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METAONTOLOGY

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Mark Tarver

Christmas Eve

1984

DECLARATION

No part of this thesis has been submitted for official examination. I declare that to the best of my knowledge all uncited material in this thesis is original.

M. Tarver

GLOSSARY OF SYMBOLS AND ABBREVIATIONS USED

The following symbols and abbreviations appear within the text; their order of presentation approximates to their order of appearance. In those cases where one symbol has been given more than one meaning, I have noted the homography.

EX	function taking a sign to its extension
$\supset$	if....then....; (or more properly, materially implies)
-	not
&	and
v	and/or (non exclusive disjunction)
$\equiv$	if and only if; (or more properly, the sign for material equivalence)
=	is identical to
$(\exists x)$	existential quantifier; meaning 'For some ...'
(x)	universal quantifier; meaning 'For all ...'
(Σx)	substitutional existential quantifier; meaning 'For at least one substitution for 'x' in the following..., the resultant substitution - instance is true'
(Πx)	substitutional universal quantifier; meaning 'For any substitution for 'x' in the following..., the resultant substitution - instance is true'
$\Box$	it is necessary that
$\Diamond$	it is possible that
oc	is ontologically committed to the existence of
$\overline{oc}$	is ontologically committed to the nonexistence of
B	2 place predicate true of couples of a person and a theory; meaning 'believes to be true'

E	sentence forming operator on predicates or definate singular terms; meaning 'has at least one thing satisfying___' or 'There is something ___ denotes'
$\longrightarrow$	sign used in rules of inference; meaning 'From ___ it is permissable to derive ___'
$\Rrightarrow$	entails
Taut	tautological inference; rule invoked when any piece of reasoning proceeds by the propositional calculus
Hyp	hypothesis; the premiss of a formal argument
CP	conditional proof: a rule of inference that enables the conclusion $A \vdash B \supset C$ to be drawn from $A, B \vdash C$
OI	O introduction: a rule of inference that derives OA from $\vdash A$
=I	= introduction: a rule of inference that allows the introduction of a = a at any stage in a formal argument
Sub	substitution rule: a rule of inference that allows the replacement of a by b throughout any formula as long as a = b and a is not in the scope of a modal operator
O Sub	O substitution rule: a rule of inference that allows the replacement of a by b throughout any formula as long as O a = b
UE	universal elimination; a rule of inference that allows any closed term to replace a universally quantified variable
UI	universal introduction: a rule of inference that allows for the inference $\vdash$Fa therefore (x)Fx
EG	existential generalisation: a rule of inference that allows for the inference Fa therefore ($\exists$x) Fx

EE	existential elimination: a rule of inference that allows for the inference $(\exists x)\, Gx$, given $(\exists x)\, Fx$ and for any arbitrary a, if Fa then Ga
$\{\ \}$	braces for a set abstract: $\{x\text{:fox}\}$ is the set of all foxes, $\{2,4,6\}$ is the set containing just the elements 2,4,6
ε	the Greek letter epsilon; meaning 'is a member of'
$\subseteq$	is a subset of
$\cap$	the intersection of
$\cup$	the union of
$\langle\ \rangle$	braces indicating an ordered n-tuple
$\approx$	has exactly as many members as
Λ	the empty set, or $\{x: -x = x\}$
$\subset$	is a proper subset of
P	function taking a set to its powerset
$\#$	function taking a set to its cardinal number
N	the set of natural numbers
$\aleph_0$	aleph zero: the cardinal number of the set of natural numbers or $\#N$
[+]	cardinal addition
F	(i) function taking a set to the set of its factors (ii) sign for a formal framework
$\mathcal{U}$	function for taking a set to the set of its ultimate factors
$\ulcorner\ \urcorner$	quasi-quotes
$\frown$	concatanation
$\leq$	(i) is a mereological proper part of (ii) is before
$\geq$	is greater than or equal to
$>$	is greater than

$<$	is wholly before
$\vdash$	in $\Delta \vdash_L A$ meaning 'the formula A is deducible from the set Δ of wffs, according to the logic/theory L'. In '$\vdash_L A$' meaning 'A is a theorem of L'
$\models$	in '$J \models A$' meaning 'J is a model of A'. In '$\models A$' meaning 'A is logically valid'
$\wr\!\!-$	in '$\Delta \wr\!\!- S$' meaning 'S is depravedly deducible from the set Δ. In '$\wr\!\!- S$', meaning 'S is a depraved theorem'
$\wr\!\!=$	in '$\mathfrak{C} \wr\!\!= S$' meaning '$\mathfrak{C}$ is a depraved model of S'. In '$\wr\!\!= S$' meaning 'S is depravedly valid'
t^{-1}	the inverse of the (1 - 1) function t

INTRODUCTION

The Ontological Question 'What exists?' dates back over two thousand five hundred years to the dawn of Western philosophy, and attempts to answer it define the province of ontology. The history of the Western philosophical tradition itself has been one of the differentiation and separation of the various sciences from the primordial stuff of ancient philosophy. Physics was first to break away from the tutelage of philosophy and established its independence in the seventeenth century. The other sciences followed suit fairly rapidly, with perhaps psychology being the last to separate.

The results for modern philosophy - of this breakup of what was once a great empire over human reason - have been mixed. An inevitable result has been that questions considered in ancient times to belong to philosophy have fallen within the ambit of other disciplines. So speculations about the material composition and genesis of the universe that interested Thales, Heraclitus and Leucippus, are continued by contemporary cosmologists in well equipped research laboratories, and not by philosophers. However ontology, unlike cosmology, has not broken away from its parent discipline and the Ontological Question as to what exists is still argued by philosophers today.

That ontology has failed to make the separation that cosmology has, is a reflection on the weakness of the methodology for settling ontological arguments. Unlike their great Rationalist predecessors, most modern philosophers do not believe that logic alone is sufficient to provide an answer as to what is. But neither do observation or experiment, in any direct way, seem to help us in deciding, for example, whether sets or intentions should be admitted to exist or not. In consequence, the status of ontology as an area of serious study has to depend on the devising of a methodology within which the Ontological Question can be tackled. The pursuit of such a methodology is the concern of metaontology and is also the concern of this thesis.

The determination of good answers to fundamental questions in ontology depends in part on the state of art in the empirical sciences. Ontology is therefore an empirical discipline itself, albeit a high-level one. Metaontology, though open to and influenced by ideas developed in science, becomes heavily involved in areas central to the interests of modern philosophers. What gives a sign meaning? What is existence? What is truth or logic? These are all questions relevant to metaontology. One advantage of pursuing these questions within metaontology is that the change in context can lead to insights that were denied in pursuing the same questions along conventional lines. Consequently much of what follows will hopefully be of interest even to philosophers whose main interests are not in ontology.

SUMMARY OF CONTENTS

Chapter 1: deals with the logical properties of ontological commitment. The conditions of ontological commitment and the relations between the ontological commitments of a person and the theories he believes are examined within a series of formal logics, (sections 1.1 - 1.2). Second part deals with the criteria of ontological commitment (section 1.3). Chapter ends with a brief look at formalisation (section 1.4).

Chapter 2: is an historical examination of the formal tradition in ontology through the work of Russell, Carnap, Goodman, Quine and Davidson.

Chapter 3: is a six-point exegesis of an original methodology for tackling ontological questions. The central idea is that formalisation is a means of testing ontological hypotheses rather than developing them. The chapter concludes with an examination of problems arising from this methodology and some reflections on falsificationism as a scientific methodology.

Chapter 4: is concerned with the conditions under which a sign acquires sense. The focus is on the problem of constructing feasible first-order languages and the discussion ranges over certain epistemological problems relevant to ontology.

Appendix I: examines the role and proper form of a criterion of identity.

Appendix II: examines the ontological consequences and limitations of accepting substitutional quantification.

Chapter 5: examines the repercussions of accepting that many theories do not define their domain of discourse (ontological elasticity). Ontological elasticity is shown to be definable neither proof-theoretically nor model-theoretically. The chapter argues that accepting ontological elasticity requires a radical evaluation of traditional accounts of logic, existence, truth and categories of being.

Chapter 6: reviews certain species of ontological reduction. The chapter ends with a critical review of Quine's throughts on ontological reduction.

Appendix III: an illustrative example of reduction through Russell's attempt to eliminate unreduced instants of time.

Chapter 7: examines the nature of logics and their relations to ontology and natural languages. Three different views on logic are distinguished as to whether there is a 'correct' logic; and the chapter concludes with a discussion on the tenability of these positions.

CHAPTER ONE

Ontological Commitment

1.1 The Logical Properties of Ontological Commitment

Ontological commitment is a feature of both theories and people who hold theories. The ontological commitments of a theory are completely independent of whoever happens to believe (or disbelieve) that theory. The ontological commitments of a person are completely determined by the theories he happens to believe.

Most philosophers would accept the foregoing statements as true: but they do invite a number of questions. To be specific:-

(1) What is meant by the use of 'theory'?

(2) Is ontological commitment a relation between a theory/person and something else, or not? If it is, what are the relata?

(3) What is it for a theory to be ontologically committed to the existence of an entity or sort of entity?

(4) What is it for a person to be ontologically committed to the existence of an entity or kind of entity?

Before proceeding to examine these questions, a word of caution. Like many terms of art in philosophy, the phrase 'ontological commitment' is bounded by uncertainty in meaning. It is therefore unwise to assume that each of the above questions must have one determinately correct answer waiting to be paired off with it. Answers are sometimes recommended by pragmatic considerations such as clarity or simplicity of usage and amount almost to prescriptions or conventions for sharpening up our analytical tools. In other areas, intuition and common usage have more to say. Effective analysis often requires a judicious balance of prescription and description - so it is here.

1.11 A Conventional Definition of 'Theory'

In ordinary speech a theory is held to be a collection of generalisations, deductively linked, concerned with explaining some pattern of phenomona. This is an undeniably vague definition; but then 'theory' is a vague word. Nothing is lost, and much is gained in the way of clarity, if the convention is adopted that 'theory' is to be taken to apply to any non-empty set of declarative sentences, or to a declarative sentence itself.[1] I should say that 'theory' as used in this stipulative sense, differs in use from the mathematical logicians' use of 'theory' to mean merely a set of wffs. Such sets of uninterpreted wffs I call uninterpreted theories, or following Quine [113], theory forms. Theory forms, like fake diamonds, are not to be confused with the real article.

1.12 Referentiality, Extensionality and Ontological Commitment

Conventional wisdom holds that the correct answer to the second question of the preceding page ('Is ontological commitment a relation between a theory/person and something else, or not?') is 'not'. The consensus is that ontological commitment is both an intentional and intensional concept; a view which is really a comment on the logical grammar of 'ontological commitment'. A few preliminary definitions are in order.

Let EX be 1 - place function which takes as arguments either (a) denoting terms, (b) function expressions, (c) predicates, (d) declarative sentences. Let Φ be any argument to EX, then EX(Φ) is defined as follows:-

EX (Φ) = the denotation of Φ, if Φ is a term.

EX(Φ) = the function Φ denotes, if Φ is a function expression.

EX(Φ) = the set of all those things of which Φ is true, if Φ is a predicate.

$EX(\Phi) = 1$ if Φ is a true declarative sentence.

$EX(\Phi) = 0$ if Φ is a false declarative sentence.

A segment of a declarative sentence S is any sequence of signs that obtain concurrently in S, (thus any declarative sentence is a segment of itself). S is extensional iff for every segment δ of S which is an argument to EX, given $EX(\delta) = EX(\zeta)$ and S' is the result of replacing one or more occurrences of δ in S by ζ, then $S \equiv S'$. S is intensional iff S is not extensional.

'Intentional' (apart from confusing with 'intensional') is so battered a coin of philosophical currency that I shall not use it.[2] Here in its place, is a newly minted word, 'referential'. I say that a declarative sentence S is referential just when:-

(1) S contains a denoting term.

(2) S contains no non-denoting term.

S is antireferential when (2) is not satisfied; and non-referential when (1) and/or (2) is not satisfied.

The claim that ontological commitment is intensional is conveniently equated with the claim that for the following sentence - frame:-

x is ontologically committed to y;

there are substitutions of definite singular terms for 'x' and 'y' which create intensional substitution instances. The claim that ontological commitment is intentional and is therefore not a relation is here treated as the claim that 'ontological commitment' is antireferential: that is, there is at least one substitution instance of the above sentence - frame which is (a) true (b) antireferential (c) substitutes a denoting term for 'x'.

Illustrative instances are not hard to find. Thus, the following sentence is true.

'Centaurs exist' is ontologically committed to centaurs.

But although EX 'centaurs' = EX 'unicorns', the following is not true.

'Centaurs exist' is ontologically committed to unicorns.

Again although 'Pegasus' is a non-denoting term, but "Pegasus exists" is not, the following is true.

'Pegasus exists' is ontologically committed to Pegasus.

These two examples prove that 'ontological commitment' is definitely both intensional and antireferential. However the force of these conclusions can be blunted by what I call Frege's option.

Frege's option was used by Frege [47] to explain the failure of salva veritate in intensional sentences. His explanation of this failure was that expressions placed in intensional contexts did not refer to their usual extension, but instead referred to their sense. It is possible to insist that this is what happens to substituends for 'x' and 'y' in the sentence - frame above. The argument then develops that ontological commitment is really a relation after all: a relation between the entity referred to by the substitution for 'x' and the indirect referent of the substitution for 'y'.

Any philosopher who opts for Frege's option acquires an obligation to explain what he takes to be the indirect reference of substitutions for 'y'. There are various ways of discharging this obligation. Here I will consider four. Each of them is unsuccessful for one or more of the following reasons.

(a) The entities appealed to as direct referents are ontologically dubious in that they have neither adequate identity conditions, nor is their existence certain.

(b) Absurdities ensue.

(c) The suggestion is contrived, or raises anomalies that abandoning Frege's option would avoid.

By far the easiest course is to accept that the intensionality and anti-referentiality of 'ontological commitment' shows that ontological commitment is not a relation.

The First Suggestion: construe the indirect referent as a possible entity

This is what Jubien [71] [72] does.[3] Thus 'Pegasus' in "Pegasus exists' is ontologically committed to Pegasus' would denote a possible winged horse. Criticism (a) applies here.

The Second Suggestion: construe the indirect referent as a universal

The idea is to see "Centaurs exist.' is ontologically committed to centaurs' and "Pegasus exists' is ontologically committed to Pegasus' as announcing a relation between "Centaurs exist.' and $\hat{x}$(centaur x) and 'Pegasus exists' and $\hat{x}$(x = Pegasus) respectively. Again criticism (a) applies. (Necessary coinstantiation is not a good basis for the identity of universals. The universals $\hat{x}$(greatest prime x) and $\hat{x}$(3-sided quadrilateral x) are assumed distinct even though they are necessarily coinstantiated.)

The Third Suggestion: construe the indirect referent as a set.

This will not do because of (b). According to this suggestion; the following sentence:-

'Centaurs exist.' is ontologically committed to centaurs

announces a relation R between 'Centaurs exist.' and $\{x\text{: centaur } x\}$. However if R ('Centaurs exist.', $\{x\text{: centaur } x\}$); then given $\{x\text{: centaur } x\} = \{x\text{: unicorn } x\}$ it follows that R ('centaurs exist; $\{x\text{: unicorn } x\}$). According to the third suggestion this is what is stated by:-

'Centaurs exist.' is ontologically committed to unicorns

which is false: therefore so is this suggestion.[4]

<u>The Fourth Suggestion: construe the indirect referent as an open sentence.</u>

A sentence like "Centaurs exist.' is ontologically committed to centaurs' is thought of as announcing a relation of R between 'Centaurs exist.' and 'centaur x'. Here, R ('Centaurs exist.' 'centaur x') iff it follows from 'Centaurs exist.' being true that there is at least one thing that satisfies the open sentence. This suggestion fits in with the intensional properties of ontological commitment since if it follows from the truth of S that 'centaur x' is satisfied, it need not follow from S that 'unicorn x' is satisfied. The suggestion is weak principally because it appears contrived and has anomalies of its own. For instance, there are an infinity of appropriate open sentences to choose as denotata ('centaur x_1', 'centaur x_2'...) and to argue for one as the denotata above the rest seems impossible. Perhaps the suggestion is really a <u>convention</u> for eliminating the embarrassingly antireferential nature of ontological commitment: in which case the appropriate open sentence can be arbitrarily selected from the appropriate range. But anomalies still arise which undermine the value of this convention. If claims about ontological commitment are really encapsulations about relations between sentences and open sentences of the home language, then translation in the ordinary sense becomes impossible. Thus "'Chairs exist.' is ontologically committed to chairs' announces a relation between 'Chairs exist.' and (say) 'chair x'. Translated into French "'Chairs exist.' is ontologically committed to chairs' becomes "'Chairs exist.' est compromettre la existence des chaises' which (according to theory) announces a relation between a French open sentence and an English sentence; a claim that is not equivalent to the English version.[5]

1.1.3 The conditions of ontological commitment

When is a theory t ontologically committed to an entity a/sort K? One answer is: t is so committed iff it is impossible that t is true but a/Ks does/do not exist. Using the accepted equivalence $\Diamond(p \mathbin{\&} -q) \equiv \Box(p \supset q)$

this answer is equivalent to:-

A theory t is ontologically committed to an entity a/sort K iff it is necessary that if t is true then a/ks exist.

Formalising this answer is not easy. Writing 'oc' as short for 'is ontologically committed to', first-order modal logic suggests

$(x)(t)\ (t \text{ oc } x \equiv \Box\ (\text{true } t \supset (\exists y)\ x = y)$

$(k)(t)\ (t \text{ oc } k \equiv \Box\ (\text{true } t \supset (\exists x)\ kx)$

But this will not do.

If ontological commitment is genuinely antireferential, then it is illegitimate to employ objectual quantifiers binding variables where nondenoting terms may stand. For instance it is legitimate to argue:-

'Pegasus exists' is ontologically committed to Pegasus

$(\exists x)$ x is ontologically committed to Pegasus;

but not to argue

'Pegasus exists' is ontologically committed to Pegasus

$(\exists x)$ 'Pegasus exists' is ontologically committed to x.

In the latter case, there is no value for 'x' for which the conclusion is true. Substitutional quantification is one way of escaping this difficulty. Following Kripke [74] I write the universal substitutional quantifier as 'Π' and the existential substitutional quantifier as 'Σ'. With substitutional quantification, as with objectual quantification, a quantified sentence has a truth-value only when the bound variables are allocated a range. In the case of the objectual quantifier, the range is some non-empty set. For a substitutional quantifier the range is some non-empty substitution set of meaningful signs. Let this substitution set be S: then the truth-conditions for substitutional quantifiers are given as follows:-

'$(\exists x)Fx$' is true iff there is some $s \in S$ and 'Fs' is true

'$(\forall x)Fx$' is true iff for any $s \in S$, 'Fs' is true.

In order to formalise the previous definition of ontological commitment, many-sorted substitutional quantification is required. Let S_T = the set of terms denoting theories; let S_K = the set of general nouns and S_X = the set of definite singular terms. S_T is the substitution set for variables '$T, T_1, T_2, T_3, \ldots$'; S_K is the substitution set for variables '$K, K_1, K_2, K_3 \ldots$' and S_X is the substitution set for variables '$X, X_1, X_2, X_3, \ldots$' Formalised substitutionally the definition of ontological commitment becomes:-

$$(\Pi T)(\Pi K)\ T \text{ oc } K \equiv \Box\,(\text{true } T \supset (\exists x)\,Kx)$$

$$(\Pi T)(\Pi X)\ T \text{ oc } X \equiv \Box\,(\text{true } T \supset (\exists x)\,X = x)$$

Read quasi-informally, these formulae amount to the following.

'For any substitution of a theory-denoting term for the letter 'T', and for any substitution of a general noun for the letter 'K' in the formula 'T oc K = O (true T $\supset$ ($\exists$ x) Kx)', the resulting substitution instance is true'

'For any substitution of a theory-denoting term for the letter 'T', and for any substitution of a definite singular term for the letter 'X' in the formula 'T oc X $\equiv$ O (True T $\supset$ ($\exists$x)x = X)', the resulting substitution instance is true.'

The revised formalisation does deal neatly with the problem of anti-referentiality: but it runs into two problems of its own.

First, the formalisation mixes substitutional and objectual quantifiers. This is a course frought with peril for the unwary. For example, the formula '(ΠF)(($\exists$ x) Fx)' is a significant one: it reads 'Any substitution instance of '($\exists$x) Fx' is true'. Reverse the order of the quantifiers and we get '($\exists$ x)((ΠF) Fx)'; and this is nonsense. Read directly, it reads 'For some x, any substitution of 'Fx' is true. The first occurrence of 'x' fails to bind the second because the second occurrence is part of the quotational name of a formula. (On a similar topic see Quine [117]). Consequently it is always wrong to interpose a substitutional quantifier between an objectual quantifier and the variables that the objectual quantifier is intended to bind.

This restriction raises a lot of difficulties concerning the inference patterns of logics which employ mixed quantification. For instance in second-order logic '($\exists$ x)(($\exists$F) Fx)' ('There is something which has some property') is equivalent to '($\exists$F)(($\exists$ x) Fx)' ('There is some property which something has'). But in a mixed quantificational logic '($\exists$ x)((ΣF) Fx)' and '(ΣF)(($\exists$ x) Fx)' cannot be treated as equivalent since one formula breaks the rule of interposition and the other does not. Since there is no current research, to my knowledge, into the limitations of mixed quantification, the practice of mixing quantifiers is best left alone.

In deporting existential objectual quantification from our formalisation, the need is created for a replacement. Let 'E' be a sentence-forming operator on names of predicates or names of definite singular terms. Where K is any predicate, 'EK' is to be read 'there is one thing that satisfies K'. Where X is any definite singular term, 'EX' is to be read 'there is something X denotes'. The quantifier '$\exists$' can then be conveniently dispensed with, and the new formalisation of ontological commitment reads as follows.

$(\Pi T)(\Pi K)\ T \text{ oc } K \equiv O\ (\text{true } T \supset EK)$

$(\Pi T)(\Pi X)\ T \text{ oc } X \equiv O\ (\text{true } T \supset EX)$

This device does not, however, serve to avoid the next problem which is rather better known than the problem of mixed quantification: this is the problem of quantifying into modal contexts (see Quine [117] again for the classic statement of this problem).

Let two pieces of paper be dropped in an urn. One is inscribed 'Unicorns exist', the other 'Unicorns do not exist'. One piece is drawn at random from the urn; it is inscribed 'Unicorns exist'. Thus we have the identity "Unicorns exist' = the theory drawn from the urn'. From the formula, '$(\Pi T)(\Pi K)\ T \text{ oc } K \equiv O\ (\text{true } T \supset EK)$', it can be inferred that:

the theory drawn from the urn oc unicorns

$\equiv O$ (true (the theory drawn from the urn) $\supset$ E unicorns)

Plainly 'Unicorns exist' oc unicorns and since 'Unicorns exist' = the theory drawn from the urn, then the theory drawn from the urn oc unicorns. It then follows that:-

O (true (the theory drawn from the urn) $\supset$ E unicorns)

But is this final sentence true? Some philosophers (qv. Smullyan [134], Hughes and Cresswell [69]) would say it was). The theory selected from the urn is 'Unicorns exist' and it is necessary that if that theory is true then unicorns exist. But some philosophers, like Quine, who have more conventionalistic tendencies, might disagree. It is a matter of contingent fact, some might argue, that the theory drawn from the urn is 'Unicorns exist'. It does not follow merely from the truth of the theory drawn from the urn that unicorns exist, unless one adds the contingent identity claim that 'Unicorns exist' = the theory drawn from the urn.

One way of cutting through this wrangle is to reformulate, once again, the definitions of ontological commitment, as follows:-

$(\Pi T)(\Pi K)\ T \text{ oc } K = ((\Sigma T_1)\ T_1 = T \ \&\ O\ (\text{true } T_1 \supset EK));$

$(\Pi T)(\Pi X)\ T \text{ oc } X = ((\Sigma T_1)\ T_1 = T \ \&\ O\ (\text{true } T_1 \supset EX));$

The topmost formula, roughly translated, reads:-

'For any substitution of a theory-denoting term for 'T' and for any substitution of a general noun for 'K' in the following formula:- 'T oc K = for some substitution of 'T_1' in '$T_1 = T \ \&\ O\ (\text{true } T_1 \supset EK)$', the result is true' the result is true.'

Applied to the urn case, what is finally derived is not 'O (true (the theory drawn from the urn) $\supset$ E'unicorns')' but:-

$(\Sigma T_1)\ T_1$ = the theory drawn from the urn & O (true $T_1 \supset$ E'unicorns').

This formula reads as:-

'For some substitution of 'T_1' in:- 'T_1 = the theory drawn from the urn & O (true $T_1 \supset$ E unicorns)', the resulting substitution-instance is true.'

This is in fact the case, since "Unicorns exist" is such a substitution.

1.2 Calculi of Ontological Commitment

The analysis of the concept of ontological commitment proceeds one stage further with the construction of calculi whose sole purpose is to exhibit the logic of ontological commitment. The calculi that will be examined are all extentions of a single calculus Q shortly presented. Q is a substitutional modal calculus with many-sorted variables with an enriched vocabulary including 'true' 'false' and 'E'. The exotic nature of Q is simply a reflection of the minimum apparatus needed to deal with the intensional and antireferential 'oc' and the intensional 'O'.

The Calculus Q

The Alphabet of Q: consists of (i) four kinds of individual constant; t_1, t_2, t_3, t_4, ...; k_1, k_2, k_3, k_4, ...; a_1, a_2, a_3, a_4, ...; p_1, p_2, p_3, p_4, ...; (ii) four kinds of variable; T_1, T_2, T_3, T_4, ...; K_1, K_2, K_3, K_4, ...; X_1, X_2, X_3, X_4, ...; P_1, P_2, P_3, P_4, ...; (iii) the substitutional quantifiers 'Π' and 'Σ' (iv) the logical constants; -, v, &, $\supset$, =, O, $\Diamond$, B, oc, $\overline{oc}$, true, false, (,), =, E.

Rules of Formation for Q

An atomic formula of Q is any of the following (nothing else is an atomic formula of Q). For any i, j $\geq$ 1 where i and j are whole numbers (i) Bp_it_j (ii) $t_i$oc k_j (iii) $t_i$oc k_j (iv) $p_i$oc k_j (v) $p_i$oc k_j (vi) $t_i = t_j$ (vii) $p_i = p_j$ (viii) true t_i (ix) false t_i (x) Ek_i (xi) Ea_i (xii) $k_i a_j$.

Any atomic formula is a wff of Q. Let w_1 and w_2 be wffs of Q: $-w_1$ is a wff of Q, (w_1 v w_2) is a wff of Q, (w_1 & w_2) is a wff of Q, (w_1 w_2) is a wff of Q, (w_1 w_2) is a wff of Q, w_1 is a wff of Q, O w_1 is a wff of Q.

Let c be any individual constant and let v be any variable of Q: v and c are <u>of the same type</u> iff (i) if c = t_i then v = T_j (ii) if c = k_i then v = K_j (iii) if c = a_i then v = X_j (iv) if c = p_i then v = P_j.

If F_c is any wff of Q containing n ($n \geq 1$) occurrences of c, then (Πv) F_v and (Σv) F_v are wffs of Q where F_v results from F_c by the replacement of m ($1 \leq m \leq n$) occurrences of c in F_c by v and v is not bound in F_c and v and c are of the same sort.

Nothing else is a wff of Q.

<u>Informal Reading of Q</u>

Although Q can be studied as a formal system, its point and philosophical interest derive from the way its symbols are read. The informal readings of the symbols of Q are as follows.

<u>Alphabet</u>	<u>Reading</u>
t_1, t_2, t_3, t_4, ...	theory-denoting terms
k_1, k_2, k_3, k_4, ...	general nouns
a_1, a_2, a_3, a_4, ...	definite singular terms
p_1, p_2, p_3, p_4, ...	person-denoting terms
Σ	'There is some true substitution for'
Π	'Any substitution for ... is a true substitution for'
T_1, T_2, T_3, T_4, ...	substitutionally bound variables whose substitution set is t_1, t_2, t_3, t_4, ...
K_1, K_2, K_3, K_4, ...	substitutionally bound variables whose

	substitution set is $k_1, k_2, k_3, k_4, \ldots$
$P_1, P_2, P_3, P_4, \ldots\ldots$	substitutionally bound variables whose substitution set is $p_1, p_2, p_3, p_4, \ldots$
$A_1, A_2, A_3, A_4, \ldots\ldots$	substitutionally bound variables whose substitution set is $a_1, a_2, a_3, a_4, \ldots$
$-$	not
$\vee$	and/or
&	and
$\supset$	if then
$\equiv$	if and only if
O	It is necessary that
	It is possible that
B	..... believes .... to be true
oc	..... is ontologically committed to ...
$\overline{\text{oc}}$	...... is committed to the non-existence of ...
E	operator on mentioned predicates and singular terms showing they are satisfied or denote
true	true
false	false

<u>Axioms of Q</u>: any substitution instance of the following axiom schemata is an axiom of Q.

A1. $Ow_1 \equiv - \Diamond - w_1$

A2. $Ow_1 \supset w_1$

A3. $O(w_1 \supset w_2) \supset (O\, w_1 \supset O\, w_2)$

A4. true t_i v false t_i

A5. - (true t_i & false t_i)

Rules of Derivation in Q: $X \longrightarrow Y$ is to be read as 'From X derive Y'

OI (O Introduction): $\vdash w_1 \longrightarrow Ow_1$

Taut (Tautology): Where Δ is any set of wffs of Q; Δ,Δ tautologically implies $w_1 \longrightarrow w_1$. In particular where $\Delta = \Lambda$, $\longrightarrow w_1$ directly.

CP (Conditional Proof): $\Delta, w_1 \vdash w_1 \longrightarrow \Delta \vdash w_1 \supset w_2$. In particular where $\Delta = \Lambda$, $\longrightarrow w_1 \supset w_2$ directly.

=I (= Introduction): Where c is any individual constant, $\longrightarrow c = c$ directly.

Sub (Substitution): Where c_i and c_j are individual constants and F_{c_i} is any wff containing c_i; F_{c_i}, $c_i = c_j \longrightarrow *F_{c_j}$, where $*F_{c_j}$ is the result of replacing c_i in F_{c_i} by c_j whenever c_i is not in the scope of a modal operator.

O Sub (O Substitution): F_{c_i}, $O(c_i = c_j) \longrightarrow F_{c_j}$, where F_{c_j} is the result of replacing one or more occurences of c_i in F_{c_i} by c_j.

Where v and c is a variable and a constant of the same sort:-

UE (Universal Elimination): $(\Pi v) F_v \longrightarrow F_c$

EG (Existential Generalisation): $F_c \longrightarrow (\Sigma v) F_v$

UI (Universal Introduction): $\vdash F_c \longrightarrow (\Pi v) F_v$

EE (Existential Elimination): Where c does not occur in any element of Δ or in w_1:- $\Delta, F_c \vdash w_1 \longrightarrow \Delta \vdash (\Sigma v) F_v \supset w_1$

The calculi examined are tagged with decimal numbers according to their deductive strengths: higher numbers indicate stronger systems.

The Calculus of Q1: results from adding to Q, two axiom schemata, A6 and A7.

A6 $(\Pi T_i)(\Pi K_j)(T_i \text{oc } K_j = (\Sigma T_k)(T_k = T_i \ \& \ O\,(\text{true } T_k \supset EK_j)$

A6 $(\Pi T_i)(\Pi K_j)(T_i \overline{\text{oc}}\ K_j = (\Sigma T_k)(T_k = T_i \ \& \ O\,(\text{true } T_k \supset -EK_j)$

A6 is the definition of ontological commitment offered earlier and the rationale of A7 is obvious given the reading of 'oc'. Some important theorems of Q1 are:-

Theorem 1 $\vdash_{Q1}$ $(\Pi T_1)(\Pi K_1)(T_1 \text{oc } K_1 \ \& \ \text{true } T_1) \supset EK_1$

1.	$t_1 \text{oc } k_1 \ \& \ \text{true } t_1$	Hyp
2.	$t_1 \text{oc } k_1$	1, Taut
3.	$t_1 \text{oc } k_1 \supset (\Sigma T_2)(T_2 = t_1 \ \& \ O\,(\text{true } T_2 \supset EK_1)$	A6, Taut, UE
4.	$(\Sigma T_2)T_2 = t_1 \ \& \ O\,(\text{true } T_2 \supset Ek_1)$	2,3 Taut
5.	$t_2 = t_1 \ \& \ O\,(\text{true } t_2 \supset Ek_1)$	Hyp
6.	$O\,(\text{true } t_2 \supset Ek_1)$	5 Taut
7.	$\text{true } t_2 \supset Ek_1$	6, A2 Taut
8.	$\text{true } t_1$	1 Taut
9.	$t_2 = t_1$	5 Taut
10.	$\text{true } t_2$	8,9 Sub
11.	Ek_1	7,10 Taut
12.	$((\Sigma T_2)T_2 = t_1 \ \& \ O\,(\text{true } T_2 \supset Ek_1)) \quad Ek_1$	4,5,11 EE
13.	Ek_1	4,12 Taut
14.	$(t_1 \text{oc } k_1 \ \& \ \text{true } t_1) \supset Ek_1$	1,13 CP
15.	$(\Pi T_1)(\Pi K_1)(T_1 \text{oc } K_1 \ \& \ \text{true } T_1) \supset EK_1$	14 UI

Theorem 2 $\vdash_{Q1}$ $(\Pi T_1)(\Pi K_1)(T_1 \overline{\text{oc}}\ K_1 \ \& \ \text{true } T_1) \supset -EK_1$

As theorem 1 using A7 instead of A6.

