be generated from the other by a substitution of "square cupola" for "stone building" or vice versa. Similarly 3. can be regarded as of the same form as 1. and 2. if substitution involving interchanging the definite and indefinite article is also allowed. 4., 5., and

6. are also of the same form if we allow that the two pairs "Ice on the road" and "Undetectable disturbances of the ether", and "the accident" and "ice on the road", are substitutable for one another.

It is clear that we cannot keep this simple notion of substitution and maintain that the sentences shown form part of an interpretation for S4 or S4.1 along the lines outlined by McKinsey. Although 2. is a true sentence and of the same form as 1., 1. is not possible. 5. and 6. are similarly not possible - though in very different ways - while 4. is of the same form and undoubtedly may be true. It is not tolerable that we should have to admit 5. and 6. as being possible, just because we wish to assert 4. to be true. It is equally intolerable that 2. and 3. cannot be regarded as true because of the clear impossibility of 1. Something has to be changed about the substitution criterion.

The first and obvious refinement is to bring the meanings of the terms being substituted more into the picture. We cannot however simply say that "square cupola" and "stone building" are not to be permitted as substitutions for one another because the former is an empty or self-contradictory notion. Such a criterion of substitutability would lead to triviality since in the sense of "logically possible" every sentence which predicates some non self-contradictory property of an object or class of objects which exists, is possible, and we should, try making this restriction, automatically build into our criterion of substitutability a provision which ensured

that all and only those sentences obtained by a permitted substitution were logically possible.

Yet we could decide to extend our field of interest beyond what is merely logically possible, and to include things which are "really" possible, or possible in accordance with the laws of nature. In this extended range of possibilities the introduction of a rule which prohibited the substitution of a non-contradictory would not immediately lead to triviality for there would remain some things which were not possible even though they were logically possible; that is, there would be assertions such as that made by 5., which contain no explicitly contradictory provisions, but are so in conflict with what we regard as real possibilities that we should have to classify them as not possible. But of course exactly the same type of problem arises in respect of a sentence such as 5., for although it is clearly not "really" possible, there are sentences obtainable from 5. by very simple substitution of terms - such as 4. - which are true. In order to conserve the impossibility of 5. we must prohibit any substitution which could lead to the production of a true sentence. While it is not possible to state on the basis of this small case study that no criterion for acceptable substitutions can be found, it is obvious that talk of "substitution" is merely a means of shifting the problem one step further without actually solving it. For as soon as we try to state the conditions on substitution, we are presented with the need for a criterion for

whether a given sentence says something which is not possible; our criterion for permissible substitutions will amount to saying that only those substitutions are permissible which do not lead to the generation from a sentence which is not possible, of a sentence which is true, or vice versa.

We have discussed the idea of a substitution criterion very summarily and in terms of a simplistic notion of substitution. Two general comments may therefore be in place. First it is more and not less difficult to find concept- or term-based criteria for substitutability when we consider less standardised forms of utterance; e.g. "Not being competitively inclined, "Jones rows just for the sake of it". The more difficult it becomes to decide just which and how many concepts a given sentence contains, the less plausible is a criterion for substitutability which requires us to reject or accept potential replacements of one concept without alteration of the others. Secondly one must be clear about which of the various levels of possibility is being discussed; we have referred here to "logical" and "real" possibility - without enumerating sorts of possibility, one can at least cite a sense of "possibility" in which just those things are possible which do not conflict with the laws of science as these are at present interpreted. Certainly "possibility" in this last sense is a very poor candidate for explication by the means suggested by McKinsey; there would have, for example, to be a rule prohibiting the substitution of any figure lower than 186,000 in a

sentence which attributed to some body a speed in miles per second greater than 186,000 - this rule however would coincide with a law of science to the effect that no body can have a velocity exceeding 186,000 miles per second. The triviality detected in the other cases would be particularly blatant here since the laws of science would be quite simply translatable into restrictions on the rules of substitution-by reference to which one would guard against the assertability as possible of anything which was undesirable (against the accepted scientific theories).

McKinsey's contribution, though an advance in that it recognizes the need to use the constants of the system to symbolize sentences, does not achieve its object of showing the usefulness of S4 or S4.1 to symbolize sentences of which some are true and others only possible. Apart from its failure to avoid the more formal objections to Lewis's systems it fails because it takes for granted the possibility of finding an explication of possibility in terms of substitutions in sentences, and such an explication is not the simple matter McKinsey implies it to be; for particularly in the case of "locical" and "scientific" possibility the obvious criteria for substitutability are based on prior decisions as to whether the sentences express things which are possible or not.

H. S. Leonard's Two-valued truth-tables

In a little-discussed paper of 1949, H. S. Leonard approaches the Lewis systems from a very different standpoint. He attempts to lay down rules whereby it can be established with the help of a truth-table like construction whether or not any given formula is a theorem*. The values which are countenanced in the truth-tables (we shall for the moment accept Leonard's extension of the term to cover his structures) are only "true" and "false", and a formula is a theorem if and only if every row of the truth-table assigns to that formula the value "true".

Although Leonard is not able to prove that the construction rules he lays down define methods of constructing truth-tables which validate all and only the theorems of particular Lewis systems, he is able to develop a system which validates all theorems of S2 plus some others, and a second system which validates at least the axioms of S4 and was not known by Leonard to validate any formula not a theorem of S4. The importance of Leonard's work lies principally in the fact that he has approached the Lewis systems from a more pragmatic point of view than other writers, and has faced the problem of laying down mechanical rules for deciding whether formulae are theorems. In Appendix I, we relate this approach to later approaches to the predicate calculus which are based on the application of

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*Prof. G. J. Massey has recently published results of (largely independent) work to develop two-valued truth-table methods for modal operators. (see 563A, 563B). One part of his forthcoming book will be devoted to a more extensive treatment of this subject.

Herbrand's theorem, and we show how the Leonard approach to modal systems lends itself especially well to the automatic testing of putative theorems by means of computer programs.

Leonard claims that his treatment "has intrinsic importance because of its intuitive validity". If this is intended to imply that his approach puts us in a better position to apply modal systems of the Lewis type in specific cases where we wish to symbolize a given deduction, then the claim is unfortunately unfounded. To see this, however, we must examine briefly the notion of a "truth-table" which Leonard employs and the rules he states for the construction of the tables.

Instead of seeing a truth-table as a column of values written under a formula, plus various auxiliary columns in which, for convenience, one writes the values assigned to proper parts of the formula being tested, Leonard sees a truth-table as one unit with a row of column headings each of which is preceded in the row by all formulae which are well-formed proper parts of it, and under each heading a column of value assignments covering all possible consistent sets of assignments.

In such a truth-table more types of rules for determining the value assignments made can be defined than are usual in a normal truth-table where the formal requirements are less precisely stated. In a normal truth-table we work with only two types of rule for assignments. Most familiar are the matrix based rules, which lay

down that for an expression whose principal operator is A, the value to be assigned in a given row is that which can be read off in the matrix for A, opposite the value(s) already determined for the argument(s) of A in that row. A second type of rule, which is perhaps less often recognised as a rule, is that where an expression has no principal connective (i.e. it is a simple propositional variable) then for every distinct assignment of values to other expressions not involving this one as a part there should be a set of rows differing only in that they assign different values to this one expression and such that in each such set all values admitted in the table are assigned to the expression once. This procedure may be described as the duplication of rows with the addition of the admitted values for the simple variable.

As his rule for propositional variables Leonard retains the rule of duplication just described. Further he uses matrix-based rules for all operators typographically identical to KC operators, and by choosing precisely the same matrices ensures that all those KC-like formulae which occur in the systems are assigned truth-table columns consisting exclusively of "true" if and only if they are transcripts of KC theorems. In addition to these types of rule, Leonard introduces a third type. These rules lay down how a given row has to be "extended" for a given heading - i.e. what value is to be assigned to that heading in the given row - not exclusively on the basis of what values have been assigned to the proper parts

of that expression in the same row, but also on the basis of the value assignments to these parts in different rows, and the assignments made in the row under consideration to expressions whose assignments in particular sets of other rows bear particular relations to the assignments to proper parts of the expression being considered.

The need for rules of this third type is arrived at as follows. First Leonard considers the effect of assigning to "◊p" the value "true" in case "p" has been assigned the value "true" in that row, and of treating "◊p" as a new propositional variable and therefore duplicating the rows for all headings before "◊p" in the row of headings and in the resulting two rows assigning "true" and "false" respectively.

Thus a table such as the following might arise:

Headings	p	q	◊p
Row 1	true	true	true
Row 2	true	false	true
Row 3	false	true	true
Row 4	false	true	false
Row 5	false	false	true
Row 6	false	false	false

It is clear that a procedure such as this could be used to provide us with truth-tables which happened to make all and only the theorems of a given system tautologies. To devise a set of rules

for constructing tables which did this for a particular system, could be regarded as an achievement, but the question would remain whether this was a happy accident, and precisely what intuitive basis there was for a truth-table in which certain expressions were sometimes treated as if they were new propositional variables and sometimes not. In short, to devise such a truth-table presentation of the Lewis systems would be an exercise of great mathematico-logical interest; however, it would not help us to be sure that the systems, whether in their axiomatic or in their truth-table versions, were suitable for the tasks of symbolization in which we are interested. From our present point of view, then, it is important to examine the reasons Leonard gives for his rules, rather than whether the rules do or do not define tables with the desired formal properties.

Leonard introduces the notion of a "valid" table, and sets out to develop tables for formulae involving " ◊ " which either are or include valid tables. Where $\underline{r}$ is a row of values, $\underline{r}$ is said to be potentially in a table, $\underline{A}$, if it has the same number of members as the set of headings for $\underline{A}$. A table is then said to be valid if and only if for every row which it potentially contains, it contains that row if and only if that row assigns to the formulae in the heading values which could be those of some interpretation of the formulae concerned. Leonard states this last condition as being that for row $\underline{r}$ and table $\underline{A}$, "J($\underline{r}$, $\underline{A}$) is not null". In this the

expression "J(r, A)" designates the class of interpretations ("I"s) identified by the row r and the list of headings of A. From a purely philosophical point of view the crucial problem about Leonard's presentation is whether his notion of an interpretation and his criteria for the existence and non-existence of interpretations in given cases, can be accepted.

He further says "'E', 'E_1' and so on (are) variables to represent formulas such as might be headings in tables". "'K', 'K_1' and so on (are) variables to represent finite, not null classes of E's". "'I', 'I_1' (are) constant interpretations that could be put upon the free variables in some E or K. .. E(I) (is) the proposition identified by the formula E and the interpretation I. It is assumed that I interprets all the variables in E, thus, with E, identifies a proposition". Having given these definitions, Leonard argues as follows:

> "Consider any r and any R (a set of E's such as might be a row headings in a table) of equal length. These two identify a possibly null class of I's. I belongs to this class if, and only if, for every E, if E is the ith member of R, then E(I) has the truth value represented by the ith member of r".

From these quotations it becomes clear that for Leonard the notion of an "interpretation" is sufficiently clear that we can in certain circumstances say that a given formula can have no interpretation in terms of a sentence with a given truth-value. Let us therefore turn to examples of his use of the claim that a given set of headings has no interpretation such that the proposition in terms

of which a given formula is interpreted has a given truth-value.

Leonard's first use of this type of argument is to justify the claim that the table displayed on p.148 includes a valid table - that is to say all rows in a valid table for the given headings are present in the table shown. He argues that this table is valid even though it does not treat "$\Diamond p$" as a new, third, independent propositional variable because "if $\underline{E}_j$ ($\underline{I}$) is true, $\ulcorner \Diamond \underline{E}_j \urcorner$ ($\underline{I}$) is also true". He sees no need to argue the matter further; instead he goes on to discuss "The Meanings of Modality Predicates" with a view to showing how to eliminate some of the rows produced by the simple rule of making duplicates with both "true" and "false" whenever the E to which assignment is being made is of the form $\ulcorner \Diamond E_j \urcorner$ for an E_j false in the current row.

Leonard's view on the relationship between the particular propositions in which modality predicates occur, and the system ("Lew" in his terms) for which we are making truth tables, is:

(i) Every proposition exemplifies many different forms; thus "New York is and is not north of Philadelphia" possesses each of the forms, $\underline{p}$, $\underline{p}.\underline{q}$, $\underline{p}.\sim\underline{q}$ and $\underline{p}.\sim\underline{p}$. The richer the algebra at our disposal the more forms of a proposition can be displayed.

(ii) "The modal properties of propositions are completely distinct from the type-properties of propositional forms ("analytic"; and so forth).. But the modal properties may nevertheless be defined by reference to those type-properties". e.g. "A proposition is

said to be possible when none of its forms is counter-analytic".

(iii) "What the formulas of Lew represent are not propositions predicating these modalities, but rather the forms of propositions predicating them ... '$\sim\Diamond(p.\sim q)$', represents the form of any proposition affirming the impossibility of a conjunction whose second factor is negative".

As will be clear from Chapters IV and V, most of the views stated here by Leonard are in agreement with those we argue. There is however one "missing link" which defeats Leonard's attempt to maintain that Lew is specially suited for symbolizing propositions which "predicate modalities". What is lacking is any means of checking that the algebra or logical system by means of which the form of a proposition is expressed, is an acceptable one.

We might, for example extend KC by adding the axiom '$\vdash p.\sim p$'. The resulting system, though inconsistent, has facilities for representing every logical form that KC can represent. Hence instead of stating the form of "New York is and is not north of Philadelphia" by using the formula '$p.\sim p$' of Lew, we could state it by using the transcript thereof in this extended inconsistent version of KC. Now if we confined ourselves to the use of expressions of this calculus, we should never find any form for the given sentence which was not a theorem of the system. It seems therefore that had it been this system rather than Lew which was under consideration we should have had to conclude that all

propositions were possible, so far as their form could be stated. Such nonsensical conclusions will be possible as long as no adequate criterion is found whereby we can rule out the use of deviant logics as grounds for the allegation that some given symbolic expression can or cannot have any interpretation.

This conclusion may be contested on the grounds that the criterion of possibility stated by Leonard in (ii) above is not that no form be assigned only undesignated values in a truth-table, but that no form be counter-analytic. To put the point conclusively we must investigate the exact definition of "counter-analytic"; this however is quickly done. We read (p.47):

8.1 E is analytic if logical considerations secure that for every I, E(I) is true.

8.2 E is counter-analytic if .... for every I, E(I) is false.

The logical considerations involved are not further expanded upon. We are thus left in the position where we must condemn the extended version of KC as not being a suitable touchstone for deciding whether propositions are possible, but must do so on the basis of our logical intuitions, unassisted by any explanation from Leonard of which direction we should take in more difficult cases.

Let us now relate the objection more specifically to Leonard's presentation of the Lewis-type systems. It should be reiterated that we are here concerned not with the propriety of the truth-table system developed nor with its accuracy as a parallel to axiomatic

formulations of Lewis systems, but with "intuitive validity" of the stated reasons for constructing the tables in just the way Leonard decides to. It is our contention that despite the interest of the truth-table presentation of the Lewis type systems, the commentary which accompanies the statement of the rules for constructing the tables, does not give the necessary explanation of how we are enabled by the Lewis type systems to use a straightforward constant expression to symbolize - satisfactorily - an utterance of ordinary speech which one might describe as saying that a given thing was possible.

Typical of the arguments Leonard employs is that which seeks to show that rows 1 and 3 of the following table can be ommitted without loss of validity:

	p	$\sim p$	$p.\sim p$	$\Diamond(p.\sim p)$
Row 1	T	F	F	T
Row 2	T	F	F	F
Row 3	F	T	F	T
Row 4	F	T	F	F

The argument is as follows:

> "..consider any interpretation, I, of "p", and the resulting proposition $[\Diamond(\underline{p}.\sim\underline{p})]$ (I). This proposition affirms that the proposition $(\underline{p}.\sim\underline{p})$ (I) is possible - that is .. that no form of proposition $(\underline{p}.\sim\underline{p})$ (I) is counter-analytic. But $\underline{p}.\sim\underline{p}$ is one form of $(\underline{p}.\sim\underline{p})(\underline{I})$; and not only is this form counter-analytic but the third column ... by being uniformly 'F', testifies to its counter-analytic character. Hence $[\Diamond(\underline{p}.\sim\underline{p})]$ (I) is false; and had we ... examine(d)

> the entire column under "$\underline{p}.\sim\underline{p}$", we should have known that by extending (any row) $\underline{r}$ by doubling so as to produce, among others a row $\underline{r}$, is to insert ... a row defining a null class of interpretations - that hence (the table) will still include a valid table if every row ... is extended with an "F" and not by doubling". (p.54)

Our immediate objection to this argument is that no attention is paid to the need to establish that '$\underline{p}.\sim\underline{p}$' is an <u>acceptable</u> form of $(\underline{p}.\sim\underline{p})(\underline{I})$. There is no doubt that it is one form of $(\underline{p}.\sim\underline{p})(\underline{I})$, viz. one of those forms which can be shown in the notation of Lew; however the acceptability of Lew as a system for investigating the forms of propositions and deciding for example whether they are counter-analytic or not, is left over to be settled on the basis of "logical considerations" by our intuition. Yet it is precisely the adequacy of Lew for this purpose which is at stake for us, and we are not convinced by a mere appeal to our intuitions.

Leonard in fact presents the matter more complicatedly than is here suggested. He talks in terms of successive truth-tables "A", "A_1", "A_k", "A_{k+1}" and presents the problem as being one of defining rules for the generation of successively larger truth-tables (each having a row of headings one expression longer than that of the previous one) in such a way that each table produced at least includes a valid table. Because of this it may appear that the argument about how the column for '$\Diamond(p.\sim p)$' is to be filled in, can be upheld on the grounds that the table consisting of all headings and columns up to and including '$(p.\sim p)$' has previously and independently been established to be valid.