Theorem 3 $\vdash_{Q1}$ $(\Pi T_1)(\Pi K_1)(T_1 oc\, K_1 \;\&\; T_1 \overline{oc}\, K_1) \supset$ false T_1

1.	$t_1 oc\, k_1 \;\&\; t_1 \overline{oc}\, k_1$	Hyp
2.	$(t_1 oc\, k_1 \;\&$ true $t_1) \supset Ek_1$	Theorem 1, UE
3.	$(t_1 \overline{oc}\, k_1 \;\&$ true $t_1) \supset -Ek_1$	Theorem 2, UE
4.	$-((t_1 oc\, k_1 \;\&$ true $t_1) \;\&\; (t_1 \overline{oc}\, k_1 \;\&$ true $t_1))$	2,3 Taut
5.	$-$ true t_1	1,4 Taut
6.	true t_1 v false t_1	A4
7.	false t_1	5,6 Taut
8.	$(t_1 oc\, k_1 \;\&\; t_1 \overline{oc}\, k_1) \supset$ false t_1	1,7 CP
9.	$(\Pi T_1)(\Pi K_1)(T_1 oc\, K_1 \;\&\; T_1 \overline{oc}\, K_1) \supset$ false T_1	8 UI

Theorems 4 to 7 each depend on the Paradoxes of Strict Implication:-

1.	$w_1 \supset (w_2 \supset w_1)$	Taut
2.	$O(w_1 \supset (w_2 \supset w_1))$	1, OI
3.	$O\, w_1 \supset O(w_2 \supset w_1)$	2, A3

1.	$-w_1 \supset (w_1 \supset w_2)$	Taut
2.	$O(-w_1 \supset (w_1 \supset w_2))$	1, OI
3.	$O - w_1 \supset O(w_1 \supset w_2)$	2, A3

We admit the Paradoxes as Derived Rules.

DR1 $O\, w_1 \supset O(w_2 \supset w_1)$

DR2 $O - w_1 \supset O(w_1 \supset w_2)$

Theorem 4 $\vdash_{Q1}$ $(\Pi T_1)(\Pi K_1) O(EK_1) \supset T_1 oc\, K_1$

1.	$O\, Ek_1$	Hyp
2.	$t_1 = t_1$	$=$ I

3.	$O\,(\text{true } t_1 \supset Ek_1)$	1, DR 1
4.	$t_1 = t_1\ \&\ O\,(\text{true } t_1 \supset Ek_1)$	2,3 Taut
5.	$(\Sigma T_1)\ T_1 = t_1\ \&\ O\,(\text{true } T_1 \supset Ek_1)$	4 EG
6.	$((\Sigma T_1)\ T_1 = t\ \ \&\ O\,(\text{true } T_1 \supset Ek_1)) \supset t_1 oc\ k_1$	A6, UE, Taut
7.	$t_1 oc\ k_1$	5,6 Taut
8.	$O\,Ek_1 \supset t_1 oc\ k_1$	1,8 CP
9.	$(\Pi T_1)(\Pi K_1)(O\,(EK_1) \supset T_1 oc\ K_1$	8 UI

<u>Theorem 5</u> $\vdash_{Q1} (\Pi T_1)(\Pi K_1)(O - EK_1) \supset T_1 \overline{oc}\ K_1$

As theorem 4 using A7 instead of A6.

<u>Theorem 6</u> $\vdash_{Q1} (\Pi T_1)(\Pi K_1)(O \text{ false } T_1) \supset T_1 oc\ K_1$

1.	$O \text{ false } t_1$	Hyp
2.	$\text{false } t_1 \supset - \text{true } t_1$	A5, Taut
3.	$O(\text{false } t_1 \supset - \text{true } t_1)$	2, OI
4.	$O \text{ false } t_1 \supset O - \text{true } t_1$	3, A2
5.	$O - \text{true } t_1$	1,4 Taut
6.	$O\,(\text{true } t_1 \supset Ek_1)$	5 DR2
7.	$t_1 = t_1$	= I
8.	$t_1 = t_1\ \&\ O\,(\text{true } t_1 \supset Ek_1)$	6,7 Taut
9.	$(\Sigma T_1)\ T_1 = t_1\ \&\ O\,(\text{true } T_1 \supset Ek_1)$	8 EG
10.	$((\Sigma T_1)\ T_1 = t_1\ \&\ O\,(\text{true } T_1 \supset Ek_1)) \supset t_1 oc\ k_1$	A6, UE, Taut
11.	$t_1 oc\ k_1$	9,10 Taut
12.	$O \text{ false } t_1 \supset t_1 oc\ k_1$	1,11 CP
13.	$(\Pi T_1)(\Pi K_1)(O \text{ false } T_1) \supset T_1 oc\ K_1$	12 UI

<u>Theorem 7</u> $\vdash_{Q1} (\Pi T_1)(\Pi K_1)(O \text{ false } T_1) \supset T_1 \overline{oc}\ K_1$

As theorem 6 using A7 instead of A6.

Theorems 4 to 7 embody what I call 'the Paradoxes of Ontological Commitment'. Theorem 4 claims, approximately, that every theory is committed to the existence of every necessary entity. Theorem 5 claims that every theory is committed to the non-existence of every impossible entity. Theorem 6 claims that every impossible theory is committed to the existence of anything and theorem 7 says that every impossible theory is committed to the non-existence of anything.

Some of the bizarre consequences of these theorems are best brought out by example. By theorem 4, if (say) numbers exist necessarily then 'Numbers do not exist' is ontologically committed to numbers. By theorem 5 'Square triangles exist' is ontologically committed to the non-existence of square triangles. By theorem 6 '0 = 1' is ontologically committed to unicorns, but, by theorem 7, at the same time denies their existence.

The Calculus Q2: arises from Q1 by the addition of A8 and A9.

A8 $(\Pi T_i)(\Pi T_j)(\Pi K_k)(O\,(\text{true } T_i \supset \text{true } T_j)) \supset (T_j \text{oc } K_k \supset T_i \text{oc } K_k)$

A9 $(\Pi T_i)(\Pi T_j)(\Pi K_k)(O\,(\text{true } T_i \supset \text{true } T_j)) \supset (T_j \overline{\text{oc}}\, K_k \supset T_i \overline{\text{oc}}\, K_k)$

A8 claims that if T_1 necessarily implies T_1 then all ontological commitments incurred by T_1 are incurred by T_1. A9 claims the same for $\overline{\text{oc}}$.

Theorem 8 $\vdash_{Q2} (\Pi T_1)(\Pi T_2)(\Pi K_1)(O \text{ true } T_1 \;\&\; O \text{ true } T_2) \supset (T_1 \text{oc } K_1 \equiv T_1 \text{oc } K_1)$

1.	O true t_1 & O true t_2	Hyp
2.	O true t_1	1 Taut
3.	O true t_2	1 Taut
4.	$O\,(\text{true } t_1 \supset \text{true } t_2)$	2 DR1
5.	$O\,(\text{true } t_2 \supset \text{true } t_1)$	3 DR1

6.	$O(\text{true } t_2 \supset \text{true } t_1) \supset (t_1 oc\ k_1 \supset t_2 oc\ k_1)$	A8, UE
7.	$O(\text{true } t_1 \supset \text{true } t_2) \supset (t_2 oc\ k_1 \supset t_1 oc\ k_1)$	A8, UE
8.	$t_1 oc\ k_1 \supset t_2 oc\ k_1$	5,6 Taut
9.	$t_2 oc\ k_1 \supset t_1 oc\ k_1$	4,7 Taut
10.	$t_1 oc\ k_1 \equiv t_2 oc\ k_1$	8,9 Taut
11.	$(O \text{ true } t_1 \ \& \ O \text{ true } t_2) \supset (t_1 oc\ k_1 \equiv t_2 oc\ k_1)$	1,10 CP
12.	$(\Pi T_1)(\Pi T_1)(\Pi K_1)(O \text{ true } T_1 \ \& \ O \text{ true } T_2)$	
	$\supset (T_1 oc\ K_1 \equiv T_2 oc\ K_1)$	11 UI

<u>Theorem 9</u> $\vdash_{Q2} (\Pi T_1)(\Pi T_1)(\Pi K_1)(O \text{ true } T_1 \ \& \ O \text{ true } T_1) \supset (T_1 \overline{oc}\ K_1 \equiv T_1 \overline{oc}\ K_1)$

As theorem 8 using A9 instead of A8.

Theorems 8 and 9 might be classed as amongst the Paradoxes of Ontological Commitment since both rely essentially on one of the Paradoxes of Strict Implication. Collectively, theorems 8 and 9 claim that any two necessary theories are ontologically committed to the existence of exactly the same things and deny the existence of exactly the same things.

Whether theorems 8 and 9 are judged acceptable is partly determined by the justice of certain philosophical views on modality and existence. According to one view of modality that did, (and still does), enjoy much influence, a sentence or theory is only necessarily true because of human conventions that determine the meaning of that sentence. This position is commonly called <u>conventionalism</u> (a good exposition being Ayer [10] chapter four). 'All triangles have three sides' is necessary, according to the conventionalist, because human beings have determined that 'triangle' shall mean <u>three sided figure</u>. It is impossible however, that the contents of the universe should be determined by the way that people use words. Consequently, it is not possible to deduce anything from a necessary truth about what exists: a position

Wittgenstein expressed in the Tractatus.

'Tautologies and contradictions are not pictures of reality'

Wittgenstein [150] (4.462)

Conventionalism fits in well with theorems 8 and 9; all necessary truths have the same commitments, i.e. none at all.

A rather older view of modality, endorsed by figures such as Leibnitz [80] and St. Anselm [3] does not see all existence claims as contingent. The older school prefers to see some of the properties that attach themselves to objects as attaching themselves necessarily to that object. This view is commonly termed essentialism. Those sentences which report on the existence of objects which necessarily exist are themselves necessarily true. An essentialist who, for example, took both numbers and universals as necessary would not wish to equate the ontological commitments of 'Numbers exist' with that of 'Universals exist'.

The Calculus Q3: results from adding A10 and A11 to Q2

A10 $(\Pi P_i)(\Pi K_j)\, P_i \mathrm{oc}\, K_j \;=\; (\Sigma T_1)\, BP_iT_k \;\&\; T_k \mathrm{oc}\, K_j$

A11 $(\Pi P_i)(\Pi K_j)\, P_i \overline{\mathrm{oc}}\, K_j \;=\; (\Sigma T_1)\, BP_iT_k \;\&\; T_k \overline{\mathrm{oc}}\, K_j$

A10 is an answer to a question in the opening page of this chapter: 'What is it for a person to be ontologically committed to a sort of entity?'. The answer A10 provides is that a person is so committed just when he believes a theory which is so committed.

Theorem 10 $\vdash_{Q3} (\Pi P_1)(\Pi T_1)(\Pi T_2)(\Pi K_1)((O\,(\text{true } T_1 \supset \text{true } T_2))$

$\& \,(T_2 \mathrm{oc}\, K_1 \;\&\; BP_1T_1\,)) \supset P_1 \mathrm{oc}\, K_1$

1.	$(O\,(\text{true } t_1 \supset \text{true } t_2)) \;\&\; (t_2 oc\, k_1 \;\&\; Bp_1t_1)$	Hyp
2.	$O\,(\text{true } t_1 \supset \text{true } t_2) \supset (t_2 oc\, k_1 \supset t_1 oc\, k_1)$	A8, UE
3.	$t_1 oc\, k_1$	1,2 Taut
4.	$Bp_1t_1 \;\&\; t_1 oc\, k_1$	1,3 Taut
5.	$(\Sigma T_1)\, Bp_1T_1 \;\&\; T_1\, oc\, k_1$	4 EG
6.	$((\Sigma T_1)\, Bp_1T_1 \;\&\; T_1\, oc\, k_1) \quad p_1 oc\, k_1$	A10, UE, Taut
7.	$p_1 oc\, k_1$	5,6 Taut
8.	$((O\,(\text{true } t_1 \supset \text{true } t_2)) \;\&\; (t_2 oc\, k_1 \;\&\; Bp_1t_1))$	
	$\supset\; p_1 oc\, k_1$	1,7 CP
9.	$(\Pi P_1)(\Pi T_1)(\Pi T_2)(\Pi K_1)((O\,(\text{true } T_1 \supset \text{true } T_2))$	
	$\&\; (T_2 oc\, K_1 \;\&\; BP_1T_1\,)) \supset P_1 oc\, K_1$	8 UI

<u>Theorem 11</u> $\vdash_{Q3} (\Pi P_1)(\Pi T_1)(\Pi T_1)(\Pi K_1)((O\,(\text{true } T_1 \supset \text{true } T_1))$

$\&\; (T_1 \overline{oc}\, K_1 \;\&\; BP_1T_1\,)) \supset P_1 \overline{oc}\, K_1$

As theorem 10 using A9 and A11 instead of A8 and A10.

Theorem 10 makes it possible for a person to have ontological commitments he does not recognise himself. A person can assent to a theory which strictly implies the existence of a kind K, and nevertheless fail to acknowledge his ontological commitment because he fails to see the implication.

<u>The Calculus Q4</u>: results from Q3 by the addition of A12.

A12 $(\Pi P_i)(\Pi K_j)\, P_i oc\, K_j \supset -\, P_i \overline{oc}\, K_j$

A12 claims that if a person is committed to the existence of a kind K, he is not committed to the non-existence of Ks. A12 is a contentious axiom. Immediate is theorem 12.

<u>Theorem 12</u> $\vdash_{Q4} (\Pi P_1)(\Pi K_1) - (P_1 oc\, K_1 \ \& \ P_1 \overline{oc}\, K_1)$

1.	$p_1 oc\, k_1 \supset - p_1 \overline{oc}\, k_1$	A12, UE
2.	$-(p_1 oc\, k_1 \ \& \ p_1 \overline{oc}\, k_1)$	1 Taut
3.	$(\Pi P_1)(\Pi K_1) - (P_1 oc\, K_1 \ \& \ P_1 \overline{oc}\, K_1)$	2 UI

But within Q4 there are theorems which definitely merit rejection; for example theorem 13.

<u>Theorem 13</u> $\vdash_{Q4} (\Pi P_1)(\Pi T_1) - (O\,(\text{false } T_1) \ \& \ BP_1T_1)$

1.	$O(\text{false } t_1) \ \& \ Bp_1t_1$	Hyp
2.	$O(\text{false } t_1) \supset t_1 oc\, k_1$	Theorem 6, UE
3.	$O(\text{false } t_1) \supset t_1 \overline{oc}\, k_1$	Theorem 7, UE
4.	$t_1 oc\, k_1$	1,2 Taut
5.	$t_1 \overline{oc}\, k_1$	1.3 Taut
6.	$Bp_1t_1 \ \& \ t_1 oc\, k_1$	1,4 Taut
7.	$Bp_1t_1 \ \& \ t_1 \overline{oc}\, k_1$	1,5 Taut
8.	$(\Sigma T_1)Bp_1T_1 \ \& \ T_1\, oc\, k_1$	6 EG
9.	$(\Sigma T_1)Bp_1T_1 \ \& \ T_1\, \overline{oc}\, k_1$	7 EG
10.	$((\Sigma T_1)Bp_1T_1 \ \& \ T_1\, oc\, k_1) \supset p_1 oc\, k_1$	A10, UE, Taut
11.	$((\Sigma T_1)Bp_1T_1 \ \& \ T_1\, \overline{oc}\, k_1) \supset p_1 \overline{oc}\, k_1$	A11, UE, Taut
12.	$p_1 oc\, k_1$	8,10 Taut
13.	$p_1 \overline{oc}\, k_1$	9,11 Taut
14.	$p_1 oc\, k_1 \supset - p_1 \overline{oc}\, k_1$	A12, UE
15.	$-p_1 \overline{oc}\, k_1$	12,14 Taut
16.	$p_1 \overline{oc}\, k_1 \ \& - p_1 \overline{oc}\, k_1$	13,15 Taut
17.	$(O \text{ false } t_1 \ \& \ Bp_1t_1) \supset (p_1 \overline{oc}\, k_1 \ \& - p_1 \overline{oc}\, k_1)$	1,16 CP
18	$-(O \text{ false } t_1 \ \& \ Bp_1t_1)$	17 Taut
19.	$(\Pi P_1)(\Pi T_1) - (O\,(\text{false } T_1) \ \& \ BP_1T_1)$	18 UI

Theorem 13 claims nobody believes a theory which is necessarily false! The immediate thought is that unpleasant consequences can be avoided by banishing A12 and staying within Q3. This immediate thought is squashed by theorems 14 and 15, which are theorems of Q3.

<u>Theorem 14</u> $\vdash_{Q3} (\Pi P_1)(\Pi T_1)(\Pi K_1)(BP_1T_1 \ \& \ O\,(\text{false } T_1)) \supset P_1 oc\, K_1$

1.	$Bp_1t_1 \ \& \ O\,(\text{false } t_1)$	Hyp
2.	$O(\text{false } t_1) \supset t_1 oc\, k_1$	Theorem 6, UE
3.	$t_1 oc\, k_1$	1,2 Taut
4.	$Bp_1t_1 \ \& \ t_1 oc\, k_1$	1,3 Taut
5.	$(\Sigma T_1)Bp_1T_1 \ \& \ T_1\ oc\ k_1$	4 EG
6.	$((\Sigma T_1)Bp_1T_1 \ \& \ T_1\ oc\ k_1) \supset p_1 oc\, k_1$	A10, UE, Taut
7.	$p_1 oc\, k_1$	5,6 Taut
8.	$(Bp_1t_1 \ \& \ O \text{ false } t_1) \supset p_1 oc\, k_1$	1,7 CP
9.	$(\Pi P_1)(\Pi T_1)(\Pi K_1)(BP_1T_1 \ \& \ O\,(\text{false } T_1)) \supset P_1 oc\, K_1$	8 UI

<u>Theorem 15</u> $\vdash_{Q3} (\Pi P_1)(\Pi T_1)(\Pi K_1)(BP_1T_1 \ \& \ O\,(\text{false } T_1)) \supset P_1 \overline{oc}\, K_1$

As theorem 14 using A11 instead of A10 and theorem 7 instead of theorem 6. Theorem 14 declares that anybody who accepts a theory which is necessarily false is ontologically committed to everything. Thus the Greek mathematicians who believed that an angle could be trisected using only a compass and straightedge (which was proved impossible in the 19th century by Wantzel) were committed, by theorem 14, to the existence of talking stones, cubic pyramids, flying saucers (or teapots for that matter) and any other phantasm imagination can suggest.

1.21 Calculi which lack the Paradoxes of Ontological Commitment

For believers in the claim that strict implication is to be distinguished from entailment, the Paradoxes of Ontological Commitment are a sure sign that something has gone wrong as early as Q1. Since the derivations of the implausible theorems 13, 14 and 15 depend on the Paradoxes of Ontological Commitment, these later difficulties will then be thought of as advanced symptoms of a disease which crept in at Q1. One option is to block off the Paradoxes of Ontological Commitment at the start.

The simplest way to do so is to insist on a distinction between strict implication and entailment; then to follow up the distinction by defining the conditions of ontological commitment in terms of entailment. Using '$\Rightarrow$' for 'entails', the revised definition of ontological commitment reads:-

A*6 $(\Pi T_i)(\Pi K_j)\ T_i oc\ K_j \equiv (\Pi T_k)\ T_k = T_i \ \&\ (\text{true } T_k \Rightarrow EK_1)$

A*7 $(\Pi T_i)(\Pi K_j)\ T_i \overline{oc}\ K_j \equiv (\Pi T_k)\ T_k = T_i \ \&\ (\text{true } T_k \Rightarrow -EK_1)$

In order to cope with the logic of the new connective '$\Rightarrow$', Q must be enriched. Anderson and Belnap's [12] System E is a likely choice for encapsulating the logic of entailment for those who wish to demarcate entailment from strict implication. The system Q* is the result of adding to Q:-

(1) the connective $\Rightarrow$ along with the appropriate accommodation in the rules of formation; namely, if w_1 and w_2 are wffs, so is $w_1 \Rightarrow w_2$.

(2) The rule of inference: $w_1 \Rightarrow w_2 \longrightarrow \Box(w_1 \supset w_2)$ ($\Rightarrow$E: ($\Rightarrow$Elimination))

(3) Certain axioms dealing with $\Rightarrow$ ie. System E.

E1 $((w_1 \Rightarrow w_1) \Rightarrow w_2) \Rightarrow w_2$

E2 $((w_1 \Rightarrow w_2) \Rightarrow ((w_2 \Rightarrow w_3) \Rightarrow (w_1 \Rightarrow w_3))$

E3 $(w_1 \Rrightarrow (w_1 \Rrightarrow w_2)) \Rrightarrow (w_1 \Rrightarrow w_2)$

E4 $(w_1 \,\&\, w_2) \Rrightarrow w_1$

E5 $(w_1 \,\&\, w_2) \Rrightarrow w_2$

E6 $((w_1 \Rrightarrow w_2) \,\&\, (w_1 \Rrightarrow w_3)) \Rrightarrow (w_1 \Rrightarrow (w_2 \,\&\, w_3))$

E7 $(O\, w_1 \,\&\, O\, w_2) \Rrightarrow O\, (w_1 \,\&\, w_2)$

E8 $w_1 \Rrightarrow (w_1 \vee w_2)$

E9 $w_1 \Rrightarrow (w_2 \vee w_1)$

E10 $((w_1 \Rrightarrow w_3) \,\&\, (w_2 \Rrightarrow w_3)) \Rrightarrow ((w_1 \vee w_2) \Rrightarrow w_3)$

E11 $((w_1 \,\&\, (w_2 \vee w_3)) \Rrightarrow ((w_1 \,\&\, w_2) \vee w_3)$

E12 $(w_1 \Rrightarrow - w_1) \Rrightarrow - w_1$

E13 $(w_1 \Rrightarrow - w_2) \Rrightarrow (w_2 \Rrightarrow - w_1)$

E14 $- - w_1 \Rrightarrow w_1$

In system E neither '$O\, w_1 \supset (w_2 \Rrightarrow w_1)$' nor '$O - w_1 \supset (w_1 \Rrightarrow w_2)$' are derivable. Thus the Paradoxes of Ontological Commitment are not forthcoming when A*6 and A*7 are added to Q* to form Q*1. By replacing '$O(\ldots \supset \ldots)$' by '$\ldots \Rrightarrow \ldots$' throughout A8 to A12, one generates a whole series of calculi parallel to Q1 to Q4.

i.e. $Q^* = Q + (\Rrightarrow E) + \text{System E}$

$Q^*1 = Q^* + A^*6 + A^*7$

A*8 $(\Pi T_i)(\Pi T_j)(\Pi K_k)(\text{true } T_i \Rrightarrow \text{true } T_j) \supset (T_j oc\ K_k \supset T_i oc\ K_k)$

A*9 $(\Pi T_i)(\Pi T_j)(\Pi K_k)(\text{true } T_i \Rrightarrow \text{true } T_j) \supset (T_j \overline{oc}\ K_k \supset T_i \overline{oc}\ K_k)$

$Q^*2 = Q^*1 + A^*8 + A^*9$

$Q^*3 = Q^*2 + A10 + A11$

$Q^*4 = Q^*3 + A12$

Q*1 to Q*3 are free from the Paradoxes of Ontological Commitment and from theorems 13, 14 and 15. Q*4 is dubious, however, since in Q*4, it is a theorem that nobody believes a theory which entails there are K_1s and a theory which entails there are not K_1s.

<u>Theorem 16</u> $\vdash_{Q*4}$ $(\Pi P_1)(\Pi T_1)(\Pi T_2)(\Pi K_1) - ((BP_1T_1 \;\&\; BP_1T_2) \;\&\; (T_1 oc\, K_1 \;\&\; T_2 \overline{oc}\, K_1))$

1.	$(Bp_1t_1 \;\&\; Bp_1t_2) \;\&\; (t_1 oc\, k_1 \;\&\; t_2 \overline{oc}\, k_1)$	Hyp
2.	$Bp_1t_1 \;\&\; t_1 oc\, k_1$	1 Taut
3.	$Bp_1t_2 \;\&\; t_2 \overline{oc}\, k_1$	1 Taut
4.	$(\Sigma T_1)\, Bp_1t_1 \;\&\; T_1 oc\, k_1$	2 EG
5.	$(\Sigma T_1)\, Bp_1T_2 \;\&\; T_2 \overline{oc}\, k_1$	3 EG
6.	$((\Sigma T_1)\, Bp_2T_1 \;\&\; T_1 oc\, k_1) \supset p_1 oc\, k_1$	A10, UE, Taut
7.	$((\Sigma T_1)\, Bp_1T_1 \;\&\; T_1 \overline{oc}\, k_1) \supset p_1 \overline{oc}\, k_1$	A11, UE, Taut
8.	$p_1 oc\, k_1$	4,6 Taut
9.	$p_1 \overline{oc}\, k_1$	5,7 Taut
10.	$p_1 oc\, k_1 \supset - p_1 \overline{oc}\, k_1$	A12, UE
11.	$- p_1 \overline{oc}\, k_1$	8,10 Taut
12.	$p_1 \overline{oc}\, k_1 \;\&\; -p_1 \overline{oc}\, k_1$	8,11 Taut
13.	$((Bp_1t_1 \& Bp_1t_2) \;\&\; (t_1 oc\, k_1 \;\&\; t_2 \overline{oc}\, k_1)) \supset (p_1 \overline{oc}\, k_1 \;\&\; -p_1 \overline{oc}\, k_1)$	1,12 CP
14.	$-((Bp_1t_1 \;\&\; Bp_1t_2) \;\&\; (t_1 oc\, k_1 \;\&\; t_2 \overline{oc}\, k_1))$	13 Taut
15.	$(\Pi P_1)(\Pi T_1)(\Pi T_2)(\Pi K_1) - ((BP_1T_1 \;\&\; BP_1T_2) \;\&\; (T_1 oc\, K_1 \;\&\; T_2 \overline{oc}\, K_1))$	14 UI

The weakness of the Q* calculi lies in the theory of entailment on which they are all founded; namely <u>System E</u>. In System E, two axioms are inconspicuosly absent. These are the axioms of disjunctive syllogism.

$$((w_1 \vee w_2) \& -w_1) \Rrightarrow w_2$$

$$((w_1 \vee w_2) \& -w_2) \Rrightarrow w_1$$

These axioms cannot be added to system E without engendering 'Paradoxes of Entailment' exactly parallel to those of Strict Implication (see Anderson and Belnap [12]). Many logicians feel that rejecting the axioms of disjunctive syllogism is a step more drastic and questionable than identifying strict implication and entailment. The axioms of disjunctive syllogism are ones that Stoic logicians said even dogs in the street recognise: when chasing their prey to a point where the road forks, they will sniff along one fork, and if they catch no scent there, they will chase along the other without sniffing.

1.22 Other Solutions

Other solutions to the absurdities of theorems 13, 14 and 15 retain the Paradoxes of Ontological Commitment, but modify the later axioms. The modifications center principally on A10 and A11; which are those axioms which relate the ontological commitments of a person to the ontological commitments of the theories he happens to believe. A10, for instance, claims that a person is committed to a kind K if, and only if, he believes a theory which is ontologically committed to Ks. However, the Paradoxes of Ontological Commitment assure us that any necessary falsehood is committed to the existence (and the nonexistence) of anything. So we deduce that any person unfortunate enough to accept a necessary falsehood is committed for and against the existence of anything.

A fairly obvious thought is that if the ontological commitments of a person were only extracted from the consistent parts of his belief-set then these difficulties would be avoided. This thought can be taken up in various ways. For instance, B10 and B11 can replace A10 and A11.

B10 $(\Pi P_i)(\Pi K_j)\ P_i oc\ K_j \equiv (\Sigma T_k)\ BP_iT_k\ \ \&\ (T_k oc\ K_j\ \&\ \Diamond\ \text{true}\ T_k)$

B11 $(\Pi P_i)(\Pi K_j)\ P_i \overline{oc}\ K_j \equiv (\Sigma T_k)\ BP_iT_k\ \ \&\ (T_k \overline{oc}\ K_j\ \&\ \Diamond\ \text{true}\ T_k)$

The extra condition $\Diamond$true T_1' prevents the derivation of theorems 13 and 14. R1 is the result of adding B10 and B11 to Q2, and R2 is the result of adding A12 to R1.

i.e. R1 = Q2 + B10 + B11

R2 = R1 + A12

But R2 is not itself free from absurdity for it is a theorem of R2 that nobody believes two contingent theories with contrary commitments.

<u>Theorem 17</u> $\vdash_{R2}\ (\Pi P_1)(\Pi T_1)(\Pi T_2)(\Pi K_1) - (((T_1 oc\ K_1\ \&\ T_2 \overline{oc}\ K_1)$
$\&\ (BP_1T_1\ \ \&\ BP_1T_2))\ \&\ (\Diamond\ \text{true}\ T_1\ \&\ \Diamond \text{true}\ T_2))$

1.	$((t_1 oc\ k_1\ \ \&\ t_2 \overline{oc}\ k_1)\ \&\ (Bp_1t_1\ \&\ Bp_1t_2))$ $\&\ (\Diamond\ \text{true}\ t_1\ \ \&\ \Diamond \text{true}\ t_2)$	Hyp
2.	$Bp_1t_1\ \&\ (t_1 oc\ k_1\ \ \&\ \Diamond \text{true}\ t_1\ )$	1 Taut
3.	$Bp_1t_2\ \&\ (t_2 \overline{oc}\ k_1\ \ \&\ \Diamond \text{true}\ t_2\ )$	1 Taut
4.	$(\Sigma T_1)Bp_1T_1\ \&\ (T_1 oc\ k_1\ \&\ \Diamond \text{true}\ T_1)$	2 EG
5.	$(\Sigma T_1)Bp_1T_2\ \&\ (T_2 \overline{oc}\ k_1\ \&\ \Diamond\ \text{true}\ T_2)$	3 EG
6.	$((\Sigma T_1)Bp_1T_1\ \&\ (T_1 oc\ k_1\ \&\ \Diamond\ \text{true}\ T_1)) \supset p_1 oc\ k_1$	B10, UE, Taut
7.	$((\Sigma T_1)Bp_1T_2\ \&\ (T_1 \overline{oc}\ k_1\ \&\ \Diamond\ \text{true}\ T_2)) \supset p_1 \overline{oc}\ k_1$	B11, UE, Taut
8.	$p_1 oc\ k_1$	4,6 Taut
9.	$p_1 \overline{oc}\ k_1$	5,7 Taut
10.	$p_1 oc\ k_1 \supset -\ p_1 \overline{oc}\ k_1$	A12, UE
11.	$-\ p_1 \overline{oc}\ k_1$	8, 10 Taut
12.	$p_1 \overline{oc}\ k_1\ \ \&\ -\ p_1 \overline{oc}\ k_1$	9,11 Taut

13. $(((t_1 oc\ k_1\ \&\ t_2 \overline{oc}\ k_1) \& (Bp_1t_1 \& Bp_1t_2))$
$\& (\Diamond true\ t_1\ \&\ \Diamond true\ t_2)) \supset (p_1 \overline{oc}\ k_1\ \&\ - p_1 \overline{oc}\ k_1)$ 1, 12 CP

14. $- (((t_1 oc\ k_1\ \&\ t_2 \overline{oc}\ k_1) \& (Bp_1t_1 \& Bp_1t_2))$
$\& (\Diamond true\ t_1\ \&\ \Diamond true\ t_2))$ 13 Taut

15. $(\Pi P_1)(\Pi T_1)(\Pi T_2)(\Pi K_1) - (((T_1 oc\ K_1 \& T_2 \overline{oc}\ K_1)$
$\& (BP_1T_1\ \&\ BP_1T_2)) \& (\Diamond true\ T_1 \& \Diamond true\ T_2))$ 14 UI

An alternative to B10 and B11 is C10 and C11.

C10 $(\Pi P_i)(\Pi K_j)\ P_i oc\ K_j \equiv ((\Sigma T_k)(BP_iT_k\ \&\ T_k oc\ K_j) \& - (\Sigma T_l)(BP_iT_l \& T_l \overline{oc}\ K_j))$

C11 $(\Pi P_i)(\Pi K_j)\ P_i \overline{oc}\ K_j \equiv ((\Sigma T_k)(BP_iT_k\ \&\ T_k \overline{oc}\ K_j) \& - (\Sigma T_l)(BP_iT_l \& T_l oc\ K_j))$

S1 results from adding C10 and C11 to Q2. In S1, axiom A12 is not independent but may be proved as a theorem.

<u>Theorem 18</u> $\vdash_{S1}\ (\Pi P_1)(\Pi K_1) P_1 oc\ K_1 \supset - P_1 \overline{oc}\ K_1$

1. $p_1 oc\ k_1$ Hyp
2. $p_1 oc\ k_1 \supset ((\Sigma T_1)(Bp_1T_1 \& T_1 oc\ k)$
$\& - (\Sigma T_2)(Bp_1T_2 \& T_2 oc\ k_1))$ C10, UE
3. $(\Sigma T_1)(BpT_1 \& T_1 oc\ k) \& - (\Sigma T_2)(Bp_1T_2 \& T_2 oc\ K_1)$ 1,2 Taut
4. $- (\Sigma T_2)(Bp_1T_2 \& T_2 oc\ K_1)$ 3 Taut
5. $p_1 oc\ k_1$
$\supset ((\Sigma T_2)(Bp_1T_2 \& T_2 oc\ k) \& - (\Sigma T_1)(Bp_1T_1 \& T_1 oc\ k_1))$ C11, UE
6. $- p_1 oc\ k_1$ 4,5 Taut
7. $p_1 oc\ k_1 \supset - p_1 oc\ k_1$ 1,6 CP
8. $(\Pi P_1)(\Pi K_1) P\ oc\ K_1 \supset - P\ \overline{oc}\ K_1$ 7 UI

However S1, though deductively strong, goes too far. In S1 it can be proved that anybody who believes a theory which is necessarily false, is ontologically committed to nothing!

<u>Theorem 19</u> $\vdash_{S1}$ $(\Pi P_1)(\Pi T_1)(\Pi K_1)(BP_1T_1 \ \& \ O \text{ false } T_1) \supset - P_1 oc K_1$

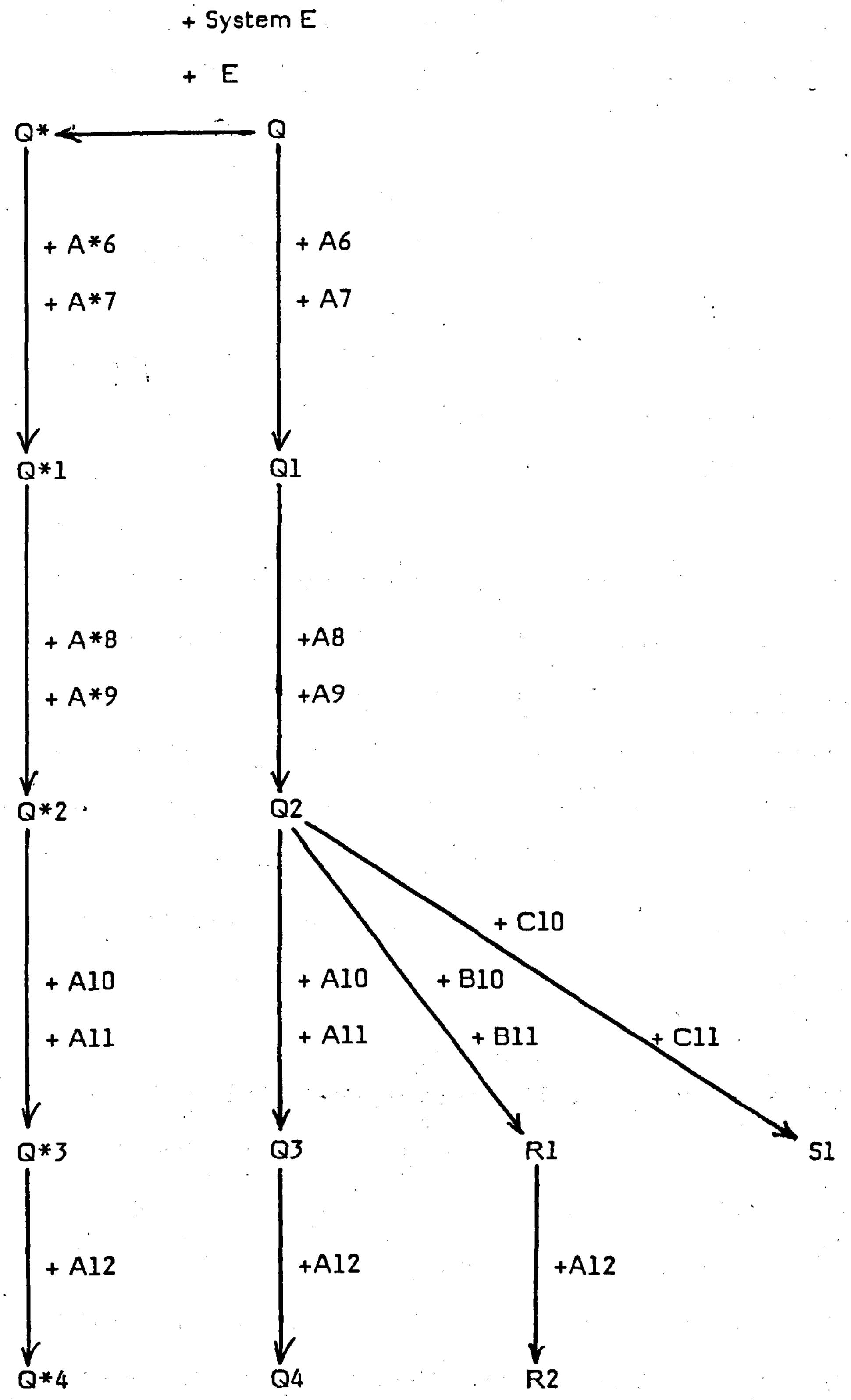
1.	$Bp_1t_1 \ \& \ O \text{ false } t_1$	Hyp
2.	$O \text{ false } t_1 \supset t_1 \overline{oc}\, k_1$	Theorem 7, UE
3.	$p_1 oc\, k_1$	
	$\supset ((\Sigma T_1)(Bp_1T_1 \ \& \ T_1 oc\, k_1) \ \& - (\Sigma T_1)(Bp_1T_1 \ \& \ T_1 \overline{oc}\, k_1))$	C10, UE, Taut
4.	$Bp_1t_1 \ \& \ t_1 \overline{oc}\, k_1$	1,2 Taut
5.	$(\Sigma T)Bp_1T_1 \ \& \ T_1 \overline{oc}\, k_1$	4 EI
6.	$- p_1 oc\, k_1$	3,5 Taut
7.	$(Bp_1t_1 \ \& \ O \text{ false } t_1) \supset - p_1 oc\, k_1$	1,6 CP
8.	$(\Pi P_1)(\Pi T_1)(\Pi K_1)(BP_1T_1 \ \& \ O \text{ false } T_1) \supset - P_1 oc\, K_1$	7 UI

The relations between the various calculi are illustrated in diagram 1.

Which 'ontic logic' best captures the logical properties of ontological commitment? The issues and problems are surprisingly similar to those surrounding modal logics: in modal logic too, we have a plethora of non-equivalent formal systems each competing for recognition. In such cases our intuitions seem too flimsy to discriminate in favour of one unique system. Perhaps there is no 'right' modal logic and no right ontic logic either.

The case, cannot, at any rate, be settled for any one system by an investigation of 'ordinary use'. The phrase 'ontological commitment' is a term of art amongst professional philosophers and a head count of philosophers would establish very little. However we can argue for the <u>selection</u> of a calculus on <u>pragmatic</u> grounds. These grounds are that a calculus should be as deductively strong as is consistent with it being free from absurdity. The

Diagram 1



+ System E
+ E
Q*
Q
+ A*6
+ A*7
+ A6
+ A7
Q*1
Q1
+ A*8
+ A*9
+A8
+A9
Q*2
Q2
+ A10
+ A11
+ A10
+ A11
+ B10
+ B11
+ C10
+ C11
Q*3
Q3
R1
S1
+ A12
+A12
+A12
Q*4
Q4
R2

calculus selected can then serve to legislate for the logical properties of ontological commitment, even where our intuitions leave off. From this perspective, R1 looks the likeliest choice for the logic of ontological commitment.

1.3 Criteria of Ontological Commitment

A criterion need not be a definition, and conversely, a definition need not be a criterion. Before embarking on an examination of criteria of ontological commitment, it is useful to have a clear idea of the distinction between the two.

A definition of a property P states what it is for an object to have P or to be a P. Thus a definition is required to have the form 'For any x, x is P if and only if ....' and not only to be true but also to be necessarily true. A definition is also required to be non-circular (a formally difficult condition to define) which requires that the definiens and the definiendum contain different expressions. In philosophy, the concept of circularity is widened to embrace 'dictionary definitions' 'x knows that p if and only if x is cognisant of p' would be counted as circular by most philosophers even though it would be good enough for the basis of a dictionary entry.

'Criterion' is a slippery word as those involved in the exegesis of Wittgenstein's later philosophy know. In ordinary speech a criterion for the prescence of a property P is a feature or procedure for basing reasonable judgements on the prescence or absence of P. Thus a reading of a temperature above 98.4°F (37°C) on a human being would be a criterion that the human being was ill. But the criterion of having a high temperature, though normally a sufficient condition of being ill, is not a necessary condition of being ill, since there are certain diseases (e.g. cancer, multiple sclerosis) which are not associated with high body temperature. A criterion need not be infallible. A yellow sky in the evening is a criterion of stormy weather to come and yet it is not an infallible criterion.

The most important feature of a criterion is that being a standard by which to make judgements it must be epistemically useful. The concept of epistemic usefulness can be defined as follows. A criterion C for the detection of a property P is epistemically useful to an agent A if and only if the possession of C by A improves the capacity of A to recognise the prescence of property P. Criteria can also be roughly divided (with certain borderline cases) into procedural criteria and nonprocedural criteria. A procedural criterion is a criterion which requires knowledge and execution of a procedure in order for that criterion to be applied. For example, the Marsh test for arsenic and the electrolysis test for distilled water require knowledge of some of the procedures of analytical chemistry to be applied successfully. Other criteria do not require to be prefaced by a procedure: e.g. forecasting the weather by looking at the clouds.

Definitions in contrast, though they may be procedural, need not be epistemically useful. An example of an epistemically useless definition is Tarski's Semantic definition of truth as satisfaction by all sequences. This definition of truth is not circular in any formal sense, but it does not improve our ability to distinguish the true from the false; to settle for instance, the Bacon/Shakespeare controversy or any unsolved problem in science or mathematics. Nor is there any reason why a definition of truth should improve our capabilities of distinguishing the true from the false. In summary, then, the leading features distinguishing criteria and definitions are these.

Criteria	Definitions
Gives often sufficient conditions, but may not be necessary.	Gives necessary and sufficient conditions.
Possibly fallible; more desirable the less fallible it is.	Infallible; necessarily true in all cases.
Epistemically useful: improves our capacity to detect the requisite properties.	Non-circular; but may not be epistemically useful.
Procedural in many cases.	

Moving from reflection on criteria in general to criteria for ontological commitment in particular, most current studies on criteria of ontological commitment are either written or inspired by the work of Quine. Quine's statements of his criterion are not always equivalent to each other or satisfactory in themselves. To gain a foretaste and a general impression of Quine's work in this area, here is one criterion of ontological commitment that Quine offers.

'In general, entities of a given sort are assumed by a theory if and only if some of them must be counted among the values of the variables in order that the statements affirmed in the theory be true.'

Quine [108] (103)

The technical phrase 'values of the variables' shows that Quine's criterion is one that requires a certain technical knowledge to appreciate. In fact, Quine's criterion is a procedural one, at least in part, and the useful application of

Quine's criterion presupposes an ability to institute a procedure Quine calls paraphrase or regimentation, but which most philosophers call formalisation. The exact nature of this procedure and the constraints under which it should be carried out is an arguing point for many philosophers. What is said here is only in the way of preparation for a more detailed discussion in chapters 2 and 3, and as an opportunity for signposting the reader to some relevant but scattered material throughout this text.

The consensus opinion amongst those analytical philosophers who favour formalisation as a philosophical tool is as follows. The best means of determining an answer to the ontological question as to what exists is to select those theories that expert opinion judges to be true, and determine to what those theories are ontologically committed. If those theories are expressed in a natural language however, frequently their ontological commitments are not immediately apparent. Most formally inclined philosophers blame this on the fact that natural languages do not provide an adequate syntactical mark of ontological commitment (see 2.2 on the distinction between grammatical and logical form). Their solution is to rephrase or formalise the theories in question into a formal language; (generally a first-order language based on the predicate calculus, but see 6.2 and chapter 7 for the consequences and principles involved in the choice of an appropriate notation). What is the relation between the original natural language theories and the formal theories which are the product of formalisation? Here formal philosophers divide. Some insist that formalisation is a meaning-preserving procedure and hence that formalisation is like translation (see 2.2, 2.6). Some formalists believe a theory has one and only one proper formalisation and this is the logical form of the theory in question (see 2.2 and 2.6 again); but other philosophers insist that there are a number of competing but equally viable formalisations of certain natural language theories (see chapter 5). Having completed the process of formalisation, one looks for theorems of the form $(\exists x)(\_\_\_\_)$ to determine what the ontological commitments are.

1.3.1 Quine's Criteria

Statements of a criterion of ontological commitment are scattered throughout much of Quine's work and the best way of coming to grips with them is to examine them one by one on their own terms. This is how, in fact, we shall proceed.

Formulation 1

'We are convicted of a particular ontological presupposition if, and only if, the alleged presuppositum has to be reckoned among the entities over which our variables range in order to render one of our affirmations true.'

Quine [116] (13)

'To show some given object is required in a theory what we have to show is no more or less than that object is required for the truth of the theory to be among the values over which the variables range'

Quine [109] (93)

These two versions of Quine's criterion are similar enough to deserve grouping together under one formulation. Nevertheless there is one subtle difference worth noticing. The first quoted version presents a criterion of ontological commitment for persons and the second for theories. Since Quine does not devote any space to examining the relations between the ontological commitments of persons and of theories, it is likely this sort of difference would not engage his attention. Actually, of course, there is a substantive philosophical issue at stake here, as we saw in 1.2., about the exact nature of the relations between the ontological commitments of theories and the people who believe them.

Formulation 1 fails principally because it treats ontological commitment as referential; which it is not. 'Dracula exists' is ontologically committed to Dracula, but Dracula is not an object to be counted amongst the values of the domain of the theory, since Dracula does not exist as an object at all.

These versions also fail to apply to cases of ontological commitments to sorts of things. The theory '(∃x) table x' is ontologically committed to tables. But it is false to say that, of any particular object, that that object is required to be counted within the range of the variable 'x' in order for the theory to be true. (See Chihara [26]).

Formulation 1 fails of ontological commitments to individual items which do not exist and also of commitments to sorts of item which do exist. Formulation 1 also fails of ontological commitments to individual items which do exist. Thus '(∃x)x = Richard Nixon' is obviously ontologically committed to the existence of Richard Nixon. Formulation 1 captures this much, since Richard Nixon has to be counted within the range of the variable 'x' in order for the theory to be true; on this score formulation 1 is successful. However, Richard Nixon = 37th President of the United States, and if Richard Nixon has to be included amongst the values of 'x' for '(∃x)x = Richard Nixon' to be true, so has the 37th President of the United States. But '(∃x)x = Richard Nixon' is not ontologically committed to the existence of the 37th President of the United States. (See Gottlieb [54]). Formulation 1 fails to acknowledge the antireferential and intensional nature of ontological commitment.

Formulation 2

'In general, entities of a given sort are assumed by a theory if and only if some of them must be counted amongst the values of the variables in order that the statements affirmed in the theory be true'.

Quine [108] (103)

This criterion represents something of an improvement over the previous characterisation. It handles '$(\exists x)$table x' rather better; for although it is true that no particular table has to be counted amongst the values of 'x' in order for the theory to be true nevertheless <u>some</u> table must be counted in for '$(\exists x)$table x' to be true. However Cartwright [25] has isolated some mistakes in this formulation too.

If formulation 2 is formalised in first-order notation then the result is something as follows; where 'T' and 'D' range over theories and domains respectively and 'x' over the universe set.

$$(T)\ T \text{ is ontologically committed to Ks} \equiv (D)\ (D \text{ satisfies } T \supset (\exists x)Kx \ \&\ x \in D$$

Cartwright points out that if no D satisfies T (as in 'Unicorns exist'), then T will be committed to any sort K. The right-hand side of the above equivalence can be altered to avoid this difficulty, thus:-

$$(T)\ T \text{ is ontologically committed to Ks} \equiv (\exists D)\ D \text{ satisfies } T \ \&\ (D')(D' \text{ satisfies } T \supset (\exists x)\ Kx \ \&\ x \in D')$$

But problems still arise. In the above case, since $-(\exists D)D$ satisfies 'Unicorns exist', 'Unicorns exist' will have no ontological commitments at all.

The only course would seem to be to abandon first order notation and interpret Quine's 'must be' in formulation 2 as requiring the prescence of the modal operator 'O' or something like it. Such an interpretation would be contrary to Quine's avowed preference for first-order notation and his abhorrence for modal logic. Cartwright reaches similar conclusions, and his version of a criterion of ontological commitment will be examined in the next section.

Formulation 3

'To show that a given theory assumes a given object or objects of a given class, we have to show that the theory would be false if that object did not exist, or if that class were empty; hence that the theory requires that object, or members of that class, in order for it to be true.'

Quine [109] (93)

This formulation makes essential use of the counterfactual conditional, an idiom which remains stubbornly beyond the scope of first-order notation and which has resisted philosophical analysis (see Campbell [17], Goodman [52]). The material conditional is obviously too weak to do the job here, since:-

(T) T is ontologically $\equiv$ (T is true $\supset (\exists x)\,Kx$)
committed to Ks

entails every false theory is committed to everything and any theory is committed to anything that exists.

But even accepting Quine's use of the counterfactual conditional, the 'criterion' stated above is at best a necessary rather than a sufficient condition of ontological commitment. For example, it is reasonable to believe that the following sentence is true.

Had Leonardo da Vinci never existed then it would have been the case that the painting of the Mona Lisa does not exist.

This sentence is equivalent to:-

Had Leonardo da Vinci never existed then it would have been that 'the painting of the Mona Lisa exists' is false.

By formulation 3 it follows that 'The painting of the Mona Lisa exists' is ontologically committed to Leonardo da Vinci. But this is false. We have to distinguish between a theory requiring the existence of an entity because, as a matter of contingent fact, had that entity not existed the theory would not have been true; and the truth of that theory necessitating the existence of an entity. It is this latter concept that formulation 4 fails to capture.

Formulation 5

'We commit ourselves to an ontology containing numbers when we say that there are prime numbers larger than a million; we commit ourselves to an ontology containing centaurs when we say that there are centaurs; and we commit ourselves to an ontology containing Pegasus when we say Pegasus is.'

Quine [116] (8)

'We can very easily involve ourselves in ontological commitments by saying, for example, that there is something (bound variable) which red houses and sunsets have in common; or that there is something which is a prime number larger than a million. But this is, essentially, the only way we can involve ourselves in ontological commitments.'

Quine [116] (12)

Chihara [27] accurately remarks that explicitly stating that a sort K exists is a sufficient condition of ontological commitment but not a necessary one. To state 'numbers exist' is, amongst other things, to commit oneself to abstract objects even if one has not explicitly stated there are such objects.

Recognition of this point seems to demand some recognition of the role of analytic truths and analytic implication: concepts Quine [108] would choose not to employ, (but possibly not Quine [120]).

Apart from this point, it is doubtful if formulation 5 is a criterion of ontological commitment. It is surely evident that we commit ourselves to centaurs if we say there are such: so evident, in fact, that it is hard to see how a statement of this fact could improve our abilities to uncover ontological commitments.

Formulation 6

'If a theory implies '$(\exists x)(x \text{ is a dog})$' it will not tolerate an empty universe; still the theory might be fulfilled by a universe that contained collies to the exclusion of spaniels and also vice-versa. So there is more to be said of a theory, ontologically, than just saying what objects, if any, the theory requires; we can also ask what various universes would be severally sufficient. The specific objects required; if any, are the objects common to all those universes.'

Quine [112] (96)

The significance of this passage turns upon the interpretation given to 'universe'.

Under one interpretation, 'universe' means set, domain or universe of discourse. Formalised, the criterion is:-

$$(T)(T \text{ is ontologically committed to Ks} \equiv \{x : Kx\} \subseteq \bigcap \{D : D \text{ satisfies } T\})$$

But consider '$(\exists x)$ dog x'; this theory is satisfied by D when D = $\{x$: collie dog $x\}$ and when D = $\{x$: spaniel $x\}$. But since $\{x$: collie dog $x\} \cap \{x$: spaniel $x\} = \Lambda$, then on this interpretation, '$(\exists x)$ dog x' has no ontological commitments. Under the other interpretation, 'universe' means possible world. But Quine has explicitly rejected the use of possible worlds and possible entities (I think rightly) for the reasons that their identity criteria are in doubt, and they are even more obscure than the modal idioms they are supposed to elucidate; (see Quine [116] and Quine [119] (245)).

1.32 Some Other Suggestions

Some of the philosophers who have noted the formal deficiencies of Quine's criteria have suggested their own remedies. Alonzo Church [29] offers the formulation:-

the assertion of $(\exists x)\mathfrak{M}\mathfrak{x}$ carries ontological commitment to entities x such that M;

where the letter 'x' may be replaced by any variable, 'M' by any open sentence containing only that variable, '$\mathfrak{x}$' by the name of the variable replacing 'x' and '$\mathfrak{M}$' by the name of the variable replacing 'M'. Chihara [27] remarks that this is only a sufficient condition of ontological commitment, since the theory '$(\exists x)$ number x' is committed to entities such that number x all right, but it is also committed to abstract objects too. In general, we have to look beyond the overtly existential pronouncements of a theory to see all of its commitments. Gottlieb's [57] criterion suffers from some of the same problems. Gottlieb's criterion is:-

'T is ontologically committed to a/Fs iff T logically implies '$(\exists x)(x = a)$'/'$(\exists x)Fx$' and '$(\exists x)$' is understood objectually.'

But logical implication, as it is generally understood, does not hold betwen '$(\exists x)$ number x' and '$(\exists x)$ abstract object x'. So again Gottlieb's criterion is only sufficient. Scheffler and Chomsky [130] suggest:

'A theory T makes a ____________ assumption if and only if it yields a statement of the form '$(\exists x)x$ is a ____________';

where '____________' can be filled by any general expression. This criterion is unsatisfactory mainly because of the vagueness of 'yields' (strictly implies, logically implies, entails?).

Cartwright is most successful; he offers:-

'An elementary theory, T, presupposes objects of a kind K if and only if there is in T an open sentence $\emptyset$ having α as its sole free variable such that (i) $(\exists\alpha)\emptyset$ is a theorem of T; (ii) it follows from the semantical rules of T that for every x, $\emptyset$ is true of x only if x is a member of K.

Cartwright [24]

Quine would baulk at the concept of 'semantic rules' (see Quine [105] for his criticisms of Carnap's use of the same concept). Aside from this, Cartwright's version seems both true and necessary and sufficient. But is it epistemically useful enough to be a good criterion of ontological commitment? This is open to debate. In a sense all that Cartwright tells us is that a first-order theory is ontologically committed to Ks iff $(\exists x)Kx$ can be deduced from that theory. Surely this was clear all along and that all that Cartwright has achieved is a measure of detail in respect of "deduce"? There is some justice in this objection, but the fact is that any criterion of ontological commitment for first-order theories will be truistic. This arises because first-order theories are ontologically perspicuous anyway (which is why formalists in ontology have advocated formalisation). We should not have to be told that '$(\exists x)$ dog x' shows an ontological commitment to dogs; our knowledge of the symbolism told us this much. So it is not clear that Cartwright's criterion will be epistemically useful to anybody whose grasp of logic is sufficient to enable him to understand it.

Is Cartwright's formulation epistemically useless after all? Where this is so or not depends not on the criterion itself, but on the procedure that prefaces its application: that is, formalisation. If Cartwright's version is supplemented by a good account of how to phrase natural language theories in the formal idiom then it will be epistemically useful, otherwise it will not. This means that the focus of attention slips away from the nature of ontological commitment, onto the nature of formalisation.

1.4 Formalisation: an initial survey

At a very abstract level, formalisation is a procedure which establishes a function from some set of natural language theories onto some set of formal theories. Obviously not just any function will do, the function must satisfy certain constraints. What are those constraints?

In his admirably lucid section on metaontology, Campbell [19] confronts this question. Commenting on the relations between a natural language theory and a canonical (formal) one, Campbell remarks:-

'The two must be reality equivalents..... That is the very claim about the world legitimately encapsulated in the natural sentence must be reproduced in the canonical one. The notion of reality equivalence is an intuitive one which resists systematic treatment. Yet so far as I can see, we cannot do without it.'

Campbell [19] (160)

What Campbell means by 'reality equivalents' is, as he admits here, obscure. One natural way of interpreting Campbell's notion of reality equivalence is that if two theories are reality equivalents then at least they must share the same ontological commitments. If theory A claims the existence of composite numbers and theory B does not, then surely A and B cannot be reality equivalents. So we might require that formalisation preserve ontological commitment; and this ushers in a very serious problem: the Paradox of Formalisation.

Grant that preservation of ontological commitment is a necessary condition of formalisation; then if we formalise a natural language theory A by a formal language theory B, we shall only be justified in accepting this formalisation if it is authoratatively established that A and B have the same ontological commitments. The ontological commitments of B ought to be clear, for it is the clarity of formal notation that is the raison d'etre of formalisation. But what of A? If the ontological commitments of A are obscure to us, we will not know if A has the same ontological commitments as B or not. Ergo, it will not be possible to determine if the formalisation is good or not. On the other hand, if the ontological commitments of B are clear, why bother formalising B anyhow? The Paradox of Formalisation concludes this reasoning by saying that formalisation either cannot be successfully practised (where we cannot determine the ontological commitments of the natural language theory, how can we tell if those commitments have been preserved?) or it is totally redundant where it can be practised (for if we already know the ontological commitments of the natural language theory, when we already have all that formalisation can give us).

The Paradox of Formalisation can either be treated as a knock-down refutation of the formal tradition in ontology, or as a mistake arising from false presuppositions about how formalisation works. Which turns out to be the case will only be determined by looking at formalisation in depth. This will be the concern of the next two chapters.

[1]In adopting this convention I assume that a declarative sentence is, at least in some cases, true or false. There is a sizeable and rather unrewarding literature on the nature of truth-bearers (see Strawson [139], White [146] and Frege [48] for examples). Certain authors prefer not to identify truth-bearers with declarative sentences, but choose propositions, statements, or thoughts instead. Much of the resulting argument has been fairly futile, and to my mind, at least some these supposedly rival identifications could be made to serve the same useful services. The advantages of choosing declarative sentences as truth-bearers are those of ontological economy and clarity of identity conditions. Related material to this convention is scattered throughout the text. See 6.3 for an examination of the concept of a declarative sentence and 5.2 for an aside on the truth-bearer argument.

[2]For a good history of the use of 'intentional' and the vagueness attached to it, see Chisholm [27]. Chisholm's article persuaded me to abandon the word altogether.

[3]See Gottlieb [54] for an exposition and criticism of Jubien. Section 4.4 and appendix II of this work contains material on the importance of identity criteria in ontology.

[4]See Scheffler and Chomsky [130], Parsons [95], and Jubien [70] for criticisms of similar strategies to this suggestion.

[5]See Church [29] for a similar criticism of Carnap [21]

CHAPTER TWO

Ontology in the Formal Tradition

2.1 The tradition itself

The formal tradition in modern philosophy originated from the work of Gottlob Frege, and emphasises the importance of the formal logic Frege invented, in the investigation of many philosophical problems. Frege's pioneering research into formal logic arose from two goals that dominated his lifetime's work. One goal was to devise a notation sufficient to express all mathematical reasoning, within which it could be effectively established whether any given sequence of formulae constituted a proof. The second was to lay down an axiom set from which, using only stipulative definitions and the rules of deduction laid down for his notation, all truths of pure mathematics could be derived.[1] Frege's own attempt to provide such an axiomatisation in Grundgesetze der Arithmetik failed at the pres ence of Russell's Paradox (see Hatcher [65]). The long term prospects of realising the axiomatisation of mathematics were destroyed, six years after Frege died, by Godel's Incompleteness theorem of 1931. But the logical system that Frege created lived on to exert a very great influence on twentieth century philosophy.

Frege was principally a mathematician who went into philosophy only insofar as it helped him to understand mathematics. It was Russell, operating as a sort of intellectual Prometheus between mathematics and philosophy, who took Frege's new logic, improved its notation and suggested its philosophical application. So Russell opened his Lowell Lectures in 1914, on the eve of a war that was to give birth to the twentieth century, with the promise of a new philosophy that, in scientific rigour and boldness, promised to match that century itself.

'The following lectures are an attempt to show, by means of examples, the nature, capacity, and limitations of the logical-analytic method in philosophy. This method, of which the first complete example is to be found in the writings of Frege, has gradually, in the course of actual research, increasingly forced itself upon me as something perfectly definite, capable of embodiment in maxims, and adequate, in all branches of philosophy, to yield whatever objective scientific knowledge it is possible to attain.'

Russell [123] (7)

Russell was not always as clear about the logical-analytic method in philosophy as he implies here. But Russell was the intellectual father of much of the formal tradition in philosophy, and particularly in ontology, Russell marks the best starting point for research.

In this chapter, five major figures of the formal tradition in philosophy are examined: Russell, Carnap, Goodman, Quine and Davidson. There is a common conviction to be found in all these philosophers as to the value of formalisation. Each of them is convinced of the value of formal logic to philosophy, and in particular, of its ability to clarify ontological problems. Three questions are, worth bearing in mind during the subsequent examination of the thought of these five men; they are as follows

(1) Why formalise?

(2) What makes a good formalisation?

(3) How does formalisation help determine an answer to the Ontological Question, 'What exists?'?

Answers to these three questions reveal profound differences between our five authorities. We cannot do better than begin with the first of them.

2.2 Russell and Logical Atomism

The philosophy of logical atomism is founded on the representationalist theory of meaning. This theory is that the words of a language depend for their meaning on things for which they stand. These things are the meanings of the words in question. Simple as this theory seems, much of the development of logical atomism is a record of an attempt to formulate representationalism in a way that would save it from criticism; the Tractatus Logico - Philosophicus being the apogee of sophistication in that development.
In 1902, while writing the Principles of Mathematics, Russell accepted representationalism as the archtypal form for any theory of meaning.

'Words all have meaning, in the sense that they are symbols which stand for something other than themselves.'

Russell [121] (51)

The entities which words stood for Russell called 'terms'. At the time of writing, Russell took a term to be any object of thought, existing or not.

'A man, a moment, a number, a class, a relation, a chimaera, or anything else that can be mentioned is sure to be a term: and to deny that such and such a thing is a term must always be false.'

Russell [121] (51)

Russell's theory of terms enabled an explanation of how nondenoting descriptions, like his later - famous 'the present King of France', could be meaningful even when there was no denotatum. The explanation was that the meaning of such a description was a subsistent, though nonexistent term, the present King of France.

In 1905, Russell abandoned this position with the vigour of a reformed alcoholic: there were no such things as nonexistent terms subsisting only for the sake of being married off to nondenoting descriptions. In his most influential article, 'On Denoting', Russell clarified the change in his views.

'The evidence for the above theory is derived from the difficulties which seem unavoidable if we regard denoting phrases as standing for genuine constituents of the propositions in whose verbal expressions they occur. Of the possible theories which admit such constituents the simplest is that of Meinong. This theory regards any grammatically correct denoting phrase as standing for an object. Thus 'the present King of France', 'the round square', etc., are supposed to be genuine objects ...... the chief objection is that such objects, admittedly, are apt to infringe the law of contradiction. It is contended, for example, that the existent present King of France exists, and also does not exist; that the round square is round, and also not round etc. But this is intolerable; and if any theory can be found to avoid this result, it is surely to be preferred.'

Russell [122]

Russell's solution, his Theory of Definite Descriptions, was essentially a modification of his earlier representationalism. His position emerges more clearly in The Philosophy of Logical Atomism nearly 15 years after 'On Denoting'.

Russell had, by 1920, come to hold that for two classes of symbols representationalism did not apply, that is, symbols in these classes could have meaning and yet stand for nothing. In the first class were the logical connectives, 'not', 'or', 'and' and so forth, whose meaning, Russell believed, was entirely explained by the contribution they made to the truth-conditions of the molecular sentences they helped to form. In the second class were incomplete symbols, of which 'the present King of France' was one. Incomplete symbols, Russell believed, were essentially abbreviations or shorthands for collections of symbols in which incomplete symbols did not appear. Thus 'The present King of France' appears as an incomplete symbol in 'The present King of France exists' because there is a procedure for eliminating 'the present King of France', in the context of a sentence, in favour of symbols whose meaning was what they stood for. When this eliminative procedure is carried out, what emerges is this:-

'The propositional function 'x is the present King of France' is (i) true for at least one value of 'x' (ii) true for at most one value of 'x.'

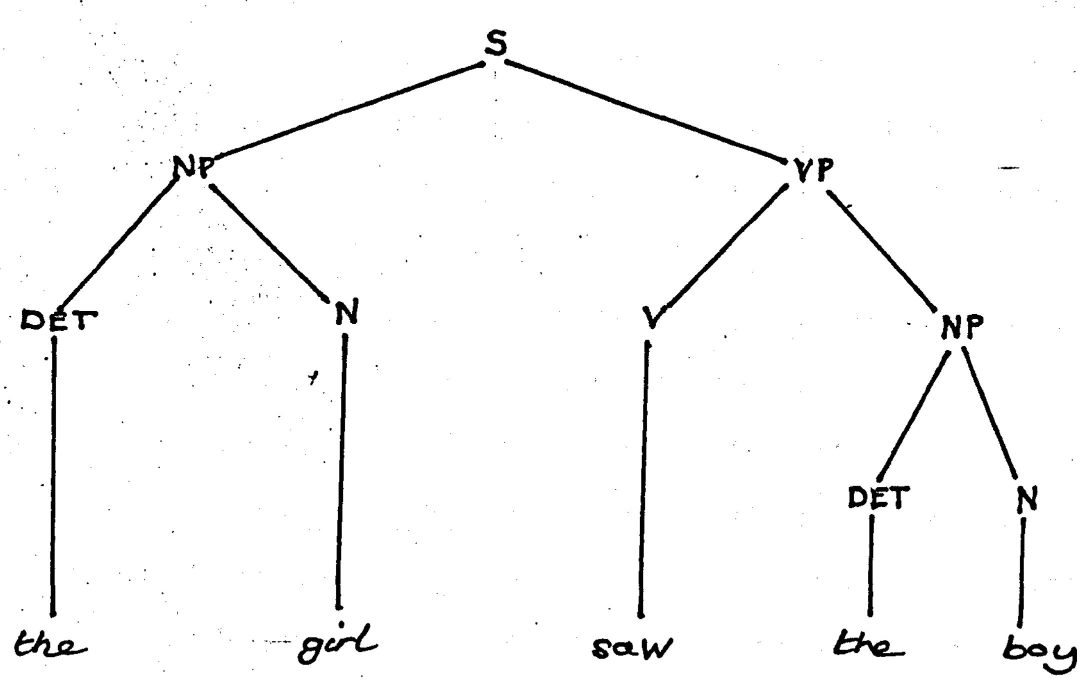
Russell [125]

There is no suggestion in the above quotation that the sentence is crediting to a subsistent present King of France, the property of existing. Existence claims turn into assertions about the properties of propositional functions. Russell drew a number of significant conclusions from his method of dealing with definite descriptions. For our purposes, his four most important conclusions relate to the distinction between grammatical and logical form, the importance of analysis, and the need to recognise logical fictions.

The phrases 'grammatical form' and 'logical form' appear quite seldom in Russell's work considering the effect that the distinction has had and the current popularity of the latter phrase in Oxonian circles. The distinction between grammatical and logical form is really the old philosophical distinction between appearance and reality, here transferred onto language.

The sentences 'I am bald' and 'The present King of France is bald' are grammatically very similar, both begin with a definite singular term, continue with a copula, and end in the same predicate. Both sentences have a very similar grammatical form. But according to Russell's ideas these two sentences work in very different ways. The personal pronoun in the first sentence is a logical name: it serves to pick out a particular, the speaker, and that particular is the meaning of the word. By contrast 'the present King of France' picks out nothing: it is an incomplete symbol which is a shorthand for a string of symbols which are not incomplete. The sentences 'I am bald' and 'The present King of France is bald' have similar grammatical forms but different logical forms.