Such an approach does not however, protect Leonard. In order to know that the smaller table is valid, we have to know the classes of interpretations defined by its headings with the various rows; but this in turn means that we know how to interpret the operators '.' and '~' in these headings and what values we should assign to the propositions resulting from given interpretations of the variables (taking Leonard's notion of an "interpretation"). This is difficult since the variables concerned are those of Lew and not those of KC, and even if we decide to interpret '.' and '~' as if we were dealing with a KC truth-table it is by no means certain that we should wish to ascribe the same truth-value to all interpretations of a formula in KC as to its transcript in Lew. As a simple example, we might well feel that these sentences suitable for use in interpreting Lew, such as "It is possible that he will come" and "It is not possible that he will come", which do not obey the law of the excluded middle (because that which is fact is no longer either merely possible or not possible); we should then have to vary our interpretation of "~" or/and our selection of propositions for which Lew is to give an acceptable symbolization.

In short, the fact that the successive tables used in Leonard's construction of the rules, share variables and constants of the system Lew, means that the notion of validity which he seeks to conserve while extending the tables remains (and starts off) relative to our acceptance of Lew as a suitable means of

representing the form of propositions resulting from the interpretation of formulae in the headings.

To round off our discussion of Leonard's system here, it is worthwhile commenting on its implications for the use of formulae consisting of constants as a means for symbolizing particular utterances. It follows from Leonard's construction that when a purely constant formula contains modal expressions, its value cannot be calculated directly from the values of the constants it contains. For example if we have to decide what the truth-value of '$\Diamond((A.B).\sim(B.A))$' will be, given that 'A' and 'B' both have the value 'false', we are not able to reach our conclusion without considering what would appear in various parts of various columns of the truth-table for the expression '$\Diamond((p.q).\sim(q.p))$'. Only after such an investigation will a set of rules such as Leonard's allow us to decide the value of the expression. In effect we have to know how to generalize the constant expression <u>before</u> we know which constant expression to use; for we shall have to assure ourselves that the expression we choose has the correct truth value. This is no insuperable difficulty but suggests that Lew is a different sort of system to those discussed in Chapter IV as typical logical systems.

It should also be noticed that here, just as for systems satisfying the desiderata considered in Chapter II, no two individual constants can be stated to be strictly equivalent to one another merely on the basis of their truth values. If two expressions other than

transcripts of KC theorems, Lew theorems their instances or negations are to be strictly equivalent it is necessary that a postulate explicitly stating this equivalence be added to the system as an assertible formula. (Thus even though 'A' and 'B' are equivalent in all respects relevant to value-assignments - they have, for example, both the truth-value 'true' - they will only be strictly equivalent if we have as an assertible formula '$\sim\Diamond(A.\sim B).\sim\Diamond(B.\sim A)'$.).

At first sight this may seem a virtue of the system in the sense that when we are dealing with sentences of ordinary language, truth-value equivalence is never a criterion for taking to sentences to be equivalent as utterances. Yet in a system which employs only two truth-values, this is an odd situation. Just as we can see such a system as being an abstraction from all other facets of our utterances apart from their truth or falsity, (cf. Chapter V) so also we may properly expect that the truth-value assignments reflect the properties of those entities used in interpreting the constants of the system - and <u>all</u> the important properties for such an interpretation.

It is an immediate consequence of this feature of Lew that when we have two constants of the same truth-value to interpret, we must make further investigation before we can assume that the same sentence can be used to interpret both. Were we not to do so, it might turn out that where 'A' and 'B' were the constants concerned

and were both of truth-value 'true', there was in the system another assertible formula, '$\Diamond(A.\sim B)$'; if 'A' and 'B' have been treated as equivalent and have been interpreted in terms of one and the same true sentence*, then we would in such a case be forced to interpret the remaining, assertible, formula as a statement to the effect that what that true sentence said could be simultaneously both the case and not the case.

It seems to us a very serious drawback of a system if, in order to interpret its constant formulae one must investigate not only their truth-values but also the truth-values of all other formulae including or ranging over these constants; yet only such an investigation is sufficient to show in all cases whether two constant expressions of the same truth value can have the same interpretation.

Possible alternative worlds

It would be impossible to treat the topic of modern presentations of Lewis systems adequately without giving some attention to the work of such writers as S. A. Kripke and J. Hintikka. It is particularly appropriate to discuss them here, as their work has affinities with both the truth-table methods of Leonard and the semantic tableau approach of Beth and Nieland which we discuss later.

These writers approach the interpretation of Lewis modal systems as a topic in the theory of modals, and define abstract structures which serve as models for the various systems. Further

*As can happen without objection in the interpretation of KC constants.

than this they comment from a more philosophic standpoint upon the intuitive significance which can be given to these models and the elements of the abstract apparatus used to define them. The approach is referred to by the writers themselves as "semantic" and is characterised by the view that the models constructed can intuitively be regarded as ordered collections of "possible worlds" which are alternative to one another in accordance with the dictates of a dyadic relation whose properties help to distinguish the model of one system from those of others.

A somewhat troublesome point about the presentations is that the terminology and level of abstractness is not uniform from one writer to the other. Even in the subsequently published versions of their contributions to one and the same symposium, Hintikka and Kripke manage to use the same abbreviation for very different things. (420T,495G) Since however their intentions are very similar, it is most economical to summarise the terminological divergences and then discuss both writers together.

The less abstract of the two presentations is that of Hintikka. He starts from the familiar notion of a model for KC: a set of formulae - those "true" in KC - can be defined independently of KC and used as the basis for a model of KC. Hintikka postulates, on the analogue of this set of true formulae, a whole range of sets of true formulae of the modal system to be modelled. For those of the formulae of the system which are transcripts of formulae of KC or

instances of these, the criterion for membership of each and every such set is the same as for the set used in modelling KC. For formulae containing the distinctive symbols of the modal system, the criterion for membership of one such set involves settling the questions (i) of whether that formula is a member of other sets, and (ii) whether the other sets concerned should be taken into account in determining the constitution of the particular set under consideration.

It is at this point more lucid if we start introducing the technical terms current in "semantic" presentations of modal logic. Hintikka's assemblage of sets of formulae is known as a "model system", and he refers to the individual members thereof as "model sets" or in abbreviated form "m.s.'s. On the m.s.'s he postulates a dyadic relation of "alternativeness" as being defined; according to the manner in which this relation orders the m.s.'s we shall say that a given one is or is not an alternative to another. In stating the criteria for membership of a m.s. no conditions are stated which determine that any formula involving the characteristically modal symbols should or should not be a member of any given m.s. The presence or absences of these formulae in given m.s.'s is determined only in the statement of the rules for the ordering relation of "alternativeness" as it applies to the model structure as a whole. Thus where $\mathfrak{Q}$ is the set of all members of the model structure, and μ and ν are two arbitrary m.s.'s the following is postulated about

the formulae '$\underline{\text{NF}}$' and '$\underline{\text{MF}}$':

If $\underline{\text{NF}} \in \mu \in \Omega$, then $F \in \mu$.

If $\underline{\text{MF}} \in \mu \in \Omega$, then there is in Ω at least one alternative to μ which contains F.

If $\underline{\text{NF}} \in \mu \in \Omega$, and if ν is an alternative to μ in Ω, then $F \in \nu$.

Hintikka's comments on the significance of the model he has defined are typical of the approach of the "semanticists" to the philosophic aspects of modal logic. He says,

> "What we have here therefore is a slightly modified version of the traditional idea that possibility equals truth in some "possible world" while necessity equals truth in all "possible worlds". Apart from using the notion of an m.s. as an explication of the notion of a description of a possible world, our only departure from the traditional idea lies in rejecting the presupposition that all "possible worlds" are on a par. .... Each statement has to be thought of as having been made in some "possible world"; and nothing can be said to be possible in such a world which would not have been true in some world realizable in its stead". (42OT;67)

Kripke's semantic approach is framed along different lines, but has the same philosophic overtones. Like Hintikka he works with a dyadic relation defined over the members of a set, and ordering its members according to a relation which he talks of as "possibility relative to". The set concerned is the basis of a "model structure" (abbreviated "m.s.") and little is said about its members save that they appear as one of the arguments of the binary function ϕ; where K is that set, G is one of its members, and $\underline{R}$ is the ordering

relation defined over the members of K, Kripke says:

> "Formally, a <u>model</u> ϕ on a m.s. (G, K, <u>R</u>) is a binary function ϕ (<u>P</u>, H) where <u>P</u> varies over atomic formulae and H varies over elements of K, whose range is the set {T, F}. Given a model we can define the assignments of truth-values to non-atomic formulae by induction". (;84).

Corresponding to Hintikka's postulates for '<u>N</u>F' we have Kripke's definition of ϕ ($\Box$ A, H) where H is an arbitrary member of K:

> "..we define ϕ ($\Box$ <u>A</u>, H) = T iff ϕ (<u>A</u>, H') = T for every H'ε K such that H<u>R</u>H'; otherwise, ϕ ($\Box$ <u>A</u>, H) = F. Intuitively, this says that <u>A</u> is necessary in H iff <u>A</u> is true in all worlds H' possible relative to H." (<u>loc. cit.</u>)

It will be apparent that the members of one of Hintikka's m.s.'s are just those formulae which would for some world H be assigned the truth-value T, by one of Kripke's model functions. In cases where each approach is usable to generate a model of a particular modal system, and where the alternativeness relation and <u>R</u> have the same properties of symmetry, transitivity etc., it will be unimportant whether we consider the presence of a given formula in an m.s. of Hintikka's presentation, or the assignment to that formula of T for the corresponding world in Kripke's presentation.

With this in mind let us turn to the more philosophical aspects of the "semantic" approach to modal logic, and concern ourselves with the conditions under which a formula is T-valued and the conditions under which an utterance which that formula might be taken to symbolize, is true.

There is an obvious structural similarity between making the truth-value to be assigned to a given formula in a given row of a

truth-table dependent upon the values assigned to other related formulae in different rows (as Leonard does) and laying down that the truth or falsity of a given formula in a given "world" is dependent on the truth or falsity of other related formulae in different worlds. Indeed we can construct a model set of the Hintikka type by considering a single row of a Leonard truth-table, and including in the set just those resulting formulae which are assigned the value "T" in that row. Perhaps because of the absence of any obvious reason to select particular truth-table rows as more relevant than others, Leonard has not hit upon the idea of making the assignments in a given row dependent upon the assignments in particular subsets of the other rows; instead he regards all other rows as equally relevant and thus starts from what Hintikka describes as the traditional view that all "possible worlds" are on a par, and reaches the desired goal of a system similar to a Lewis system by means of rules which put certain rows - or possible worlds - beyond consideration (they are in practice never generated, due to the restrictions on "doubling").

We alleged that the rules which Leonard employs to decide when "doubling" should occur and when not, required further justification and were by no means "intuitive" as they were presented. Such an attack cannot be transfered to the "semanticists" in the direct form of challenge to the adequacy of the set of "worlds" they consider; they do not have anything which corresponds to doubling,

nor occasions where doubling is "unnecessary" or "counter-intuitive". But what they do have, and do not attempt to explain the significance of, are "alternativeness" relations. Instead of not admitting certain "worlds" to the discussion, the move of these writers is in effect to admit them but only subject to the condition that they are declared to be irrelevant ab initio; they are irrelevant to the determination of the truth values of formulae in certain other worlds because these are not genuine alternatives to the other worlds. What we have to ask is, "What is the purpose or philosophical significance of the alternativeness relations used, other than the futile one of matching the interpretation to the still suspect Lewis systems?" (This we ask, of course, in the knowledge that the writers concerned have been starting from the Lewis systems and seeking a model for them; there is therefore no implication that these writers purport to find in everyday speech or any other philosophically interesting potential interpretation, factors which force them to the adoption of a Lewis-type modal system. Others however are likely to read this claim into what has been achieved in giving this "semantic" model for the Lewis systems).

From the point of view of view of one constructing an abstract model of an axiomatic system, it is perfectly acceptable to proceed as Hintikka and Kripke do, talking in terms of sets of expressions and sets of these sets, or of sets over whose members a dyadic ordering relation is defined. So long as unambiguous rules can be

stated for finitely determining whether a given expression is mapped onto T or F by the model, the model is mathematically adequate. But being adequate in this way is a property which does not guarantee that an abstract model gives any worthwhile insights, or helps us in interpreting the system which it models. Suppose a system KC' is presented to us as an abstract system and we are asked to judge it; being well acquainted with boolean algebras we succeed in demonstrating that the theorems of this system can be fully and satisfactorily modelled with the help of the notations of Boolean algebra and the notion of mappings into the set $\{0,1\}$. Such a discovery brings us no further towards finding a use for KC' as a logic, unless we go on to make the connection between Boolean algebras of two elements, truth values, and assertions which are true or false. Our model is indeed of mathematical interest and may for that reason be of philosophic interest; but until we go farther and devise rules relating the formulae to constant expressions which we can interpret in terms of ordinary language, we shall be able to make no pronouncement on the philosophically interesting question which is whether this KC' is suitable for symbolizing the elements of our everyday speech and argumentation. This situation with respect to the Lewis systems is no different; the model we are given is framed in terms of mathematical concepts and although the use of such concepts guarantees that the model is stated in accordance with mathematical standards of rigidity, it does not establish that the system is of

any more than mathematical interest.

The approach of Kripke and Hintikka is hence also open to the sort of criticism brought several times before against other presentations - viz. that no attention is paid to the question of how constants of the system are to be paired off with sentences such as they might be used to symbolize. This can be shown by the analogy with KC'; KC', like the semantic construction models for Lewis systems, is so set out that we are able to prove that all and only the theorems of the system can be mapped onto a set of entities whose members can be selected from a set of entities onto which all formulae of the system can be mapped, by means of a set of rules which make no reference to the system itself. Both formulae and theorems, however, are stated in terms of variables, and it is only possible to apply the system when we take the further steps of stating (i) what constants the system is to contain in addition to the logical constants, (ii) under what conditions these can replace the variables in formulae and, (iii) what equivalence classes are to exist among formulae containing only constants.

It is easy for us to settle these matters for KC' - at least if we allow ourselves to be influenced by its uncanny resemblance to KC. We find that an indefinitely large number of constants can be allowed, substitution for variables may take place without restriction, and the formula, including constants standing alone, can be divided into two equivalence classes in such a way that

membership of one of these can be recursively determined in accordance with the "truth matrices" for the logical operation constants. Although the theorems of the Lewis systems are reminiscent of those of KC, no attention is paid in the semantic constructions to the respects in which their differences from KC are accompanied by differences between the Lewis systems and KC in the matter of the existence, substituability and equivalence class membership of constants. The actual difficulties in giving rules for the use of constants have already been adequately played upon in commenting upon other writers.

Beth and Nieland's semantic tableaux and trees - and truth tables

To conclude our discussion of the various formulations and "constructions" which have been given for Lewis systems, let us consider the presentation given by E. W. Beth and J. J. F. Nieland in their paper "Semantic Construction of Lewis's Systems S4 and S5" (165R/610E). It will suffice for present purposes if we show the relationship of this version to the presentations already considered and dispel any illusion that either the semantic tableaux or the tree-structures used in the exposition help us to avoid the shortcomings found in those other versions.

This model for the Lewis systems can be usefully compared with both the semantic constructions of Hintikka and Kripke (which are referred to in Beth and Nieland's paper) and the truth-table method of Leonard. The ease with which such comparisons can be stated

argues further for the basic unity of approach among these three presentations.

Crucial to this formulation is the notion of a "valuation". As illustration we shall take S4, which is dealt with in all three approaches. An "S4-valuation" is defined as follows:

> "..an ordered quadruple $\underline{P}, \leqslant, \underline{p}^{o}, \underline{f}$; here $\underline{P}$ is a set, $\leqslant$ a partial ordering of $\underline{P}$, $\underline{p}^{o}$ the first element of $\underline{P}$ in accordance with that ordering, and $\underline{f}$ a function which associates with each node $\underline{p}$ in the tree $\underline{P}$ a pseudo-valuation $\underline{w} = \underline{f}(\underline{p})$; such a pseudo-valuation $\underline{w}$ connects with every formula $\underline{U}$ a truth-value $\underline{w}(\underline{U}) \varepsilon \{0,2\}$ in accordance with the following conditions:
>
> (S1) If $\underline{w}(\underline{U}) = 2$ and $\underline{w}(\underline{V}) = 0$, then $\underline{w}(\underline{U} \rightarrow \underline{V}) = 0$; otherwise, $\underline{w}(\underline{U} \rightarrow \underline{V}) = 2$.
>
> (S2) If $\underline{w} = \underline{f}(\underline{p})$ and if every $\underline{p}'$ and $\underline{w}'$ such that $\underline{p}' \leqslant \underline{p}$, $\underline{w}' = \underline{f}(\underline{p}')$ we have $\underline{w}'(\underline{U}) = 2$, then $\underline{w}(\underline{NU}) = 2$; otherwise, $\underline{w}(\underline{NU}) = 0$". (165R;17)

From these specifications it will be clear how the model of Beth and Nieland is to be related to the constructions of Hintikka and Kripke; the ordering relation ' $\leqslant$ ' is the "alternativeness" relation, the elements, $\underline{p}^{k}$, of $\underline{P}$ are the 'worlds', and from an S4-valuation we can generate a corresponding model system - in the sense of Hintikka - by taking as the members of our model sets just those formulae which are assigned the truth-value 2 by the various pseudo-valuations $\underline{w}$, and then ordering these model sets just as the $\underline{p}$'s (from which these pseudo-valuations, $\underline{w}$, are derived as $\underline{f}(\underline{p})$.) are ordered. Each S4-valuation can thus be mapped onto a model system answering to the description given by Hintikka.

That some relationship is to be expected between a formulation which admits the use of semantic tableaux for validity testing and

a presentation of the same system in terms of truth-tables, can be seen by considering the relationship between semantic tableaux and truth-tables in the case of the intuitionist calculus and KC.

The philosophic justification of Beth's use of semantic tableaux rests upon his dictum:

> "An inference or proof has demonstrative force if and only if it does not admit of a counter-example". (165T;58)

The semantic tableau provides a systematic way of laying out an argument to the effect that any assumption that a given sequent has a counter-example, is an assumption which leads to a contradiction and therefore cannot be entertained. When a semantic tableau is not closed it gives us a basis from which we can systematically construct a valuation - i.e. an assignment of truth-values to the formulae in the sequent being tested - which shows the sequent not to be valid (that is, in Beth's terminology, a solution to the "valuation problem").