Russell never gave a precise definition of 'grammatical form' and I think one comes closest to a satisfactory definition through the resources of generative grammar; a subject that was not developed until nearly 40 years after Russell's lectures of 1918. (See Grinder and Elgin [52] or Lyons [86] for an introductory exposition of generative grammar). Generative grammar is concerned with the determination of the formation rules for the sentences of various natural languages. Integral to generative grammar is the concept of constituent structure, in which the grammatical structure of a sentence, (in terms of its composition into words, phrases and clauses) is represented. Constituent structure is often represented by means of a tree -structure or phrase - marker like this one:-



Represented as a derivation from a phrase structure grammar, the above phrase-marker would appear like this:-

1 S

2 NP VP

3 DET N VP

4 DET N V NP

5 DET N V DET N

6 The N V DET N

7 The girl V DET N

8 The girl saw DET N

9 The girl saw the N

10 The girl saw the boy

Call one of the entries 1 to 10, a constituent level of the sentence 'The girl saw the boy'; and the number tagging a constituent level a constituent index of that level. Two sentences have the same grammatical form to the degree that their constituent levels are identical down to a given constituent index i. The higher the value of i in relation to the highest (or deepest) constituent level attainable, the more similar the grammatical forms of the two sentences. This definition of 'grammatical form' makes the phrase scalar in application rather than non-quantative, but that, I believe, is how the facts stand anyway. Although 'I am bald' and 'The present King of France is bald' have similar grammatical forms they have different logical form. (A better example is Quine's 'I did it for my wife's brother' and 'I did it for my wife's sake'. both sentences are very similar in grammatical form, but different in logical form. There is not an entity my wife's sake to be ranked along with her brother). Russell believed that in a perfect language such misleading locutions as 'The present King of France' and 'my wife's sake' would not appear. In a perfect language, logical form and grammatical form would coincide, and the way that the sentence appeared on paper would reflect the state of affairs that it was about. Russell identified the perfect language as a first-order language.

'In a logically perfect language, there will be one word and no more for every simple object, and everything that is not simple will be expressed by a combination of words, by a combination derived, of course, from the words for the simple things that enter in, one word for each simple component. A language of that sort will be completely analytic, and will show at a glance the logical structure of the facts asserted or denied. The language which is set forth in Principia Mathematica is intended to be a language of that sort.'

Russell [125]

Many traditional philosophical problems arose, Russell argued, because philosophers took the grammatical form of a sentence as a good guide to its logical form. So Russell circa 1900 had been misled by grammar into thinking that the phrase 'The present King of France' works as a name, picking out an object, because, like a logically proper name, it appears in the subject position. This misconception had generated a baroque ontology of terms. In a logically perfect language, in which every sentence appears in its true colours, mistakes like these would not be made.

'I think the importance of .... grammar is very much greater than it is generally thought to be. I think that practically all traditional metaphysics is filled with mistakes due to bad grammar, and that almost all the traditional problems of metaphysics and the traditional results - supposed results - of metaphysics are due to a failure to make the kind of distinctions in what we may call philosophical grammar [logical form] with which we have been concerned.'

Russell [125] (269)

Even over 40 years later, in 1959 Russell's opinions on this matter had not changed. In reply to Strawson's ordinary-language 'On Referring', Russell wrote:-

'I..... am persuaded that common speech is full of vagueness and inaccuracy, and that any attempt to be precise requires modification of common speech as regards vocabulary and as regards syntax...........In philosophy, it is syntax, even more than vocabulary, that needs to be corrected.'

Russell [128] (241 - 242)

The cure for the traditional mistakes of traditional philosophy lay through analysis and the exposure of logical fictions.

The present King of France is a logical fiction. Natural language makes it appear that the present King of France is in some fashion; whereas in a perfect language the phrase 'the present King of France' would disappear in the manner suggested by Russell's Theory of Definite Descriptions. To say that X was a logical fiction meant for Russell that when talk of X was analysed into the symbolism of a perfect language, no mention of X would appear. As Russell progressed in his philosophical career, he uncovered a surprisingly diverse number of logical fictions apart from the present King of France; numbers, classes, and even material objects and egos were to become logical fictions. Numbers were logical fictions because number-talk could be analysed into class-talk, and classes were logical fictions because talk of classes could be analysed in terms of propositional functions.

The use of the word 'analysis' in this context raises fairly obvious parallels with the physical sciences; particularly chemistry. In chemical analysis, the chemist begins with a chemical whose formula and molecular structure are unknown and he obtains knowledge of them by analysis: his goal is simply to describe accurately what he has got in his sample. The imputation of the phrase 'logical analysis' is that the philosopher engaged in logical analysis is only concerned with making clear what is already contained in an assertion, and is not concerned in adding or subtracting from what is there, or judging whether that assertion is true. Wittgenstein interpreted analysis in this way and summed up the doctrine in the Tractatus Logico - Philosophicus.

'Philosophy aims at the logical clarification of thoughts.
Philosophy is not a body of doctrine but an activity.
A philosophical work consists essentially of elucidations.
Philosophy does not result in 'philosophical propositions', but rather in the clarification of propositions.
Without philosophy thoughts are, as it were, cloudy and indistinct: its task is to make them clear and give them sharp boundaries.'

Wittgenstein [144] (4 - 112)

On this view, analysis was concerned with the explication of the meanings of signs. Since this activity required not special knowledge bar the familiarity with language that an old fashioned liberal education affords, analysis was heartily endorsed by many Oxbridge philosophers. By taking analysis to be the concern of philosophy, the philosophical establishment at Oxford were able to find refuge for themselves in a century in which, as Russell correctly predicted, those innocent of science would be pushed to the wall. In due course the activity of analysis was to be watered down even further in the shape of ordinary language philosophy.
Gilbert Ryle's 'Systematically Misleading Expressions', written in 1931 is a classical statement of analysis viewed through the foggy and grey-tinted lenses of the classically grounded philosopher interpreting analysis according to his own lights. Ryle's article will be used to illustrate Ryle's approach to analysis, what he thought it could achieve for philosophy, and also why it does not work.

Ryle opens up his article with an observation that Russell made; namely, that there are sentences in common use whose grammar is apt to be seriously misleading to philosophers. Lewis Carroll provides a delightful example in Alice through the Looking-Glass.

'I see nobody on the road' said Alice.
'I only wish *I* had such eyes' the King remarked in a fretful tone. 'To be able to see Nobody! And at that distance too! Why, it's as much as *I* can do to see real people, by this light!'

Lewis Carroll [23]

Needless to say, the White King misunderstands the use of 'nobody', which unlike, say 'Robinson' is not used to designate an entity. We might explain this to the King by saying that when Alice said that she saw nobody on the road, she meant that it was not the case that there was somebody she saw on the road. In Ryle's opinion, as in Russell's, mistakes occur in philosophy because of philosophers being confused like the White King about the use of words which are philosophically misleading. The cure (and the activity) of philosophy consists in replacing sentences which mislead by sentences which do not. So 'Carniverous cows do not exist' means what is meant by the less misleading 'No cows are carniverous' and does not refer to a group of subsistent carniverous cows. 'Unpunctuality is reprehensible' is less misleadingly recast as 'Whoever is unpunctual merits reproof.'

The preceding two examples are Ryle's. It is when Ryle turns the focus of inquiry on more interesting and also more problematic examples that his method unravels. For example, Ryle considers the sentence 'The idea of taking a holiday has just occured to me' as less misleadingly recast as 'I have just been thinking that I might take a holiday'. Since Ryle adopts a behaviourist stance and rejects the existence of mental entities, it is easy to see why Ryle prefers the latter sentence. But it is also true to say that Ryle is right in saying that the second sentence is less misleading than the first <u>only if he is right in supposing that there are no mental entities, especially ideas!</u> This means, in effect, that in the most interesting and strongly contested ontological questions do not admit of resolution by Ryle's technique. Ryle, ontologically speaking, stocks the deck from the outset by condemning as systematically misleading all expressions which impute the existence of entities outside of his ontology. Methodologically, as far as ontology goes, Ryle's version of analysis is circular.[2]

Russell, in contrast to Ryle and his Oxford contemporaries, was more influenced by mathematics and the sciences than by the supposed forms of correct speech. Thus Russell's version of analysis is considerably less gentlemanly and respectful to ordinary use, than that developed by Ryle and others; it is also much more interesting. To distinguish Russell's version of analysis from Ryle's, I shall refer to Russell's version as 'revisionary analysis', of which these were the main features:-

(1) The purpose of revisionary analysis is to minimise our ontological commitments and to substitute logical fictions for assumed entities.

(2) The only entities which should be assumed are those entities which are given to us in experience.

(3) Revisionary analysis (unlike descriptive analysis) does not attempt to preserve meaning, but only structure.

I shall explain these points in order.

Russell was the end of a line of great philosophers, beginning with Descartes, who saw epistemology as the most important area of philosophy. The question of the scope of human knowledge occupied Russell throughout his career. Like Descartes, Russell believed that both scientists and ordinary people were prone to accept a great many assertions that could not be defended in philosophy. In many cases, what made these assertions philosophically doubtful was that they assumed the existence of entities whose existence was rationally undemonstrable. These entities Russell called 'metaphysical'. Part of the job of the philosopher was to reframe those assertions in such a way as to give them the best chance of being true: and this meant getting rid of metaphysical entities from the common ontology. The end result of the philosophers' labour would be the elimination of a large number of different metaphysical entities in favour of a small number of assumed entities: entities of what may be called the minimum domain, (the smallest domain of entities necessary to support our assertions). This is how Russell interpreted Ockham's Razor.

'Suppose, e.g., that you have constructed your physics with a certain number of entities and a certain number of premises, suppose you discover that by a little ingenuity you can dispense with half of those entities and half of those premises, you have clearly diminished the risk of error, because if you had before 10 entities and 10 premises, then the 5 you have now would be all right, but it is not true conversely that if the 5 you have now are all right, the 10 must have been. Therefore you diminish the risk of error with every diminution of entities and premises.'

Russell [125]

Revisionary analysis was the procedure whereby metaphysical entities were eliminated from our ontology. The technique of revisionary analysis was to substitute logical fictions for metaphysical entities, or as Russell puts it:-

'Wherever possible, substitute logical constructions for inferred entities ...'

Russell [125] (146 - 147)

What this meant was that assertions that reported on the existence of metaphysical entities were reinterpreted so as to refer to logical fictions. As you remember, to say X was a logical fiction was to say that in a perfect language (like that in PM) all statements about X could be analysed into a symbolism in which no mention of X appeared. The entities which were mentioned in the perfect language when analysis had reached an end, wold be the elements of the minimum domain. So the program of revisionary analysis had two parts (i) a substitution of logical fictions for metaphysical entities as the objects of reference for ordinary language sentences (ii) the elimination of logical fictions in favour of entities of the minimum domain.

By 1920 Russell believed that the entities of the minimum domain should be sense-data. In this respect Russell was very much the British empiricist, for he held that the elements of experience were sense-data and experience was the only basis for knowledge in respect to what exists. Russell was by no means always constant as to what the entities of the minimum domain were. For example in The Problems of Philosophy (1914), universals and selves were assumed entities. Roughly speaking, as Russell got older, his budget of minimum entities became smaller, (see Quine [115] for a statement of Russell's ontological development)

Russell believed that revisionary analysis would often carry one away from the meaning of the analysandum. A case in point is Whitehead's analysis of 'point' and 'straight line' in The Concept of Nature (an analysis Russell admired and often referred to). Whitehead analyses talk of points and instants of time in terms of abstractive sets of regions of space-time. There is no reason to suppose, and it would be implausible to suggest, that Whitehead was engaged in an analysis of the meaning or ordinary usage of 'point' or 'straight line'. In 'The Philosophy of Logical Atomism', Russell confesses that meaning may not be all that pertinent to the procedure of revisionary analysis.

'I think that the notion of meaning is always more or less psychological, and that it is not possible to get a pure logical theory of meaning, nor therefore of symbolism.' Russell [125] (40)

What restrictions do bind revisionary analysis if preservation of meaning is not one of them? Russell answers many pages later.

'........I think that any valid kind of interpretation ought to leave the detail unchanged, though it may give a new meaning to fundamental ideas. In practice, this means that structure must be preserved. And a test of this is that all the propositions of science should remain, although new meanings may be found for their terms.' Russell [125] (161)

What Russell meant by 'structure' emerges by example from his earlier writings. In order to interpret sentences within the process of revisionary analysis, but also at the same time, to preserve their structure, one had to find some way of assigning extensions to the elements of those sentences which would preserve the truth of those deemed true, and the falsehood of those deemed false.[3]

A good example is the Frege - Russell definition of cardinal number. In Frege and in Russell, a cardinal number is an equivalence class of similar classes. The cardinal number series, 0_C, 1_C, 2_C, 3_C..............is defined as follows.

$0_C = \{x: x \approx \Lambda\}$

$1_C = \{x: x \approx \{0_C\}\}$

$2_C = \{x: x \approx \{0_C, 1_C\}\}$

$3_C = \{x: x \approx \{0_C, 1_C, 2_C\}\}$

....

Cardinal addition [+] is defined thus; where m and n are cardinal numbers.

$$m\,[+]\,n = \hat{x}: (\exists y)(\exists z)(y \in m \;\&\; z \in n \;\&\; y \cap z = \Lambda \;\&\; x \approx y \cup z)$$

From the definition, together with other definitions and axioms, [+] can be proved to be commutative and associative. Indeed if we assign [+] as the extension of '+' and fix EX('0'), ... so that EX('0') = 0_C, EX('1') = 1_C,.....then we can recover many of the basic truths of arithmetic, including Peano's axioms. The extensions attached to the elements of the language of arithmetic may not be familiar. For Russell this did not matter. The important thing was that in interpreting arithmetic in this manner it was possible to preserve what was valuable in arithmetic (what mathematicians accepted as true) and, at the same time, to disperse with an ontology of unreduced natural numbers. It was never important to preserve the meanings attendant on arithmetical sentences. Whatwas important was that, syntactically, the same sentences should be counted in as true, after revisionary analysis, as had been counted in before. Any meaning-shift that transpired during this process was irrelevant. The philosophical heritage that Russell bequeathed to twentieth century philosophy was a rich one. He was, as William James wrote of Charles Sanders Peirce, 'a goldmine of ideas for the coming generation'. All of the succeeding figures in this chapter owe a lot to Russell; but not all of his ideas were of equal value. His belief that every sentence had a unique logical form was a weak element. Indeed, Russell in his Introduction to the Philosophy of Mathematics, was already aware of the existence of competing, but equally satisfactory, set-theoretical formalisations of arithmetic. His decision to base his ontology on sense-data was also mistaken in retrospect (see 4.4). Butfor the realisation of the potential of symbolic logic to philosophy, he was ahead of the mass of his generation. In seeing that it was structure and not meaning that was important in formalisation; he was not only ahead of his own time, but writing over sixty years ago, still ahead of philosophical logic at Oxford today.

2.3 Carnap

An early work in the formal tradition was Carnap's Der Logische Aufbau der Welt, published in 1928, and translated and published in English as The Logical Structure of the World in 1967. Carnap's work is philosophically notable because it was the first sustained attempt to carry out the Russellian project of exhibiting a system by which all statements of fact could be analysed into reports about sense-data. That this project is fundamentally misguided does not detract from the value of the methodological remarks that Carnap has to make in that book. The fact that Carnap conveniently separates out his metaontological remarks in the first half of the book (p1 - 105), from his exposition of his sense-datum ontology in the second, only makes the task of assessment much easier. Consequently in what I have to say I will concentrate almost entirely on Carnap's metaontology.

Carnap pursued ontology through the creation of a constructional system. In order to understand Carnap's metaontology, the elements of a constructional system have to be grasped.

One of these elements is that a constructional system contains an array of constructional definitions written in a constructional language. It is clear from Carnap's remarks that he envisaged the constructional language as modelled closely on the sort of language one finds in Principia Mathematica i.e. a first-order language. Constructional definitions were essentially rules which allowed certain symbols to be defined in terms of others and were formulated in a way appropriate to the symbols concerned. Carnap offers as an example the constructional definition "'x is a prime number' is coextensive with 'x is a natural number whose only divisors are 1 and x itself" (alternatively '(x) prime number $x \equiv (y)$ divisor of $(y,x) \supset (x = y \vee y = 1)$)'. The statement can be thought of as a rule enabling us to eliminate all occurrences of the propositional function 'x is a prime number' in favour of 'divisor of', 'natural number' and '1'.

The purpose of this system of constructional definitions was to exhibit a method whereby the elements of the constructional could be reduced to a few fundamental expressions which Carnap called <u>basic concepts</u> or <u>undefined concepts</u>. Any defined concept could be eliminated in context by recourse to the constructional definitions. The purpose of this process was to show that the only objects with which the constructional language was concerned were the objects which fell under the extension of the basic concepts. Carnap called these objects, '<u>basic objects</u>' (= Russell's <u>simples</u>, the elements of his minimum domain). The objects which were the apparent subjects of the <u>defined concepts</u>, Carnap called <u>logical complexes</u> (= Russell's <u>logical fictions</u> or <u>logical constructions</u>). A constructional system can be thought of as a logical machine for demonstating the <u>reducibility</u> of a manifold of apparent objects, the logical complexes, to a few, the basic objects.

The second element of a constructional system is that it is also a means for achieving a <u>unified science</u>. The idea was that in a constructional system, axioms written in the constructional language could be formulated, from which the important truths of various branches of the sciences could be deduced. Carnap envisaged a constructional system as an axiomatised theory, in the manner of <u>Principia Mathematics</u>, from which the laws of science would be theorems.

'A theory is axiomatised when all statements of the theory are arranged in the form of a deductive system whose basis is formed by the fundamental concepts. So far, much more attention has been paid to the first task, namely, the deduction of statements from axioms, that to the methodology of the systematic construction of concepts. The latter is to be our present concern and is to be applied to the conceptual system of unified science. Only if we succeed in producing such a unified system of all concepts will it be possible to overcome the separation of unified science into unrelated special sciences.'

Carnap [18]

Carnap thought that, in order for a constructional system to be satisfactory, the constructional definitions had to satisfy a requirement: this requirement I call (since Carnap gives it no name) extensional adequacy. What is extensional adequacy?

To this question, Carnap gave two answers which he thought were equivalent, but in fact which are not. I shall tag them 'Answer A' and 'Answer B'.

Answer A

Carnap's first answer was that in order for a constructional definition to be extensionally adequate it had to relate definiens and definiendum which were extensionally coextensive. The point of this requirement was that such a definition would, in the context of a first-order language, allow from the systematic replacement *salva veritate* of the definiendum, wherever it occurred, by the definiens. In this way constructional definitions would allow statements which dealt with logical complexes to be broken down so as to preserve truth-value, into basic statements that dealt with basic objects.

Goodman [53] has offered a number of criticisms of Carnap's answer (A). I shall not repeat them here. By far the most telling of all criticisms of extensional adequacy, as a criterion of the correctness of any constructional definition, comes from the reduction of arithmetic to set theory.

In the section on Russell, we saw one set-theoretical interpretation of natural numbers in terms of equivalence sets of similar sets. It is also quite feasible to use ordinal instead of cardinal numbers to do the same job. Ordinal numbers are definable thus[4]: let ON be the property of being an ordinal number; then ON is defined:-

$$(x)\ ON\ x \equiv (CON\ x\ \&\ TRANS\ x).$$

CON and TRANS are thus defined:-

$$(x)\ CON\ x \equiv ((y)(z)((y \varepsilon x\ \&\ z \varepsilon x\ \&\ - y = z) \supset (z \varepsilon y \vee y \varepsilon z))$$

$$(x)\ TRANS\ x \equiv (y)(y \varepsilon x \supset y \subset x)$$

This definition defines the following series of sets as ordinal numbers, the series itself being well-ordered by ε.

$$\Lambda.\ \{\Lambda\}.\ \{\Lambda\{\Lambda\}\}.\ \{\Lambda\ \{\Lambda\}\ \{\Lambda\{\Lambda\}\}\},$$

The numerals '0', '1', '2', '3'........ can be interpreted so as to denote the elements of this series. Moreover if the account is supplemented by sufficient set-theoretical machinery, then it is possible, by means of the above identification, to derive the Peano axioms. One can create a constructional system X for arithmetic based on cardinal numbers and one Y based on ordinal numbers and either will satisfy arithmetic. But in the context of Carnap's answer (A) at least one of X and Y must be wrong. In system X, based on cardinal (CARD) numbers, the definition of natural number (NUM) is:-

$$(x)\ CARD\ x \equiv NUM\ x.$$

In system Y, the definition is:-

$(x)\ ON\ x \equiv NUM\ x.$

If both definitions are true then $(x)\ CARD\ x \equiv ON\ x$. But plainly cardinal and ordinal numbers are not the same. Therefore, by Carnap's criterion: at least one of X or Y is wrong. Since both systems are perfectly adequate to the demands of arithmetic, Carnap's answer (A) cannot be right.[5]

Answer B

In his commentary on Carnap in The Encyclopaedia of Philosophy, Norman M. Martin gives an interpretation of extensional adequacy somewhat different from answer (A).

'A concept x is said to be reducible to a set of concepts Y if every sentence containing x can be transformed into sentences concerning concepts belonging to Y (with preservation of truth-value). This transformation is carried out by means of a rule, or constitutional [= constructional] definition. Although such a rule is formally a definition, it need not be a definition in the sense of a purely verbal transformation; that is, it need not be the case that the objects indicated by the definition [definiens?] are the same objects as those indicated by the definiendum.'[6]

Martin [89] (Vol II. 26)

Martin's interpretation is somewhat more than slightly misleading because Carnap did believe that coextensiveness was the principle condition required to guarantee the adequacy of constructional definitions. Nevertheless Carnap did offer a functional definition of extensional adequacy very similar to, if not identical with, Martin's. Unlike Martin, Carnap was unaware that he was offering a substantially different account of reducibility and the adequacy of constructional definitions than in (A). Consequently there is little development of answer (B) in the Aufbau. Carnap's comments are limited to a short passage.

'The purpose of construction theory is to order the objects of all sciences into a system according to their reducibility to one another......In view of this task, it is advisable to express the criterion [of extensional adequacy] in still another form so that we no longer speak of propositional functions and their logical relations, but of states of affairs and their factual relations.... We now arrive at a factual criterion of reducibility which is wanting in logical strictness, but allows easier application to the empirical findings of the individual sciences. It is the following: we call an object a "reducible to the objects b,c,......." if, for any state of affairs whatever relative to the objects a,b,c..., a necessary and sufficient condition can be indicated which depends only upon objects b,c...'

Carnap [18] (79 - 80).

Carnap follows this up with an argument to the effect that his 'factual criterion' is equivalent to answer (A). In fact it is not. A first-order language L can have a number of isomorphic models with distinct domains. If I and J are isomorphic models of L and we have a specification of the isomorphism between I and J, then we can systematically 'translate' L-sentences about objects in the domain of I, under the interpretation I, into sentences about objects in the domain of J under interpretation J . Whatever object can be described under I , necessary and sufficient conditions for that object being involved in a given state of affairs in I, can be stated in terms of J . The language of arithmetic $\{0,',+,x\}$ has a number of isomorphic models and we have seen that it can be legitimately interpreted to the domain of cardinal or ordinal numbers.

Had Carnap pursued answer (B) he would have avoided several difficulties and reached a number of conclusions about structional systems that would have fitted in well with his later thought. First, he would have avoided the most telling objection to answer (A): namely that from cardinal and ordinal numbers. Secondly, he would have been compelled to recognise that there could be rival constructional systems, each adequate at yielding as theorems, the fundamental laws of science, but each based on a different set of basic objects. This recognition would have fitted in well with many of the things that Carnap had to say in 'Empiricism, Semantics and Ontology'. To understand how Carnap arrived at his position in that article, it is necessary to explain something of Carnap's metaphilosophy.

Carnap, like his contemporary logical positivists, drew a distinction between genuine statements and pseudo-statements. A genuine sentence could be of three kinds: (i) a statement of logic or mathematics, these were deducible from stipulative definitions and the rules for manipulating signs; (ii) statements of the empirical sciences; these were distinguished by the fact that observation sentences could be deduced from them or in conjunction with other observation sentences, new observation sentences could be deduced (iii) statements of logical syntax, which were essentially statements about statements and were the concern of philosophy. Pseudo-statements were found in traditional metaphysics and were distinguished by, (a) the fact that, like the statements of empirical science, they purported to be statements about the nature of the world, but (b), unlike statements of empirical science, no observations could be made which would determine their truth or falsity.

A language framework (also a linguistic framework) was similar in many ways to Carnap's earlier constructional system. A language-system was essentially a formal theory written in a formal language. L - rules (or as we would say now, deduction rules) permitted the derivation of one formal sentence from a set of sentences and mathematical truths were those that could be deduced from stipulative definitions laid down. L - truths were just those truths which were the combined sum of the logical and the mathematical truths. P - rules were, like L - rules, rules of transformation for moving from a set of sentences to a new sentence. Unlike L - rules, P - rules were not though ot as specifically 'logical' in character, but were thought as embodying inferences based on physical laws such as Newton's principles of mechanics, Maxwells's equations of electromagnetics and the like.

It is fairly obvious that if a language - system could be made to incorporate the findings of human beings, that all specifically mathematical or scientific questions that could be answered in terms of present-day knowledge, could be answered by addressing oneself to the theorems of the system. Where does that leave philosophy? Carnap believed that whereas the statements of logic, mathematics and the empirical sciences were to be found within the system, philosophy was concerned with statements about the system; or as he puts in Philosophy and Logical Syntax, philosophy is about logical syntax.

It was Carnap's opinion that in taking philosophy to be concerned with logical syntax, philosophers could learn to avoid the sterile debates and pseudo-statements of traditional metaphysics. Any genuine set of philosophical questions and answers could be reformulated in terms of questions and answers about the logical syntax of a language-system. So for instance, a question about the nature of wisdom, could be reformulated as a syntax question about the L - rules and semantic postulates concerning the word 'wise'. This brings us to Carnap's distinction between the material and the formal modes of speech.

A sentence was in the material mode of speech when, syntactically, it looked as if it was a sentence concerned with extra-linguistic reality; but in content it was really concerned with language. Carnap [19] gives as an example the sentence 'That A is older than B, and B is older than A is an impossible state of affairs', which is written in the formal mode as "A is older than B and B is older than A' is contradictory'. This latter sentence was in the formal mode because syntactically it presented itself as a question about language. Carnap believed all philosophical activity should take place in the formal mode.

The consequences of applying Carnap's metaphilosophy to metaontology are interesting ones.

Ontological questions like 'Do numbers exist?' and 'What are numbers?' are questions addressed in the material mode of speech. Properly reformulated is the formal mode of sppech they have to be referred to one or another language-system. When this is done pseudo-problems in ontology disappear. Carnap illustrates:-

'To take a case in point, in the different systems of modern arithmetic dealt with logically, numbers are given different status. For instance in the system of Whitehead and Russell numbers are treated as classes of classes, while in the systems of Peano and of Hilbert they are taken as primitive objects. Suppose that two philosophers get into a dispute, one of them asserting: 'Numbers are classes of classes', and the other: 'No, numbers are primitive objects, independent elements'. They may philosophise without end about the question what numbers really are, but in this way they will never come to an agreement. Now let them both translate their theses into one formal mode. Then the first philosopher makes the assertion: 'Numerical expressions are class-expressions of the second-order', and the other says: 'Numerical expressions are not class-expressions, but elementary expressions'.

In this form, however, the two sentences are not yet quite complete. They are syntactical sentences concerning certain linguistic expressions. But a syntactical sentence must refer to one or several specific language-systems; it is incomplete unless it contains such a reference. If the language-system of Peano is called L_1 and that of Russell L_2, the two sentences may be completed as follows: 'In L_1 numerical expressions are elementary expressions,' and: 'In L_2 numerical expressions are class expressions of the second order'. Now these assertions are compatible with each other and both are true; the controversy has ceased to exist.' Carnap [19]

These conclusions reach their fruition in 'Empiricism, Semantics and Ontology' where Carnap distinguished between external and internal questions in ontology.

External questions are questions like 'Are there numbers', 'Are there minds' which unrelativised to any language-framework are pseudo-questions. If these questions are relativised to a language-framework then they are sensible. A question like 'Are there (really) space-time points' is an external question. A question like 'Does this language-framework presuppose space-time points' is significant and an internal question. Within a language-framework the distinct categories of entity presupposed are marked out by the use of variables which range over entities of that type.

(The use of variables to mark out the distinct categories of things presupposed in a language-framework created a misunderstanding by Quine [109]. Quine wrongly mistook the internal-external distinction to mark out a distinction between questions of the form 'Are there Xs?'; where Xs were a proper subset of the range of some variables of a language-framework; and questions of the form 'Are there Ys?', where Ys were the range of certain variables of that language-framework. Quine then goes on to criticise Carnap, arguing that merely by adjusting the range of variables, external questions can become internal and conversely. It should be clear that Quine has misinterpreted Carnap. (See Ayer [11] for an indictment of Quine's misunderstanding)).

Carnap's distinction between internal and external questions has been rejected on the grounds that it depends on the acceptance of something very much like the Verification Principle (Cornman [31], Goldstick [51]). Certainly there are passages in 'Empiricism, Semantics and Ontology' which have a strong positivistic flavour, like this one.

'Suppose that one philosopher says: 'I believe that there are numbers as real entities. This gives me the right to use the linguistic forms of the numerical frameworks and to make semantical statements about numbers as designata of numerals'. His nominalistic opponent replies: 'You are wrong, there are no numbers. The numerals may still be used as meaningful expressions.. But they are not names, there are no entities designated by them ....' .. I cannot think of any possible evidence that would be regarded as relevant by both philosophers, and therefore, if actually found, would decide the controversy or at least make one thesis more probable than another.... Therefore I feel compelled to regard the external question as a pseudo-question, until both parties to the controversy offer a common interpretation of the question as a cognitive question, this would involve an indication of possible evidence regarded as relevant by both sides.'

Carnap [20] (219).

Under one interpretation, a very natural one, the argument is:

All declarative sentences which are neither verifiable or falsifiable are meaningless.
All declarative sentences which make ontological commitments to numbers etc., are neither verifiable or falsifiable.

All declarative sentences which make ontological commitments to numbers etc., are meaningless.

The criticism then is that the major premises of this syllogism is nothing better than a version of the discredited Verification Principle. Actually, this interpretation does little credit even to the <u>consistency</u> of Carnap's position!
If the conclusion of the syllogism is true then many of the sentences within the linguistic framework themselves are meaningless: namely, those that record

their commitment to numbers, propositions and the like. A much fairer interpretation of Carnap runs, oratio dicta, somewhat as follows.

'Traditional ontology has produced a series of controversies about the existence of numbers, propositions and the like which have been sterile because no methodology has existed for their resolution. It is doubtful whether expressions like 'real' and 'exists' have any application to the items of the ontologist; for there appear to be no rules to settle their application. Whatever good can be found in ontology can only be found by agreeing to adopt a common methodology for the practice of ontology. This is to be found in my linguistic framework and in the systematic translation to the formal mode of speech.'

The conclusion of Carnap's thinking that external questions are pseudo-questions which do not answer to rational discussion, seems contrary to Quine's view of ontological questions. Quine views all existence questions as logically, on a par, and questions about the existence of numbers or propositions differ only in their degree of generality from questions about the existence of King Arthur's crown or of subatomic particles like quarks.

'Our theory of nature grades off from the most concrete fact to speculations about the curvature of space-time, or the continuous creation of hydrogen atoms in an expanding universe; and our evidence grades off correspondingly, from specific observation to broadly systematic considerations. Existential quantifications of the philosophical sort belong to the same inclusive theory and are situated way out at the end, farthest from observable fact.'

Quine [12] (98)

In fact Carnap and Quine are not as divergent in their views as conventional wisdom believes, nor, perhaps, as either Carnap or Quine believe. In 'Empiricism, Semantics, and Ontology' Carnap admits an important fact: that certain language-frameworks are better than others and hence we can have rational reason to prefer one language-framework to another.

'To accept the thing [= physical object] world means nothing more than to accept a certain form of language.... The decision of accepting the thing language, although not itself a cognitive nature, will nevertheless usually be influenced by theoretical knowledge, just like any other deliberate decision concerning the acceptance of linguistic or other rules.... The efficiency, fruitfulness, and simplicity of the use of the thing language may be among the decisive factors.

Carnap [20] (208)

'A question like 'Are there (really) space-time points' is ambiguos...., it may be meant in the following sense: 'Are our experiences such that that the use of the linguistic forms in question will be expedient and fruitful?' This is a theoretical question of a factual empirical nature.'

Carnap [22] (213)

'The acceptance or rejection of abstract linguistic forms, just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required.'

Carnap [22] (221)

Carnap's pragmatic criteria, efficiency, results, expedience, simplicity, fruitfulness which he regards as desiderata in selecting a linguistic framework are the same criteria that Quine invokes as desiderata in selecting an ontology.

'Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged.'

Quine [116] (16)

If there is some linguistic framework L, and L is distinguished by its fruitfulness, economy etc and L presupposes the existence of a kind K, and, further, this presupposition is essential to L retaining its pragmatically desirable features then surely this is evidence that Ks do in fact exist. If so then the statement 'Ks exist' and the question 'Do Ks exist' is not pseudo-statement and a pseudo-question respectively. Each is amenable to rational discussion in relation to objective features of language-frameworks; and Carnap's hard distinction between external and internal questions vanishes.

2.4 Goodman

Nelson Goodman's contributions towards a nominalist ontology in The Structure of Appearance and (with Quine) in 'Steps towards a Constructive Nominalism' have been important and original. His important metaontological contributions though are more limited, and occur principally in the first chapter of The Structure of Appearance. Essentially, Goodman's metaontology is much the same as Carnap's in the Aufbau, except that Goodman differs from Carnap in arguing that coextension between definiens and definiendum in a constructional definition is not a necessary condition of the definition being adequate; though it is, according to Goodman, a sufficient one.

Goodman argues that coextension is not what to look for between definiendum and definiens but *extensional isomorphism* is. What Goodman has to say about extensional isomorphism is largely contained in the following long passage.

'If we now look more closely at the very divergent definitions of a given concept that were equally legitimate, we find that they possess in common one feature that every illegitimate definition lacks; namely, that in each legitimate definition, the extension of the definiens is *isomorphic* to the extension of the definiendum. The necessary and sufficient condition for the accuracy of a constructional definition seems to be that the definiens be extensionally isomorphic to the definiendum. *More generally, the set of all the definientia of a system must be extensionally isomorphic to the set of all the definienda*. I shall explain first and illustrate the kind of isomorphism I mean and then consider whether this criterion is satisfactory.
We may think of the extensions of the definienda and definientia in question as relations - that is, classes of couples, classes of triples, and classes of longer sequences of any uniform length. While sequences may in turn be construed as classes, it is simpler to disregard this for our immediate purposes. A class of individuals or other one-place sequences may be considered as a monadic relation. By the *components* of a sequence I shall mean the elements that occupy entire places in the sequence. Thus the sequence

<<a,b>,c>,<d,e>

is a couple: its components are <<a,b>,c> and <d,e>, not the couple <a,b> or any single individuals. On the other hand, if we progressively dissolve each component that is a sequence into its components any every component that is a class into its members, and continue this until we reach elements that have no further members, we have what I call the *ultimate factors* of the sequence.

Here they are a and b and c and d and e. The ultimate factors of a relation or other class are reached in a similar fashion. For our purposes in the present chapter, a sequence is not considered to be identified, as by the Wiener-Kuratowski definition, with a class. An ultimate factor is always either an individual or the null class.

A relation R is isomorphic to a relation S in the sense here intended if and only if R can be obtained by consistently replacing the ultimate factors in S. Consistent replacement requires only that each not-null ultimate factor be replaced by one and only one not-null element; that different not-null elements; and that the null class be always replaced by itself. Since the replacing elements need not be ultimate factors (eg. h,k might replace t) this sort of isomorphism is not symmetric; for if R is isomorphic to S, still there may be no way of replacing the ultimate factors in R so as to obtain S. Nevertheless, if R can be obtained by consistently replacing the ultimate factors in S by certain elements of R, it will also be true that S can be obtained by replacing those elements in R by the correlated ultimate factors of S. It is often more convenient to work in this direction in establishing that R is isomorphic to S, but it should be noted that this does not establish the isomorphism of S to R. Every relation is, or course, isomorphic to itself. Also any class having the same number of members as a given class of individuals is isomorphic to it, but a class is not necessarily isomorphic to every class having the same number of members or ultimate factors.'

Goodman [53] (13 - 14)

There are some peculiarities and mistakes in this passage. For example Goodman writes of the ordered pair <h,k> as 'h,k' rather than the usual '<h,k>'. I shall follow the usual practice. His reference to <u>the</u> Wiener-Kuratowski definition of ordered pair is wrong since Wiener and Kuratowski offered different definitions of ordered pairs. Kuratowski's definition; viz:-

$$\langle a,b\rangle = \{a, \{a,b\}\}$$

would serve Goodman's purposes well since the ultimate factors of $\langle a,b\rangle$ are a and b and likewise with $\{a, \{a,b\}\}$. (Wiener's definition, $\langle a,b\rangle = \{a, \{b,\Lambda\}\}$ would obviously <u>not</u> suit Goodman's purposes, since the ultimate factors of $\{a, \{b,\Lambda\}\}$ are a,b and Λ. It is useful, in the interests of formal clarity, to do what Goodman did not: which is to formalise the above account. Unlike Goodman I shall identify ordered pairs with sets in the manner of Kuratowski. Where A is any set, a <u>factor</u> of A is given by the following equivalence:-

$$(x)\ \text{factor}\ (x,A) \equiv (x \in A \lor (\exists y)\ (\text{factor}\ (y,A)\ \&\ x \in y))$$

This definition is not circular; its content is reproduced by the following inductive definition.

For any x, and for any y:-

(1) If $x \in A$, x is a factor of A

(2) If y is a factor of A, and $x \in y$, then x is a factor of A.

(3) If x is not a factor of A by (1) and (2), then x is not a factor of A.

F (A) is the set of factors of A. The set $\mathcal{U}(A)$ of <u>ultimate factors</u> of A is given by the identity:

$$\mathcal{U}(A) = \{x: -\ \text{set}\ x \lor x = \Lambda\} \cap F(A)$$

Where A and B are any sets, A is <u>extensionally isomorphic</u> to B if any only if there is an <u>extensional isomorphism e defined from B to A</u>. Where e is a function which satisfies the following conditions:-

(i) The domain of e is F(B).

(ii) Where x and y are ultimate factors of B:-

$$x = \Lambda \equiv e(x) = \Lambda$$

$$-(x = y) \supset -(e(x) = e(y))$$

(iii) Where z is any non-ultimate factor of B:-

$$e(z) = \{x: (\exists y)\ y \varepsilon z \ \&\ e(y) = x\}$$

$$\{x: (\exists y)\ y \varepsilon B \ \&\ e(y) = x\} = A$$

In sum Goodman's account of extensional isomorphism is as follows:-

A constructional definition of the form $\phi =_{df} \psi$ is O.K.

if, and only if

ψ is extensionally isomorphic to ϕ

that is, if and only if

$EX(\psi)$ is extensionally isomorphic to $EX(\phi)$

that is, if and only if

there is an extensional isomorphism defined from $EX(\phi)$ to $EX(\psi)$

Goodman's extensional isomorphism criterion produces some strange results. For instance, Goodman states extensional isomorphism is not necessarily symmetrical. Consequently it would be possible for a constructional definition of the form $\phi =_{df} \psi$ to be O.K. by Goodman's criterion but not one of the form $\psi =_{df} \phi$. From a logical point of view, the order of the definia presented in a definition should not influence the adequacy of the definition, since, logically, both are equivalent.

Another weakness of Goodman's account is that in identifying the adequacy of a system in terms of the adequacy of all of its constructional definitions, insufficient emphasis is placed on the role of the theorems of the system. So, let C_1 be a constructional system which takes as its set of definienda, the arithmetical expressions of basic arithmetic. Suppose C_1 offers constructional definitions of those expressions in a way that identifies the natural number series with cardinal numbers and the relations and operations over this series with relations and operations over the set of cardinal numbers. Each definition of C_1 should be O.K. by Goodman's criterion. Suppose C_2 offers an alternative constructional system using the same set of definienda but basing the reduction of natural numbers on ordinal numbers. C_2 might be O.K. too. It is obvious that if, (in the Goodmanian sense) C_1 and C_2 are O.K. as constructional systems so should any C_3 where $C_3 \subseteq C_1 \cup C_2$. But plainly a constructional system that identified natural numbers with cardinal numbers, but used the definitions of arithmetical operations and relations appropriate to a system based on ordinal numbers would be totally inadequate. Within such a system it would be impossible to recover the basic truths of arithmetic as theorems of the system.

A final failing of Goodman's criterion is one that it shares with Quine's criterion of ontological reduction (see chapter six). Extensional isomorphism demands that we admit the objects of the definiendum into our ontology as unreduced objects. Consider an ontologist who defines natural numbers in terms of cardinal numbers. He cannot claim to have shown, on this basis, that we can dispense with an ontology containing unreduced objects; for to assume there are no unreduced natural numbers is to assume $\{x\text{: natural number } x\} = \Lambda$ and hence that there is not extensional isomorphism from the set of natural numbers to the set of cardinal numbers and that his definition is not O.K.. His only alternative then is to admit the existence of natural numbers as a natural consequence of the adequacy of his definition. But since the point of the exercise was to show the redundancy of natural numbers, this seems self-defeating. The final course - admitting both natural numbers and cardinal numbers but identifying the two - runs into the same essential difficulties that Carnap's definition (A) of extensional adequacy did: namely, there are equally satisfactory nonequivalent definitions of the same arithmetical concepts.

2.5 Quine

As one of the greatest living formalists, Quine's work in metaontology has been as important as Russell's. At the cost of a certain inevitable artificiality, Quine's contribution to metaontology can be divided into three areas.

(1) The statements of his criterion of ontological commitment. These were reviewed in chapter one.

(2) Quine's remarks on formalisation and the truth of ontological hypotheses. These are the subject of this section.

(3) His views on ontological relativity and ontological reduction. These are dealt with in 6.5.

Quine's initial position to ordinary language sentences is precisely the same as Russell's: they are grammatically misleading and ontologically unperspicuos.

'The trouble is that there is no simple correlation between the outward forms of ordinary affirmations and existences implied. Thus, granted that the construction exemplified by 'Agnes has fleas' can often be accorded the forthrightly......existential sense intended by '$(\exists x)\ Fx\ \&\ Gx$', there remain abundant cases like 'Tabby eats mice' and 'Ernest hunts lions' that cannot. Reflective persons unswayed by wishful thinking now and again have cause to wonder what, if anything they are talking about.'

Quine [119] (242)

Quine's solution is broadly the same as Russell's: the solution to this tangle is to rephrase our existing theories into an ontologically more perspicous notation. Quine believes this notation will be a first-order language and the process of rephrasing which I have called 'formalisation', Quine calls 'regimentation' or 'paraphrase'.

At the end of the previous chapter, it became clear that the constraints which bind the enterprise of formalisation are of crucial importance in metaontology. Without any guide as to what constitutes a good formalisation there seemed to be no point to any of Quine's criteria of ontological commitment, and with the wrong guides, problems like the Paradox of Formalisation arose. It would be reasonable to expect that Quine would expend much effort on clarifying the nature of formalisation and the relations between natural and formal language theories. But although Quine does offer some examples of formalisation with respect to belief sentences, his remarks on the criteria for good formalisation are brief and not altogether helpful. What he does say is limited to the quotation below.

'[Preservation of meaning is not] to be claimed for the paraphrase. Synonymy, for sentences generally, is not a notion that we can readily make adequate sense of.......and even if it were it would be out of place in these cases. If we paraphrase to resolve ambiguity, what we seek is not a synonymous sentence but one that is more informative by dint of resisting some alternative interpretations. Typically, indeed, the paraphrasing of a sentence S of ordinary language into logical symbols will issue in substantial divergences. Often the result S' will be less ambiguos than S, often it will have truth values under circumstances under which S has none..... and often it will even provide explicit references where S uses indicator words....[The] relation [of S'] to S is just that the particular business that the speaker was on that occasion trying to get on with, with help of S among other things, can be managed well enough to suit him by using S' instead of S. We can even let him modify his purposes under the shift, if he pleases'. Quine [119] (159 - 160)

Quine's remarks hardly provide a shopping list of positive features any formalisation should satisfy: instead we are given a few features that formalisation does not have to satisfy and then left to muddle through as best as we can.

Quine's cavalier attitude to the problems of formalisation does have a justification which is internal to his metaontology. Quine believes not only that ordinary language is grammatically confused but that it is so confused as to merit its complete rejection in the processes of ontology. Ontology can only be seriously practised within first-order notation or something like it. From Quine's point of view, the interest in the ontology of a speaker only begins when that speaker expreses himself in a formal language; so it really does not matter what criteria the speaker uses to formalise his ordinary discourse. From this perspective, Quine's sketchy picture of how formalisation should proceed is not a symptom of carelessness, but of professional and conscious disinterest. This is how Quine puts his case:-

'Futile caviling over ontic implications gives way to an invitation to reformulate one's point in the canonical notation. We cannot paraphrase our opponent's sentences into canonical sentences for him and convict him of the consequences, for there is no synonymy; rather we must ask him what canonical sentences he is willing to offer, consonantly with his own inadequately expressed purposes. If he declines to play this game, the argument terminates. To decline to explain oneself in terms of quantification, or in terms of those special idioms of ordinary language by which quantification is directly explained, is simply to decline to declare one's refential intent'

Quine [119] (242 - 3)

Quine's deliberate abandonment of a systematic theory of formalisation has two immediate dividends and one long term loss. The most immediate dividend is a saving of work. The next most immediate dividend is that Quine really sidesteps the Paradox of Formalisation. The Paradox of Formalisation depends on the assumption that formalisation should preserve ontological commitment. Quine makes hardly any assumptions about how formalisation should proceed, and certainly does not make that one.

Now for the long-term loss. However interesting the byways of metaontology the prime purpose of the subject is to uncover a methodology of ontology which is capable of rationally determining our ontological beliefs. This requires of a good metaontology that it provide a means for rationally distributing truth-values to ontological sentences. Writing one's ontological prejudices in canonical notation does not help in itself to determine if these prejudices have any foundation or not. What is needed (and from Quine) is a method by which we can judge the results of our formalisation to see if the ontological commitments there recorded are well-founded or not. This is where Quine incurs his long-term loss and this is what Quine has to say.

'Now how are we to adjudicate between rival ontologies? Certainly the answer is not provided by the semantical formula 'To be is to be the value of a variable'; this formula serves rather, conversely, in testing the conformity of a given remark or doctrine to a prior ontological standard. We look to bound variables in connection with ontology not in order to know what there is, but in order to know what a given remark or doctrine, ours or someone else's <u>says</u> there is ... .

Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the overall conceptual scheme which is to accommodate science in the broadest sense; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or physical part, are not different in kind from the considerations which determine a reasonable construction of the whole.'

Quine [116] (15 - 17)

'Existence statements differ in no way, epistemologically, from theoretical sentences generally. They are parts of an inclusive theory whose overall claim to acceptance resides in the systematic simplicity, or something like that, with which the whole theory accommodates our observations. I am sorry that I have nothing new to say by way of illuminating this vague matter of the acceptability of theories.'

Quine [114] (95)

Quine is right in saying that his criteria of acceptability are vague: but his admission leaves him exposed to Carnap's attack on ontology. If the criteria are as loose and as vague as even Quine admits they are, is this not good evidence to show Carnap is right in saying that traditional ontological questions are pseudo-questions without good means of settling them?
These is a core of a Quinean response in both the quoted passages above. It is this. The criteria for evaluating the acceptability of scientific theories are no less vague than the criteria for assessing ontologies: in fact they are the same. So if ontology is to be rejected because those criteria are too vague, so should science. The reply is effective because very few philosophers would be heroic enough to reject Western science. But is the comparison between science and ontology a good one? Principally, are ontological questions on a par with scientific ones as Quine says they are?

In one very important sense, ontological theories (as Quine presents them) and scientific theories seem to differ, and Quine's comparison between the two suffers from the Fallacy of Division. (The Fallacy of Division holds that what is true of the whole must be true of all its parts. Thus the argument 'I am made of my molecules; my molecules are not alive, therefore I am not alive' commits the Fallacy of Division). Quine is right in thinking that the criteria which govern the acceptability of scientific theories in general are vague. But when we come down to specific scientific theories the picture is very different. Very often it happens that scientists are well aware of the exact observations and results that would discredit or confirm a theory and the criteria of acceptability are not at all vague. This is true even of the more architectonic pieces of science like Relativity Theory which in its generality and abstractness should approach ontological theories more closely than most portions of science. It is only when we draw together all scientific theories and ask what acceptability for all cases amounts to, that the philosophical platitudes ('simplicity', 'accommodates our observations') plod in. (Compare answers to 'What is the point of brushing your teeth?' and 'What is the point of anything?').

But ontology has not been comparable to science in this respect: ontologists have not agreed on the sorts of evidence relevant to a given ontological hypothesis and the history of ontology is not one of precise hypothesis and experiment. This was of course what Carnap's complaint about ontology was really about; that ontologists lacked criteria for resolving their own disputes.

Quine's failure to define clearly what makes an ontological hypothesis acceptable, derives in part from his abandonment of any theory of formalisation. Had Quine produced such a theory he could have characterised a good ontological hypothesis thus:- a good ontological hypothesis is committed to a kind K if and only if Ks are quantified over in a theory T_C, where T_C is a canonical theory which is the proper formalisation of a true theory T_n. As it is, Quine's counsel to show 'tolerance and an experimental spirit' to ontologies is rather redundant. In the absence of a clear idea of what experiments to perform, tolerance of a flaccid kind seems our only option.

2.6 Davidson

Davidson is unique amongst the five formalists considered in this chapter in placing his theory of formalisation squarely in the theory of meaning. (Indeed, to a large extent, Davidson's theory of meaning is his theory of formalisation.) According to Davidson, an acceptable theory of meaning M for a language L provides for each sentence s of L, a true statement as to what s means. Since to understand M and to know M is true is to know the meaning of every sentence of L; and to know the meaning of every sentence of L is to be a master of L; we can also characterise an adequate theory of meaning for a language L as something that when understood, and known to be true, will make us masters of L. This is how Davidson puts it:-

'.......someone who knows the theory can interpret the utterances to which the theory applies' Davidson [37] (315)

A second feature of an adequate theory of meaning M for a language L is related to the fact that languages are learnable. If natural languages were such that every sentence that had never been heard by a speaker was not understandable to him, and had to be learnt as an unfamiliar word is learnt, then languages would be humanly unlearnable. This point, Davidson conceeds, depends on a number of empirical assumptions:-

'....for example, that we do not at some point suddenly acquire an ability to intuit the meanings of sentences on no rule at all; that each new item of vocabulary takes some finite time to be learned; that man is mortal.'

Davidson [34] (388)

Nevertheless the point is true.

The question then arises as to how finite intelligences operating in a finite lifespan can learn to use natural languages. The answer Davidson gives (and he is surely right) is that we learn to understand sentences by understanding the words in them and how their mode of combination helps determine the meaning of the sentences of which they are a part.

'When we can regard the meaning of each sentence as a function of a finite number of features of the sentence, we have an insight not only into what there is to be learned; we also understand how an infinite aptitude can be encompassed by finite accomplishments.'

Davidson [34] (387)

Davidson links this view with an important constraint on a theory of meaning.

'.....a satisfactory theory of meaning must give an account of how the meanings of sentences depend the meanings of words. Unless such an account can be supplied for a particular language, it is argued, there would be no explaining the fact that we can learn the language: no explaining the fact that, on mastering a finite vocabulary and a finitely stated set of rules, we are prepared to produce and to understand any of a potential infinity of sentences.'

Davidson [37] (304)

A theory of meaning M for a language L should tell us how the meanings of L sentences are a function of the meaning and arrangement of L words. Obviously, if M is merely an infinite set of assertions of the form 'S means that p', where S is a structural name of an L sentence and p is some sentence, then M will not be an adequate theory of meaning for L. M will not explore the structure of L sentences nor the meanings of L words. This possibility can be avoided if M is required to be finite. In this case, M would consist of a finite number of assertions concerning the rules of meaning for L from which it would be possible to deduce the meaning of each L sentence. Davidson's idea was that any finite theory of meaning would be forced to explore the semantic structure of L sentences, to stay finite.

This left Davidson with the problem of the exact form in which a theory of meaning is to be couched and how it is to be tested. One way of 'generating' an infinite number of assertions from a finite number is by constructing an axiomatic system in which there are a finite number of axioms but an infinite number of theorems to be deduced. Perhaps what is needed is an axiomatic system, containing a finite number of axioms, from which theorems of the form 'S means that p' can be deduced.

But the phrase 'means that' is philosophically obscure. So Davidson suggested 'S means that p' be dispensed with and replaced by 'S is T $\equiv$ p', where T is some as yet unknown predicate. Davidson's idea was that if the right restrictions were placed on T and on M itself, that from $\vdash_M$ S is T $\equiv$ p, we would be able to infer that S means that p.

Davidson further suggested that 'true' replace 'T'. Theorems of M thus include sentences of the form 'S is true $\equiv$ p'. An example of such a theory for English might be:-

'Snow is white' is true $\equiv$ snow is white.

Students of Tarski's theory of truth will recognise the above sentence as a substitution - instance of Tarski's T-schema 'S is true $\equiv$ p'. Tarski showed, for any first-order language, how to construct an axiomatised theory that would provide, for each sentence of the first-order (object) language in question, a theorem of the form 'S is true $\equiv$ p'. S was the metalanguage name of an object language sentence and p was a metalanguage sentence. Tarski allowed the derivation of an infinite number of such theorems from a finite number of axioms which stated satisfaction conditions of the object language variables. Davidson seized on Tarski's means of generating equivalences of the form 'S is true $\equiv$ p' as a way of generating the same equivalences within his theory of meaning. Tarski had a way of deducing an infinite number of these equivalences from a finite number of axioms which went into the structure of the object language sentences. This was the sort of thing that Davidson thought a theory of meaning should do. So why not equate a Tarski-style theory of truth for a language L with a Davidsonian theory of meaning for language L? Davidson did.

'We have such theories, I suggest, in theories of truth of the kind Tarski first showed how to give.'

Davidson [37] (318)

A Tarski-style theory of truth for a language L was the proper form for a theory of meaning M for L. Suppose that L was English. Davidson envisaged that a more-or-less formal portion of English (let us call it Formal English) would receive a Tarskian explication of the truth-conditions of its sentences. So in such a theory we might have as a theorem:-

'$(\exists x)$ dog x' is true $\equiv (\exists x)$ dog x.

Davidson believed a feature of this Formal English is that, although it would not include every English sentence, anything that could be stated in English could be stated in Formal English. Since Davidson expresses a clear preference for using first-order languages, this amounts to the belief that what can be stated in any natural language can be reproduced in a first-order language. Here one must interject an element of doubt about the whole Davidsonian program. It is by no means evident that any formal language yet constructed can match the subtleties and expressive capacities of a natural language as rich in nuances of meaning as English. Strawson for instance, puts up fairly convincing arguments to show how the meanings of logical constants differ from the accepted readings given to them. I doubt if '$\supset$' means what is meant by 'if...then' and I doubt that 'if...then' has only one meaning in use. (See Austin [6] for an analysis of the different meanings of 'if'). Such features of English as tone seem neglected on Davidson's truth-conditions theory of meaning. It is ironic that Tarski, from whom Davidson draws so much inspiration, should have precisely these doubts.

'Whoever wishes, in spite of all difficulties, to pursue the semantics of colloquial language with the help of exact methods will be driven first to undertake the thankless task of a reform of this language... It may however be doubted whether the language of everyday life, after having been 'rationalised' in this way, would still preserve its naturalness and whether it would not rather take on the characteristic features of the formalised languages.'

Tarski [141] (267)

Whatever the hopes for the Davidsonian program in the long term, Davidson himself is optimistic. Once a Tarskian truth-theory for Formal English has been given we can extend this theory to the whole of English by associating the Formal English sentence with all those English sentences which mean the same. Davidson believed Chomsky's work in transformational grammar offers help in this task: for transformational grammar is largely concerned with transforming the output of certain PS-grammars into sentences each of which are identical in meaning. This close parallel between this aspect of Davidson's work on the theory of meaning and Chomsky's ideas has led Harman [62] [63] to identify Chomsky's deep structure with Davidson's logical form[7]. So, returning to our previous T-sentence, '$(\exists x)$ dog x' is true $\equiv (\exists x)$ dog x'; transformational rules would associate '$(\exists x)$ dog x' with various synonymous sentences in Informal English. The structure of a Davidsonian theory of meaning is illustrated in diagram 2.1.

When does such a theory of meaning M give a proper representation of the meaning of L sentences? Once again Davidson borrowed off Tarski. Tarski held any good theory of truth for a language L should have as a consequence all substitution-instances of the schema 'S is true $\equiv$ p' where s is the structural name of p. This was Tarski's material adequacy condition on any theory of truth. Following a policy of taking truth for granted and defining for meaning, Davidson adapted Tarski's material adequacy condition. Davidson argued that M gave a good account of the meanings of L sentences just when all theorems of M which were of the form 'S is true $\equiv$ p' were true.

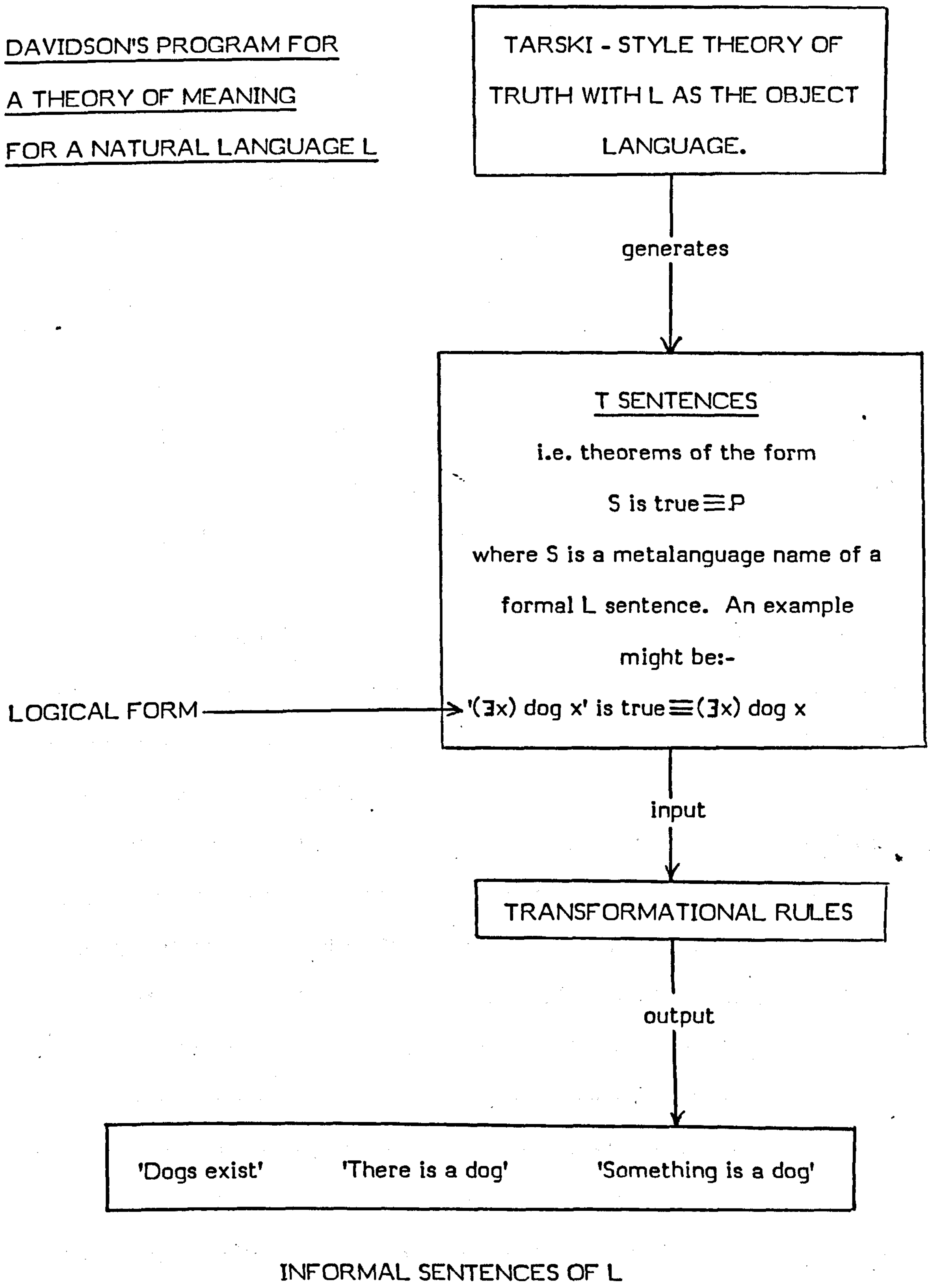
Davidson summed up,

'It is enough to demonstrate that a theory of truth [= a theory of meaning in Davidson's view] is empirically correct, then, to verify that the T-sentences are true ....' Davidson [37] (321)

This requirement is Davidson's convention T. (It should be said that Davidson undoubtedly envisages that if 'S is true $\equiv$ p' is a T-sentence of M, and S is asociated by transformational rules with S', then 'S' is true $\equiv$ p' is also to be counted as a T-sentence of M).

How do Davidson's theories about meaning relate to the metaontological questions with which we are concerned? Simply, many philosophers consider that Davidson has provided a methodology for correlating each sentence of a

Diagram 2.1


DAVIDSON'S PROGRAM FOR
A THEORY OF MEANING
FOR A NATURAL LANGUAGE L
TARSKI - STYLE THEORY OF TRUTH WITH L AS THE OBJECT LANGUAGE.
generates
T SENTENCES
i.e. theorems of the form
S is true≡P
where S is a metalanguage name of a formal L sentence. An example might be:-
LOGICAL FORM
'(∃x) dog x' is true≡(∃x) dog x
input
TRANSFORMATIONAL RULES
output
'Dogs exist'
'There is a dog'
'Something is a dog'
INFORMAL SENTENCES OF L

natural language L with the logical form of that sentence. The logical form of any L sentence is thought of as a meaning-preserving formal equivalent of that sentence. Because any sentence and its Davidsonian logical form are ontologically equivalent, the logical form of an L sentence reproduces with canonical clarity the ontological commitments of that L sentence. In determining the ontological commitments of all true L sentences, by examination of their individual logical forms, the ontologist detemines what there is.

I shall not explore Davidson's theory of meaning any further than this.[8] The important issues have already been defined by what has been written so far.

It is generally recognised that Davidson's convention T is insufficient as a guarantee that a theory of meaning M for a language L is a good theory of meaning for L. Platts summarises why this is.

'First, given a truth theory which serves up only true biconditionals, we can construct quite automatically any number of other truth-theories which also serve up only true biconditionals, yet which pair quite different metalanguage sentences with each object-language sentence. For example, we can construct a theory that yields on the RHS of each T-sentence the conjunction of that served up by the previous theory with a truth, say, 'Snow is white'. A moment's reflection shows that 'p' and 'p & snow is white' will agree in truth-value; so if the original truth-theory satisfied...[convention T]..., so will this new one.... The second objection is more evidently substantial... A theory of meaning, we have maintained throughout, must connect with speakers' understanding of their language. One concrete instance of this is that we should not credit them with an understanding they do not have. Now consider at backward community who have a term for 'water', but lacking a developed science, know nothing of its structure. Taking any sentence of theirs in which the term for water is used, we shall obtain a true T-sentence if on the RHS we replace 'water' by 'H_2O'; for 'water' and 'H_2O' are extensionally equivalent. But to use the H_2O sentence on the RHS is mistaken since it credits the native speakers with an understanding, a knowledge, they lack.'[9]

Platts [100] (65 - 66)

Criticisms like these have led Davies [41] to distinguish between a theory of truth for a language, which satisfies convention T, and a theory of meaning for a language which satisfies convention T <u>and</u> other criteria too. It is the 'other criteria' that prove the problem to the development of Davidson's work. McDowell [91] suggests that a good theory of meaning for a language L should represent L speakers as rational in their beliefs. But this is much too vague to pass muster.

These difficulties in Davidson's work are well known to those current with the literature. What does not attract interest, but what is also just as important, is whether the theory of meaning is the best framework within which to develop a theory of formalisation. The gulf between the old-style formalists such as Russell, Carnap, Goodman and Quine and the host of philosophical logicians at Oxford and elsewhere who practice formalisation in the wake of Davidson, could hardly be greater on this point. Yet it barely passes mention in the current literature. All of the formalists considered prior to Davidson did not consider preservation of meaning to be a requirement of formalisation. The reason why this was, is that these philosophers, being grounded in mathematics and science in a way that their successors at Oxford are not, were aware that some of the most impressive formalisations, most particularly of mathematics, had shown no concern with meaning at all. The goal had always been to preserve what Russell loosely called the 'structure' of the original theories.

We have already seen two examples of structure-preserving formalisations of arithmetic in terms of cardinal and ordinal numbers. There are others. A case in point is Dedekind's and Cantor's rival constructions of the real numbers. Dedekind defined a real number as a section or cut of the rationals. Peano and Russell followed suit. Cantor however defined a real number as an equivalence class of Cauchy sequences. Dedekind's and Cantor's approaches are interestingly different since they use diverse set-theoretical constructions. Neither Cantor nor Dedekind were interested in whether their approaches captured the 'meaning' or the 'logical form' of sentences which mentioned real numbers. Nor have the mathematicians who followed them been inclined to sterile wrangling over the question of the logical form of real number sentences. The question never arose. All that was important was whether these set-theoretical sentences mirrored the sorts of arithmetical properties of real numbers that mathematicians were interested in keeping. Both Dedekind and Cantor, in their different ways, were successful in accomplishing this task.

The issues here are plain enough to see for those with the ability to learn: meaning is not where the action is in formalisation. But there are ways in which Davidsonian disciples can try to wriggle off the hook. One way of evading the issues is to invoke the Quinean thesis of the Indeterminacy of Translation. The defence is as follows. Davidson accepts the Indeterminacy thesis and he accepts that there can be diverse theories of meaning for one object language, each such theory being fully satisfactory in itself. Why not regard Cantor's and Dedekind's rival treatments of real numbers as evidence for an Indeterminacy present in arithmetic?

There are two reasons why this reply will not do. The first is that if the Indeterminacy of Translation is taken into the Davidson theory of meaning, then the doctrine that each sentence has one and only one logical form has to go. Different, but equally satisfactory theories of meaning for an object language may (and will in some cases) assign competing formal sentences to the same object language sentence. But second, and more importantly, it is most implausible to view Dedekind's or Cantor's work with real numbers as being competing views on the proper interpretation of the meaning of real number sentences. At the time of their foundational work, mathematicians, as a linguistic community, were either mostly either ignorant of, uninterested in, or suspicious of the infant discipline of set theory. There is not the slightest reason to credit these men with utterances that presumed a deeper technical embrace of set theory than that they possessed.

Despite garbled attempts like Evans [43] to illuminate the concept of logical form, not a great deal of success has been achieved. The attempt to elucidate the logical forms of various natural language sentences has resembled Lewis Carroll's Hunting of the Snark. Not only is the beast mythical, but there is a lack of agreement amongst the participants about what they are supposed to be looking for. At Oxford the new parlour game of 'find the logical form' has replaced their previous preoccupation with the trivialities of ordinary use. The deficiencies of this approach are best illustrated by example. The field in contributors is a rich one. I will choose, at random, Kaplan's [73] treatment of propositional attitudes: his work illustrates the pitfalls as well as any other. Kaplan formalises 'There is someone Holmes believes to be the murderer' as :-

$(\exists y)(\exists\alpha)\,[R(\alpha,y,\text{Holmes})\ \&\ \text{Holmes}\ B\ulcorner\alpha = \text{the murderer}\urcorner]$

where

R = 'the name........represents.........to.....'