The method here employed bears striking resemblances to that known as the "shorter truth-table method" for deciding whether formulae of KC are theorems or not. Here again one attempts to derive from the assumption that a counter-example to the purported theorem being examined exists (thus that that formula has a 'O' or 'F' in some row of its truth-table) an explicit contradiction on the strength of which we can deny that a counter-example is possible and therefore accept the formula as a theorem. In the case of the

shorter truth-table method, however, we proceed by writing truth-values under the formula and its well formed parts; none the less it will be clear that this procedure is analogous to that followed in the semantic tableau, viz. writing the formula and its well-formed parts under the truth-values. Where the semantic tableau yields a counter-example the shorter truth-table method also yields one with the same assignment of truth-values to the atomic formulae.

Since Leonard has developed a form of truth-table for modal functions, it is only to be expected that a corresponding shorter truth-table method can be defined. Further, if the systems of Beth and Nieland and of Leonard are both S4, it is to be expected that the semantic tableaux presented by Beth and Nieland will provide truth-value assignments allowing disproof by the shorter truth table method whenever the semantic tableau is not closed.

It is beside the point to discuss this farther here; it can none the less be of interest to comment upon the relationship between valuations or pseudo-valuations as described by Beth and the rows of truth tables as these occur both in traditional truth-tables and in Leonard's tables. Since Beth explains how to construct the valuation showing a given sequent to admit of a counter-example, whenever that sequent's semantic tableau is not closed, we shall in the discussion which follows automatically cast light on the relationship between semantic tableau and truth-table procedures for solving the valuation problem.

We have noted that on the basis of the model sets described by Hintikka it should be possible to construct rows of Leonard truth-tables. It is equally possible to construct a row of a Leonard truth-table on the basis of a pseudo-valuation such as those defined by Beth and Nieland; the row consists of just those truth-values - written under the various well-formed parts of the formula being investigated - which are assigned to these parts by the pseudo-valuation in question.

If we cast our minds back to the case of the valuation problem for KC, we shall be struck by an apparent asymmetry in the cases of S4 and KC. The valuation problem in the case of KC was solved when we found a function which assigned the truth-value "2" to all formulae in the antecedent of the sequent and the truth-value "0" to all formulae in the consequent; we could then construct a row of a truth-table in which just those values were written under the various formulae where these appeared as headings in the table, hence generating a row such as the shorter truth-table method could have provided. But in that case it was the valuation which served as our basis for constructing a row of the table; here we are using, not the valuation itself but a "pseudo-valuation" as the basis for the crucial row of the table, and this difference requires explanation.

First let us remark that not all pseudo-valuations are of equal significance; the solution to the valuation problem for the

sequent $\underline{K}/\underline{L}$ demands that for some S4-valuation the function $\underline{w}^{o}$ assign "2" to all formulae of $\underline{K}$, and "0" to all formulae of $\underline{L}$. This $\underline{w}^{o}$ is the pseudo-valuation defined as '$\underline{f}(\underline{p}^{o})$' where '$\underline{p}^{o}$' and '$\underline{f}$' are the last two members of the quadruple which specifies the S4-valuation in question, that is, $\underline{p}^{o}$ is the first element of $\underline{P}$ and $\underline{f}$ is a function specific to this valuation and generating a pseudo-valuation for each node.

What is crucial for the solution of the valuation problem or the rejection of a formula whose validity is on trial is then the pseudo-valuation $\underline{w}^{o}$ of each S4-valuation which is possible for the formula(e) and its (their) sub-formulae. The additional pseudo-valuations serve only to help to determine what values will be assigned by $\underline{w}^{o}$. When writing out a full truth-table we should therefore keep the rows generated from these pseudo-valuations other than $\underline{w}^{o}$'s somewhere apart from the set of rows which we consider when asking whether the formula is a tautology.

Intuitively these rows can be used to form a third dimension for truth-table. Thus we alter our concept of the truth-table from that of a plane rectangular table into that of a three-dimensional box shaped table, and we identify the rectangular table on the basis of which questions of tautologousness are decided with the "front" face of the box. To save terminological confusion we continue to talk of "rows" and "columns", and in terms of our visual image mean by these terms respectively a series of entries horizontally

arranged across the front face or parallel with such a series, and a series of entries vertically arranged up and down the front face, or parallel with such a series. A series in the new dimension, thus from the front face to the rear face parallel to the "roof" of the table, will be referred to as a "street".

Thus the first value in a street of our three-dimensional table will be one assigned by the $\underline{w}^0$ whose value assignments are represented by the row in which it occurs. The remaining elements in that street are the assignments made by $\underline{w}^1$, $\underline{w}^2$, ... etc. of the S4-valuation concerned, to that same formula. If we know the ordering of the elements $\underline{p}^1$, $\underline{p}^2$, ... of $\underline{P}$ and hence that of the various $\underline{w}$'s we can in accordance with rule (S2) infer the value to be assigned to a formula involving the modal symbol 'N' in the first position in the street, i.e. in the row which is part of the front face of the table. Since the only one of the rules governing assignments of values which refers to the ordering of the elements of $\underline{P}$ is that concerning the relationship between the value of $\underline{U}$ and that of $\underline{NU}$, we see that the value assigned to a formula not containing the modal symbols 'N' or ' ◇ ' will be the same in some row on the front face as it is in each position in the street whose first element has that formula as its heading (that is, every consistent assignment will occur in each street and in the column under that formula).

In short, the whole horizontal plane of the table which has row j as its front row can be seen as being a projection onto the horizontal of the front face of the table - at least so far as the columns under formulae containing no modal symbols are concerned. Where only a single formula in the row of headings is of the form 'Nx', we can determine the value of this formula for row j by considering the result of applying the rule (S2) to p's whose corresponding pseudo-valuations could produce the value assignments displayed in the various rows on the same horizontal plane as row j, and considering all partial orderings of P which retain as the first element, the p whose corresponding w yields row j. For the case of the first or only formula with a modal symbol it is clear that all these partial orderings give the same result, and all the potentially different cases with row j in the front face collapse into one - in which Nx is assigned "2" if and only if "x" would have been a tautology in a KC truth-table.

The process may be continued recursively leading into further ramifications of images of the potentially different row j's which could have arisen had (S2) not dictated the same assignment in all cases, and the duplication of the images of all other rows on the front face taken in all partial orderings with the images of these different extended row j's; the degree of involvement will depend upon the Lewis (or other) system being used and its rule corresponding to (S2).

The reason why Leonard is not quite forced into working with three-dimensional truth-tables can now be seen. In the case of S4 one can at least start one's truth-table building by making the truth-table for the formulae not involving modal symbols, and then extending this recursively for each new formula which arises. The rule used need not postulate a replication of the already existing rows and a consideration of all partial orderings of them because it is possible to work with the interrelations of the values in the rows of the front face itself. Leonard follows this path until he finds that the sequence in which expressions containing modal symbols are assigned values makes a difference to the final result; he then falls back on the intersection of the various tables produced by making the assignments in all possible different orders, and uses this as the basis for his putative decision procedure for S4. One of the principal handicaps which he suffers is that he has not hit upon the idea of a <u>partial</u> ordering of the rows; in effect he assumes them to be fully ordered and devises rules for deleting undesired rows; the problem here is that because one retains a two-dimensional table and a tautology concept which tests the whole column under the formula concerned, cases arise where in order to establish the theoremhood of certain formulae, one is forced to eliminate all those rows which assign it an undesignated value even where the rows concerned do not correspond to rows on the front-face of a three-dimensional table. In eliminating such rows by <u>ad hoc</u>

rules one tends to eliminate others which should have an influence on the values to be assigned to further formulae.

We may conjecture at this point that S5 can be given a truth-table which is a simple three-dimensional array. S4 adds the difficulty that the ordering of the nodes is a partial ordering only, and hence the "streets" must take account of the forking of the tree. Other Lewis systems are less easy to visualize. It must in principle be possible to produce a system in which the three-dimensional table is itself mirrored, plane by plane, in a four-dimensional table; in view of our doubts about the practical usefulness of the systems with a three-dimensional table, we leave these matters at the level of conjectures.

To conclude, let us recall the recurrent difficulties and see whether trees, tableaux, or n-dimensional truth-tables can resolve them. That the tree structure differs so little from the models of Hintikka and Kripke as to offer no help, will be obvious. The tableaux and the truth-tables have on the other hand the aura of being very concrete approaches, and in particular it seems as if we are at last talking in terms of actual individual constants of the system when we consider the value assignments and the search for a counter-example.

However it is not a concrete counter-example which is sought in the semantic tableau method or the shorter truth-table method; instead it is a sketch of the ways in which truth-values could turn

out in practice with among these possibilities at least one which shows that the supposed certainty of the formula being tested would fail. This is thus a mere characterisation of a possible counter-example; and as such its worth rests on our ability to "cash" the characterisation - that is to choose particular utterances which have the characteristics ascribed to our imaginary counter-example. Thus the decision methods for KC are of interest because we can with ease select utterances which are "true" and "false", and because we therefore have some interest in knowing that the system is not such that all formulae are tautologous. But given a system which we cannot use because we cannot find rules for matching constants to the utterances we wish to symbolize, it is of little interest to know that many formulae admit of "counter-examples"; only if the characterisation given of those counter-examples allows us to identify some of them will solutions of the decision and valuation problems help establish what uses the system is suited for.

The Generation of Concrete Counterexamples to Supposed Modal Theorems

Our discussion of Lewis systems has tended to be highly abstract and it is therefore advisable to close our consideration of them by displaying in concrete terms the problems which arise even if we accept the more precise formulations of the systems as given by Lewis's followers. We take it that no justification is needed for the supposition that it makes sense to talk of assigning the truth-values "true" and "false" to propositions symbolized in a modal

system in just the way we talk of such assignment when considering the models for KC. The generation of a counterexample to an alleged theorem is then in principle a simple matter - it consists in so assigning values to the well-formed parts of the formula concerned that we are forced by the rules of the operators used in the system to assign the value "false" to the formula as a whole.

Here the further factor of being "forced" to assign a certain value has entered the picture. It is the combination of a given operator with the appropriate number of arguments which determines the assignment of the value to the whole. Thus the value to be assigned to 'P.Q' for KC is determined by the rules in KC for the use of '.' and the values which we - according to an unrestricted choice - assign to 'P' and 'Q'.

It may be said that the rules governing the assignment of values to complexes as a function of the assignments made to their parts, are rules which do not have to be justified. Certainly they do not have to be justified as components of the system of logic concerned - they are elements which help to constitute that system; it would not be the same system without them, and to entertain the supposition of that "same" system without one such rule is self defeating. Hence it is true that no justification need be sought for having included any such rule in a given system. On the other hand as soon as we make assumptions about what the system is <u>for</u>, then it does become relevant to ask whether a given rule is justified.

Prior's discussion of "contonktion" (681R,155P, and articles referred to in 682H) provides an interesting counterpoint to our discussion here. The basis of his paradox is that he makes use of the familiarity of the normal terminology for specifying a logical connective in a formal system, but specifies a connective for which we expect to have no use. This notion that we should not wish to use a system which allowed inference via contonktions is easy enough to explain and points the question whether the modal operator "N" is not another more subtle such case. The reason why we do not want systems with 'tonk' is that we in our everyday manipulations with assertions have insufficient interest in a relation amongst them such as that which contonktion would symbolize, and therefore have no wish to see this incorporated in a system. That a system with contonktion and the normal attributes of KC would not be consistent in Post's sense, is a quite separate point; it is not a great defect of arithmetic that all numbers which can be written in well-formed formulae are taken as "existing", and we may on this analogy, feel that a logical system in which all well-formed formulae were provable could also have its interest*.

In order for us to have gained any extra information about the interpretation and uses of the Lewis systems it is necessary not only for the "counterexample methods" (the semantic tableau and the

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*Just as the system without any theorems described in C. L. Hamblin's Article "One-valued logic" has its interest. (385H)

truth-table methods) to frame a counterexample in terms of assignments to formulae of the system, but also the appropriacy of such an assignment to a formula of the structure shown and in the context given. In KC this was simple; all atomic formulae could have either truth-value assigned quite independently of the assignments made to any other atomic formula, and further the assignment to every complex formula was fully determined by those to the atomic ones it had as well-formed parts. This was satisfactory because we recognised that classes of utterances could be found where the truth of one utterance was always independent of that of the others, and we accepted that it was interesting to see what the combination of truth-values by means of truth-value determinate operators would yield - since we felt that there was at least a rough resemblance between the effect of these on truth values and the effect of joining the utterances in question by ordinary language connectives.

Let us proceed along the formulae in the heading of a truth-table for a modal expression, and examine the process of generating the real, ordinary language counter-example to some purported theorem, such as we can - presumably - do on the basis of a row of the truth table. The row concerned when we consider a three-dimensional table is of course a "front" row.

Let us suppose that the first element of the heading which we encounter is an atomic formula, P, to which a truth-value 'true' is

assigned in the row under consideration. It seems that we are here presented with a case exactly the same as we would have been in the case of KC, and that we may simply choose any true assertion as the corresponding part of the ultimate counter-example. We choose, accordingly, some assertion such as "Pigs cannot fly". The next heading encountered is the negation of the first; and it is naturally assigned the truth-value 'false'. In KC we could have chosen any false assertion to correspond to this, but would normally use the negation of the assertion first chosen - if only for convenience. In this case we appear to have the same freedom and can choose a middle path between using the negation of the assertion corresponding to P, and using an arbitrarily chosen falseassertion; we choose "Pigs can fly" as corresponding to this heading (for this row of value assertions).

Next comes the formula consisting of the previous formula preceded by the modal symbol 'N' or '$\sim \Diamond \sim$'. Let us suppose that the row concerned contains an assignment of "false" under this formula as heading. Were we to be working in accordance with the pattern for KC, we should now be able to proceed by choosing an arbitrary false assertion to correspond with this formula in this case, but might for the sake of making potential applications of the system clearer, use an assertion related to that corresponding to the contained formula. This last course seems to be what the various writers have in mind. In particular there seems to be an

idea that the symbol 'N' symbolizes and should be 'translated' as "necessarily". If we try this we reach the assertion "It is necessary that pigs can fly".

So far we have succeeded in maintaining in our counter-example precisely the truth-values and the normally assumed desirable semantic relations amongst the assertions corresponding to well-formed parts of the formula being tested. We have found nothing however which seems related to the third dimension of the truth-table, or to the pseudo-valuations which go to determine the actual valuations in the Beth method.

Suppose then that we must also find an assertion to correspond to 'NP', and t we for the reasons outlined above choose 'It is necessary that pigs cannot fly', and that the truth-value assigned by the row in question is "true". If we are prepared to accept that this assertion is indeed true then no problem arises; but we may equally well suppose that the fact of pigs not being able to fly is purely fortuitous, and hence not wish to say that 'It is necessary that pigs cannot fly.' is true. Should this latter be the case it seems that we have been led up a blind alley in our search for a concrete counter-example, and that we cannot merely use the form 'It is necessary that.." to render occurrences of the operator 'N'. If we further accept that all the assertions to be taken into consideration must be either true or false and not both, then it is clear that in all cases where 'NP' can have two different values, we may

be unlucky enough to generate an "It is necessary that.." assertion which has the inappropriate truth value. While this may seem an unduly simplistic approach in view of the ramifications of the models given for modal systems, it is in fact the approach with the closest analogy to that suited to well-established logical systems. Let us however attempt to modify it to take account of difficulty.

The difficulty may be stated as being that if we use an arbitrary assertion of appropriate truth-value to correspond to 'NP' in the counter-example then we fail to do justice to the supposedly special relationship between "P" and "NP", and show no reason why a sign such as 'N...' should be used instead of just another variable whose value is related to that of 'P' by means of some asserted formula or whose value is fixed (i.e. using a constant); yet if we follow the plausible path of generating an assertion to correspond to 'NP' simply by prefixing 'It is necessary that...' to the assertion which we used to correspond to 'P', then in a substantial proportion of cases (perhaps even 50%) we shall end up with assertions whose truth value does not correspond to that given in the row which we are using as the recipe for the counterexample.

One manoeuvre whose futility is worth pointing out, is that of using, not the form "It is necessary that ...." to correspond to 'NP', but just the same form of words as was used to correspond to 'P'. Such a move could well occur to one when reflecting on the clear intention in the models for Lewis systems to display the modal symbols

as covering properties and relations which hold of given things irrespective of what truth-value they happen to have. Thus it might be plausible to say (i) the formula 'P' is to be matched with an actual assertion which we are forced to accept or reject on the basis of our assessment of its truth value, but (ii) such an assertion can sometimes be indisputably of one truth-value and sometimes be definitely of one or the other truth-value but still not be indisputably and inevitably the one we judge it to be. On the basis of this last consideration we might wish to see 'P' symbolizing the assertion as a simple true statement and 'NP' as symbolizing it as an assertion which purports to reveal something which is necessarily so. An assertion which purports to reveal something as not merely being so, but being necessarily so, is not bound to use any modal words, but is still clearly separate from the assertion of that same claim as fact. The upshot of this approach would be to say that the truth-value assigned to 'NP' is not simply dependent upon that assigned to 'P', and that its partial independence allows us to use it to represent our agreement or disagreement with the purported necessity of what is said. (Given of course that that which is said is true, and the independence hence arises).

Our example shows that this idea does not help. 'Pigs cannot fly' is a perfectly understandable assertion. It can certainly be presented as a statement expressing something necessary, and the

purported necessity of what is said can be put in question by showing that 'Pigs cannot fly' is untrue. However given that it is true that pigs cannot fly, we shall have to come down either for or or against the necessity of pigs being unable to fly. Whichever we do we are faced with the problem of those cases where the truth-value assignment in the row we are dealing with conflicts with that we decide to give to the assertion we are trying to incorporate into the counter-example.

It will be clear that this is a dichotomy. Either we regard the 'NP' type of formula as being properly matched with an assertion which is to be chosen arbitrarily* except that when 'P' is matched with a false assertion this chosen assertion must be a false one, or we postulate a semantic relation between the two assertions used and run the risk that the rule used to generate one from the other leads us into conflicts over truth-values.