α = a variable ranging over names.

B = '............believes....... is true.

If it is true that Kaplan's formalisation does capture the logical form of 'There is someone Holmes believes to be the murderer' then the formal sentence above and the sentence of which it is the logical form ought to mean the same. In other words:

'There is someone Holmes believes to be the murderer' means the same as '$(\exists y)(\exists \alpha)[R(\alpha,y,\text{Holmes})$ & Holmes $B\ulcorner\alpha =$ the murderer$\urcorner]$'.

Is this true? To what authority can one appeal to establish its truth or falsity? Unreinforced intuition has nothing to say here. I cannot draw upon my resources as a competent speaker of English to settle the matter. Nor can the above statement be thought of as a report on the usage of words. If Kaplan had argued that the French word 'vin' means what the English word 'wine' means then it would become possible to draw some empirical content from his assertion. But plainly Kaplan is not reporting on a correlation in verbal behaviour between two language communities. Is he reporting on an identity of meaning between the sentence 'There is someone Holmes believes to be the murderer' as used by the ordinary English speaker and the formal sentence '$(\exists y)(\exists \alpha)[R(\alpha,y,\text{Holmes})$ & Holmes $B\ulcorner\alpha =$ the murderer$\urcorner]$' as used by Kaplan? If so, then what Kaplan has to say in his article belongs in his autobiography and not in a journal of philosophy.

In the final analysis, claims like Kaplan's to have isolated the logical form of various sentences often amount to pseudo-statements. We can only usefully claim an identity of meaning between one word or one sentence with another when there is one or more language communities in which those words or sentences have a use. Claims to the effect that word A and word B are synonymous are elliptical for predictions about the way those words are used in the communities in which they are understood. Statements about the logical form of various sentences do not cash in in terms of linguistic observations of usage. We cannot hope to capture what ordinary people mean when they use belief-sentences, in the idiom of formal logic. Since the ordinary man knows no formal logic there is no possibility of comparing the usage of a complex logical sentence with a formally unstructured natural one. Nor can any philosopher predict how, if he learnt formal logic, the ordinary man would choose to use formal language sentences. Research into logical form, in this context, is largely a waste of paper.

2.7 Summary

This chapter opened with three questions.

(1) Why formalise?

(2) What makes a good formalisation?

(3) How does formalisation help determine an answer to the Ontological Question, 'What exists?'?

The table opposite summarises the contents of this chapter by comparing the responses of each of the five philosophers studied to the three questions above.

Diagram 2

	RUSSELL	LATER ATOMISTS	CARNAP	GOODMAN	QUINE	DAVIDSON
Why formalise?	To guarantee human belief, and also human science, safe from philosophical attack by re-casting its assertions in a more ontologic-ally secure way.	To reveal more perspicuosly the ontological commitments of natural language sentences by re-casting them in a less grammati-cally misleading way; (isolating their logical form).	To create a constructional system with an ontology of basic objects All assertions of science will be rewritten in this language. The system will be axiomatised so as derive the true assertions of the sciences as theorems.	As Carnap	To recast ordinary language theories in a formal language which is ontologically more perspicuos than natural languages. We determine our ontological commitments in relation to the formal theory alone.	To construct a theory of truth/ meaning for any language L, that correlates every every L-sentence with its logical form. The logical form of an L sentence S reveals the ontological commitments of S.
What makes a good formal-	Preservation of structure. The sentences whose	Preservation of meaning; which is a constraint	All constructional definitions of the system shall be	All construct-ional definit-ions shall be	Insignificant. As long as the formal theory serves	Preservation of meaning: in Davidson's view

	ophical thought takes to be true should still be true; but the meanings of the logical variables involved can change.	general.	definiens and their definiendum shall be co-extensive.	extension of the definiens is extensionally isomorphic to the extension of the definien-dum.	purpose the natural language counterpart did.	theory of meaning is finite and satisfies convention T.
How does formal-isation help determine an answer to the Ontological Question 'What exists?'?	By determining the minimum domain of entities we need to fund our most indispensable beliefs, we determine all that needfully exists.	What exists is what is required to exist by those sentences which are the logical forms of the sentences that are true.	The only objects that exist are the basic objects of the constructional system. All other objects are logical constructions out of these objects.	As Carnap	Pragmatic factors. The convenience and simplicity of our formal theories, and their acceptability in dealing with experience dictates we should accept their ontology under these circumstances.	By determining the logical forms of all true L sentences we determine what exists.

[1] Frege made an exception in the case of geometry, which he regarded as a collection of synthetic apriori statements about the nature of space,, after the manner of Kant.

[2] David Lewis [83] provides a very good unsolicited example of the weakness of Ryle's analytical technique as applied to ontology. Lewis treats 'There are many ways things could have been besides the way they actually are' as a systematically misleading way of saying 'There are possible worlds different from the actual world'. Ryle would probably assert that it was the latter sentence which was systematically misleading. Who is right can only be settled by determining whether there are possible worlds.

[3]See appendix III for a worked example of this sort of technique as applied by Russell.

[4]In intuitive set theory, an ordinal number is also defined as an equivalence set of well-ordered sets under order isomorphism. See Hatcher [65] (146-148). The above is the definition in Z-F set theory.

[5]See Benecerraf [13].

[6]My emphasis.

[7]See especially the footnotes at the end of Harman [63] for an example of how transformational grammar is supposed to operate on formal sentences.

[8]There is more to Davidson that is stated here. For those interested in the more:- Davidson [37] is most important. There are discussions of Davidson's

ideas on meaning in Harrison [64], Platts [99], Platts [100], Davies [41] and in Evans and Mc Dowell's collection Essays on Semantics.

[9] See Foster [45] and Putnam [107] for the originals of these criticisms.

CHAPTER THREE

Model Worlds and Formal Frameworks

3.1 On Justification in General

Faced with any theory or statement the most challenging and important question we can ask of its proponent is 'Can you justify that?' In a very few pages the reader will be faced with my exposition of how ontology should be practised and it is natural that the same question should occur to him. Since the ideas introduced in this chapter are fundamental and their consequences continue until the very end of this work, it may not go amiss to say a few words on the subject of justification in general and how the justification of a theory or statement can proceed. In particular I want to distinguish between retrospective justification and consequential justification.

Retrospective justification is commonly found in mathematics, and less successfully in the great metaphysical systems of the seventeenth century rationalists. The technique of retrospective justification is to justify an assertion A, by deriving A by logic alone from a set of assumptions $B_1, \ldots. B_n$ where $B_1, \ldots. B_n$ are statements whose truth is held to be beyond doubt. The theorems of Euclid were long held to be paradigms of the retrospective justification of many substantial statements about the nature of space. The Euclidean paradigm has had such a grip on the imagination of philosophers that, even today, the demand for justification sends philosophers racing to assemble the materials for a textbook logical argument.

Retrospective justification is sometimes successful, and it reaches its metier in mathematics where from stipulative definitions and simple axioms it is possible to derive an extraordinary fertility of substantive theorems. Outside mathematics, and particularly in philosophy, retrospective justification is notably less successful. It is rarely possible to derive an interesting and substantial proposition from a set of assumptions which are little better than truisms unless one is notably fortunate in picking on just the right propositions. Frequently such attempts turn out to depend on importing a number of suppressed premises, which later critics take delight and sustenance in pointing out. But there is another alternative to trying to build brick houses out of straw bales and this is consequential justification.

Consequential justification takes its beginning from the recognition that a theory is a tool to solve a problem.[1] What justifies the employment of any tool in preference to another is that it performs in its allotted task better than any other. Likewise what justifies our selecting a theory is very often the fact that that theory is an effective problem-solver: more effective than any alternative we have to hand. A theory is consequentially justified when we justify it by pointing out its success in resolving tangles, straightening out obscurities, and explaining phenomena that we had little place for previously. Consequential justification differs from retrospective justification in that we do not reason to the theory but from the theory to its consequences which either vindicate the theory's effectiveness or show it is ineffective. The thinking behind the consequential justification was embodied succinctly nearly two thousand years ago:- 'By their fruits ye shall know them' (Matthew VII, 20) It is by the fruits of this chapter that what is said therein stands or falls. Since it will require several chapters to develop those ideas and gather the fruits in, the best advice that I can offer to the prospective reader is to read carefully and sympathetically; bearing in mind that with consequential justification, reasons rarely come first.

3.2 The Elements of a New Metaontology

I shall begin by laying down six of the most important elements of the metaontology here put forward and then discuss them in order.

(1) Ontology is concerned with the devising and testing of ontological hypotheses. Formalisation does not end by extracting an ontology from natural language theories. Rather, an ontological hypothesis is where to begin and formalisation is an attempt to see if that hypothesis is tenable.

(2) There is no methodology for the formation of ontological hypotheses. Ontological hypotheses are chosen on the basis of their intuitive attractiveness.

(3) The proper object of formalisation is the entirety of organised human knowledge: that which we call 'science'. Since science is largely empirical, so, indirectly, is ontology.

(4) In formalisation, it is not necessary to preserve either the sense or the ontological commitments of the sentences under formalisation.

(5) In order to talk significantly about the putative entities of an ontology, a characterising language is required in order to express that talk. The expressions of this language must make sense, and therefore it must be possible to divine when a sentence of that language counts as true relative to the ontology it presupposes.

(6) Formalisation is achieved in the construction of formal frameworks. A formal framework is an axiomatic machine for correlating natural language sentences with formal language sentences. Since this is its sole purpose, construction of a formal framework need and should not presuppose any ontology. Formal frameworks merely talk about signs, that is, they are written in the formal mode.

An ontological hypothesis is, in essence, an attempt to partition the universe set. Such a hypothesis has the form:-

$(x)(K_1x \vee \ldots \vee K_nx)$; and for any i,j where $-(i = j)$ and $1 \leq i \leq n$, $1 \leq j \leq n$, $(y)(K_iy \supset -K_jy)$; and $(\exists z)K_1z \ \& \ \ldots\ldots \ \& \ (\exists z)K_nz$.

The disjoint sorts $K_1,\ldots K_n$ that purport to exhaust the range of things that are, are sometimes dignified by the title of <u>categories</u>, and the scheme of categories chosen to partition what is, is also what individuates an ontology. An ontology is never founded on a logical truth. 'Only material objects exist', 'Everything is either a mental object or a physical object' are ontological hypotheses determining ontologies. 'Everything is either a shoebox or not a shoebox' is not an ontological hypothesis since it is an instance of the valid theorem $(x)Kx \vee -Kx$. To proceed then, with the six main points of our hypothetical metaontology.

(1) 'Ontology is concerned with the devising and testing of ontological hypotheses. Formalisation does not <u>end</u> by extracting an ontology from natural language theories. Rather, an ontological hypothesis is where to <u>begin</u>.'

Many philosophical logicians have not taken the attitude expressed immediately above. The conventional view of formal ontology is that the ontologist, beginning with a corpus of natural language sentences deemed true, determines the shape of his ontology, by determining what he has to existentially quantify over in formalising those sentences. Formalisation then becomes a 'black box', receiving on an input, natural language sentences, and giving as an output, formal language sentences.

This current model of formalisation that has impressed itself on so many of the best minds, has certain points of affinity with Francis Bacon's early model of scientific procedure, as set forth in his Novum Organum. Stripped of much important detail, Bacon's scientific methodology was essentially a black box that received as inputs, observation-statements, and gave as an output the laws of nature. The contents of this black box were largely inductive principles. The philosophical problem that attaches to Bacon's approach is the Problem of Induction. The problem that attaches to the conventional model of formalisation is the Paradox of Formalisation. Both problems are too substantial to be ignored.

In place of the conventional model is the hypothetical model I present here. The ontologist does not approach his task devoid of ontological prejudices. He begins with a clear idea of what he takes to exist, in the form of an ontological hypothesis. Formalisation is a procedure for testing such a hypothesis. In outline, this method is reminiscent of Popper's hypothetico-deductive method sufficient to deserve the parallels drawn out in diagram 3.

(2) 'There is no methodology for the formation of ontological hypothesis. Ontological hypotheses are chosen on the basis of their intuitive attractiveness.'

This point speaks for itself. There may be psychological reasons why one philosopher prefers to formulate an ontological hypothesis based on materialism, whereas another asserts dualism. But it is not part of metaontology to descriminate for or against any one ontology, or to suggest what steps a philosopher should take in the formalisation of an ontological hypothesis. Any hypothesis is welcome so long as it is properly put and subject to test.

Diagram 3

CONVENTIONAL MODEL OF FORMALISATION	INDUCTIVIST METHODOLOGY
successful	accurate
THEORIES	OBSERVATIONS
are	are collected, and confirm by
FORMALISED	INDUCTION
to reveal their	the truth of certain
ONTOLOGICAL COMMITMENTS,	SCIENTIFIC LAWS,
the totality of which	the totality of which
determine what our	determine what our
ONTOLOGY	SCIENCE
shall be	shall be

NEW MODEL OF FORMALISATION	HYPOTHETICO-DEDUCTIVE METHODOLOGY
freely chosen	freely chosen
ONTOLOGICAL HYPOTHESIS	SCIENTIFIC HYPOTHESIS
is tested by being	is tested by being
fitted against	fitted against the
SUCCESSFUL THEORIES	WORLD OF EXPERIENCE
by the	by the
FORMALISATION	DEDUCTION
of those theories.	of observation statements from
The results of this enterprise	the hypothesis given certain
determine	boundary conditions. The results
what our	determine what our
ONTOLOGY	SCIENCE
shall be	shall be

(3) 'The proper object of formalisation is the entirety of organised human knowledge: that which we call 'science'. Since science is largely empirical, so, indirectly, is ontology.'

A scientific hypothesis gains in stature just insofar as it survives more searching and critical tests. A familiar occurrence in the history of science is one where a hypothesis, deemed true of all domains, applies only when the variables of that hypothesis are restricted to range over a limited domain. Consequently scientists are careful to test a hypothesis under as wide a range of conditions as they can muster. (Thus in Galilean mechanics, an object is deemed to have a uniform acceleration in a gravitational field. In Newtonian mechanics an object has increasing acceleration inversely to the distance of the object from the centre of the field. If the variables of both theories are restricted to objects in free fall close to the surface of the earth, both theories work almost equally well. Only in relation to bodies in free fall and celestial bodies in space, does Galilean mechanics go significantly astray).

The ontologist sets out to vindicate his ontological hypothesis by showing it can handle the widest range of successful theories that can be mustered. Suppose an ontologist successfully formalises a theory T: who knows - if he extended T in some way, either by adding to the assertions of T, or by enriching the vocabulary of T, perhaps the new theory T' so formed would prove resistant to the sort of formalisation carried out on T. Extrapolating this reasoning to its conclusion must force the admission that it is <u>the entirety of science that is the object of formalisation</u>. This point leads to two parenthetical remarks.

The first is that, in setting himself the task of formalising human science, the ontologist is free to choose the <u>language</u> that science is expressed in, as long as the language suffices to express our consensus theories. Therefore it is not necessarily required that the ontologist attempt to formalise every sentence of a natural language, but only as much as is strictly required for scientific research. For instance, the ontologist may shun attributives like 'large' on the grounds that they may be better replaced by statements of measure. Demonstratives might be deported on the grounds that they are a formal nuisance and are only expedient in conversation. The definite article 'the' can be tightened up so as to imply uniqueness, and so on. Whether a kind of vocabulary is essential to science depends on whether its work can be carried out as well or better, by some other stretch of vocabulary, in terms of precision, clarity of meaning and information-content.

Given that the ontologist is ultimately engaged in the formalisation of science, then the results of ontology are inevitably going to share the same uncertainties that many of the sciences do. Science is inherently incomplete and provisional, subject to increment and amendment from second thought and experiment; and so, necessarily, ontology becomes the same. An ontology capable of accommodating mid-nineteenth century physics may be inadequate when faced with the physics of the late twentieth century. Though fundamental changes in our ontology are slow to transpire, and often depend on drastic changes in our scientific outlook, ontology remains an _empirical_ subject whose results depend on the state of current science. It is the generality and foundational nature of ontological hypotheses, rather than their alleged _apriori_ character, that makes them of interest to philosophers.

(4) 'In formalisation, it is not necessary to preserve _either_ the _sense_ or the _ontological commitments_ of the sentences under formalisation.

An ontological hypothesis is a hypothesis which is not logically true stating that only certain specified disjoint things exist. According to anybody who accepts such a hypothesis, however provisionally, whenever a person makes a true assertion about the universe he inhabits, he is referring to a configuration of objects recognised by the hypothesis. To maintain otherwise is to reject the hypothesis as false. But if an ontologist affirms such a hypothesis, then he is required to develop it. In particular, for each sentence S of a language L sufficient to express human science, it is required that the ontologist state what objects S purports to refer to. Most importantly, where S is true, we require some statement as to what objects of the hypothesis itself S refers to and in what configuration they are found. (Thus if S = 'The orbit of the moon is approximate to an ellipse' and the hypothesis is that only material objects exist, then we require that S be construed purely as a reference to material objects).

Preservation of sense is not a necessary condition of successful formalisation, and consequently, formalisation has nothing to do with the construction of a theory of meaning in Davidson's terms. To see this, we need only mark the Fregean distinction between sense and reference. Thus even though Reynolds = Russia's leading spy and the Blue Club = New York's most exclusive club, it does not follow that 'Reynolds is a member of the Blue Club' and 'Russia's leading spy is a member of New York's most exclusive club' have the same sense. It is not a necessary condition of two sentences concerned with the same objects that they have the same sense. So if an ontologist tries to vindicate an ontological hypothesis by transcribing L sentences into a formal language, preservation of sense is not a constraint on his exercise.

Preserving sense is not a requirement on formalisation. More surprisingly, neither is preserving ontological commitment.

For example, suppose that we accepted the ontology of physicalism. What physicalism amounts to is arguable, but let it be defined here as the doctrine that (i) the only entities that exist are those required by physics, (ii) and that all events (including actions of living organisms) that can be explained at all can be explained from the laws of physics. A small step towards clarifying the physicalist hypothesis would be to recast it as following. There is a formal language L_p, which can be identified as the language of physics; (to be thorough about this, we should have to give some effective means of determining the elements of L_p). The physicalist hypothesis entails that every true assertion can be formalised using only logical variables drawn from L_p. The physicalist then is faced with the problem of formalising natural language sentences which use a vocabulary removed from L_p. For instance, 'Jones is in pain' may be a true assertion but neither 'Jones' nor 'in pain' may be elements of L_p. The physicalist may respond by redescribing Jones as, say, a cluster of space-time points forming a mereological whole. 'Jones' being in pain might be identified with a physical event, say excitation of the medulla oblongata. Thus let '(ιx) Fx' be taken to denote the mereological whole that is Jones scattered through space-time and let 'G' be a predicate true of an event when it is a pain-process occurring in the body. Using Goodman's [53] '$\leq$' as short for 'is a proper part of', letting 'e' range over events, 'Jones is in pain' might emerge as:-

'$(\exists e)\, G(e)\ \&\ e \leq (\iota x)\, Fx$.'

'Jones is in pain' and the above formal sentence do not agree in sense; that is beside the point sofar as formalisation is concerned. Nor do they agree in respect of their ontological commitments. 'Jones is in pain' is committed to Jones and somebody who is in pain. There is no imputation of space-time coordinates, meredogical wholes, physical pain-processes etc., in this innocent statement; nor could the existence of such items be deduced from Jones being in pain. The physicalist will claim this does not matter. He will claim that, as a matter of fact, Jones is really just a cluster of space-time coordinates, pain is just a physical process and whether these facts are registered in 'Jones is in pain' is irrelevant.

The physicalist is right to stand his ground. As observed in chapter one, ontological commitment is an intensional relation, and there is no inconsistency between two sentences reporting on precisely the same features of the universe but differing in their ontological commitments. Simply, the ontological commitments of a sentence are a function of its sense; and preserving sense is not the issue in formalisation.

This leads to an important consequential justification of our new metaontology. The Paradox of Formalisation that loomed so threateningly at the end of chapter one, is felled at its initial premiss. It ws essential to the formation of the Paradox, as stated, that it be assumed that in formalisation, ontological commitments be preserved. No such assumption is made in our new metaontology and in fact one would expect in most substantive formalisations the assumption would be false.

(5) 'In order to talk significantly about the putative entities of an ontology, a characterising language is required to express that talk. The expressions of that language must make sense; and therefore it must be possible to divine when a sentence of that language counts as true <u>relative to the ontology it presupposes</u>.'

This point requires rather more explanation than the preceding points. The issues involved are vital, but also somewhat more involved; I shall begin by example.

Voodoo (or <u>voudon</u> as the devotees prefer to call it) is a religion that resulted the combination of native African religions with Catholicism, by the slaves shipped to Haiti during the seventeenth century to work on the sugar plantations. As with many religions, Voodoo has evolved its own language and its own ontology. Amongst the entities recognised by the <u>houngans</u> (priests) and <u>mambos</u> (priestesses) of Voodoo, are the <u>loa</u> or pagan deities. In the

Voodoo ceremony, a snake representing the loa is caged on an altar in a circle. The mambo seats herself on the box and is then 'penetrated' by the snake; 'she writhes, her whole body is convulsed and the oracle speaks from her mouth' according to the French observer, Moreau de Saint-Mery.

From the point of view of an observer of such a ceremony, coming to terms with the ontology and ceremony of Voodoo, there are two different perspectives from which he can view this ceremony. He can take the viewpoint of the houngans or the mambos themselves and assent to sentences like 'The loa has taken possession of the mambo' or equivalently 'It is true that the loa has taken possession of the mambo'. To use the language of voodoo disquotationally in the manner is to commit oneself to the ontology of voodoo. Alternatively the observer can be more circumspect. He can rather choose to say; 'Amongst Voodoo worshippers, in this situation, it is usual to say 'The loa has taken possession of the mambo''. Such a statement can be consistently qualified by 'But of course it is all nonsense, there are no loa'. In making his initial remark, the observer is not referring to loa, but to the customs, practices and language of Voodoo. His remark leaves it open to him to decide if he wants to accept the voodoo ontology or not. To use the language of Voodoo quotationally, or as I shall say in future, in the formal mode is to separate oneself from the beliefs of Voodoo and to place oneself outside its ontology.

To take another example, consider set theory. Set theory has its own ontology of abstract objects and its own language. To participate in the practice of set theory and to use its language is to commit oneself to the ontology that the language of set theory characterises. An ontologist cannot say 'There is only one empty set' or 'There are a denumerable number of finite ordinals' and then add 'But of course, really, there are no such things' without contradiction. On the other hand he can learn to say 'Amongst those who accept set theory and its practices, it is true to say 'There is only one empty set' and 'There are a denumerable number of finite ordinals" and <u>then</u> add 'But of course, really, there are no such things' without contradiction.

In order to master the art of understanding when a judgement counts as true relative to an ontology presupposed, it is only necessary to learn the language used to characterise that ontology. For those skeptical of this ontology, this process of learning is akin to a game or a piece of anthropological study. The anthropologist who studies Voodoo worship has to master the sense of the special language of Voodoo in order to see when a judgement is deemed true relative to the native ontology. An anthropologist who studies colonies of university logicians can come to learn enough set theory to predict what will count as true in the characterising language of set-theory.

The point of learning to judge of truth in a characterising language comes out directly in formalisation. For instance, suppose an ontologist A is asked to formalise the sentence.

'There are as many electrons as protons in every uncharged atom'.

A responds to the challenge by producing:-

'$(x)(\text{ uncharged } x \ \& \text{ atom } x) \supset \# \{y\text{: electron } y \ \& \ y \leq x\} = \# \{y\text{: proton } y \ \& \ y \leq x\}$'

This says that for any uncharged atom x, that the cardinalitiy of the set of all electrons that are parts of x is identical to the cardinality of the set of all protons that are part of x.

The natural language sentence 'There are as many protons as electrons in every uncharged atom' may not agree in sense with the above formal sentence; but that is not the point. Presumably though, A will wish to claim that both sentences are true in virtue of the very same state of affairs, and hence that they are at least materially equivalent. Now scientists inform us that there are as many electrons as protons in every uncharged atom; so the natural language sentence can be taken as true. But is the formal language sentence true? There is obviously a clear ontological commitment to sets in that formalisation, and if sets do not exist then the formal sentence is false. Therefore in order to be justified in asserting that the formal sentence is true, A should first be justified in asserting sets exist. But this is just the kind of justification that the procedures of ontology are supposed to provide.

The point of this example is not to raise the question of set theory's ontological qualifications. The point is that if a necessary condition of successful formalisation is that material equivalence be maintained between the formalising sentence and the natural language sentence formalised, inevitably the ontologist will be propelled into making some truth-value assessment of the formal language sentences. But these same formal language sentences are often ontologically biased in favour of certain ontologies. It then becomes impossible to make large scale decisions about their truth-value without first deciding for or against the ontologies they characterise. But such prejudged decisions mean, in effect, that the issues on which formalisation is supposed to pronounce, have already been pronounced upon.

The way out of this problem is to insist that the ontologist vindicate his ontology, by showing that he can systematically parse, not truths into truths and falsehoods into falsehoods; but instead truths as they are generally deemed within science into sentences which are true relative to his ontology (or true, in his characterising language). To put the matter most succinctly, the ontologist displays the merit of his ontological hypothesis by showing that whatever truth we want to claim of the world as it is, can be rewritten as a truth about the world as the ontologist conceives it to be.

There are two incremental points to make here. The first is that since an understanding of the sense of a characterising language is essential to grasping what counts as true in that characterising language, the study of how the elements of characterising languages can have sense is of great metaontological interest. Metaontology intersects with the primary goal of semantics in its interest in what gives a sign meaning. For the semanticist, the justification for this interest is that this is what semantics is about. For the metaontologist, the justification is that in studying the sense-conditions of formal language (the conditions under which their elements have sense) we learn what conditions a formal language has to satisfy in order for judgements about truth relative to an ontology (that characterising language), to apply. In the next chapter, the sense-conditions of first-order languages will be explored in depth.

The second point to make is about that fragment of some natural language used to express our science. The same remarks that were made about the language of Voodoo and set theory apply to this language too. Science, with its talk of vectors, chain-reactions, fields, drives and psychoses is shot through with as many apparent ontological commitments as set theory or Voodoo. It would be quite feasible to adopt an independant attitude to the language of science and talk of truth relative to the ontology it characterises. The facts are however, that I and whoever is likely to be reading this work, are both involved directly or indirectly with the community of beliefs and practices that constitute Western science. We do not use the language of scientific theorising in the formal mode, as observers of our own linguistic science. We use the language of science disquotationally as participants, experts or amateur, contributors or passive recipients, in the body of doctrine within which we have been raised. Whether this body of doctrine and practice is in any way superior to that of Voodoo is a question that would take us far beyond the confines of this essay into an examination of what Wittgenstein would call Forms of Life. Feyerabend [44] has intimated that as far as he is concerned the credentials of Voodoo are as good as those of science: neither has any rational foundation. I for one disagree, and though I recognise a difference in sense between 'true relative to the ontological presuppositions of modern science' and 'true', in practice they will be taken here to be materially equivalent. To follow this course is to accept, however provisionally, what experts in their various fields take to be true. This is the price of employing the language of science disquotationally: that one accepts whatever ontology modern science requires.

(6) 'Formalisation is achieved in the construction of formal frameworks. A formal framework is an axiomatic machine for correlating natural language sentences with formal language sentences. Since this is its sole purpose, construction of a formal framework need and should not presuppose the correctness of any ontology. Formal frameworks merely talk about signs, that is, they are written in the formal mode.'

The task of the ontologist is to systematically associate each sentence of the (natural) language of science with a formal sentence of his chosen formal language. This chosen formal sentence is designed to represent what the natural language sentence is concerned with; but in an ontologically more perspicuos manner. Let us follow the practice of calling the formal sentence S_f that is placed to formalise the natural language sentence S_f, a formal image of S_n, and S_n an informal image of S_f. A formal framework is that which is designed to associate formal and informal images together.

There are a denumerable number of sentences in any natural language and it is to be expected that the language of science will be no less rich in this respect. The association of formal and informal images cannot proceed by enumeration alone, since such a procedure would be endless. But the means of associating sentences of denumerably rich languages together is essentially simple, if, as Tarski and Davidson have shown, an axiomatic approach is followed. From a finite number of axioms, an infinite number of theorems can be deduced. In order that a formal image be associated with an informal image, both would have to be brought together in a theorem. So let F be a formal framework, S_f is a formal image of S_n just when there is a theorem:-

$$\vdash_F (S_n \ldots\ldots S_f)$$

where '........' indicates our ignorance, for the moment, of what the proper form of such theorem might be.

Let S_n = 'There are as many protons as electrons in every uncharged atom' and let S_f = '(x)(uncharged x & atom x)$\supset \#\{$y: proton y & y $\leq$ x$\}$ = $\#\{$y: electron y & y $\leq$ x$\}$. One possible candidate for completing '.....' might be ' '. Thus there would be:-

$\vdash_F$ There are as many protons as electrons in every uncharged atom
$\equiv$ (x)(uncharged x & atom x)$\supset \#\{$y: proton y & y $\leq$ x$\}$ = $\#\{$y: electron y & y $\leq$ x$\}$

But this is not a convenient form for theorems of F to take. It cannot be determined if the above theorem is <u>true</u> without determining whether S_f is true. What is wanted is that S_n be true relative to the ontology of science iff Sp be true relative to our chosen ontology. But a very simple device will put this right. Encase both S_n and S_f in quotes, turning them into structural names. Append 'true in the ontology of science' to the structural name of S_n (or just 'true$_s$' for short) and 'true in our ontology O' (or 'true$_o$') to the structural name of S_f. Finally join the sentences thus generated by an equivalence sign '$\equiv$' to generate the archtypal form of a theorem of F:-

$\vdash_F$ 'S_n' is true$_s \equiv$ 'S_f' is true$_o$.

It can then be determined whether this sort of equivalence is true, independently of what ontology we are prepared to accept.

The easiest way to show how an axiomatised theory can give theorems of this form is to construct such an axiomatised theory and then to draw attention to the salient features. This is how I will proceed.

3.2.1 A Fragment of a Formal Framework

The axiomatic theory shortly to be presented is not a formal framework in the truest sense. The range of sentences formalised falls far short of that required to express our science. Nevertheless it is sufficiently representative of what a formal framework would look like for it to be useful in illustrating the issues at stake.

Initially, a portion of natural language is required to be formalised. I call the language under formalisation the target language. Here, L_t is the target language. In sum, L_t consists of the following elements:-

(a) a list of English proper names 'Vesuvius, Italy, Leibnitz, Manhatten....'

(b) The copula 'is'

(c) The numerals '0,1,2,3,4,5,6,7,8,9'

(d) The nominalised adjective 'length'; also 'long'

(e) The prepositions 'in' and 'of'

(f) The unit of measure, 'miles'

The vocabulary of L_t permits the formation of sentences like the following:-

'Manhatten is 11 miles long'

'Manhatten is 11 miles in length'

'The length of Manhatten is 11 miles'

'The length in miles of Manhatten is 11'

The formal language L_f that we shall use to formalise L_t is the canonical language of the formal framework. Here L_f is a first order language composed as follows:-

(a) The usual list of logical constants from the predicate calculus plus a plentiful supply of variables.

(b) Primitive closed terms divided into two classes

  (i) the names found in L_t, 'Manhatten, Italy...' etc.

  (ii) terms formed by joining a numeral to the word 'miles'; as in '11 miles'.

(c) The 1 - place function-expression 'the length of'.

In addition to L_t and L_f, there is a third language required, L_m. L_m is the metalanguage of the formal framework; that is, the language used to talk about L_t and L_f. L_m is another first-order language composed as follows:-

(a) The usual list of logical constants from the predicate calculus plus a plentiful supply of variables.

(b) Primitive closed terms which are structural names of all the elements of L_t and L_f.

(c) The sign for concatanation '$\frown$'; (this can be treated as a 2-place function expression)

(d) The 1-place predicates; 'true L_t' (meaning 'true in the characterising language L_t') and 'true L_f' (meaning 'true in the characterising language L_f') and 'NAME' and 'NUM'.

The axioms of the formal framework are as follows. First 11 axioms which define what a NUM (numeral) counts as:-

(1) NUM '0'

(2) NUM '1'

(3) NUM '2'

(4) NUM '3'

(5) NUM '4'

(6) NUM '5'

(7) NUM '6'

(8) NUM '7'

(9) NUM '8'

(10) NUM '9'

(11) (x)(y) (NUM x & NUM y) $\supset$ NUM $x^\frown y$

The second set of axioms defines the extension of 'NAME'. For simplicity, I shall use only one axiom;

(12) NAME 'Manhatten';

although it would be easy to extend the list indefinitely to include other NAMES.

In L_t the sentences

'Manhatten is 11 miles long'

'Manhatten is 11 miles in length'

'The length of Manhatten is 11 miles'

'The length of Manhatten in miles is 11'

are treated as equivalent. Axioms (13)-(15) sum this up.

(13) (x)(y) (NAME x & NUM y) $\supset$ (true$_{L_t}$ ($x^\frown$'is'$^\frown y^\frown$'miles'$^\frown$'in'$^\frown$'length')

$\equiv$ true_{L_t} (x $\frown$ 'is' $\frown$ y $\frown$ 'miles' $\frown$ 'long')

(14) (x)(y) (NAME x & NUM y) $\supset$ (true_{L_t} (x $\frown$ 'is' $\frown$ y $\frown$ 'miles' $\frown$ 'in' $\frown$ 'length')

$\equiv$ (true_{L_t} ('The' $\frown$ 'length' $\frown$ 'of' $\frown$ x $\frown$ 'is' $\frown$ y $\frown$ 'miles')

(15) (x)(y) (NAME x & NUM y) $\supset$ true_{L_t} ('The' $\frown$ 'length' $\frown$ 'of' $\frown$ x $\frown$ 'is' $\frown$ y $\frown$ 'miles')

true_{L_t} ('The' $\frown$ 'length' $\frown$ 'of' $\frown$ x $\frown$ 'in' $\frown$ 'miles' $\frown$ 'is' $\frown$ y)

(16) relates L_t sentences to L_f sentences.