But what of the "possible alternative worlds"? Hintikka has managed to invoke the traditional idea that that which is necessary is that which is true in all possible worlds, and to claim that the models such as his for the Lewis systems, are refinements on this type of analysis of necessity. The explanation, one might therefore think, for the conflict between the truth-value in the row we

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*For simplicity we are still discussing only the case of the only expression of the form 'Nx' in the row of headings; where several such expressions occur the restrictions become more complicated, but they are never adequate to determine completely what value such an expression should be given, and hence an analogous dichotomy can be stated.

are considering and the truth-value we would in fact wish to assign to the assertion which seems the appropriate one to match with 'NP', could arise because the row we are working with is not the row representing this possible alternative world. That is, we happen to have found a counter-example which is 'in' a possible alternative world other than this, the real world.

Such a supposition is equally fruitless. The formula to which we are producing the counter-example could perfectly well be the 'NP' which is now being considered; when this is so and the row from which we are generating the counter-example assigns 'false' to 'NP' whereas we ourselves tend to think that the assertion generated by the normal rules from that corresponding to 'P' is in fact a true assertion, then it does not help in the least to say that that assertion could be false in a "possible alternative world". A counter-example in order to be of any use must be a real counter-example in the real world, and not one in a "possible" world.

To put this last point in another way, we should realise that the whole truth-table for KC is intended as a summing up of all the possibilities and that where a row of this truth-table assigns a formula the value 'false' this indicates that in a good interpretation of the system it is possible to generate a counter-example where an assertion corresponding to this formula clearly is false. Similarly the rows or front-face rows, tableau assignments for a Lewis system must be taken to sum up all possibilities for giving

assignments to the formulae of the system. Our row with a 'false' assignment under a given formula, or our tableau for that formula which remains unclosed, represents such a possible assignment case. The whole point of having a decision problem or valuation problem solution, is that with its help we are able to find any such possible assignment case which may exist, and as a direct consequence thereof can say that the purported theorem is not a theorem. Yet if this formal counter-example does not correspond to a concrete counter-example in the real world which we are interested in symbolizing, then we are in no way served by having the system with or without its solution to the decision problem. In particular the idea that the formal counter-example may turn out to correspond not to a real counter-example, but to a counter-example in another 'possible' world, is tantamount to the idea that a purported theorem of the calculus under consideration may fail to be a theorem because of a 'counter-example' which is of relevance only if the system were being used to symbolize a world which by the supposition of the case we are not interested in symbolizing. If we are interested in symbolizing this world, we shall prefer a system which deals with the possibilities and the facts of the real world even at the expense of the possibilities in alternative worlds.

We feel that enough has now been said to establish that the Lewis systems have not provided the "answer" which makes all new foundational work on modal logic superfluous. Not only has no

philosophically attractive interpretation of these systems emerged from our discussions, but we have remarked in many quarters analogous shyness about giving concrete examples. The remainder of this study is intended to be a more constructive approach to the formalisation of a modal logic for necessity - which is taken as a paradigm case. The mire of imprecision and light-hearted transplanting of concepts into contexts where their application is no longer unambiguous, such as we were subjected to when studying Lewis's writings and which despite the devoted efforts of his followers has only in small measure been alleviated, leads us to the conclusion that the most promising start to such a study lies not with the other writers whose systems enjoy smaller followings than the Lewis systems, but rather with a fundamental examination of how one sets about symbolizing any subject matter, and then an examination of what aspects of "necessity" are worth while symbolizing. When these matters are settled - thus beyond the scope of the present work - it will become to the point to consider just how far existing systems live up to the requirements, and just how far alternative systems are desirable and possible.

PART 2

CHAPTER IV

SYMBOLIZATION

In Part I we have shown how one particular group of systems which are supposed to be suitable for symbolizing the notion of necessity, have been developed in a way which obscures their semantics, and, despite more sophisticated recent developments, fail to provide us with a ready means for carrying out specific tasks of symbolization. It would be dreary and pointless to go through the multiplicity of other systems proposed as modal logics (the most notable being those of Łukasiewicz, H. B. Smith, and Church) using the approach applied to the Lewis systems. Instead it is better to develop in more detail the general considerations applying to the whole process of logical symbolization, so that the criteria for assessing such systems as systems can be clear. It will then be possible (in Part 3) to go on to discuss the particular problems associated with the notion of necessity itself, and finally to sketch out the lines along which a satisfactory logic of necessity should be sought.

The purpose of this part, therefore, is to lay down some principles of calculus construction. This will be done in two stages. First, we shall present symbolic calculi in terms of a series of levels of sophistication, indicating the types of symbolic apparatus to be found on each level, and the criteria which are relevant for making assessments of the success of these (we introduce

the term "formalism" as a general one for symbolic apparatus at any of these levels). Later (in Chapter V) formalisms will be discussed from the point of view of their subject-matter - a topic which when applied in the case of logics is usually known as that of "logical form".

The levels of sophistication at which we consider formalisms will be referred to as those of (i) signs or sign-sets, (ii) sign-systems, (iii) symbolisms,* (iv) sub-logics, (v) calculi or logical algebras. Of these the fifth corresponds most closely to a familiar classification - that of a formal logic for which a matrix formulation is specified. The remaining levels are more or less arbitrary but are chosen so that each of the five, by its differences from the others, serves to focus attention on one or more of the characteristics found in axiomatized systems. It is not our intention to claim that there cannot be axiomatized systems which lack some of the characteristics of, say, logical algebras, but we do claim that those axiomatized systems which normally figure in the discussions of logicians should fall into the category of logical algebras if there is to be much hope of fruitful outcomes to such discussions.

To provide a concrete example illustrating the various levels of formalism, we shall introduce a series of related formalisms

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*The term "symbol" is used throughout this Part in a wide sense - such as it normally has - and is not to be seen as specifically related to the term "symbolism". The italicisation of the latter will perhaps serve as a reminder of this.

which have as their intended interpretations the currency of the United Kingdom. At the lower level the examples will be of formalisms which merely symbolize certain coins; but later the 'subject-matter' is extended to sums of money in general and a notion of provability from axioms is introduced. Where appropriate other examples are also given at various stages.

Signs or Sign-sets

If it seems likely that one will forget some important matter, then it is a good idea (so we are told) to tie a knot in one's handkerchief so that the next time one wishes to use the handkerchief the presence of the knot will serve as a reminder of the fact that there is something one ought to be remembering, and so that thus in the end one is able to avoid forgetting the important thing in question. If this is true, then the knot on the handkerchief serves as a symbol of the fact that there is something to be borne in mind and not forgotten. It is a peculiarly simple sort of symbol in that it is specific to one person and symbolizes one, ad hoc, thing, (in the normal case that thing is the need to remember - it might equally well be that someone else uses a knot in his handkerchief to symbolize something quite different - purely ad hoc).

This and other mnemonic devices whereby one is enabled to remember the monarchs of one's country or the sequence of colours in the visible spectrum, can be put into the first level of sophistication of formalisms. This category - of signs - can be extended a

little beyond such examples so that it covers all cases where we apply to something about which we might think, some conventional correlation by which we are able to use the thought of the correlate either in place of, or as a cue for, the thought of that with which we have correlated it. As will be clear from the examples already given, the notion of "convention" is here used in such a sense that it makes sense to talk of conventions made with oneself; the criterion by which one may distinguish a convention made with oneself from an association which one knows to be made by one's own mind, is that in the case of the convention one may well talk of "forgetting" or of "remembering" and - where it is complicated - may set out to "learn" it, whereas in the case of an association the terminology used is that of its "being made" or "not being made" on a given occasion and its "disappearing" and "reappearing" over a period, while what one would set out to do would be to "strengthen" or "establish" it rather than learn it*. Similarly the convention

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*Had the word "objectify" not acquired many overtones in the history of phenomenalist and existentialist thought, one might well describe the difference between knowing that one habitually makes certain associations and knowing a convention that one has established for oneself, in terms of the convention's being an association which had been objectified in one's mind so that the failure of the association to be made in some case was evidence of a failure on the part of the mind, and not of a mere change in its behaviour.

The explanation of a "convention" as something about which it would be possible on a given occasion to be mistaken, is related to this explanation but it seems to us that it must follow immediately from any normal definition of "convention" that mistakes are possible, and that this explanation is thus of little interest when stated so baldly.

is able to be "used", while an association could only be "relied upon.

The extent of the category of signs and sign-sets (these latter being collections or sets of signs) is in practice quite wide. Many of the words which we use in everyday speech may be regarded as signs. In particular, proper names and common nouns normally can be used in this way, and it is the ease with which such words draw our attention to objects or classes of objects, that leads to the tendency to say that the words "stand for" or "mean" those objects. There is certainly much truth in the view that common nouns and proper names can be used simply as signs - consider the writing of a shopping list or an invitation list for a party; these are cases where it would be bizarre to claim that the individual words would be meaningless or could not be understood if not taken as being embodied in a sentence, (the rest of which was inexplicit in this case and had to be "understood")*.

As a final illustration of this category, one may consider how one would set about devising a formalism which would be of this level and could be applied to the coins of the United Kingdom. We can solve this problem simply by choosing tokens of some sort, which are such that the thought of one of them is conventionally associated with the thought of a coin of one or other sort, and that each token is thus associated with one and only one of the sorts of coin.

*It is however, another matter whether such words always have this sort of use, and these examples do not weigh against the contention that words, as used in sentences, should be regarded as not having pre-established meanings which might be investigated apart from the investigation of the meanings of the sentences as wholes.

For example, we could use a set of photographs of the coins in question (i.e. of specimens of each type). Such a formalism might serve as a way of indicating to a purchaser of some commodity how much he was to pay for the quantity supplied. Or again, we might simply use pieces of card on which were written the English words for the coins concerned - viz. "halfpenny", "penny", "threepenny", "sixpence", "shilling", "florin", and "half-crown". These cards could be used in a similar way. Moving away from visible tokens one might suggest a set of spoken signs such as the English words for the coins - as these words are spoken and understood by English speakers.

In each of these cases the signs in the sign-set which is proposed as a formalism for the coins, are to be taken as requiring an explicit clause in the convention correlating each one separately with a coin type. It is not intended that the formalism be established by a general convention that "The photograph of a coin of any type, is associated with a coin of that type"; similarly in the case of the words, no reference is supposed to have been made in the convention to the fact that "halfpenny", "threepenny" and "sixpence" are words which appear to stand for multiples of a penny; their only significance for these purposes should lie in an explicit convention linking each separately with a type of coin.

Hence further sign-sets for the coins can be made up taking individual signs from each of the three sets mentioned. Were one not to make this restriction, it would be difficult to maintain that these signs were comparable with knots in handkerchiefs and mnemonics. For the knot in the handkerchief, although it serves for us as a symbol of or sign for something, does not do so in accordance with any general rule or principle of symbolization; nor do mnemonics employ consistent conventions - for example the mnemonic* for remembering the colours of the visible spectrum cannot be guaranteed t to operate on the same principle as a mnemonic for remembering kings of England, nor as any other mnemonic in particular, and thus it is not possible to be sure that a pair of mnemonics taken together as a sign-set, will display just one principle of symbolization. For this reason the term "sign" and the notion of a "sign-set" are applicable whether or not one single principle of symbolization has been employed in generating the symbols involved.

The criteria for comparing sign-sets or signs, will depend upon one's intended use for the formalism. The formalism which takes as signs the spoken words for the coins, would not be very practical for a shopkeeper wishing to use the formalism to indicate the prices of goods on display, whereas the formalism employing cards on which were written the appropriate words would be a reasonably suitable

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*The mnemonic is here taken as a single sign which enables us to remember a list.

one since these cards could be displayed beside the appropriate goods without any need for tape-recordings or assistants to speak continously beside the display stands.

On the other hand auctioneers might well prefer the spoken signs. Factors such as the speed with which the signs may be manipulated and the ease with which they may be differentiated under difficult conditions, will also have an influence on our choice. A slightly different type of factor is that of the universality of the convention governing the use of the formalism; it is preferable to employ better-known formalisms instead of less well-known ones, if one has an interest in communication. However this last factor does not enter into discussions where the hope is to develop a new formalism which will be put to new uses and has from the very nature of the case yet to be popularized.

The criteria which are used for sign-sets tend not to weigh very heavily when assessing the merits of formalisms which are sign-systems as well as sign-sets, since the characteristic properties of the former have a practical value much overshadowing the inconveniences of particular sets of signs. Naturally, though, it is desirable that the signs in a sign-system should constitute the most convenient possible sign-set.

Sign-systems

When a sign-set is such that one (possibly complex) principle is involved in the choice of the signs to represent various things, it

is a sign-system. When one is able to see a sign-set as manifesting a principle of symbolization in this way, one can conceive of the extension of the formalism to deal with other cases and can form a clear opinion of what sort of thing would or would not be suitable for use as a new sign in extending the formalism. Moreover, when one sees a principle at work in the selection of the members of a sign-set, one is able to start talking of the formalism as symbolizing in a certain respect, (we return to this topic below, p.219).

As has been suggested an example of a rule which might be employed to determine the sign used in a sign-system to symbolize a coin is the rule that the coin be symbolized by a photograph of one such coin. In the light of this rule one would be able to say that it was most probable that the sign which, in an extended version of the system, would symbolize a crown coin, would be a photograph of such a coin.

Another type of sign-system for the coins would be that which consisted of the signs:

h p t s S F H

In this case the coins are being symbolized in respect of the initial letter of the English words which specify them, subject to the auxiliary rule* that coins above a certain value have as signs

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*As indicated the principle used for generating the signs can be complex as it is here. A consequence of this is that mere extrapolation from one's experience of a sign-system may be an inaccurate guide to the principle which it employs; for instance, the auxiliary clause in question could hardly be predicted if we were only familiar with the signs "h", "p", "t" and "s". In proposing sign-systems however, one naturally has to state the principle involved in its full complexity and face any critique arising where that principle is inadequate to deal with extensions of the formalism which are felt desirable.

capital letters. In this case too, we are able to speculate with reasonable certainty about the sign to be used in extending the formalism to deal with the crown coin.

Other sorts of sign-system are those which result from the application of rules for producing a written form of spoken words (for example a phonetic spelling system) or of rules for the transliteration of words from one type of script into another (say from Cyrillic to Latin script). In these cases the written forms produced are, like the originals from which they were produced, signs but these, instead of being matched with what they symbolize* merely by arbitrary conventions which have to be learnt in each new case, are matched with what they symbolize by means of some rule laying down that a certain sound in the standard spoken form shall be represented in a particular way (possibly subject to exceptions so long as these are stated in terms of general conditions and not in such a way as simply to list the deviant cases specifically) or that a certain combination of letters in one script shall be represented in a particular way in the other script (again subject to possible exceptions if these are stated generally). Similarly shorthand systems and other consistent systems of abbreviation generate sign-systems for the matter to which they are applied.

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*Qua written forms of spoken words (or transliterations) they symbolize the spoken forms, (or the originals); we do not intend to deny that written words can and often do symbolize things in just the same way as spoken words can do and without any need for mediation by way of symbolizing the spoken forms of the words - e.g. written lists. There is no need to regard given signs as functioning as symbols in only one way, and writing down what a person said is not the same as oneself saying in writing just what he said.

In order to consider the comparison of sign-systems let us introduce another sign-system for the coins. This system operates by assigning to each coin the sign consisting of the result of putting the normal symbol for the value of that coin in place of the '...' in 'C(...)'. The signs are

C(½d) C(1d) C(3d) C(6d) C(1/-) C(2/-) C(2/6)

This system clearly has as its principle of symbolization the rule that the actual symbol for the value of the coin is enclosed in the shown frame; this rule can therefore be applied to any coin the value-symbol for which is known. It can also be extended by the use of other frames to cover things other than coins, for which value-symbols are known - for example Postal Orders and banknotes, using the frames 'PO(...)' and 'B(...)' respectively.

It will be noticed that this system for symbolizing coins (we shall call it "System C") is particularly convenient for reckoning the total value of collections of coins; thus if we have a collection which we represent as:

six of C(1d) two of C(3d)

nine of C(1/-) four of C(2/6)

we are able by the application of the normal multiplication rules for sums of money to calculate merely on the basis of the information derivable from that representation that the value of the collection is 20/-. This is however not determined by the rule we have given for producing the sign-system, since it is nowhere stated in that

rule that the value-symbols should be such that we can do such calculations upon them; so far as the rule is concerned there is no presumption that the value-symbols are any more easily manipulable than the English words referred to in the rule for the alphabetic system. Were we to wish to, it would be possible to introduce a supplementary set of rules which laid down the methods of calculation which would apply to the symbols "h", "p", "t", "s", "S", "F", "H" in the alphabetic system, and thus the two could be made comparable again. However the addition of such rules raises the formalism from the level of a sign-system to that of a sub-logic, a type of formalism which will be discussed later.

The criteria which we shall apply in deciding which of two sign-systems is superior, will deal with the differences between the rules of symbolization they employ - since matters connected with the signs as such will be dealt with by the criteria for signs and sign-sets and matters like the rules for manipulating the signs within the formalism arise only when considering the formalism at a higher level. When one has two sign-systems for symbolizing the same subject-matter, it would seem normal to prefer that which employs the more familiar or generally accepted principle of symbolization; but this preference may be qualified by factors such as the risk of confusion if one system involves signs not clearly distinct from others likely to be used in the same context. Furthermore the familiarity of principles of symbolization is likely

to be relativized to the purpose which one has in using the sign-system; the use of principles based on the sound of spoken words is familiar in shorthand systems and in systems for the transliteration of written text between different types of script, but sound need not always be a useful basis for symbolizing linguistic entities.

The criteria which are employed to compare sign-systems, like those employed for sign-sets, are largely concerned with the psychology of the users of the formalisms - or the intended users. In view of this it is worth commenting on the view that logic should be as independent of psychology as possible. While this is a creditable doctrine if it means that the canons of logic should as far as possible be indifferent to the subject-matter of any discussion in which they are applied and to the interests and intentions of the person applying them, it is possible to be misled by the doctrine into thinking that logic should be independent of any interest or intention at all. But in a trivial sense the prime interest of logic is in being logical; in less aphoristic terms one may say that insofar as one of the criteria for judging a logic is its ability to depict argument forms which lead only to true conclusions (given true premisses), there is a sense in which we are not neutral as to the principle of symbolization employed by a system which purports to be a logic.

Even if we allow that it is in principle possible that a formalism should be a logic in the sense that it provided facilities

for depicting valid argument forms, while it employed a principle of symbolization which bore no relation to the truth or falsity of that which is symbolized, we should still be able to say that the formalism concerned employed a sign-system which was inconvenient and probably misleading. This would be the case, for example, if on the basis of a sign-system which symbolized utterances in respect of their length we were able to set up the remaining apparatus giving us a sub-logic or a calculus which answered to our desires as logicians; the independence of length and truth as properties of utterances - which we have supposed - is sufficient to ensure that this formalism is inconvenient and confusing to use, and is probably sufficient to indicate that the success it has had in symbolizing truth relationships amongst utterances so far considered has been fortuitous and cannot be guaranteed for future applications. We shall of course return to the criteria for comparing logics at a later stage, but it is important to point out at this stage that the prospective use of a formalism should influence the way we set about symbolizing a given subject matter, even at the level of sign-systems.

Thus although the basis for preferring sign-system to another may be the relative familiarity of the principles of symbolization for the intended users of the formalisms, a sign-system which is also a formalism at one of the higher levels may well employ a principle of symbolization dictated by the uses for which its higher-level attributes makes it especially suitable.

Symbolisms

It has been noted that System C, in addition to its properties as a sign-system, employs symbols for which we can easily state manipulation rules. These rules, since they generate value-symbols and not coin-symbolizing signs are not strictly rules for the manipulation of the symbols in System C alone, but they do illustrate the notion of symbols in a formalism having some systematic inter-relation amongst themselves which can be defined apart from the system itself. It is when the symbols used in a formalism are thus systematically interrelated that the formalism is called a symbolism.

A good example of a symbolism can be found by considering a typical "metric". Suppose we wish to devise a formalism for representing the points on a finitely long line. We may choose one end of this line as the origin and express the position of every point on the line in terms of its distance from the origin. Now if the various points were to be symbolized in our formalism by unconnected and randomly chosen tokens (or symbols), then the formalism would be a sign-set. If instead of such, randomly chosen, tokens we chose the tokens according to some system or principle of symbolization but chose some property of the points other than their distance from the origin as the basis according to which the principle generated the sign, then we should have a sign-system. However, if the principle of symbolization generated signs for the points on the basis of their distance from the origin, and these

signs were real numbers, then we should have a symbolism.

All that is necessary to make a sign-system into a symbolism is that we define some ordering or interrelation among the signs, which is independent of what these signs symbolize in this particular formalism. Such a defined ordering or interrelation would not be independent, for example, if the properties assigned by it to a sign were assigned to that sign in virtue of its symbolizing a certain thing or a thing with certain specified properties. The ordering or interrelation thus defined will be referred to as the "symbol-system" for the symbolism. By the very description thus given to it, a symbol-system is capable of being possessed by several different symbolisms.

Clearly this definition of symbol-system is so wide that almost all signs which one ever has cause to use are in some sense parts of symbol-systems. For instance the alphabet is a symbol-system and hence the alphabetic system for symbolizing the coins has a set of signs among which an ordering is defined and which therefore are a symbol-system. The set of photographs which were suggested earlier as signs for the coins, will probably not have any ordering or interrelation in particular amongst them, but there is no reason why we should not establish one here and now; the set could then be. used in other symbolisms.

If a sign-system can so easily be turned into a symbolism, what is the point of distinguishing the two? Cannot a mere sign-set

also be turned straight into a symbolism by defining an ordering on the signs it employs? These are two questions which immediately press themselves forward, and must be answered before farther exposition of the notion of a symbolism.

The reason for distinguishing sign-systems from symbolisms, is that there are two separate questions involved when one asks of a formalism (a) in what respect the signs symbolize the subject-matter, and (b) what system of interrelation of the signs is built into the formalism. It is difficult to think of actual sign-systems which employ signs on which no ordering is defined and for which we have no normal interrelation; this fact proceeds largely from the natural preference for using familiar symbols when constructing new formalisms. A possible example of a sign-system whose symbols are not necessarily interrelated or ordered would be a system of metaphors* which used as symbols (signs) for various types of event, typical products of those types of event - for example corpses to symbolize war, sheaves to symbolize sowing; in such a case a principle of symbolization could well be discerned but the signs generated would not be such as to have an interrelation or ordering apart from that arising from this metaphorical system itself**. Having seen this example as a sign-system it is possible, as always, to go on and turn it into a symbolism by inventing rules for connecting the signs; that this is a separate matter may be seen from the fact that the rules

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*to be used in a particular genre of poetry, perhaps, or as a "secret" language.

**We are indebted to Mr. Iain C. Orr formerly of Linacre College, for this example.

invented would also serve to connect those signs in any other case where they might be used for symbolization.

Suppose we have a mere sign-set - its members might be : a cat, ".", middle G on the piano - and someone proposes an ordering amongst the signs;* shall we allow that the resulting formalism is a symbolism? This is largely a pragmatic question. Although a symbol-system has been created, there can be no reason to suppose that any new sign added to the sign-set can be incorporated into the symbol-system except by a purely ad hoc device. In the case of a sign-system whose signs form a symbol-system, it is clear that whenever we can discern some pattern or ordering amongst the symbolizanda in respect of the range of properties by reference to which the principle of symbolization lays down which sign is to be used, then we can also define an analogous pattern or ordering amongst the signs themselves. This analogous pattern or ordering will clearly be such that whenever another symbolizandum is dealt with according to the principle of symbolization, the sign which symbolizes it will be related to the other signs already in the symbol-system in a way analogous to that in which the symbolizandum is related to the previously symbolized things.

If the pattern or ordering is only in relation to properties irrelevant to the choice of symbolization then there can hardly be the expectation that an analogous pattern can be defined for the set of signs in such a way that signs introduced when the set of

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*e.g. by arbitrarily assigning real numbers to the signs.

symbolizanda is extended must fit into that pattern or ordering. For example, if we symbolize ten members of a football team with respect to their weight then it will not follow from our ability to map the numbers on their jerseys onto the weight-based symbols, that we shall be able to use the same rule for mapping from the number on the jersey of the 11th player to the weight-based symbol we assign to him.

The restriction of the term symbolism to sign-systems for which there are symbol-systems, seems to be acceptable in the context of a study such as logic. We shall be concerned here with the symbolization of subject matter which we believe to contain some pattern, and it would be troubleseome to have at each stage to consider the possibility that a symbolism might be being used to symbolize a random selection of things amongst which no order or pattern was to be presumed. In other fields it might well be worth considering sign-sets whose signs are symbol-systems: an obvious example is in the lexicographical treatment of ordinary language words, since at least some sets of words are mere sign-sets, but the fact that they may be alphabetically ordered when written down is the basis of our ability to make and use dictionaries.

The criteria for assessing symbolisms proceed naturally from the consideration of what interests we have in the development of symbol-systems for the signs in sign-systems. As has been suggested, the basing of the principle of symbolization for a sign-system upon

the same range of properties as are involved in our appreciation of some pattern in the symbolizanda, leads to the possibility of our defining an analogous pattern in the signs generated by the principle of symbolization. Thus no matter what the typographical form of the signs generated the fact that one's principle of symbolization bases the sign upon the height of the symbolizandum will enable one to define relationships amongst the signs which will parallel the relationships amongst the heights of the symbolizanda; when another symbolizandum is added, one will be able to accommodate the sign generated for it into the symbol-system in such a way as not alter the symbol-system for the other signs and that all the relations in the symbol-system which previously mirrored relations amongst the heights of symbolizanda still mirror the same relations. This feature of the behaviour of symbolisms is the one which is most frequently used and of most interest in the present context.

Where the signs employed in a symbolism have an ordering defined for them which is not mirrored in the symbolizanda, the symbol-system in question is redundant and irrelevant.

It should be noticed that there are two different levels of ignorance in which we may approach a formalism when seeking to regard it as a symbolism. On the one hand we may already have decided what principle of symbolization is involved in the production of the sign-system; in this case we need only look for the parallelism between the interrelations of the appropriate properties of symbolizanda and

those of the signs in the symbol-system. On the other hand we may be in ignorance even of the basis of the principle of symbolization, and in this case all interrelations of the symbolizanda whatever will potentially be mirrored by the relations in the symbol-system. In this latter case it would be possible to go from the discovery of a mirroring relationship between the interrelations of the symbolizanda and those of the signs, to the discovery of the principle of symbolization which would generate those signs on the basis of the properties in terms of which the interrelations of the symbolizanda are described. When we have not decided what the basis of the principle of symbolization is, it is almost impossible to state categorically that a symbol-system for the signs is an irrelevant one, because the number of possible bases for interrelating most sorts of entities* seems infinite.

Another important point is that the discussion so far has not taken into account the way in which the rule of symbolization does the specification of the signs assigned to symbolizanda. In

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*The situation is a little different in the case of the symbolization of "abstract entities". If such abstract entities are mere abstractions from real things and have as such only certain specified properties then it does seem possible to say whether the symbol-system concerned is relevant or not, since the principle of symbolization can only be based on properties not effaced in the process of abstraction.

principle this specification might take place by the device of quoting or of describing; however in cases such as metrics the specification is given in terms such as "the number giving the number of times the unit quantity can be taken from the quantity possessed by the symbolizandum" and in such cases it is clearly a pre-requisite for our actually doing the symbolization that we should understand the real number system. It might seem as if the symbol-system which is "discovered" in the symbolism is in fact determined by the principle of symbolization which is already at work in the sign-system. Now while it is certainly the case that there can be principles of symbolization which lay down rules for assigning signs to symbolizanda in terms of which it automatically follows that the signs will form a symbol-system (e.g. the rule that successive integers be used to symbolize items to be symbolized*, would be a simple rule of this sort) it is by no means necessary that this will always be true. The example of the system of metaphors shows this, and one may well imagine a principle which assigns to symbolizanda successive members of a list of quoted signs which have no interrelation other than their membership of this list; the fact that a genuine principle will

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*That is, each different sort of item would be assigned an integer as its symbol. If every individual item presented were to be assigned a separate integer, we should obtain a system which dated the presentation of items. The dating of objects and occurrences seems to be a procedure yielding a symbolism as here described; however this is not the normal intention of dating things, since we do not represent that which is assigned a date by that date. We tend father to use the date to relate things to one another in a more precise way. The whole topic is too complex and important to be dealt with in this context. The degeneration of attempts to symbolize the interrelations of events, into mere symbolizations of their temporal sequence is perhaps of more direct interest. Cf. the remarks of H. Hiż on the "clock axiom" in his review of J. Łoś's Podstawy analizy metodologicznej Tranonow-Milla. (425A and 534F).

presumably give some instruction for choosing which sign is to be used, and will base the instruction upon some fact about what is to be symbolized, does not in itself lead to the generated sequence of signs being a symbol-system at this stage, (though of course, once we know the basis of the principle we can proceed to define relations to hold amongst the members of the sign-system - relations which will produce a symbol-system much more relevant than that which resulted when successive integers were used to symbolize each new item presented).

Finally one should note that the possibility of defining new symbol-systems for a given set of signs will always exist. Thus whenever a sign-system can be developed it is an open question whether this will be turned into a symbolism, but there is very little doubt that it can be. Our criteria for judging symbolisms are to apply to just those formalisms which are at the moment endowed with symbol-systems for otherwise we may spend the rest of our lives assessing new and unwanted possible symbolisms.

Following through the example of the symbolization of coins it is worthwhile illustrating the notion of a symbolism by one final example. Here instead of symbolizing coins, the formalism is for symbolizing sums of money, and is in fact the one which will come into use in the United Kingdom after decimalization of the

currency (to the base £1). Thus all sums are expressed as multiples of, or decimal parts of, £1. The coins which were symbolized earlier have as their values, in this system; £0.002083 £0.00416 £0.0125 £0.025 £0.05 £0.1 £0.125. The normal rules for ordering real numbers apply to these symbols as they would to the numerical part of them and thus the formalism is a symbolism.

Finally to recapitulate, the appropriate criterion by which we judge symbolisms is that of the relevance of the symbol-systems they employ. The relevance of a symbol-system is a measure of the extent to which the interrelations defined amongst its symbols mirror the interrelations which exist among the symbolizanda in respect of the range of properties on the basis of which the principle of symbolization operates. The principal interrelation of sums of money (with respect to the "amount", which we take to be that on which the symbolization shown is based) is that they are ordered and additive according to the normal rules. This feature is shared by the symbol-system in the symbolism, and so that symbolism is at least to some degree a relevant one.

Sub-logics

A sub-logic is a symbolism the signs of which include both "simple signs" and "complex signs". A "simple sign" is a sign no part of which is a sign of the same symbol system; a "complex sign"*

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*How such a sign is arrived at in the process of symbolization is taken up in Chapter V.

is a sign at least some part of which is itself a sign of the same symbol system. In this explanation the notion of "being part of" remains undefined; it would not be satisfactory to define it purely in terms of the properties which signs may be regarded as having qua material objects, sounds, etc. since this would make "c" a part of "o" when written, and "a" a part of "I" when spoken with some accents. Yet a definition which allows as 'parts' only those things which figure in the symbol system in such a way that the behaviour of the complex is a function of the behaviour of its parts, would leave us with the notion of the "arguments of function" as primitive instead - since the application of a function can only be defined in terms which refer to the parts of the expressions in which that function is applied.

In a sub-logic each complex sign is equivalent to some simple sign; that is to say that we may define certain functions which map n-tuples of simple signs onto simple signs, in such a way that any complex sign will be regarded as equivalent to the result of some series of applications of such functions to simple signs which are either themselves parts of the complex sign or are simple signs onto which the functions map n-tuples of simple signs which in their turn are part of the complex sign. Many complex signs may be equivalent to one and the same simple sign.

Normally a complex sign will be written in such a way as to make this explicit by the graphic inclusion in the complex sign, not

only of the simple signs referred to, but also of marks which indicate the functions concerned and the groups of simple signs (or complex signs for which are to be taken the equivalent simple signs) which are in each case the arguments of these functions.

Arithmetic is a familiar sub-logic. We write the natural numbers as simple signs, and from these we may construct complex signs such as "6 x 4" and "5 - 3" which are equivalent to natural numbers. Similarly we may discern a sub-logic implicit in the propositional calculus, KC: here the simple signs are the two signs "T" and "F", and the complex ones are constructed recursively out of signs by the application of "~" to one sign or by enclosing in brackets the results of joining the successive member of an ordered pair of signs by "."; thus giving complex signs such as "~T" or "(T.~(T.F))".

The symbolism for sums of money outlined in the previous section can easily be turned into a sub-logic by the introduction of complex signs involving "+" and "-" and equivalent to the sums of money signs formed by prefixing "£" to the result of the addition or subtraction of the numbers in the pairs of signs concerned. It will not however make any sense to introduce the function of multiplication into the sums of money formalism in the normal way; while it is possible to multiply a sum of money by a certain number, it is not possible to multiply one sum of money by another. This is

a worthwhile caution against assuming that because sub-logic contains a given function, any other sub-logic which uses typographically similar signs (but not necessarily to symbolize the same things or to symbolize in the same respect) must admit of the introduction of that same function. This point is clearly of relevance to the discussion of "transcription" in Part 1.

In clarifying the criteria for judging sub-logics it will be helpful to return to the task of symbolizing coins. The two formalisms which have been set out already can both be regarded as symbolisms provided an ordering of the signs in them is defined. In the case of the alphabetic formalism this ordering may be exhibited as follows:

h p t s S F H

An anlogous ordering may be imposed upon the other "value-based" signs by reference to the magnitudes of the value of the sum-of-money signs they contain. It should be noted first that the ordering imposed upon the alphabetic signs is additional to the ordering already done by their membership of the ordered system of signs known as the "alphabet", and that on the basis of that ordering alone the formalism could have been regarded as a symbolism, albeit one with a symbol-system of little relevance. Secondly the idea of imposing another ordering on the letters of the alphabet is a quite normal one employed by the Greeks and Romans in developing their systems for enumeration; for example in the Roman system:

i v x l c d m

Once the symbol-system is laid down, it is immediately possible to define functions which will map simple signs or n-tuples of simple signs into the set of simple signs. The function "Inverse" might well be defined which takes one argument and has the values. H, F, S, s, t, p, h, respectively in the cases where the arguments are h, p, t, s, S, F, H. Using this and similar functions of more arguments we may construct complex signs in such a way that to each complex sign there will correspond an equivalent simple sign which is discoverable by the application of the functions contained in, or appropriate to, that complex sign.

As the difference between sub-logics and symbolisms lies in the existence of complex signs in the former and the notion of a function, by means of which we explain the equivalence of complex signs to simple signs, the basis for comparison of different sub-logics will lie in the variety of types of function which may be employed in reducing their complex signs to simple signs. Let us therefore consider a pair of functions which might be introduced in the formalism for the coins.

The two functions "+" and "×" are such that the tables set out below indicate the simple sign onto which the function in question maps the ordered pair of simple signs consisting of the signs written respectively opposite the row in question, and above the

column in question:

+	h	p	t	s	S	F	H
h	p	p	t	s	S	F	H
p	p	t	t	s	S	F	H
t	t	t	s	S	S	H	H
s	s	s	S	S	F	H	H
S	S	S	S	F	F	H	H
F	F	F	H	H	H	H	H
H	H	H	H	H	H	H	H

⁎	h	p	t	s	S	F	H
h	H	S	p	S	H	h	h
p	F	t	F	h	p	p	p
t	s	H	p	t	t	t	s
s	h	t	s	s	s	S	H
S	s	S	S	S	F	h	s
F	F	F	F	H	p	S	p
H	H	H	h	t	F	t	h

Both of these functions are full-defined in the sense that for any pair of arguments they give a value; '⁎' is also complete in the sense that every sign is the value of at least one case of the application of "⁎" to a pair of arguments. Little more can be said about the two from a formal point of view. None the less there are grounds for preferring "+" to "⁎" given that this is a formalism for symbolising coins.