(16) (x)(y) (NAME x & NUM y) $\supset$ true_{L_t} ('The' $\frown$ 'length' $\frown$ 'of' $\frown$ x $\frown$ 'is' $\frown$ y $\frown$ 'miles')

true_{L_f} ('the length of' $\frown$ x $\frown$ '=' $\frown$ y $\frown$ 'miles')

From (1) - (16), it emerges as a theorem that:-

true_{L_t} 'Manhatten is 11 miles long' $\equiv$ true_{L_f} 'the length of (Manhatten) = 11 miles'

Granted Manhatten is 11 miles long, then the equivalence is true if and only if 'the length of Manhatten = 11 miles' is true_f. Whether this will be so or not will depend on the ontology presupposed by the canonical language L_f. If this ontology includes impure numbers (of which 11 miles, 5 kilos, are examples) then it will be true (at least approximately) that 'the length of Manhatten = 11 miles' is true. If on the other hand, the ontology under examination excludes impure numbers but includes natural numbers, then an alternative formalisation is called for - and an alternative canonical language.

To illustrate, let L_t and L_m be as before. However L_f is changed, as follows L_f now contains.

(i) The usual list of logical constants from the predicate calculus plus a plentiful supply of variables.

(ii) Primitive closed terms divided into two classes.

   (a) the proper names of L_t

   (b) numerals

(iii) The 1 - place function expression, 'the length in miles of'

The axiomatisation is exactly the same as before except instead of axiom (16) we have:-

(16)' $(x)(y)\ (\text{NAME}\ x\ \&\ \text{NUM}\ y) \supset (\text{true}_{L_t}\ (\text{'The'} \frown \text{'length'} \frown \text{'of'} \frown x \frown \text{'is'} \frown y \frown \text{'miles'})$
$\text{true}_{L_f}\ (\text{'the length in miles of'} \frown x \frown \text{'='} \frown y))$

From (1) - (16)' it follows as a theorem that

true L_t 'The length of Manhatten is 11 miles' $\equiv$ true L_f 'the length in miles of Manhatten = 11'

This formalisation is suited to an ontology which countenances natural numbers.

In passing one obvious feature of formalisation is worth noticing. Though I have presented two formalisations, they are both formalisations of one target language. Statements of measure can be construed either to an ontology of impure numbers or to an ontology of natural numbers. The question of which formalisation best captures 'the logical form' of these statements is a spurious one that deserves no answer, and indeed has none. The capacity of a theory to sustain competing formalisations to different domains is a measure of the ontological elasticity of the theory. (Ontological elasticity will be examined in chapter 5).

An important feature of a formal framework is that since it is concerned merely with correlating sentences, it is purely metalinguistic, or as I shall say, it is written in the formal mode. The extensions of the variables in L_m are purely concerned with syntactical items. A syntactical item is any fragment of a language, a letter, mark, sign, word, phrase, clause, sentence, or series of sentences. Where t is any first-order theory, t is in the formal mode if, and only if:-

(1) The range D of the bound variables of t (if any) includes only syntactical items

(2) Where v is any logical variable of t:-

$(x)\ x \varepsilon \mathcal{U}(EX(v)) \supset x$ is a syntactical item

i.e. every element of the set of ultimate factors of the extension of v is a syntactical item.

The requirement that formal frameworks be written in the formal mode poses certain constraints on the way axioms are laid down. For example, suppose that it is hypothesised that sets exist and the canonical language L_f contains the language of set theory. It may be desirable, in the interests of deriving substantive theorems in our formal framework, to include certain axioms of set theory. One such axiom is the Power Set Axiom.

$$(A)(\exists B)(C)(C \varepsilon B \supset C \subseteq A)$$

Where 'A', 'B' and 'C' range over sets. But expressed in the form above, the Power Set Axiom is not in the formal mode. The Power Set Axiom cannot be accepted in its usual interpretation without providing at least some argument for recognising the existence of sets. Consequently the truth of any formal framework containing the Power Set Axiom, cannot be established unless it is first established that sets do exist. This is just the sort of circularity that must be avoided at all costs. It can be avoided if the Power Set Axiom is ontologically neutered by being put in the formal mode, thus:-

'$(A)(\exists B)(C)(C \varepsilon B \supset C \subseteq A)$' is true_f

Another constraint that expression in the formal mode produces is that if we wish to reason about the canonical language, then the principles of reasoning have themselves to be stated. For instance we may wish to infer 'true_f '$(\exists x)$ $x = 11$ miles'' (it is true in the ontology L_f characterises that 11 miles exists) from 'true_f 'the length of Manhatten = 11 miles'. This cannot be done straightforwardly as in :-

$$\frac{\text{true}_f \text{ 'the length of Manhatten = 11 miles'}}{\text{true}_f \text{ '}(\exists x)\ x = 11 \text{ miles'}}$$

Since this has the form of a logically invalid argument $Fa \vdash Fb$. Instead the principles of reasoning appropriate to L_f have to be stated axiomatically. Such an axiomatisation will state the permitted syntactical transformations used in L_f derivations. To give a fully worked example of such an axiomatisation would necessarily involve much space. Instead I shall give a partly worked example sufficient to illustrate the technique.

To expand the formal framework state previously so as to handle derivations in L_f the language L_m has to be expanded. Let L_m+ be the expansion of L_m, defined as follows.

L_m+ contains

(i) L_m;

(ii) the 1 - place predicate 'VAR' where EX'VAR' is the set of bindable variables of L_f

(iii) the 1 - place predicate 'CLT' where EX'CLT' is the set of closed terms of L_f;

(iv) the 1 - place predicate 'STR' where EX'STR' is the set of strings in L_f. A string in L_f is a concatanation of n ($n \geq 0$) elements of L_f. The blank string ' ' is a string in L_f;

(v) the 2 - place predicate 'OCC' where EX'OCC' is the set of ordered pairs <a,b> where a is a character and b is a string such that a is occurs in b;

(vi) the 3 - place function expression 'SUB' where EX'SUB' is a function such that SUB (a,b,c) is the result of substituting the string a for the string b throughout the string c. (e.g. SUB('ket','k','mark') = 'market').

The significance of 'VAR', 'CLT', 'STR', 'OCC', and 'SUB' would be stated axiomatically in the way that 'NAME' and 'NUM' were. Supposing this were alone, the rule of <u>Existential Generalisation</u> could be stated in L_m+ for L_f

$$(w)(x)(y)(z)(\mathrm{VAR}\ w\ \&\ \mathrm{CLT}\ x\ \&\ \mathrm{STR}\ y\ \&\ \mathrm{STR}\ z) \supset (\mathrm{true}_{L_f}\ (y^\frown x^\frown z)$$
$$(-\mathrm{OCC}\ (w, y^\frown x^\frown z) \supset \mathrm{true}_{L_f}\ (\mathrm{SUB}\ (w, x, \text{'}(\exists\text{'}^\frown w^\frown\text{')('}^\frown y^\frown x^\frown z^\frown\text{')'})$$

In semi-formal English, this axiom states:-

for any variable w, closed term x, and strings y and z; if $y^\frown x^\frown z$ is true_{L_f}, then so is the result of substituting w for x throughout $\text{'}(\exists\text{'}^\frown w^\frown\text{')('}^\frown y^\frown x^\frown z^\frown\text{')'}$ where w does not occur in $y^\frown x^\frown z$.

The constraints in expressing a formalisation in the formal mode may seem onerous; but the formal mode is good insurance against building ontological presuppositions into a formalisation. Expression in the formal mode also allows for an extremely simple criterion of adequacy in formalisation. This is that <u>every theorem of a formal framework be true</u>. By an <u>adequate</u> formal framework is meant one which satisfies this criterion. An adequate formal framework is, amongst other things, a true finitely axiomatised theory written in the formal mode.

Let $\mathcal{F}$ be any adequate formal framework and let the target language L_t of $\mathcal{F}$ be rich enough to express any scientific assertion we might reasonably wish to make. Let Γ be the set of all and only those sentences of L_t which are true. The set Δ is the set of all those sentences s of L_f where s is a formal image under $\mathcal{F}$ of some element of Γ. Now let Δ be expanded to Δ' by rendering Δ' deductively closed in respect of Δ. Δ' will be what I call a model world.

Model worlds are extremely interesting both from an ontological and a metaontological point of view. Ontologically they are important because they represent the fruition of an ontological point of view or hypothesis. A model world purports to describe the contents of the universe; their properties, and the laws that regulate them. We can say that a model world is the ideal end of the ontologist's efforts: it reflects one way of answering, in depth, the question as to what there is. Metaontologically, model worlds are interesting because they offer a means of defining that extremely intractable concept, existence. In conjunction with ontological elasticity, looking at existence in terms of model worlds challenges some classically held opinions about the logical structure of the universe (see 5.422).

3.3 Problems and Tentative Theories

According to Popper [105] (164), there is a general structure to the evolution of human thinking which is represented by the schema:-

$$P_1 \longrightarrow TT \longrightarrow EE \longrightarrow P_2$$

P_1 is an initial problem and TT is a tentative theory, designed to solve P_1. TT is then examined and criticised in the process of error exposure EE which throws up problem P_2; the cycle then repeats itself.

In this work, the problem P_1 has been to devise some methodology for answering the ontological question as to what there is. TT, the tentative theory, is the methodology outlined in this chapter.. In this section the focus is on EE, the process of criticism and evaluation, and on P_2, the problems which are generated as a result of error exposure. I should record here, for posterity, a debt to Dr. J.E. Tiles for his help in the process of error exposure. Any deficiencies in the tentative theories designed to meet the problems that error exposure generates are my responsibility.

The essence of the methodology suggested previously is that formalisation is a means of testing ontologies, in a manner analogous to the role that experiment and observation play in testing scientific hypotheses. In Popperian methodology of science, a scientific hypothesis is said to be corroborated, if the observations and experimental results that are made, are the ones that can be expected if the hypothesis is true. I say that an ontological hypothesis is corroborated just when the ontologist succeeds is systematically parsing our most successful theories into a notation of his own choosing, so that these theories are mapped to assertions which are true relative to his chosen ontology. But does corroboration of an ontological hypothesis entail verification of that hypothesis and does failure to corroborate the hypothesis entail falsification of that hypothesis? If not, can ontological hypotheses be verified or falsified at all? Again, if the answer is 'no', is there any point to the pursuit of ontology? There is a compelling argument to the effect that all these questions should be answered in the negative. I shall state it and then consider what its real significance is. The argument splits into two parts; the first establishing that corroboration does not entail verification, and the second that lack of corroboration does not entail falsification.

As regards the impossibility of verification, the argument runs as follows. Science is subject to change; new theories are suggested to replace old theories which have failed to compete as successfully, new observations are constantly made, improvements of measurement are recurrent and new ideas bring new expressions into the language of science. The ontologist who tries to vindicate his hypothesis H has to work within an artificially frozen model of science. The ontologist has to isolate a language Ls euphemistically called 'the language of science' ignoring the influx of new vocabulary that is constantly occuring. The ontologist qua ontologist has to accept the current valuation of Ls sentences though he must be aware some of these valuations will be wrong. If he succeeds in constructing a 'satisfactory' framework then he will have had to make a number of presumptions prove to be wrong then his carefully constructed framework will be wrong too. Since it is never possible to be completely certain of the results of science one can never claim that an ontological hypothesis is verified i.e. proved to be true. All one is entitled to say is that such a hypothesis is compatible with all that is currently accepted in professional scientific circles as true.

As regards the impossibility of falsification, the argument continues: to fail to corroborate a hypothesis against current science is not to have that hypothesis falsified. First, it may be that the current state of sciences is wrong and wrong in some ontologically vital area of research where results seem to be inconsistent with the hypothesis. Second, failure to find an adequate formalisation enshrining the hypothesis does not entail there is not such a formalisation. Perhaps if the search proceeded for a little longer eventually the apparent difficulties would be solved. Since ontological hypotheses are neither verifiable or falsifiable, the argument winds up, they are not scientific hypotheses in the truest sense.

Now though I believe the conclusion of this argument to be unjustified by the observations that precede it, it must be admitted that the above criticism does raise epistemological questions of the first importance: the principal are being whether we are ever justified in ascribing a truth-value to a sentence in the absence of absolutely conclusive evidence as regards its truth or falshood.

It is only right to begin by acknowledging that ontological hypotheses are neither verifiable nor falsifiable in the strong sense intended by the critic. I take this mean something like this: for any substantive ontological hypothesis H, and for any given moment t, we can never say with certainty that evidence will not be available after t which will lead to a reevaluation of truth-value assessment at t of H. However I also think precisely the same could be said of a great many theories of natural science and so if ontology is to be rejected on these grounds, it will at least go down in distinguished company.

That unlimited generalisations can always be overturned by unexpected observations is the foundation of the Problem of Induction and Popper's philosophy. This fact has let most philosophers to recognise that no matter how well corroborated a scientific theory is it can never be said to be verified and the supercession of Newton's by Einstein's theories of motion is often offered as a paradigm case of a well-corroborated theory turning out to be false. Falsification is often claimed to be asymmetrical to verification in this respect: that an unlimited generalisation can be falsified but not verified.

Though of course it is true that the truth of any statement of the form $(x)\, Fx \supset Gx$ is disproved by the truth of one of the form $Fa\ \&\ -Ga$, it is an oversimplification to suppose that the refutation of any scientific theory can proceed in so direct a manner without any ancillary assumptions. Such ancillary assumptions often include assumptions as to the reliability of the scientist's equipment and its accuracy in performing its allotted task. Sometimes those assumptions go badly wrong as they did ith Kaufmann in his 1901-1906 experiments of the relation of the inertial mass of electrons in relation to their velocity in terms of the speed of light. Apparently Kaufmann's results 'refuted' Einstein's theories on the subject. It was only ten years later that it was generally realised that Kaufmann's equipment was inadequate. Even the famous eclipse observations made in 1919 to test the fruits of Einstein's General Relativity Theory were equivocal. The Sobral data gave a displacement of the stars' light of 1.98 seconds with an error of ± 12 seconds while the Principe experiment gave a displacement of 1.61 with an error of ± 30 seconds! (Einstein's prediction was 1.72 seconds). Nevertheless scientists like Eddington were prepared to abandon Newton for Einstein on the assumption that the data furnished was sufficient. (See Bernstein [15] (80-83, 177-119)).

In other cases, the auxiliary assumptions relate not equipment, but to auxiliary hypotheses, which have been assumed in the apparent 'refutation'. A case instance was Prout's hypothesis of 1815: that the atomic weights of all pure chemical elements were whole numbers. The history of chemistry for the next 100 years was of attempts to test Prout's hypothesis with discouraging results: the atomic weights of many elements were not whole numbers. Only in this century, with the discovery of isotopes was it realised that it was not possible to get a pure sample of any one isotope by purely chemical means. But before the existence of stable isotopes was demonstrated in 1912 by Thomson, many scientists such as Stas and Maxwell thought Prout's hypothesis had been refuted by the persistant lack of success that chemists had in finding pure samples by chemical means that had whole atomic weights. (See Lakatos [78] (53-54), Crosland [32] (269-279).

One may sum this up by saying that many observation statements are only falsifying under certain theoretical assumptions, and because of this fact, rejecting a theory on the basis of such observations is often to lay oneself open to contrary evidence against one's decision later. Both acceptance and rejection of a scientific theory often have to proceed in the knowledge that later evidence may prove decisions wrong; therefore acceptance and rejection must function independantly of verification and falsification as they have been defined here.

Precisely the same conclusion applies to ontology. We cannot reasonably hope for either total verification or a quick kill. What we do have the right to expect is that it be possible to make reasoned assessments of the truth-values of ontological hypotheses as we can do for scientific ones. In this respect our right to call an ontological hypothesis 'true' is founded on much the same general principles as any scientific hypothesis. If the ontological hypothesis successfully handles the data we have already got, then we are justified in accepting it as true as long as it continues to be successful.

In respect of calling an ontological hypothesis 'false' the issues are more complex. On the basis of what I call 'dogmatic falsification' <u>theories</u> are demarcated as scientific or unscientific according to the prescence of a set of falsifying basic statements (see Popper [103]). Popper, sometimes following dogmatic falsificationism, recognises that there are certain hypotheses which must consequently be ranked as unscientific. Indefinite existential statements such as 'There are tachyons' become unscientific, because no observation sentence is inconsistent with it. Similarly probability statements about the distribution of a property in a denumerable or very large population are, strictly speaking, unscientific, since no accessable sample can give results which are actually inconsistent with that hypothesis.

This is a mistake; fundamentally there are no <u>falsifiable</u> <u>theories</u> at all, and the appellations 'scientific' and 'unscientific' apply not to theories but to theorising and indirectly to theorists. To illustrate.

The field of paranormal psychology is one which lies, at the present, on the borders of respectable science. It is a subject which has always engendered fierce argument, and very roughly there have been three attitudes to it

(A) <u>Total Scepticism</u>. All 'paranormal phenomena are the result of concious or unconcious deceit or illusion.

(b) <u>Partial Compromise</u>. There are genuinely paranormal phenomena for which researchers at present have no explanation. But one day all such phenomena will be accommodated in a framework of explanation which uses principles which are part of natural science in the same way that comets and eclipses were gained for science from occultism.

(c) Total Acceptance. There are genuinely paranormal phenomena which can only be explained in the context of a spiritual or religous framework.

Professors X and Y are total sceptics. Over the years of patient research, X and Y uncover cases of honest mistake and dishonest fraud, X and Y uncover cases which are recalcitrant to interpretation in the light of total scepticism. Professor X admits that the total sceptical position looks to be false. Professor Y refuses to accept this conclusion and continues to believe, on the basis of past experience, that these cases are not cases of bona fide paranormal phenomena.

Question: is the hypothesis of total scepticism falsifiable or not?

The answer to this question is that there is no answer. We can say'As interpreted by X, it is falsifiable, but as interpreted by Y, probably not'. The actual syntactical form of the hypothesis - the fact it takes the shape of an unlimited generalisation - does not say how it is to be used. As Popper [104] has observed, any theory can be rescued from apparently falsifying evidence, if we are prepared to make the appropriate concessions. The natural corollary to this is that it is wrong to apply the criterion of falsifiability to a theory, but only to apply it to an interpretation or use of a theory, by a theorist. Methodological falsificationism refuses to play the game of dividing theories into falsifiable and nonfalsifiable, scientific and unscientific.[3] Methodological falsificationism argues that it is theorists and not theories that are scientific or unscientific. Interpreted methodologically, the criterion says that a theorist A is scientific in respect of an adopted hypothesis H just when A is willing to specify some experimental situation E, such that if result R came from E, A would abandon H.

A consequence of methodological falsificationism is that indefinite existential and probability statements are restored to respectability. A probability hypothesis H about the distribution of a property P amongst a very large S, may not be 'falsifiable' in the strict sense. (i.e. such a hypothesis may be consistent with any set of observation statements we can reasonably be expected to evaluate). Nevertheless H can be used or treated scientifically, if it is stated clearly under what conditions H will be abandoned. Such a statement would state the minimum size of any sample of S sufficient to overturn H, and the minimum deviation from the predicted occurrence of P in that sample to overturn H. This minimum sample and minimum deviation need not <u>entail</u> the falsity of H.

Methodological falsificationism leads into the subject of epistemology, and, in particular, the rational determination of belief. To require that a hypothesis be accompanied by a statement of the conditions under which it should be abandoned, is not to state how those conditions, in general, are to be arrived at. Thus methodological falsificationism shifts interest away from the study of <u>truth</u> (and falsehood) <u>conditions</u> to that of <u>assertability</u> (and denial) <u>conditions</u>.

I believe that one of the principal problems of epistemology is to determine the general methodology whereby assertability and denial conditions are determined. Previous epistemology has concerned itself too much with a model of inviolate and celestial <u>knowledge</u> based on <u>total</u> evidence and too little with the rational determination of <u>belief</u> in the light of <u>partial</u> evidence. This is not a work in epistemology and so this is not the place to embark on such a grand design as the one suggested for epistemology. However as regards <u>ontology</u>, the methodology for arriving at the assertability and denial conditions of ontological hypotheses is very much within the ambit of meteontology. The ensuing remarks concerning the assertability and denial conditions of ontological hypotheses are, however, generalisable to other areas of human discovery.

The model of the evolution of theory formation offered by Popper offers a good entry for this discussion. This model was represented by the schema:-

$$P_1 \longrightarrow TT \longrightarrow EE \longrightarrow P_2.$$

The model is oversimplified in an important respect. In many problem situations where a problem P_1 is a focus of interest, there is not one and <u>only</u> one tentative theory TT put forward but <u>many</u> tentative theories TT_1, TT_2, TT_3,...... . Each of these theories is subject to evaluation and each can generate its own particular problems. Consequently, in place of Popper's simple linear model of problem and theory generation there is a more complex tree structure.

$$\begin{tikzcd}
 & TT_1 \arrow[r] & EE_1 \arrow[r] & P_1 \\
P_0 \arrow[ur] \arrow[r] \arrow[dr] & TT_2 \arrow[r] & EE_2 \arrow[r] & P_2 \\
 & TT_3 \arrow[r] & EE_3 \arrow[r] & P_3 \\
 & \cdots & \cdots & \cdots
\end{tikzcd}$$

In this kind of problem situation it frequently happens that for any TT_i and TT_j, where $-(i = j)$, that TT_i and TT_j are inconsistent with each other. Consequently no competing theory can become established without doing so at the expense of its rivals. In other words the assertability and denial conditions of TT_1, TT_2, TT_3, ... etc., are logically tied together to this extent:- that for any TT_i, if TT_i is warrantably assertable then for any TT_j, TT_j is warrantably deniable. This fact gives some ingress into the problem of determining under what conditions it is correct to (provisionally) deny an ontological hypothesis and under what conditions it is correct to affirm it.

Unlike certain hypotheses of empirical science, the denial conditions of an ontological hypothesis cannot be identified with a possible result R from a crucial experiment E. We cannot perform a laboratory test to detect the existence of sets or ideas in the way we might test for the existence of a new subatomic particle. Our suspicions that an ontological hypothesis is unworkable increase so long as efforts to construct an adequate formal framework enshrining it go unrewarded. But it cannot be said that any ontological hypothesis H ought to be abandoned, if, after x man-hours of effort, no satisfactory formal framework has been found to incorporate H. First, any value we fix for x will be more or less arbitrary. Second, there are other parameters which determine whether a formal framework of the right kind will be discovered e.g. the intelligence of the ontologist, his imagination, the degree of his application, the research facilities available to him. These parameters are not easily quantifiable, and it does not seem possible to state the denial conditions precisely in terms of them.

The solution is to define the denial and acceptability conditions of an ontological hypothesis in the context of a program of ontological research. In a program of ontological research a number of competing ontological hypotheses H_1, H_2, H_3,... are tested out in answer to the ontological problem as to what there is. The subject of formalisation is a natural language, or a fragment of such, of which there is a general agreement that that language is rich enough to formulate our consensus theories. The ontologist operating, from the standpoint of his chosen hypothesis, endeavours to represent the consensus theories by formal images which are true relative to his own ontology. Our confidence in his hypothesis grows as he shows that he can successfully accommodate more and more of our current science without overstepping the self-imposed boundaries of his ontology. But willingness to accept that hypothesis is constrained by the success of rival ontologists, working within the program, to formalise the same target language from the standpoint of rival hypotheses. The overall attitude of the rational observer is somewhat similar to that of the inscrupulous punter who charges his bets depending on which horse is in the lead. The assertability conditions of any hypothesis are fulfilled just when it is first in encompassing the largest range of consensus theories. Correspondingly the denial conditions of the trailing pack are fulfilled as long as the pack continues to trail. To prefer an ontological hypothesis on its ability to accommodate our currently most successful theories in the largest number possible, is to reason in much the same way as the empirical scientist who chooses a theory on the basis of its ability to organise and explain the widest range of empirical phenomena.

Denial and assertion, unlike falsification and verification, are essentially revisable. An ontologist who assents to the recommended methodology commits himself to denying any hypothesis, including his own, so long as it continues to be less successful than one which rivals it. But if an ontological hypothesis H thus fulfils its denial conditions then this need not mean that H should be dropped from the program of ontological research. The ontologist may continue to work at developing H; (metaontology issues no prescriptions on how to use up leisure time) in the hope that eventually H will assume the position of premiere hypothesis. Methodologically, it is advantageous to have a research program with many competing hypotheses as possible, so that survival of the fittest operates to best effect.

[1] In relation to what follows Popper [105] (106-150, 241-244) is relevant. Popper also views a theory as a problem-solving tool and has a number of interesting things to say.

[2] There is a modest but notable exception where metaontology does have an ontological verdict to pass: this is that physical objects exist. This conclusion puts paid to Idealism however (see 4.4).

[3] See again Lakatos [78]. I read Lakatos about midway in the evolution of this section, and the conclusions of his work were so in accordance with the development of my own thinking, that they more or less fused together.

CHAPTER FOUR

Making Sense: how to construct a formal language

4.1 What this is all about

This chapter is about how to avoid talking nonsense; for the production of nonsense is a vice as besetting to philosophers as insincerity is to politicians. In both cases the vice is only dangerous if it goes undetected, and I hope that anybody who reads this chapter will be able to avoid some shortcomings and perhaps detect some new ones in contemporary philosophy.

The principal concern of this chapter is with formal languages, specifically first-order languages: the reasons are not hard to find. In formalisation we endeavour to systematically replace each sentence of L by a canonical sentence which formalises it. The canonical sentence is a sentence of a formal language and therefore it should have some sense; it should not be merely a collection of ink marks. Now syntactically, of course, there is nothing to distinguish a collection of meaningless ink marks, provided they are put together to form a grammatical sentence, from a meaningful sentence. This fact has been the foundation of much misfortune to philosophy and the only corrective I know of is to examine the conditions of sense, and, by carefully determining what they are, to avoid the mistakes that philosophers have embroiled themselves in. This means that we become involved in the study of the sense-conditions (if that is the right word) of first-order languages.

In order to study the sense-conditions of first-order languages we have to look sideways, occasionally, at natural languages, to discern the general principles by which expressions of these languages gain sense. An analogy may help clarify.

Imagine that a man devoted himself to constructing the first heavier-than-air aircraft. He would be confronted with a task daunting even to the most persevering and heroic intelligence. He would be aware that there would likely be many parameters which would influence the airworthiness of any craft he built and that he knew hardly any of them. He would be confronted with the prospect of repeated bungles and painfully learnt lessons. Human intelligence, when confronted with titantic obstacles, naturally and rightly looks for a short way round them. One way in which our inventor might seek to circumvent some of the labour is by studying naturally occurring 'aircraft': that is, birds. By a judicious examination of naturally occurring flighted creatures he may learn enough to avoid some, if not all, of his worst bungles. He may for instance, reach the conclusion that his aircraft should have two slightly curved planes attached to some roughly cylindrical middle with a similar, but smaller plane at the back. Similarly in constructing artificial languages, it is useful to look at natural languages to see the sort of principles which should govern our construction. But a word of warning should intrude here. I am not about the dissection of the semantic principles of natural languages for their own sake. The kind of close, detailed examination of the usage and purpose of ordinary language idioms as is found in, say, Austin's <u>How to Do Things with Words</u> is not to be found here. Instead I am interested in the broadest and most significant features of natural languages: the ones that enable us to communicate at all. I shall deliberately ignore nuances, borderline cses, special instances and the like, in order to concentrate on what is important, (in a way that Austin might have frowned upon, were he alive today). Unlike Austin and some of his Oxford contemporaries who are still functioning, I do not place much value on linguistic observations carried out with no especial purpose but to make such observations. Philosophers, like scientists, should begin with a problem, and what observations about language are important are defined in relation to what is needed to solve the problem.

The preceding sentence is a guide to what the construction of formal languages is about. An expression has sense when there is somebody ('is' in the timeless sense peculiar to philosophy) who grasps that sense. A language has sense (can be employed for communication) when there is a language-community who can find an employment for it. To show that a canonical language has sense and hence that the canonical sentences have sense, it is necessary to show that such a language-community is possible. It is an Aristotelean dictum that if a thing is, it is also possible. So the most direct way that an ontologist can discharge his obligation of showing that his canonical language has sense is to create a language community which understands his canonical language. Such a community I call a <u>model language community</u> and designate any such arbitrary community by the letter 'M'. The ontologist creates a model language community by <u>teaching</u> his canonical language.

Faced with such a task, the ontologist may demand that the metaontologist supply some effective procedure, or at least some detailed methodology, for constructing a model community. The metaontologist should resist his demand. There is no effective procedure for constructing such a community; nor is the problem of finding the best methodology a <u>philosophical</u> problem. There are many ways in which the sense of an expression can be taught, and which is best is dependant on the nature of the expression. In some cases the sense of an expression may be communicated by associating it with a decision procedure that effectively settles its application. Secondary school students in science are taught many of their basic concepts in this way, e.g., x is acid if and only if x turns litmus paper red. But not all expressions are effectively applicable. 'Is a theorem of first-order logic' is not effectively applicable; neither is 'is lying' or 'is thinking', nor, most significantly is 'true'. Ostension is another popular device. But this technique is not suitable for 'is a proton', 'is a unicorn', 'is a unit set'.

Effective procedures and ostension are well-documented means of teaching parts of a language. There are others. Some of these belong in the realm of science-fiction, but may one day become fact; e.g., hypnotic learning, implantation of micro-translators in the brain, grafting speech-centres from one brain to another.

The question of how a language might or should be communicated is not a question of philosophy, but a question that concerns educationalists, psychologists and linguists. Wittgenstein in his classic, Philosophical Investigations, recognised and made use of the fact that there is no effective procedure for communicating the sense of an expression in order to refute representationalist theories of meaning. Whatever ceremony we imagine presented to the learner to help him grasp the sense of a word, it is always possible, Wittgenstein pointed out, for the learner to fail to grasp the point of the ceremony. In such a case the learner will fail to use the word appropriately. Once Wittgenstein's point is taken to heart, it is futile to search for an effective way of constructing a model community.

There is a task which the ontologist can and should require the metaontologist to undertake. The metaontologist should supply an account of the exact nature of the understanding that the members of the model community should have, in order to be truly said to understand the canonical language. Not how this understanding is produced (which is not a philosophical problem) but what such understanding amounts to (which is a philosophical problem).

The canonical languages that will be examined in this chapter are first-order languages, not only because first-order languages are accorded pride of place in logic and formal philosophy, but also because there is philosophical reason to do so (see chapter 7). The elements of the sentences of a first-order language are divided into logical constants like '$\vee$', '$\supset$', '&', '$\equiv$' etc., and logical variables. The senses of the logical constants are unproblematic given their extensive use in logic and the widespread knowledge of their semantics. The ontologist may reasonably be excused from explaining the senses of expressions we already understand. His job is to teach the senses of the logical variables to members of M. The metaontologist has to lay down the conditions by which it can be assessed whether these variables have come to have an agreed sense amongst members of M. The specific question the metaontologist addresses himself to is, 'What is it to grasp the sense of a variable?' Answers to this specific question vary according to the nature of the variable, as will become clear in subsequent sections.

4.2 E variables

Logical variables can be partitioned into predicates, function-expressions and names. The simplest way in which any person can display a grasp of the sense of a variable is through being able to identify the objects that fall under the extension of the variable. Variables whose sense is understood by X only when X has the capacity to identify items of their extension, I call 'extensionally accessable variables' or 'E variables' for short. The grasp of sense appropriate to E variables is the sort of grasp parents try to produce in their children when teaching them their earliest words. Unless a child recognises that a car is correctly described by 'car' then adults take it that he does not grasp what 'car' means. This sort of case is so familiar that it tends to produce a philosophical blindness to the important aspects of E variables. Consequently a step back or two into abstraction is useful in restoring the proper perspective.

In order for a variable V in a model language community M to have a sense within M, V must be usable in an act of communication or speech-act between two members of M. In any act of communication there are at least two participants, namely a speaker S and a hearer H. In order for S to communicate successfully with H via a speech-act which uses V there are 4 conditions which must be satisfied.

(1) S must attach a sense to V.

(2) H must attach a sense to V.

(3) The senses S and H attach to V must be the same.

(4) H must know (1), (2) and (3).

These conditions are evident after a little thought. If S should remark 'The snurd needs repairing' to H, and S attaches no sense to 'snurd' then no act of communication has taken place. Even if S does mean something by 'snurd', if H attaches no sense to the word, communication has broken down. Supposing that S and H do attach a sense to 'snurd', but that the sense each attaches is different, the H will misinterpret what S has said. Lastly, even supposing that S and H do mean the same by 'snurd', meaning door, unless H knows that S means 'door' then H will not be able to infer 'S said that the door need repairing'. H will not have the ability to extract the information-content from S's phonic act.

'Snurd' in this imaginary case is an E variable since 'snurd' means door and doors are easily recognised. The key features of the use of 'snurd' in relation to the 4 conditions laid out above are as follows:-

(1) S attaches a sense to 'snurd'

Comment: If this is true it is because S's usage of 'snurd' is regulated to particular items (i.e. doors) and it therefore predictable, as opposed to random, and unconsidered. A corollary of this is that if S attaches a sense to an E variable like 'snurd' then the outside observer can predict his usage of 'snurd' and thus acquire the same use of 'snurd' as S. Therefore a condition of S attaching a sense to an E variable is that it is possible to know what that sense is.

(2) H attaches a sense to 'snurd'.

Comment: As (1) with 'H' replacing 'S'.

(3) The senses S and H attach to 'snurd' must be the same.

Comment: If (3)' holds it is because S and H will identify the same objects as snurds even if screened from one another under laboratory conditions. If it is true to say of 'snurd' that it means door, this will be because members of M independently identify the same objects, doors, as snurds and identify the same objects as non-snurds. Conversely if 'snurd' has no uniformity in its application by members of M then it cannot be said to have a sense (though it may have a sense for a given speaker). Following Harrison [64] (3 - 21) I call the ability of a sign to have a meaning independent of any meaning fancy may attribute to it the autonomy of that sign. The autonomy of an E variable consists in the fact that members of the model community in which that variable has a use apply that sign to the same kinds of objects.