It will be observed from the table for "+" that it is a function mapping pairs of simple signs onto the simple signs which symbolize whichever coins are nearest in value to the sum of the values of the coins symbolized by the pair of signs, provided that where two coins are equally near in value to this sum, the sign for the higher-valued is chosen. The table for "⁎", however, does not seem to display any "system" of this sort at all, and it is reasonably certain that even if some economical rule could be stated for producing the mapping

which ⁎ defines for pairs of signs, this rule would not have any connection with such familiar properties of coins as their values. A difference between the sub-logic with "+" and that with "⁎" is thus revealed by a consideration of possible bases for finding a link or systematic connection between the things symbolized by arguments of these functions and the things symbolized by the corresponding values of the function; in the one case such a basis is found in the values of the coins concerned, and in the other case no basis is (yet) discovered, and it may even be thought that none exists. This difference, like those already found at others levels, will be a pragmatic one, and so it might well be given a name showing that emphasis; thus we may say that in the case of the symbolization of coins, the sub-logic with alphabetic symbolism and "+" is more useful than that with alphabetic symbolism and "⁎"; the reason for this usefulness is the presumed primacy of our interest in the values of coins and the interrelations of these values.

In the sub-logic of arithmetic it is not so easy to find a relatively useless function like "⁎". However one may introduce one if we accept the notion of a list, A, which is an infinitely long list of random numbers. "U" our relatively useless function, may then be taken as the function which maps pairs of natural numbers ontc members of the list A, in such a way that the successive members of A are assigned as images to successive members of the series of ordered pairs, (x, y), which is arranged in ascending order of magnitude of

$2^x.3^u$. Thus if the list A starts with the five numbers, 89, 1, 47152, 900237, 5561, 10, then the complex signs "1 U 0" and "1 U 1" will be equivalent to the simple signs "1" and "10" respectively. Since the members of A are randomly selected this would appear to be a function whose usefulness is almost nil.

We might analogously introduce a useless function into the sub-logic for KC by means of a list, B, of randomly selected "T"s and "F"s, and some principle which would lay down a unique ordering for the n-tuples of simple signs which the function might take as its arguments*.

Once we have decided on our assessment of the usefulness of the functions in a sub-logic, we may go on to inquire into a special property which sub-logics may possess. This property, which we shall refer to as "coherence", links the principle of symbolization by means of which the signs in the sub-logic are assigned to specific symbolizanda, and whatever view we have taken of the system of relations among the things symbolized in order to assess the

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*The functions about which we talk take simple signs as arguments and have simple signs as values - or map n-tuples of simple signs into the set of simple signs; the functions should not be confused with the marks which may appear in complex signs and may be thought of as indicating which functions are relevant for determining the simple sign to which the complex sign is equivalent. In the KC sub-logic we shall have the mark "." as a constituent of complex signs; from the presence of this mark we know that the conjunction-function is the one to be used in finding the simple sign to which this complex one is equivalent. However since the conjunction-function only maps pairs of simple signs into the set of simple signs, it is necessary for us to apply this function, not directly to the pair of parts of the sign which occur on either side of "." and within the pair of brackets closest to it, but to the simple signs which are equivalent to (or are) those parts.

usefulness of the functions in the sub-logic.

It is clear that in order for us to conclude that a certain function in some sub-logic is a relatively useful one, we must be in a position to appreciate the (systematic) existence of a particular relationship amongst the things symbolized by the arguments and the value of that function in each case where it can be applied in the sub-logic. The usefulness consists in the fact that the function provides a means of defining in the formalism a set of relations among the signs which will parallel a set of relations among the symbolizanda - a set of relations in which we have a specific interest.

Now in order for any relationship to exist between a pair of things, it seems that these things must at least have properties in terms of which the relationship may be stated. Thus if one object is greater than another, each must have some dimension by measuring which we can discover this relationship; only in the case of "abstract entities" such as sizes themselves or numbers, does it seem plausible to suggest that the things being symbolized could be related without there being some particular aspects of them or properties of them which we see as being the basis of the relationship in question. The significance of this point lies in the fact that such properties as we may see as giving the basis for the interrelations of things, may also be the properties by reference to which the principle of symbolization prescribes the signs which

are to be used to symbolize the particular things.

A sub-logic is said to be coherent, when the properties on the basis of which we see the interrelationship of significanda when we find its functions to be the most useful ones, are also the properties by reference to which the principle of symbolization operates. Other sub-logics are not coherent in our sense. It is clear that a given sub-logic can possibly be regarded in different ways and its functions seen as more or less useful according to the interrelations among significanda which are thought to parallel the functions; in this case the sub-logic may be taken in the way which makes its functions most useful.

Now that our formalisms include both simple and complex signs, problems such as the word problem* become statable in more interesting ways. Thus we may ask whether there is a general method for showing whether or not two signs are equivalent in a sub-logic. It will be clear that there will always be such a method. Our definition of a sub-logic entails that there are no words in the formalism which are not equivalent to some simple signs, and we presume it possible to distinguish the simple signs one from another; the problem thus reduces to that of whether there is a general method of determining whether two simple signs are equivalent; while no mechanical procedure can be laid down for showing two simple signs equivalent or non-equivalent, it again seems clear that all simple signs should be treated as non-equivalent because the principle of

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*c.f. Trakhtenbrot (879A; chs 4,12,13) for a clear introduction to this topic.

symbolization can only generate different signs for two symbolizanda in case they differ one from the other,* and so if we are ever to use the formalism it will be desirable that the simple signs be non-equivalent.

At this level too it becomes worthwhile introducing some more formalized elements into our metalanguage. Thus the behaviour of our functions may be described by the use of variables of the meta-language to range over the simple signs of our sub-logic, and some constant of our metalanguage to designate the function itself. For example the variables "x", "y", "z" may be introduced to range over the set of signs, {T, F} while "K" may designate the conjunction function in the KC sub-logic; then, given the usual convention for the application of a function to arguments, we may say of the sub-logic that where a complex sign consists of a pair of simple signs, x and y, joined by "." the complex sign is equivalent to a simple sign which is the value of K (x,y) - that is, T in case both x and y are T, and F otherwise. Such semi-formal metalinguistic talk is familiar enough and no farther examples need be given. One should however stress that the "x" and "y" in this example are metalinguistic variables ranging over the (constant) signs "T" and "F", in the sub-logic; they are not variables within the sub-logic, for a sub-logic as such contains no signs which can be regarded as variables.

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*We may however, in applying the formalism, choose to represent the diverse origins of different occurrences of the same simple sign by using typographical variants of it. (Cf. p.232).

Calculi or logical algebras

A calculus* is like a sub-logic but in addition to simple and complex signs possesses "variables" and signs of "assertion" (and sometimes, as in Łukasiewicz's later systems, of "rejection").

By "variable" is meant a mark, distinct from the simple and complex signs so far described and from any parts of them, which may occur either alone or in a complex with signs of other variables when such a complex is constructed like a complex sign except that one or more variables appear in this complex just as simple signs might appear in a complex sign. Thus any notation or convention which can be applied in the construction of complex signs may also be applied in the construction of complexes involving variables; such complexes are referred to as "non-constant formulae" and in case they contain no simple signs may be referred to as "fully-variable formulae".

To each variable of a calculus is assigned a "range". The range of any variable is the set of things symbolised by some set of the simple signs in the calculus and it is possible for different variables to have different ranges. We may define the "substitution set" for each variable as the set of signs symbolizing its range.

By the "evaluation" of a complex sign is meant the finding - by means of the appropriate functions applied in sequence to the simple

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*We shall not repeat the alternative term "logical algebra" each time we use the term "calculus"; the two are to be taken in entirely equivalent senses.

signs it contains - of the equivalent simple sign, as described in the section on sub-logics. A non-constant formula is said to be "realized" by the sequence of simple signs which results from the evaluation of the series of complex signs produced when the variables in that formula are replaced by the simple signs within their respective substitution sets - this replacement being according to some systematic procedure applicable to all non-constant formulae, and being carried out so that no complex sign is produced in which different simple signs have been used to replace occurrences of the same variable in the formula.

Two non-constant formulae are "equivalent" if and only if their realizations - according to the same systematic procedure for replacement - are identical.

The assertion sign does not figure as one of the signs and marks which may appear within the expressions of a calculus when these are regarded as symbolizing particular things or ranges of things. It is however used in the calculus as a prefix to any sign which is or has as its evaluation, some particular "designated" simple sign (or one of a range of such signs), or as a prefix to a formula the realization of which consists only of designated simple signs. It is thus clear that the assertion sign may appear in front of any complex sign derived - by replacement of variables by simple signs - from a non-constant formula if that formula itself may have the assertion sign prefixed to it. The converse is also true, but in

cases where an infinity of simple signs exist this is not of practical use.

These definitions do not contain any stipulation which would ensure that the functions used in evaluating complex signs should ever map an n-tuple of simple signs onto a designated simple sign, nor anything which would lead to some variable's being assigned a range which consisted only of designated simple signs. Hence, there is no need for any calculus to contain theorems. However if a calculus does contain theorems then it must contain at least the trivial rule of inference which produces a replica of the theorem to which it is applied.

Calculi, as we have described them, are clearly related to what one normally understands as a logical calculus. This presentation, however, makes it a matter of investigation whether or not it is possible to develop a formalism with axioms and rules of inference; such an investigation starts with the examination of the best way to symbolize the particular subject-matter concerned. It seems to us that this sort of approach is most suitable when considering modal logics, and that very little can be hoped for from an approach which starts from abstract systems framed in terms of axioms and theorems and then tries to find interpretations for them.

In the case of the formalism for sums of money, for example, it is only possible to produce highly uninteresting theorems when we turn from the sub-logic to the calculus. The marks, "x" "y" etc.

are introduced so that the prefixing to one of them of the "£" sign produces a variable whose substitution set is the entire set of signs (simple or complex) for sums of money. As in the sub-logic addition and subtraction are the only functions used in evaluating complex signs and so the addition and subtraction notations are the only ones by which more complex formulae may be built up.

The choice of the designated sign is easy to make in this case. Only one sign has any properties in terms of addition and subtraction such that theorems could emerge in the system, and that is "£0"; with this in mind the assertion sign "NUL" is introduced and may be prefixed to any formula in case its realization consists entirely of occurrences of "£0", and to any other signs in case they are or evaluate to "£0". Typical of the theorems which thus arise are:

NUL £x - £x and NUL (£x + £x) - (£x + £x)

The rules of inference which exist in this calculus are (1) the rule which produces a replica or a realisation of the theorem to which it is applied, and (2) the pair of rules which produce formulae consisting of (a) the joining by '+' and bracketing, and (b) the joining by ' - ', of the pair of theorems to which they are applied.

In the case of the sub-logic for coins, it is equally simple to introduce variables; however the choice of a designated simple sign is considerably more difficult. Certainly so far as variables whose substitution sets are the whole set of simple signs are concerned, there are no theorems which can be stated in terms of

'-' and '+' no matter which simple sign is chosen as designated; the same seems likely to hold for '*', although a rigorous proof of this would be hard. If one abritrarily chooses a particular simple sign as the designated one, then there will be some assertable signs and formulae, such as that sign itself, any complex signs which happen to evaluate to it, and such formulae as are produced by writing such sequences as "-(x-x)" after assertable signs (where 'x' is a variable).

The most familiar calculus is of course KC itself. Its sub-logic has already been described. The calculus itself is produced by adding to the sub-logic a set of variables, "p", "q", etc. all having substitution sets consisting of both "T" and "F", and by taking "T" as the designated simple sign. The assertion sign is written as " $\vdash$ " and the following theorems are found to be suitable as axioms given only the rule that generates β from a pair of theorems of the respective forms α and $\sim(\alpha . \sim\beta)$*:

$\vdash \sim(p. \sim(p.p))$

$\vdash \sim((p.q). \sim p)$

$\vdash \sim(\sim(p. \sim q). \sim(\sim(\sim(q.r). \sim\sim(r.p))))$

The other example which has been used earlier is that of arithmetic using the real numbers. In this case too something analogous to a calculus may be developed, however there may be some doubt about the exact status or nature of the assertion sign in this

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*adapted from those given by Copi in Symbolic Logic (254A).

formalism. The introduction of variables (usually written as "x", "y", "z" etc.) into the sub-logic of real number arithmetic yields the basis of the system normally known as "algebra". In algebra there is not one particular simple sign which, by its overwhelming importance merits being taken as the designated sign, however there is a relationship between a pair of signs - viz. that of their being equal - which does attract attention and upon which something like an assertion sign can be based.

This pseudo-assertion sign of algebra is the sign of equality as it is normally employed; it may be applied (by interpolation between, instead of prefixing) to a pair of signs in case they are or evaluate to the same simple sign, a pair of formulae in case their realizations (taking the substitution sets in the same sequence in both cases) are the same, and to a pair consisting of a formula and a sign in case the realization of the formula consists exclusively of occurrences of the simple sign which that sign is, or evaluates to.*

Although algebra may appear to be deviant in this respect, it is clearly possible for us to introduce into algebra another assertion sign which does conform to the description given. For example the sign "0" might be chosen as the designated one (or possibly the sign "1") and then all formulae which could appear as right- or left-hand parts of equations, the other part of which was "0", could be written with the assertion sign prefixed to them. It is of

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*Prior has recently suggested an analogous view of the logical equality sign (683A).

course possible that some other formalisms could be developed which would be like algebra in giving greater prominence to a relationship between two expressions than to a property of one, but we may be reasonably sure of detecting such cases by a consideration of the properties of the symbolizanda in which we have a particular interest, and we may also be reasonably sure that a logic will not be such a formalism if it is concerned with assessments of "truth" "falsity" and other properties among which are one or two in which we have a particular interest (cf. on the topic of the preference of logic for certain values). In these circumstances the account given of the assertion sign should be adequate for the purposes of a modal logic as well as those of KC*.

Finally turning to the question of criteria for assessing calculi, the most important criterion will naturally be that the sign or signs chosen as designated should be ones which symbolize things in which we have a particular interest. The exact nature of this interest is not important here, but it will be evident that if the calculus in question has a principle of symbolization which takes as

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*The remarks on the role of the equality sign as a pseudo-assertion sign for algebra may suggest that modal logic may also be a sphere where a "two-place" assertion sign is appropriate. Such a supposition, however, leads at once to the idea that the "deducibility sign" be related to the assertion sign of first order propositional logic in the same way as is the equality sign - viz. as a predicate in a metalogic or as a second-order operator.

a basis a certain range of properties and is a coherent calculus, then our interest in the things symbolized by the designated signs should revolve around their possession of a particular one or particular ones of those properties*.

As has been seen in the case of the formalism for sums of money the choice of a designated sign which will allow us to have theorems need not lead to the choice of a designated sign which symbolizes a thing in which we have such a particular interest. In other cases - such as that of the coin formalism - the choice of designated sign may not be sufficiently determined by considerations of our particular interest in things symbolized, and even if it were, this might not lead to there being any theorems. Thus formalisms may be compared first in respect of the satisfactoriness of the choice of designated signs, then in respect of the theorems thus produced and the possibilities of axiomatic formulation with rules of inference.

The last point upon which calculi can be compared is that of the variables they contain. Although in the examples discussed there has been no need for variables with anything less than a range covering the whole class of things symbolized by the calculus, it is quite compatible with the description given that there should be variables with ranges which differ one from another and cover only parts of the field symbolized. In cases where such variables are used, the appropriacy of their ranges and uses is a very·

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*rather than on some extraneous attribute of these things.

difficult matter to gauge; it does not seem to be a general principle that the widest range should be sought for all variables simultaneously since there may well be uniformities in the relationships which certain clearly-defined sub-classes of the things symbolized have to the members of the whole class, and these may permit the existence of theorems involving specialized variables ranging over only such sub-classes but no theorems when all variables range over the whole field. On the other hand a variable whose range is restricted so that there is only one member in its substitution set hardly seems a very welcome addition to the facilities of the calculus since that member of the substitution set can always replace it; variables with less restricted substitution sets can also be unwelcome since the relationships amongst the symbolizanda which they are used to pick out may appear not to be important or to be "contrived" such as would be the relationship between a member of set S and a non-member of that set, if the set were to consist merely of those things symbolized by the three signs which are earliest in the symbol-system ordering of signs. In short, one may say that the variables used will be judged according to our view of how genuine a division of the field of things symbolized is defined by the range of the variables.

Lewis's system as a calculus

This section serves merely to focus attention to two aspects of the earlier discussion of the Lewis systems which can be underpinned

by the account of symbolization given in this chapter. The first of these is the account given in Chapter II of the semantics of value-matrices, and the second is the intuitive account given of Lewis's mistake in formulating the problem for which the Lewis systems purport to provide a solution.

The accounts of a sub-logic and a calculus given in this chapter make no reference to the possibility of an indefinite number of constants all having exactly the same position in the symbol-system and the same behaviour in the system (e.g. assertability, membership of the realizations of various formulae). In particular the description of KC makes no mention of constants other than "T" and "F". Yet in Chapter II account was taken of the fact that systems like KC are sometimes set up* in terms of an infinite number of simple constants, each of which is either true or false in the sense that it is a member of one or other of two equivalence classes arbitrary members of which are related just as "T" and "F" are in the presentation given. The use of such a multiplicity of constants calls for comment, and the presentation of KC without them, as is done here, constitues an underpinning of the intuitive understanding of value-matrices which was put forward in Chapter II (pp.86-92) in that it shows such a multiplicity to be superfluous.

In some particular context where there are two different true assertions to be symbolized, one person may symbolize both by using

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*For example by McKinsey whose article is discussed in Chapter III (p.127 ff.).

"T" twice, while another may choose to use the two constants "A" and "B" - each of which is an assertable constant of the system he is using. The difference between these two cases seems very like that between a use of algebra merely for the purpose of obtaining a solution to an equation and a use of algebraic symbolism in the context of some scientific application. In the latter case it may be desirable to retain the separate identity of two parts of a formula even though in the present case they are the same in value (or range of values) simply because they are the results of quite separate measurements and may be expected in other cases to differ; when it actually comes to working out results in the case of the applied algebra (say in dynamics) then the normal techniques are used and the equality of the two parts is relevant, but up to that point there is a value in the formula as representing some group of measurements which are taken in the situation (witness the temptation to refer to the variables as "parameters", when this word would more properly apply to the dimension, measures of which are the basis for symbolizing the range of that variable).* Thus when discussing which of two formulas to apply in order to obtain a mathematical representation of some phenomenon it is important not to confuse two variables or constants merely because they are known to have identical values in the present case and hence seem interchangeable from the point of view of the subsequent manipulations;

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* cf. K. Menger (584B, 584E).

the non-identity of the things symbolized is the reason for keeping them separate.

In the case of logics it is also tempting to use different constants to symbolize different assertions; but it should be noted that the significance of such a procedure is the same as in the case of the use of algebraic symbolism in applied science - the difference between two constants both of which symbolize a true assertion lies only in the fact that the assertions involved were not regarded as necessarily identical; this is of no relevance to the manipulations done with these symbols in the KC system and for those purposes they may be regarded as mere notational variants of one single constant "T" which might be used to symbolise any and every true assertion.

It has been suggested that the basis of Lewis's intuitive approach to his systems was a mistaken equivocation between constants and variables. It would be helpful to consider cases where the constants of a calculus could be farther analysed without such difficulties, and to compare this with the case Lewis has to deal with.

Such a comparison may be made. The system which admits of farther analysis of constants is constructed by a grafting of the coin value formalism onto the sum of money formalism. The coin-value symbols are first of all replaced by the corresponding decimalised ones and the list is thus C(£0.002083) C(£0.00416) C(£0.0125) C(£0.025) C(£0.05) C(£0.1) C(£0.125). It is then

noted that any sum of money may or may not be made up by the addition of the values of some number of these coins; the set of designated simple signs for the sum of money calculus is then extended to include not only £0.0 (which can be made up by zero number of these coins) but also those seven simple signs which symbolize sums of money which are equal in value to the coins. The use of the "C" prefixed to the sum of money sign enclosed in brackets now becomes the assertion sign for the calculus. The extended calculus is called "CVS".

CVS has exactly the same theorems and rules as the calculus of sums of money. It will also have a set of signs which symbolize sums of money which are able to be made up by a group of coins - these sums will be called "coin-recursive" - and we may well want to mark out these signs in some way. Now one feature of the coin-recursive sums is that if β and $\alpha-\beta$ are both coin recursive then is coin-recursive; this relationship can be stated in terms of the signs used to symbolize these sums, and the prefix "CR" which is applied to any sign symbolizing a coin-recursive sum. Since all the designated signs may have "CR" prefixed to them, we are now in a position to produce a "system" analogous to that of axioms and rules for the calculus itself; in this "system" the axioms are the designated signs, the rule is that α is CR if and if there is some β such that β and $\alpha-\beta$ are both CR; and the theorems are all the signs for coin-recursive sums of money, or "recipes" for making them.

It is thus possible in CVS to give axioms and rules of inference for the demonstration of theorems and also to give "axioms" and "rules of inference" by means of which any constant may be discovered to be or not to be in a certain class. In CVS there is little cause for confusion because the concepts of coin-recursiveness and coinhood and the corresponding prefixes "CR" and "C(...)" are quite distinct. But if they were not it would become difficult to distinguish the application of the one (like coin-recursiveness) which is appropriate only for constants and that of the other as a true assertion sign.

What seems to have happened in the case of Lewis's approach has been that he considered the KC formula 'p ⊃ q' which has as its realization the sequence (T F T T): he felt that the first of the evaluations (when 'p' and 'q' are both replaced by "TW") yielding the members of this sequence, was the evaluation of a sign which could be used to symbolize a variety of different types of pairs of assertions. Thus far nothing can be criticised; the first point which is at fault is the importation into the discussion of the evaluation 'T ⊃ T' of considerations dependent upon particular things that the "T"s may in some cases symbolize. Such a slip is easily fostered when different letters are used as synonyms for "T" in the manner described above, because it is felt that the identity of the symbolized assertion is somehow preserved by the use of the particular letter chosen - however this letter qua ingredient in the calculus is totally undistinguishable from any other synonym of "T". Once one has taken the different synonyms of "T" as if they were different in the way that the different sum of money signs are different, then it becomes easy to open discussions on what basis there may be for a particular pair of them both being truth-symbolising signs.

Given a system in which genuinely different signs were used for symbolizing different true assertions one could imagine that a prefix might be definable which could be applied to a pair of constants to indicate that they stood in some relationship which other pairs did not stand in, and that the applicability of this prefix might be recursively decidable along lines similar to those for the prefix "CR" in CVS. Such a prefix might even serve as a two-place assertion sign like that of equality in algebra*. However it cannot be that the relationship of a pair of constants is decided on grounds independent of the identity of those constants; this is in fact what Lewis appears to propose. While starting from the point of view that the pair of signs are constants, he gives as the ground for applying the prefix to them their satisfaction of criteria which can only apply to pairs of fully-variable formulae. The dilemma which is posed is that on the one hand if these are in fact constants then the fully-variable formulae Lewis desires to consider are not shown to be in any way systematically related to the constants presently under consideration, and indeed would have to appear in some other calculus, while on the other hand if the pair of expressions which are to be tested for the applicability of the prefix, are fully-variable formulae then the problem is mis-stated and what is under consideration is a two-place assertion sign the applicability of which should be decided on the basis of realizations in terms of simple signs or groups of simple signs, and some particular designated sign or signs.

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*cf. our comments above (p.229).

CHAPTER V

LOGICAL FORM

In this second part of the consideration of symbolization we turn to the notion of logical form. The nature of logical form is a topic which has a substantial history in philosophical logic, and it seems worthwhile to tie in some remarks on this with the views expressed in Chapter IV; this is particularly true because some recent approaches to modal logic and related topics have taken as their starting points what seem to be attempts at finding the logical form of modal assertions, and so our approach to modal logic may take as a backcloth, views on logical form which can be compared with those. (Rescher:711B and Kenny:473A).

The view of logical form to be put forward is that giving the logical form of some assertion consists in representing in a systematic way those factors which influence the assessment one makes of some status of that assertion. The statuses concerned will vary from case to case, but in the familiar controversy the status concerned is that of being true or false. The first task is to state our position in the traditional controversy since the satisfactoriness of the doctrine in that field will be a prerequisite to its being found convincing in others - such as that of modalized assertions.

The traditional problem of logical form

There are two principal doctrins which we contrast with our own. First there is that which bases logical form upon the notion of truth, but holds that logical form can be detected by consideration of the constituents of the sentence used to express the assertion whose logical form is sought. The second view denies that logical form can be found out by a consideration of the constituents which are, or might be, used in a sentence expressing the assertion, but does not see the detection of logical form as being particularly connected with the factors which influence our appraisal of a sentence as true or false.

A concise statement of the first position can be found in G. H. von Wright's paper "Form and Content in Logic" (954F; 1-21). His starting point is the notion of a valid syllogism; he wishes to explain the point of our saying that such a syllogism is "true in virtue of its form alone". He also considers syllogisms of propositions and truths based on relations and their inverses.

The present concern is not with the logical form of arguments as such, but it seems that von Wright conceives his comments as also applying to the assessment of the form of particular assertions - for example on page 5 he considers different types of logic and classifies them according to the type of variable which can occur.

In relation to the classical syllogism he distinguishes two types of words; "words for which other words can be substituted

without affecting the truth of the proposition expressed by the syllogism. We shall call them variable words. Words for which it is impossible to substitute other words without affecting the truth of the proposition expressed by the syllogism. We shall call these words (logical) constants. The constants in our example are the words 'if-then', 'all', 'and', and 'some'".

Analogously in the case of arguments at the propositional level he says

> "The variables are those parts of the sentence for which we could substitute other fragments of language without affecting the truth of the proposition expressed by the if-then sentence... the constants are the words 'if-then' and 'not'".

When we come to consider truths of logic in the realm of relations, however, von Wright finds this story less easy. The simple word substitution test does not work for inverse relations like those of teacher and pupil, and so he quite casually drops word-substitution:

> "Instead of 'pupil' a logician might say 'teacher converse', and similarly instead of 'child' he might say 'parent converse' and instead of 'servant' he might say 'master converse'. And if he then said 'if somebody is x of somebody, then somebody is x-converse of somebody', he would say something the truth of which is independent of the meaning of x in the same sense in which the truth of 'if x, if y, then not y, if not x' is independent of the meaning of x and y". (page 4)

and a few lines later he actually comments on this shift of emphasis from word-substitution to concept-substitution, without seeming to see in it any cause for modifying his statement of what logical form is:

> "It is of some interest to observe that there are logical constants for which ordinary language has no name and for which the logician must invent a technical term. This fact probably offers a partial explanation why some branches of logic were much retarded in their development before the rise of so-called symbolic logic". (page 5)

Perhaps von Wright simply does not consider it important to distinguish here between concept-substitution and word-substitution, and would not mind our understanding "concept" where he says "word". Yet there is a real difference and his reference to words tends to suggest that he believes either (a) that it genuinely is a matter of word substitution, or (b) that words designate concepts in the way Carnap alleges those believing in the "Name-relation" would hold.* Further it is reminiscent of the procedures described by Ryle for the detection of category mistakes and the construction of "logical grammar"; the relation between logical form and category mistakes is however a matter of substance which should not simply be made to appear trivial by imprecise terminology.

Three arguments can be brought against the view that word substitution can provide a criterion for assessing logical form. First, despite the odd use of a hyphen in "if-then" this "constant" is not one word but two. If we are to allow constants of two words why not countenance constants of six or seven words? Indeed in that case every sentence can be regarded as "(logical)" constant followed by a full stop, since the words up to the full stop cannot always be replaced by a similar number of other words without

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*cf. Meaning and Necessity (207K; Chap. III).

affecting the truth-value of the proposition expressed.

Secondly there are difficulties because we sometimes wish to take a whole range of different words as expressing the same constant (in truth-functional terms). Thus "and" and "but" are often assimilated and "although" differs little from these. Now the word substitution criterion will have to give some means for distinguishing between the substitutability of "and" for "but", and the substitutability of other terms for words in variable-positions; otherwise "and" will be classed as marking the position of a variable. One might lay it down that only words which are not intersubstitutable salva veritate in all contexts should be used in such tests, but this would appear to make all the words in the sentence into "(logical)" constants.

Finally a difficulty arises when we consider the logical form of sentences which are expressed in an ambiguous fashion. The familiar example - in Strawson's version - is that of the ambiguity between the particular and general uses of the definite article in a sentence such as: "The cat is a hunter". While it would be reasonable enough that we should have to resolve the ambiguity before setting about finding the logical form,* it will be quite impossible to carry through a pure word-substitutability test because each time we substitute a word for the word "cat" the same type of ambiguity is liable to arise again, and before we can assess whether or not

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*von Wright's criterion is based upon the truth of the 'proposition' and this, unlike the sentence, may be presumed unambiguous.

the proposition expressed is true, we must resolve the ambiguity anew. It is clearly important that the ambiguity be resolved in the same way each time, but it is equally clear that there is nothing in the words actually used which could lead us to such consistency. In fact we require to say that the replacing word is to be taken as being used in the same sort of way as the replaced word, but this would be to abandon mere word-substitution as the criterion and allow that it is necessary to consider the concepts being used.

In the light of these objections it seems that von Wright's account is not acceptable. None the less we can agree with him in all but the last eight words of the following passage (page 7):

> "...in distinguishing between the form and content of expressions we presupposed an insight into the truth of the propositions which they express and how this truth may be affected by the substitution of parts in the expression".

The second view of logical form which deserves discussion can be found in the writings of W. v. O. Quine and P. F. Strawson. These two writers present the matter in different ways but it seems to us that they are united in (i) rejecting the word-substitution procedure as an adequate one for discovering logical form, and (ii) giving accounts which while making the assessment of logical form dependent upon our understanding of the assertions concerned, do not invoke the crucial factor which is the assessment we make of the assertion's truth or falsity.

In "Two dogmas of Empiricism" (690L; §1) Quine defines a "logically true" statement as one which, when we have a prior inventory of logical particles "is true and remains true under all reinterpretations of its components other than the logical particles". At this point he does not seem to think that we have any means of gaining this inventory of logical particles other than by understanding the language and knowing which word is of which sort.

In Word and Object (690T; Ch.V) when he discusses the "regimentation" of statements so that they become suitable for symbolization according to mechanical rules, it is again apparent that he thinks that before we can symbolize correctly we must understand the symbolizandum and thus be in a position to accept the regimented form as a fair paraphrase of the original.

Generally, however, Quine does not take up this topic explicitly and so his discussions need not occupy too much of our time. But it is clear that when he produces a paraphrase of a given statement into regimented canonical form he is in effect concerned with the logical form of that statement. It is thus of interest to read on page 191 of Word and Object that some such paraphrases are satisfactory because they "were such as to meet most or all of the likely purposes for which the originals might be used, except insofar as such needs include brevity or familiarity". It is at this point that Quine and Strawson diverge. Not unnaturally so, since Strawson is concerned with the best

philosophical account of the truth-functional logic (in Introduction to Logical Theory (840C) whereas Quine is concerned with establishing the claim that truth-functional logic is adequate for all circumstances and all purposes. Quine's introduction of the purposes for using the sentences concerned seems a step in the right direction, but does not seem to account for the need for regimented paraphrases to have as parts expressions which Strawson might call "referring expressions" and in some cases to introduce explicit parts where the original was inexplicit about the relevance of a particular factor. Such aspects of regimented paraphrases do not seem necessary merely to the fulfillment of the same purposes as the original - why, for example, should not the paraphrase be less explicit instead of more? It is left as an unexplained outcome of the need for us to accept paraphrases as good ones, that they should have just the number and arrangement of parts they do have.

Strawson takes as the touchstone of logical form the "formal powers" of the statement whose form is sought. The difference between Quine and Strawson could be put by saying that while Quine believes that logical form can be reached by abstracting from everything about the statement except its purposes (but retaining a sense of "goodness" about the paraphrase which is related to the form), Strawson believes logical form to be reached by abstracting from everything about a statement except its formal powers. For Quine the matter is one of the point of saying a certain thing,

whereas for Strawson it is the role which that which is said plays in valid arguments that counts.

Strawson's account runs as follows (840C;50):

> "Then let us restrict our attention to the general classes of inference (&c) which interest the formal logician. We may then speak of the formal powers of a statement, meaning by this the range of parts it could play in inferences (&c) belonging to such general classes. ... We may say that two statements are of the same logical form when we are interested in an analogy between their formal powers; ..."

He carefully and rightly points out that one statement can be regarded as having a multiplicity of different logical forms, depending upon the purposes we may have in examining it - particularly what comparisons we make. He is also much more careful than von Wright about relations, and is chary of introducing any "manufactured constants" as it would seem one has to if one adopts the alternative view of the matter whereby we would distinguish canonical forms and talk of putting given statements "into logical form". He is however kind to this alternative approach, saying that it leads to no difficulties even though it is less easily generalised to cover all the preoccupations of formal logicians.

Strawson's account makes our assessment of the logical form of a statement dependent upon our knowledge of what constitutes valid inference for statements of the sort in question. Now if one has a concept of valid inference which lays down that an inference is valid just in case the truth-conditions for the premisses include those for the conclusion, then it will turn out that Strawson's

view of logical form comes close to, or even coincides with the view argued for here. However it is by no means necessary that one should have such a view.

In particular in the case of attempts to treat utterances other than straightforward true-or-false statements, the question of what constitutes a valid inference will be one which is less universally agreed. Now if we take Strawson's view of logical form, we shall turn first to the question of what constitutes a valid inference in this particular field; yet it seems to us that no satisfactory systematic account of valid inference can be given except in terms of the conservation of some valuation which the utterances are assessed as having. A good example of the sort of approach we have in mind - though not the results - is that of Anthony Kenny in "Practical Inference" (473A); Nicholas Rescher in his "The Logic of Commands" (711B) sees the relation between validity and assessments of this kind, but is less explicit than Kenny.* It does not seem to us to make sense to ask which inferences are valid in a certain field without being in possession of any idea of what property of the utterances in question it is that one wishes to find in conclusions whenever it is in premisses. The only alternative to basing the notion of valid inference upon the conservation of such properties, would be to argue that we had an insight of validity which allowed us to pick out certain sequences of utterances as being valid inferences; but so far as the evidence about our

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*an early writer along these lines is Alf Ross (730A).

insights goes, it seems to be the case that we do have strong feelings about the correctness of conclusions drawn from premisses and. that these feelings sufficiently often mislead us - through being based on the effects of rhetoric not logic - that we are led to formalisation and calculation as a safeguard. If logic is to be any sort of an improvement on the use of pure intuition, then the systematization involved must be based on something over and above the actual insight of connection upon which we wish to improve. This is not to say that the insight is irrelevant - obviously in order to have confidence that the logical system will help us to avoid the mistakes we must have some confidence that there is a connection in principle between the basis of the systematization and the insight itself. The connection concerned is naturally that the systematization should be based on the relationship of properties of utterances, which one would assume to exist if one accepted the insight in some case; thus since the insight of validity in the case of statements would lead us to expect that the conclusion of the argument was true if the premisses were, we require a systematization based upon the truth or falsity of the statements concerned. In this way it seems that even though we may have some such insights as that of the validity of some putative inference, so far as logic is concerned the notion of valid inference is one which should be based on the interrelation of the properties ascribed to utterances

of the field concerned.*

Although it is very well possible that Strawson should agree with us about the logical form of any sentence we may discuss, since we may in fact both adopt the same value systems, it seems that to base the notion of logical form upon that of valid inference is to put the cart before the horse and risk creating confusion.

A solution and its extension

In criticising von Wright, Quine and Strawson, we have given some signs of our own view in this matter. The writer with whom we are most in sympathy is Stuart Hampshire (in his article: "Logical Form" (387A)).

Hampshire starts by judiciously pointing out that ordinary language is by no means primarily designed for stating facts objectively or for tracing connections amongst them; on the other hand it is just this sort of use of language which is of interest to the logician. His view of logical form is that it "is not shown in the grammar or the form of sentences it is shown in the use". As an instance he cites the contrast between (1) "I am honest" and (2) "I am tired", saying "I suggest that when we distinguish the complexity of meaning of sentence (1) from sentence (2), the question we ask ourselves is so distinguishing, is what is the method which I use

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*Strawson actually relies (p. 46) on "our sense of the formal similarity between inferences"; at this point he might surely have referred also to our feelings that certain inferences are valid, and posed if not answered the question of how we are to give a general description of the conditions for an inference being valid.

to verify this statement?" Again he says (p. 45)

> "Consider the statement 'The thunderstorm gave me a headache'. In order to discover the symbolic equivalent of a statement of this type in the framework of Principia Mathematica, we must first ask ourselves - how is it verified or falsified? In other words, - what is its logical form?"

Hampshire very effectively criticises Quine's version of his own procedure by pointing out that the job of sweeping away unnecessary grammatical forms and showing reductions to extensionally symbolizable paraphrases, is of precisely the type under consideration:

> "In order to decide whether one sentence-form X can be eliminated in favour of another sentence-form Y, the question we ask is - 'In what contexts of argument, or for what purposes do we normally want to use English sentences of this grammatical form X? Do we follow the same procedure in establishing the truth of Y?"

While we cannot agree with Hampshire's implicit restriction of logic to the logical systems such as KC which symbolize with respect to truth and falsity, nor with other incidental doctrines in his paper, we are quite in sympathy with what seems to be his main point. That is that in the truth-functional logic we treat sentences as complex insofar as their truth-conditions are complex, and the displaying of the logical form of a sentence is the rewriting of it in such a way as to make clear and explicit the references to truth-conditions which we find to be needed if the sentence is to be verified. Thus in such cases as the logic of definite descriptions the process of finding the appropriate logical form commences with an examination of how sentences involving the device in question

are to be verified.

Such an account of logical form can hardly be regarded as surprising, given the view that KC is a calculus for symbolization with respect to truth-values. The whole basis upon which we are able to use KC is that anything which is to be symbolized may either be directly adjudged either true or false, or else may be represented as a complex of items which can be directly adjudged true or false and of truth-functionally determinate function-signs. The logical form of an assertion is thus the display which shows the various directly adjudged assertions and the relevant functions which would have to be used in verifying the assertion in question. For example the assertion "It is still raining" might be displayed as "It is raining and it has been raining (for some time up to now)" prior to its being symbolized as '(T.T)' in case it should be true, and as '(T.F)', '(F.T)' or '(F.F)' in case it should be false as a result of one or more of the two assertions in the display of the logical form being false.

It is more usual for logical form to be discussed in relation to the predicate calculus. In this case too it is natural that the logical form be a display of the result of analysing the assertion into some function of various features relevant to the truth of the whole. In this case, however, it is not normal to refer to the features by using true or false assertions combined by truth-functional operators; instead it is usual to rewrite

sentences so that nouns can be replaced by individual constants predicates and relational words by predicate constants, and conjunctions by operators. This sort of display however may be replaced by one which does use full assertions as elements in the display of the logical form, and this is precisely what von Wright sets out to do in his essay "On the Idea of Logical Truth". In "Form and Content in Logic" he proposes such an attempt, saying on page 14:

> "The truth value of any proposition to the effect that there is a thing which satisfies a certain one of the conditions depends on the content of the n names of the properties which occur in the expression".

He then makes the proposal that all quantified discourse about properties be analysed into complexes of propositions about the satisfaction by individuals of certain conditions. If we ignore his view (in any case modified) that the actual words used in the original sentences will yield the list of predicates to be used in stating conditions to be satisfied, we can see that the proposal amounts to suggesting that we display the logical form of these assertions by means of truth-functional complexes of what he calls "existence-constituents" - statements ('propositions' in his terms) specifying the existence of things satisfying truth-conditions for the original assertion. To this extent then von Wright unwittingly adds to the plausibility of our view.

On the other hand von Wright's reason for attempting the reduction seems to be an unhappiness about making the truth-value

of a complex depend upon anything other than truth-value assessments and logical functions of them. From our point of view this is not a worry. In the case of symbolizations in truth-functional predicate calculus, my account of logical form is that the logical form of an assertion is given by representing in some systematic way ("displaying" as we have called it above) those factors which influence our assessment of that assertion as being true or false. One such representation would be that proposed by von Wright - given that we could directly verify the existence-constituents. But there might well be other ways of representing the factors involved, and amongst these would be the usual paraphrasing of the utterance in the form of a canonical sentence, the symbolization of which could be done without reference to its meaning*.

Two particular objections which might be brought against this account, are worth answering here - especially because the answers may make the details of the doctrine more clear. The first of these objections is that because there is just one set of truth-conditions for a given assertion, we are committed to there being only one logical form for each utterance; the second is that because logical form is based on verification the logical form will be affected by every shift in our verification procedures and that particularly in the case of assertions which are also made by scientists the logical form of an assertion might well be unknown to a (lay) person making that assertion because he did not in fact

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*We do of course often simply write down the formula of the calculus by which such a canonical form would be symbolized, and say that that gives the logical form of the utterance.

know all the details of how to verify that assertion.

What Quine (69OT;160) calls "the maxim of shallow analysis" is no less applicable in the case of the present view of logical form, than it is on his view. When presented with a particular assertion and asked to symbolize it, we shall not simply attempt to find the most detailed possible logical form that it could have; it is surely clear that one may be indefinitely specific about the truth-conditions for a given sentence by ruling out all sorts of abnormal conditions, defining various terms and procedures etc.* If this is so there can be no absolute logical form which would be based on the most detailed possible analysis. Instead we shall look at the context and see whether or not we can get by, simply representing it by a single constant of whatever truth-value we may think the assertion has. If however parts of the context depend for their truth on factors which also influence the truth or falsity of the present assertion, then we shall probably wish to display this assertion as depending upon that factor for its truth or falsity, and subsequently to display the other assertions in the context as being also so dependent.

Thus a sentence like "John quickly threw the ball back to the bowler" may be regarded in different contexts as having a very large number of different logical forms. Were the argument one

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*The apparent absence of any end to the degree of specificity possible has been one of the main factors in bringing into disfavour the straightforward verificationist theory of meaning.

about who was and who was not a good fielder and it were accepted that the relevant criterion were throwing the ball back to the bowler quickly, then this sentence might well demand no more than analysis as having the logical form of a predicate applied to an individual constant. If, on the other hand the context is one where the action on the cricket field is being described and in other assertions we find the ball, the bowler, John, throwing, and speed, all separately involved in what is being described, then the logical form might well have to be considerably more complex. When it becomes relevant to the discussion in hand that the "bowler" is just that person whom the captain of the fielding side has selected and who is doing this or that particular thing, then the number of factors to which the assertion must be regarded as implicitly referring will vastly inflate the number of factors influencing our ascription of truth and falsity and requiring to be represented in the logical form.

Having pointed out in this way that it is not necessary to treat an assertion as having one absolute logical form which never varies from context to context, it should not be necessary to say too much about the allegation that a person may be unaware of the logical form of some utterance he makes. Clearly a doctrine which made us say any such thing would not be satisfactory. The reason for this unsatisfactoriness is that the point of discerning logical form is to give us a basis for producing the symbolization of an

utterance in a given calculus by a purely mechanical process : now if we have a situation in which something said is symbolized in such a way that there are significant parts of the symbolic version which can interact with other symbolic expressions in a way not analogous to that in which the speaker imagines his utterance could interact with other utterances, then it is clear that we have not symbolized what the speaker wanted to say but only something which we may think he said, wanted to say, or ought to have been saying (given the words he used).

To take a concrete example, imagine a report of the death of some person - "x is dead". Let us suppose this report to be made by a person of great medical knowledge, and to be made to someone who is a complete layman as regards medicine. It may well be that the person who gives the report has a very recondite set of criteria for death and in order to satisfy himself that x was in fact dead has carried out a variety of tests which the person hearing the report simply does not know of or imagine. The question which arises is whether the report has a logical form which displays factors affecting the reporter's assessment of whether "x is dead" is true, or merely displaying factors which affect the hearer's assessment.

The answer to this question is that the logical form will depend upon how we describe this situation. If we describe it as one in which the fact of the death of x was communicated by the reporter to the hearer, then it will follow that there is no more in

the logical form of what the reporter said than is in common between the sets of truth-conditions applied by the two parties in assessing the assertion in question. Yet it might also be that the reporter was being exceedingly careful in his choice of words and did not necessarily wish to communicate to his hearer what the hearer though was being said; in such a case if we are content to describe only what the reporter <u>said</u> as opposed to what he <u>communicated</u> then the logical form may well be based upon all sorts of truth-conditions unimagined by the hearer. Finally it may be that even though the wish of the reporter was to communicate successfully he failed because the hearer had in fact a concept of death quite different to what the reporter expected; in such a case, of course, we can say that the reporter's utterance was <u>understood</u> as one having the logical form Z, even though Z represents factors not related to his set of truth-conditions.

These considerations do not in fact properly relate to the question of logical form, since they raise the whole question of precisely what it is that is being symbolized in a given logical formalisation of some argument or set of descriptions. The lesson which emerges is that it will not make sense to claim to have formalised the two sides of an argument between two people who simply misunderstand one another; before they can even disagree they must understand what is being claimed on each side, and it is these claims <u>as mutually understood</u> which are symbolized when we

symbolize some argument.

As a final note on the case of the scientific overtones implicit in many assertions which the layman also seems to understand, one should note that when two scientists talking together use assertions such as might also be used by laymen, it may well be that they understand one another in such a way that the assertion has a logical form quite unlike that it would have for the laymen. Indeed it does become difficult to separate the logical form of the assertion from the representation of it in the symbols of whatever theory the two scientists may share concerning this subject. For example assertions about "electrons" may well be made by persons with varying degrees of familiarity with the present theoretical viewpoints about them; on the one hand we must not reckon into the logical form of what is said by laymen on the subject of electrons reflections of the sophisticated criteria appropriate to a scientist's discourse about them, while on the other hand two scientists may not be understanding each other properly unless the logical form of their talk about electrons does reflect the scientific theory about them.*

Thus we suggest that the logical form of an utterance is a systematic representation of those factors which affect the assessment of it (with respect to truth and falsity in the case of logical

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*Of course when the subject matter of the discussion is what the scientific theory itself ought to be, they will fall back on a less recondite use of terms and the logical form will alter appropriately; they may then be able to introduce new truth-conditions for statements about electrons made thereafter.

form regarded from the KC point of view); the range of factors included will differ according to the context, there being no point in including as separate factors the different constituents of some complex truth-condition if none of them ever appear alone as truth-conditions of other utterances in the context; and, logical form cannot be discerned until one has clarified the meaning of the utterance concerned - it being understood that this meaning may sometimes be different for speaker, hearer or an objective third party who knows the language.

Logical Form, valuations, and respects of symbolization

It will be clear that this account of logical form is intended to be sufficiently general to cover cases other than the propositional and predicate calculi of truth-functions. In the case of these calculi it has been suggested that the appropriate basis for assessing utterances is with respect to their truth or falsity and that logical form is thus tied in with the truth-conditions for the assertions made. The generalization of the account relates it to the argument of Chapter IV, and takes as the basis for assessing utterances in each case the range of properties with respect to which the calculus concerned is said to symbolize them. It is assumed that only coherent calculi are being considered.

It is difficult to know whether it requires much argument to defend the view that logical form is relative to some calculus or other. This is implicit in the account given here, and it seems

to us to be amply attested by the example quoted earlier (p.243) and the observation that the logical form of a classical syllogism may or may not show us that it is "true by virtue of its form alone", depending upon whether we have the sense to consider it in relation to Aristotelian predicate logic rather than in relation to KC.

It is certainly not a very controversial matter that there may be forms of valid inference other than those of KC and the related predicate calculi; controversy arises over whether these arise in virtue of the introduction of "meaning postulates" (cf. Carnap's paper of that name published as appendix B to 2nd edition of Meaning and Necessity) or because of the existence of different types of logic for different subject-matters.

We have no brief for the view that there is a special type of logic appropriate to certain specific subject matters - e.g. ethics (cf. Hare and many others). But that is not to deny that there may be different ways of treating the subject-matter which KC symbolizes with respect to truth and falsehood; and there may be ways of symbolizing subject-matter which because of the non-applicability to it of the categories "true" and "false" cannot usefully be symbolized by KC at all.

Given such a view it will be particularly important that when the question "What is the logical form?" is asked of some utterance, it is clear what calculus it is proposed to use for symbolization. So far as it is possible to argue against a downright denial that

logical form is relative to particular calculi, the argument runs that if this denial is accepted then there cannot be a range of different calculi applicable to the same subject-matter.

It is however evident that at least in somewhat uninteresting ways alternative calculi can be used for symbolizing the same utterances. Consider a pair of utterances written in succession and understood as being thereby conjoined (for instance both being products of the same pen). These are written material and as such may be regarded in a variety of ways. The familiar way is as understandable assertions. The printer however regards them as typesetting exercises, and is particularly interested in whether the pair will fit onto one line or other unit of space. Both ways of regarding the pair of written-down utterances can lead us to symbolization. In the first case we take the pair as a unit and with a view to KC symbolization we discern its logical form as being the conjunction of two truth-functionally determinate parts. In the second case we take the pair as a unit and discern its logical form - with a view to algebraic symbolization - as being the addition of two parts of numerically determinate length. A somewhat trivial example, it is true, but none the less one in which different interests lead to different types of symbolization and different ascriptions of "logical" form. Had it been necessary to undertake a less "shallow analysis" in these cases, we could have proceeded in one case either to give the forms of the conjoined

parts relative to predicate calculus or relative to KC itself, and in the other case to give the forms of the added parts as sums or differences of other parts.

It would be much more interesting to consider whether utterances might also be assessed in terms of their plausibility, rudeness, or strength of expression, and compare the logical forms resulting in these cases with those resulting in the case of truth assessment with a view to KC symbolization. However in the case of these other properties we do not yet have calculi relative to which the logical form could be set out, and so in arguing this point it is best to rely on the case of symbolization of length.

The reason why it will not do, simply to claim that we have discovered a logical form just as soon as we have devised some way of displaying the factors influencing our ascription to an utterance of some predicate or other, is that there is no guarantee in that fact alone that we shall be able to devise a calculus for symbolizing the utterances in respect of their possession of the properties referred to, and that even if there is one such calculus it may not be unique.

Suppose that an utterance (let us call it "Utt") has some property which we call "Prop" and that we have decided to try to symbolize Utt with respect to Prop. Choosing some level of depth of analysis, we shall be able to set out the factors which cause us to ascribe Prop to Utt, and when we have a generalizable* convention

*As will be seen this is in fact a complicated business; the convention must be able to cater for factors leading us to ascribe Prop to some utterances and not to others, and even factors leading us to ascribe predicates for properties related to Prop to various utterances.

for setting out those factors we have reached the stage of having given the "form" of Utt relative to Prop - but not yet the "logical form" since there is no calculus.

Prop and whatever related properties we choose to pick out among utterances, will give us a basis for making a sign-system symbolizing any collection of utterances. When it comes to making a symbolism by establishing some ordering among the signs generated by the principle of symbolization we shall be presented with choices. If we have recognised only Prop and the absence of Prop as conditions in which utterances can be, then it is likely - but not, of course, essential - that we shall order the signs concerned as the elements of a Boolean algebra. If a wider range of signs is available, the range of possible orderings will naturally increase.

The introduction of complex signs in the transition to a sub-logic may well not be possible in such a way as to produce a coherent formalism. For example if we have a sign, Sig, corresponding to the unit element of a Boolean algebra, by which we symbolize Utt (and a fortiori any other utterance which has Prop), then it does not necessarily follow that Prop is such that any pair of utterances, both of which have it, can be systematically related to some complex utterance, (or some utterance regarded as a complex of those two) which has Prop. Finding this, we might go back and try to alter the ordering of the signs. However, in the case of a property such as that of "being good style" it seems that there simply are not

utterances which are, or can be regarded as, complexes of other utterances and which bear a systematic relationship to those other utterances with regard to being or not being good style. One could, of course, say that any utterance which was stylistically acceptable was to be regarded as the product of some operation on any pair of stylistically acceptable utterances; such a move would preserve the position of the sign for stylistically acceptable utterances as the unit element of a Boolean algebra, but fails to give any acceptable notion of "part" as we have required when discussing sub-logics (p.213). Such a case should be sharply distinguished from one such as is described on page 252; in KC the symbolization with respect to truth or falsity leads us to consider the meaning of the utterance as the first step in symbolization, and it is therefore quite possible that a grammatically simple utterance should be regarded as a complex of parts each of which is directly verified. The fact that in KC 'T' is equivalent to any complex sign evaluating to 'T', does not entail that an utterance which under shallow analysis is symbolized immediately as 'T' can under deeper analysis be symbolized by any complex sign evaluating to 'T':* our choice of complex signs will be determined by our understanding of the utterance, and one can appreciate how difficult it would be to understand the example of page 252 as a disjunction with one false disjunct or as a material

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*We are of course here discussing "applied logic" - on the analogy of "applied mathematics" (cf. Chapter IV page 232ff).

implication with two false arguments. It is because neither this "understanding" of utterances or the reverse one which immediately understands a complicated context as one utterance capable of being categorised as true or false by a single inspection, are easy that the use of the KC·sub-logic has any attraction, for its only point is in allowing us to calculate the equivalence of the simple and complex signs which might in the respective cases be used for symbolization.

Thus to devise a sub-logic when given a certain principle of symbolization (namely that with respect to the property of utterances chosen in the case concerned) may not be possible. If it is possible then we shall demand that the sub-logic be coherent in the sense outlined in Chapter IV.

The possible difficulties of devising a calculus on the basis of a sub-logic have been fully explored already. We are thus left in the case of the various special logics which have been suggested for various types of utterance or subject-matter, with the need (i) to choose some property with respect to which we wish to conduct symbolization and investigation of logical form, and (ii) to attack the problem of whether this choice of property is such as to allow us to construct either a coherent sub-logic or a calculus.