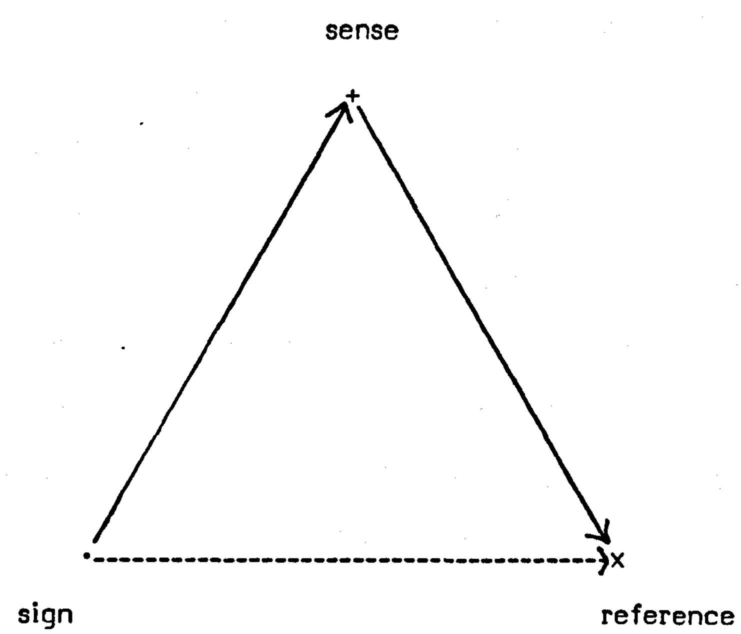
(4) H knows (1), (2), (3).

Comment: (4) is satisfied, if it is satisfied at all, because H has the ability to observe another's use of the word 'snurd' and to determine whether he applies the word to just the same objects H would. This goes for S too. Corollary: unless it was possible for H to recognise that S took as snurds the same objects as H did, (4) would not be satisfied. Call any object for which it is possible to determine that a language-user has referred to or identified that object, a public object. Unless the objects of the extension of an E variable were public objects, that E variable could not be used in an act of communication; it would be senseless.

Two important conditions of an E variable having sense, then, is that its sense be autonomous and its extension include public objects. These twin conditions will figure largely in the discussions that follow.

E predicates like 'car', 'boy', 'dog' have so far come under examination, but the case for E function-expressions is similar. The sense of an E function-expression is grasped only by being able to demarcate its extension: what counts as an argument to the function it denotes and what value is associated with that argument. The 2-place arithmetical function-expression '+' is an E function-expression. The sense of '+' is understood by anybody who knows how to add. Computable functions are in general signified by E function-expressions. Anybody who possesses the appropriate specification of computable function also has the means to effectively determine the value that function gives for any argument.

The later Wittgenstein possessed an advantage over his predecessors, especially Frege, in understanding how it was the E function-expressions stood for functions. Frege's explanation was that it was the sense of the function-expression that determined its reference; where the sense of an expression was an abstract object. i.e.



The broken line represents the indirect relation of referring whereby the sign is connected to its reference by its sense.

Frege's model is deficient in one vital respect that completely nullifies its explanatory value. There is no explanation of what binds a sign to its sense (or for that matter what binds the sense to the reference). On Fregean grounds we can either postulate a third entity to stand between a sign and its sense to bind the two together; but this would obviously begin a vicious regress; or we can say that the relation between a sign and its sense is ultimate and unanalysable. But in the latter case if we take this way out then we might as well say that the relations between the sign and its reference is ultimate and unanalysable and dispense with the assumption of a third mediating entity called a sense.

The later Wittgenstein recognised that for some arithmetical expressions what 'glued' the expressions to their extension was a human agreement as to what fell under the extension of that expression. Commenting on this sort of case, Wittgenstein remarked:-

"But are not the steps then <u>not</u> determined by the algebraic formula?' - The question contains a mistake.

We use the expression: 'The steps are determined by the formula....' <u>How</u> is it used? - We may perhaps refer to the fact that people are taught by their education (training) so to use the formula '$y = x^2$', that they all work out the same number of 'y' when they substitute the same number for 'x'. Or we might say: 'These people are so trained that they all take the same step at the same time when they receive the order 'add 3". We might express this by saying that for these people, the order 'add 3' completely determines every step from one number to the next'.

Wittgenstein [150] (185)

In natural languages, names are the odd kind out of the three different kind of E variable that obtain. First, names do not have a <u>meaning</u> in the way the predicates or function-expressions do. There is no synonym for 'George Washington'. Nevertheless names do have a sense. Names can be inserted into sentence-frames to make sentences and the truth-values of any declarative sentence so formed depends on the name used. If Dummett is right in saying that an expression has sense when it contributes to the truth-conditions of the declarative sentences in which it is used, then names have sense.

Second, in natural languages a name cannot be classed as either an E variable or a non-E variable. For example the name Joseph Stalin may have been an E variable to contemporary Party members such as Lenin or Trotsky. Either of these gentleman might have been able to identify Stalin or have given directions for the location of Stalin in a certain office of the Politburo. But nobody at this present time grasps the sense of 'Josef Stalin' as an E variable. A contemporary of today has to grasp the sense of 'Josef Stalin' through their historical knowledge of Stalin. The name 'Josef Stalin' itself is neither an E variable nor a non - E variable. Of a name in a natural language we can only ask of a given person at a given time, whether he grasps the sense of that name as an E variable or not.

What is involved in grasping the sense of a name as an E variable? The unproblematic answer is: being able to pick out the nominatum. But this answer is too facile. It is better that this question be approached obliquely. Imagine that S walks up to a dog, points at it, and says 'Snapper!', and walks away. What has S done? Here are some possibilities.

S was marking the dog as dangerous.

S was identifying the breed.

S was trying to get the dog to sit up and beg.

S was trying to get the dog to bark.

S was scolding the dog.

S was naming the dog 'Snapper'.

The bare description of S's act leaves all of these possibilities open. Whether an act of baptism takes place then, depends on more than the circumstantial details surrounding the act. Whether S actually <u>named</u> the dog 'Snapper' or not depends on the use to which he puts his action and 'Snapper'. Wittgenstein, with his ability to cleave through illusion to the essential, saw this too.

'It is quite true that, in giving the ostensive definition, for instance, we often point to the object named and say the name.... This is connected with the conception of naming as, so to speak, an occult process. Naming appears as a <u>queer</u> connexion of a word with an object...'

Wittgenstein [150] (38)

'....naming is something like attaching a label to a thing. One can say that this is preparatory for the use of a word. But <u>what</u> is it a preparation for?'

Wittgenstein [150] (26)

If S has really named the dog 'Snapper' then this is a preparation for using the name as a subject-term in a series of subject-predicate sentences. We might say: the ceremony of naming is only possible in a language which has the resources to employ names; and this means employing them in conjunction with general expressions. A language that consisted only of names would be impossible. Now if we think of what makes an expression a name as not being the product of the occult process of <u>naming</u>, but more a function of how it is employed in the sentences, then we will be less inclined to thinks that the paradigm case of grasping the sense of a name consists of pointing at the nominatum and saying the name. Instead the attention will be focussed on the use that a person makes of the name in his assertions. The question that should be addressed here is: what sort of ability with sentences containing a name N do we expect of a person who grasps the sense of N as an E variable (that is to say, who has effective access to the nominatum)?

Returning to S for a moment, and supposing that S did name the dog 'Snapper', let the previous question be addressed to this situation. S has Snapper in front of him, and this means that S is in a privileged position to answer certain questions about Snapper. For instance S should be able to tell us whether Snapper is large, small or middling, whether his coat is long or short, curly or straight, what colour his eyes are, perhaps even the breed if S has any knowledge of dogs. S will not be able to speak with such authority on whether Snapper's mother is still alive, when Snapper was last taken to the vet and who his owner or his 'real' name might be. S will be able to confidently allocate truth-values to sentences which make straightforward observational reports about Snapper, but sentences which require collateral information S will be more spotty on. Reflecting on this position it becomes clear that the sentences containing 'Snapper' which S is qualified to pronounce on the truth-value of, in virtue of his grasp of 'Snapper' as an E variable, are just those sentences which concatanate 'Snapper' to an E predicate. To have effective access to Snapper is to be able to settle what E predicates are true of him and which are not. To grasp the sense of a name N as an E variable is to be able to decide correctly on the truth-values of atomic sentences containing N that employ only E variables.

This covers what it is for 'Snapper' to be a name and what it is for S to grasp the sense of 'Snapper' as an E variable. But what is it that binds 'Snapper' to Snapper and makes 'Snapper' the name of the dog that S named? The answer is that it is by reference to the properties of the dog he named that S settles the truth-value of sentences that join 'Snapper' to some E predicate. We could say: it is what S is prepared to allow as true about sentences of this form that individuates the dog he named as the nominatum of 'Snapper'. Consequently if S should go on to found a model language community M, in which each member grasped the sense of 'Snapper' as an E name, then what would make 'Snapper' the name of a particular dog (the one S named) for M would be an agreement amongst M members as to what E predicates it was correct to adjoin to 'Snapper'; and the dog named would be that dog which satisfied all those predicates. The ability of 'Snapper' to have sense (within M), that is to have autonomy, depends on an independant agreement on when it is truly employed in a range of sentences which use only E variables.

E variables may therefore be characterised as variables whose sense is not only grasped by identifying their extensions; but also as variables an agreement as to what their extensions are is the foundation of their having autonomy.

An *ontologist* who introduces a canonical language C is required to state precisely which variables of C he takes as E variables. Having made this decision he is then required to show that the members of his model community M had a grasp of the sense of these variables appropriate to their status as E variables. For instance if Φ was a predicate classified as an E predicate we would expect members of M to identify the same objects, independantly of one another, as falling under Φ.

One difficulty in testing for whether an E variable like Φ is appropriately understood by M members is specifying the conditions under which we would accept that it is so understood. For example we could specify the acceptability conditions as follows,

'Φ is validated as an E variable amongst M members if and only if for any x,y,z, if x and y are M members, and z is any object, and z is presented to x and y independantly, then x agrees z satisfies Φ if and only if y agrees z satisfies Φ'

But this condition is too demanding, for human error and borderline cases will create less than perfect agreement. Plainly what is required is a numerical measure of agreement that will circumvent recourse to vagaries like 'most' or 'a substantial majority'. Such numerical standards can be attained with a semantic matrix test.

To validate Φ as an E variable amongst M members, two samples are required: an object sample and a subject sample. The object sample consists of a heterogenous array of objects; the subject sample consists of a section of M. Each element of the subject sample is presented, in isolation from other elements of the subject sample, with each element of the object sample. For each such object, the subject is required to state whether or not the object falls under Φ . The collected results for all subjects can be displayed in a semantic matrix. A specimen of such a matrix is given below.

Subject sample

	Φ	A	B	C	D	E	F
	a	1	0	0	0	0	0
	b	0	1	1	1	1	0
Object Sample	c	1	1	1	0	0	0
	d	0	1	0	1	1	1
	e	0	1	0	1	1	1
	f	1	1	1	0	1	0

'1' indicates the subject classified the object as falling under Φ. Thus above, C classifed as b as satisfying ϕ. '0' indicates that the subject classified the object as not falling under Φ . (Abstention is not allowed). Thus A classified b as not satisfying Φ.

The closer to perfect agreement in the employment of the users of the subject sample approach, the more nearly a semantic matrix for Φ has rows which consist either entirely of '1's or entirely of '0's. The more random the us

the more nearly '0's and '1's in any row tend to balance each other in n

This simple fact can be used to construct an effective statistical pro

which ends in giving a numerical value for the agreement in use of ϕ; thi

is the index of autonomy of Φ, as calculated from a given semantic

The index of autonomy is designated by the letter 'ι' (iota); a su

indicating the variable in question and a superscript indicating the se

matrix from which the index is calculated. Thus ι^{S}_{ϕ} is the index of au

calculated for Φ from semantic matrix S. The effective meth

calculating ι^{S}_{ϕ} is as follows.

(1) Let $r_1, r_2, r_3, \ldots r_n$ be the rows of S. Let r_k be any row where $1 \leq$

Let the number of '1's in r_k be a_k, and the number of '0's be b_k.

defined as following.

(2) If $a_k > b_k$ then $x_k = a_k - b_k$

(3) If $b_k > a_k$ then $x_k = b_k - a_k$

(4) If $a_k = b_k$ then $x_k = 0$

(5) Let t be the number of members of the subject sample and u the

of members of the object sample.

(6) The index ι^{S}_{ϕ} is given by the following formula:-

$$\iota^{S}_{\phi} = \frac{\sum_{y=1}^{y=n} x}{t\,u}$$

Example: Let S be the semantic matrix illustrated on the previous p

has rows $r_1, r_2, r_3, r_4, r_5, r_6$. We have:-

$a_1 = 1$ and $b_1 = 5$ therefore $x_1 =$

$a_2 = 4$ and $b_2 = 2$ therefore $x_2 =$

$a_3 = 3$ and $b_3 = 3$ therefore $x_3 = 0$

$a_4 = 4$ and $b_4 = 2$ therefore $x_4 = 2$

$a_5 = 4$ and $b_5 = 2$ therefore $x_5 = 2$

$a_6 = 4$ and $b_6 = 2$ therefore $x_6 = 2$

$t = 6$ and $u = 6$. Putting these values into the formula $\iota_{\phi}^{s} = \frac{\sum_{y=1}^{y=n} x}{t\,u}$ we derive:

$$\iota_{\phi}^{s} = \frac{\sum_{y=1}^{y=6} x}{t\,u} = \frac{4+2+0+2+2+2}{6 \times 6} = \frac{12}{36} = 0.3$$

The value of ι_{ϕ}^{s} for any Φ and for any S is always between and inclusive of 0 and 1. The closer the value to 0, the more random the use of Φ; the closer the value to 1 the index returns, the more uniform the use of Φ. By setting the minimum acceptable level of ι_{ϕ}^{s} to a given value (eg. 0.9), precise conditions for the determination of whether the use of Φ shows an acceptable uniformity can be laid down. The use of a semantic matrix test is subject to the usual abuses and warnings that statistical tests are subject to: the larger and the more varied the sampling, the more authoratative the results. Semantic matrix tests vary according to the nature of the variable tested in a way that I will now describe.

The simplest case is that where the test variable is a 1-place predicate whose extension includes only physical objects. Here the test procedure consists simply of a presentation of various physical objects with the accompanying standing question as to whether each such object satisfies the variable or not. Physicalistic predicates of n places, where $n>1$, demand that the presentations consist of n objects, placed in some kind of ordering, with the appropriate standing question. Physicalistic function-expressions require a similar treatment; if a function-expression of this kind contains n argument places, then a presentation will consist of n objects, placed in order, with an n+1th object and the standing question will be whether the n objects, so ordered, constitute an argument to the appropriate function and whether the n+1th object is a value to that argument. Names denoting physical individuals which are classified as E-variables receive a slightly different treatment from the foregoing. The mark of grasping the sense of a name as an E variable is the capacity to judge the truth-values of atomic sentences containing that name which use only E -variables. Call an atomic sentence which contains only E variables an atomic E sentence. Then the test for a name N being an E variable amongst members of M, is that they should independantly agree on the truth-values of atomic E-sentences containing N. Objects of the object sample are atomic E-sentences containing N, and the standing question of each such object is 'Does this sentence count as true?'[1] Assent is registered by '1', dissent by '0'.

Nothing has been said so far about E variables which are taken to include non-physical objects such as numbers; the predicate 'is odd' being an example. Such predicates do raise profound metaphysical and epistemological issues which will be the subject of discussion for the next three sections. The immediate problem is how such a predicate can be tested in a semantic matrix since the objects which satisfy it, odd numbers, cannot be presented in the way that physical objects can. The solution is to use terms that purport to refer to such objects, namely numerals, in place of numbers. Instead of presenting the number 2 and asking 'Does this number satsify 'is odd'?' we enquire 'Is the atomic sentence '2 is odd' true in the characterising language of arithmetic?' Assent is registered by '1', dissent by '0'. The treatment of non-physicalistic variables (or non P variables) is then similar to the treatment of physicalistic names (or P names) in that the elements of the object sample are atomic E sentences rather than non-linguistic objects. The treatment of other non P variables that are also classified as E variables is very similar too.

So far, with adaptations, semantic matrices have proved remarkably flexible in providing the means by which the autonomy of E variables can be tested. It is time that we begin to grapple with some of the philosophical problems that have been submerged in discussion so far. Putting this previous discussion to one side for the moment; consider the following case.

S and H are two members of the ontologist's model community M. The ontologist endorses an ontology including natural numbers as unreduced objects, and in order to characterise his ontology he uses a canonical language incorporating many arithmetical expressions. Amongst these expressions is the predicate 'is odd' which the ontologist classifies as an E predicate. The ontologist claims that S and H, amongst others of M, attach the same sense to 'is odd' because, if they are asked to write down odd numbers beginning with 1 until told to stop, they both independently write:-

'1, 3, 5, 7, 9, 11, 13, 15, 17, 19, 21, 23, 25, 27, 29 .....'.

Ignoring the inductive uncertainties of the test, (the fact that either S or H can go 'off the rails' if asked to carry on the series past a certain point), does the test establish that S and H attach the same sense to 'is odd'? On the realist view of mathematics which the ontologist adopts, which distinguishes between numbers and numerals, the test is only convincing if S and H denote the same numbers by the same numerals. But do they? Is it not possible that S and H may have fixed their attention on different numbers using the same numerals? In such a case their 'agreement' is merely a verbal one of no substance whatsoever (just as a man in England may agree it is 4p.m. and so might a man in America and yet agree to different things). This philosophical doubt here cannot be dispelled by asking S and H purely arithmetical questions like 'Is 5 prime?', 'Does 3 + 7 = 11?', 'What is the square root of 49?'. Even if S and H both answer '5 is prime', '3 + 7 = 11', ' 49 = 7^2', these answers only mark a genuine agreement if S and H mean the same by 'prime' '+' and '$( )^2$'. These suppositions can be tested by asking S and H questions about the extension of 'prime, +' and '$( )^2$', but then the answers S and H provide will use numerals; therefore any agreement in these answers will depend on whether S and H in fact denote the same numbers by the same numerals. Of course this is where the doubts began.

The problem that has issued here is the problem of demonstrating that numbers are public objects. Only if the extension of an E variable consists only of public objects can we say that E variable has sense. To be a public object O has to be recognisably referred to by two separate speakers by use of some individuating apparatus. Unless numbers are public objects, then arithmetical sentences will consist only of senseless marks. The vindication of realist mathematics as even tenable, depends on demonstrating that numbers as abstract objects are public objects.

There is a far older problem for realism in mathematics, which might be termed 'the Problem of Epistemic Access'. The problem is most easily put forward as a sceptical argument.

Human beings are physical beings, animals equipped to detect limited kinds of radiation and limited frequencies of sound, evolutionally prepared to survive in a macroscopic physical world. By access to electron microscopes, X-ray diffraction photographs, cloud chambers and the like, humans have improved their epistemic access to areas of the physical world which are not within the reach of their unaided senses. However the possibility of human beings doing such things depends on the existence of causal interactions from the microscopic and submicroscopic worlds to the domain of perceptual objects and events. Without such causal interactions, human beings would be shut off from events outside the domain of the perceptual and they would be restricted in their knowledge to what is perceivable.

Now the realist mathematician believes in a domain of abstract objects: numbers. Being abstract objects, numbers exist neither in space or time.

Consequently it is impossible that events should take place in the domain of numbers, or that there should be causal links between the domain of numbers and the perceptual domain. Therefore knowledge of the properties of numbers and mathematical knowledge in general is impossible. But this is absurd, since plainly we do possess a great deal of arithmetical knowledge about so-called 'numbers'. Therefore the realist mathematician is wrong.

Both the problem of the publicity of numbers and epistemic access challenge the realist mathematician for answers; though the problem of publicity is prior. If we cannot even talk of numbers, then how we come to know of them hardly arises. The problems have been formulated for realist mathematics; but they could be extended to cover many kinds of non-physical ontologies. Seeing how these problems can be met in general is an important metaontological task.

4.3 First Epistemological Interlude: the Doctrine of the Mind's Eye

One of the oldest answers to these problems is the Doctrine of the Mind's Eye. This doctrine maintains that, in addition to their five physical senses, human beings have a sixth sense which is purely intellectual and directed towards such objects of reflection as numbers and propositions. The adherents of the doctrine insist that intellectual perception is responsible for human beings' capacity to talk and know about abstract objects.

Sight being the principal sense of human beings, and also the source of much of their knowledge, it is not surprising that the Doctrine of the Mind's Eye has been a popular answer to the problems just aired. It would have been stranger (but perhaps not much stranger) if the doctrine had been that we had the capacity to intellectually smell abstract objects!

How old the Doctrine of the Mind's Eye actually is, is partly conjectural. The earliest generally accepted authority for the doctrine is Plato. In The

Republic, Socrates poses the doctrine to Glaucon.

Socrates: '...we say that the particulars are objects of sight but not of intelligence, while the Forms are the objects of intelligence but not of sight.'

Glaucon: 'Certainly'.

Socrates: 'And with what part of ourselves do we see?'

Glaucon: 'With our sight'

....

Socrates: 'Apply the analogy to the mind. When the mind's eye is fixed on objects illuminated by truth and reality, it understands and knows them and its possession of intelligence is evident; but when it is fixed on the twilight world of change and decay [the physical world] its vision is confused and its opinions shifting and it seems to lack intelligence'.

Plato [99] (Stephanus pages 567b - 508d)

Ancient though it is, the Doctrine of the Mind's Eye is by no means defunct. Many able mathematicians have held to the doctrine more or less in the form that Plato taught it. Elements of the doctrine are to be found in Frege [48] and Russell [124]. In Frege's system, every significant sentence expresses a thought (gedanke) which is the sense of the sentence. This thought is an abstract object which is grasped or perceived by anybody who understands that sentence. In Russell, the sentence '2 + 2 = 4' deals exclusively with universals with which we are acquainted; our apriori knowledge of the truth of this sentence is founded on our ability to be directly acquainted with universals.

G.W. Hardy, speaking in 1940, remarked:-

'For me, and I suppose for most mathematicians, there is another reality, which I call 'mathematical reality'; and there is no sort of argument about the nature of mathematical reality among either mathematicians or philosophers...

A man who could give a convincing account of mathematical reality among either mathematicians or philosophers would have solved very many of the most difficult problems of metaphysics... I believe that mathematical reality lies outside us, that our function is to discover or observe it, and that the theorems we prove, and which we describe grandiloquently as our 'creations' are simply notes of our observations. This view has been held, in one form or another by many philosophers of high reputation from Plato onwards.'

Hardy [61]

Kurt Godel, writing in 1964, defended the ancient Doctrine of the Mind's Eye.

'I don't see any reason why we should have less confidence in this kind of perception i.e. in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decided in the future. The set-theoretical paradoxes are hardly any more troublesome for mathematics than deceptions of the senses are for physics.'

Gödel [50]

In assessing the claims of these various mathematicians as to how we perceive the properties of numbers, a comparison with the facts of physical perception is called for.

In humans, sight is the ability to distinguish the size and shape of objects without actually touching them. A beginning is made at explaining sight when physiology is applied to the phenomena. Physiologists tell us that physical objects generally reflect light of certain wavelengths which enter the human eye. This light is focussed by the lens in the eye onto the retina. The rods and cones in the retina respond to the bombardment of photons of light, by transmitting electrical impulses via the optic nerve to the brain. The brain then transmits electrical impulses along the efferent nerves to the muscles. The muscles then move the body in a way appropriate to the behaviour of a sighted organism.

One significant feature of the physiological explanation is that the facts it is designed to explain (the behaviour of a sighted person) are significantly distinct from the facts used in the explanation itself (the transmission of light to the eye; the connection of the brain to the eye by the optic nerve; the connection of the brain to the muscles of the body). A certain kind of pseudo-explanation occurs when the 'explanation' is simply a reaffirmation of what has to be explained. An 'explanation' of this kind was provided by Moliere's medical doctor in his satire Les Malades Imaginaires. When asked to explain why opium makes people sleepy, the doctor replied, to general applause, that a virtus dormitiva in opium was responsible. Effectively the doctor had replied that opium makes people sleepy because there is something in opium that makes people sleepy!

The Doctrine of the Mind's Eye is another pseudo-explanation from the same stable. There is no independent physiological account of 'seeing with the Mind's Eye' to explain our appreciation of abstract objects, of the kind that physiology has provided to explain our appreciation of physical objects. 'Intellectual perception' has the same explanatory value as 'virtus dormitiva'. To say that S knows 5 is prime because S perceives 5 is prime does not advance our understanding of mathematical knowledge. In respect of mathematical knowledge, the Doctrine of the Mind's Eye fails to link together the processes of proof and calculation, by which we gain mathematical knowledge, with the occult faculty of intellectual perception.

For example, Lindemann proved that π was a transcendental number many years after transcendental numbers had been defined. Prior to Lindemann nobody knew whether transcendental numbers existed or not. How are we to explain a century or so of ignorance from the perspective of the Doctrine of the Mind's Eye. Why did so many mathematicians fail to see the transcendality of π. Did they fail to look hard enough? What role did Lindemann's faculty of intellectual perception play in his discovery?

One way of telling the story is as follows: Lindemann was a brilliant mathematician so his intellectual vision for mathematical objects was very acute. Lindemann perceived that π was transcendental in a way that his less able colleagues could not. Realising that many mathematicians did not have his vision, Lindemann constructed a proof of the transcendental nature of π, based on easily observable characteristics of π. The proof was a means of boosting the intellectual vision of other mathematicians like a telescope may improve the powers of the naked eye. The situation is analogous to that where a long-sighted man points out a distant object (e.g. a horse) to a near-sighted friend and presents him with a telescope to verify what he has seen.

The analogy has a certain gloss. Mathematicians and logicians are familiar with first feeling that a given proposition is true and then looking for a proof of it. But the gloss is thin and patchy and fails to disguise the elements of disanalogy. Consider the physical analogy where the long-sighted man sees and identifies a distant horse and says, with the directness and simplicity common only to small children and philosophers: 'I know I am looking at a horse'. The short-sighted companion queries the claim and is handed the telescope. The claim is verified.

Now suppose Lindemann had, without possessing proof, claimed before an assembly of his colleagues: 'I know π is transcendental'. Asked to give a proof, Lindemann replies he has no proof and his claim is based on intellectual perception. No mathematician would accept a claim like this on such a basis. Now further suppose that, at a later meeting, Lindemann turns up with a proof of the transcendental nature of π. Would this proof verify his earlier claim to knowledge at the time that claim was made? No. Unlike the telescope, the proof does not verify the earlier claim to knowledge because the proof was not in Lindemann's possession when the claim was made. Proof does not verify our claims to know through intuition, though it may verify what we intuit. Intellectual perception is not a sufficient condition of mathematical knowledge; neither is it a necessary condition. If the act of intellectually perceiving is a private mental act, then it is logically distinguishable from the physical acts of calculation and proof. It is therefore quite possible, logically speaking, that Lindemann may have lacked the faculty of intellectual perception, though in all apparent respects he was an extremely competent mathematician. In such a case, would Lindemann's proof that π was transcendental verify his claim to know or not? If we say 'yes' then intellectual perception is not a necessary condition of mathematical knowledge. If we say 'no' then we would be saying that a man could be, in all respects that we could ever observe, extraordinarily gifted at mathematics and yet fail to know the first piece of mathematics! Such a view leads to a queer sort of mathematical solipsism; i.e. nobody could have reason to believe anybody but himself understood mathematics.

What sort of explanation does the Doctrine of the Mind's Eye afford of the publicity of abstract objects and numbers in particular? In miniature, the explanation is that when S writes or utters '5', he concentrates his intellectual attention on 5 and H does the same. Obviously there is something unsatisfactorily mystifying about this concentration of intellectual attention on an abstract object. But even if this is put aside, the explanation still fails to give numbers the status of public objects. Intellectual perception is a private mental act. How then can it be known that S and H focus their intellectual perception on the same abstract object when writing or uttering '5'? Neither as an analysis, nor as an explanation of our knowledge of abstract objects, does the Doctrine of the Mind's Eye succeed in its purpose.

4.4 Individuating Relations: the Refutation of Idealism

Out of the many philosophical positions that Wittgenstein examined with such penetration in the Philosophical Investigations, one of these was the doctrine that all mental processes are essentially private, and therefore known directly only to the person who has them. A consequence of this view is that expressions which describe such mental processes are learnt by each person in relation to his own mental processes. Wittgenstein argued that, if this were indeed true, then expressions descriptive of mental processes could have no sense: they could play no part in an act of communication.

'If I say of myself it is only from my own case that I know what the word 'pain' means - must I not say the same of other people too? And how can I generalise the one case so irresponsibly?

Now someone tells me that he knows what pain is only from his own case! - Suppose everyone had a box with something in it: we call it a 'beetle'. No one can look into anyone elses box, and everyone says he knows what a beetle is only by looking at his beetle. - Here it would be quite possible for everyone to have something different in his box. On might even imagine such a thing constantly changing. - But suppose the word 'beetle' had a use in these people's language? -If so it would not be used as the name of a thing. The thing in the box has no place in the language-game at all; not even as a something: for the box might even be empty. - No, one can 'divide through' by the thing in the box; it cancels out, whatever it is.'

Wittgenstein [150] (293)

Wittgenstein argued that in order for ascriptions of thought and sensaton to oneself to be significant, they had to be correlated to public and observable behaviour and actions. These public happenings defined the 'inner process'. It follows that these behavioural traits could be used to ascribe thought and sensations to others. So a condition of sensibly talking about one's own thoughts and sensations is that one can, at least in some cases, recognise them in others.

'.........if anyone said 'I do not know if what I have got is pain or something else', we should think something like, he does not know what the English word 'pain' means; and we should explain it to him. - How? Perhaps by means of gestures, or by pricking him with a pin and saying: 'See, that's what pain is!' This explanation, like any other, he might understand right, wrong or not at all. And he will show which he does by the use of the word, in this as in other cases.
If he now said, for example: 'Oh, I know what 'pain' means; what I don't know is whether this, that I have now, is pain' - we should merely shake our heads and be forced to regard his words as a *queer reaction* which we have no idea what to do with.'

Wittgenstein [150] (288)

Wittgenstein's point is that the sensation of pain is personal to the person who has it - nobody can feel my pain except me - the concept of being in pain has to be defined in relation to observable behaviour and events. So we identify pain as that sensation which is produced in normal human beings by injuries of various kinds. The sense of the word 'pain' is taught to novices in connection with these sorts of scenarios. When we tell a crying infant who has grazed his knee that he feels pain and try to console him, we are in effect saying to him that he is to recognise as pain whatever sensation is qualitatively similar to the sensation he feels in his knee. Because pain is defined in this way, it is futile to speculate whether what one feels, having been hurt, is pain. Pain and hurt are logically tied together. (Which is why in English we say 'Does it hurt (give you pain)? and 'Where (on your body) are you hurt?) The thread of Wittgenstein's reasoning can be summed up in a dictum which deserves to be engraved on the mind of every ontologist.

Objects and processes that would otherwise be private become public objects by being defined through their public relations to public objects and processes.

Applying this dictum to ontology in general, we conclude something like this. If an ontologist wishes to introduce a kind K into his ontology, and Ks look to be private objects of which nothing useful can be said, then he requires another kind K_1, to define Ks, by virtue of their relation to Ks. But if K_1s are similarly placed as Ks; that is, their publicity is in doubt, then K_1s will need another kind K_2 by which *their* publicity can be guaranteed. If circularity and infinite regress are both to be avoided then there must be a kind K_n whose elements are guaranteed to be public. The elements of K_n will fund the reference to all other entities of the ontologist's ontology. The elements of K_n I call *basic objects*.[2]

The category of basic objects is nothing more than the category of physical (i.e. spatio-temporal) objects. The evidence for identifying physical objects as basic is extremely strong. Only physical objects satisfy the following three conditions.

(1) There is no reasonable doubt as to the existence of physical objects. Traditional philosophical arguments to show that our epistemic access to physical objects is problematic are unsound.[3]

(2) Physical objects are objects which we already perceive and refer to. There are no grounds to doubt their publicity.

(3) Since the problem of publicity rose over non-physical objects, it would be a *petitio* to identify as basic anything else but concrete objects.

Protagonists of the Idealist and Phenomenalist traditions in philosophy will disagree with these remarks. According to the phenomenalist tradition, all that a human being actually perceives are sense-data (sounds, colours etc) in his own private space. Once the phenomenalist position is accepted then a whole range of philosophical problems arise. For example, how can a person ever know of the existence of things outside his mind since all he sees is a picture show of his perceptions? Locke adopted the representationalist view: what the mind sees is a representation, accurate in parts, of what lies beyond. Berkeley, the archtype Idealist, rejected Locke's account and argued that there was nothing beyond except other minds; God's in particular. Hume declared the whole debate insoluble and advocated backgammon. Kant concocted a noumenal world as the beyond, in which space and time did not exist. Carnap in the <u>Aufbau</u> tried to merge what lay beyond and what appeared before, by analysing reports about what lay beyond as reports about experience or possible experience.

All these programs were flawed from their very inception, since the assumption on which all were constructed was false. If the only objects with which humans are acquainted are private mental percepts then it would be impossible ever to say anything of sense about those percepts. If S should make the statement 'I am now experiencing a yellowish sense-datum' it would be utterly unclear what S meant. S could be talking about anything in his private mental box. Even 'yellowish', if taken to be meant by S to refer to his private percepts, could have any meaning. In order for remarks about sense-data to be more significant than disturbances in the atmosphere, sense-data must be defined in relation to public objects. S can say his sense-datum is <u>like</u> a yellow dot on a piece of paper, or it is the sort of thing that is <u>caused</u> by looking at sodium lamps or sunflowers. But however sense-data are defined, remarks about sense-data only make sense because there is a public physical world to anchor sense-datum language to. Idealism is therefore false, for if it were true, language would not be possible. A minimum ontology must recognise the existence of physical objects.

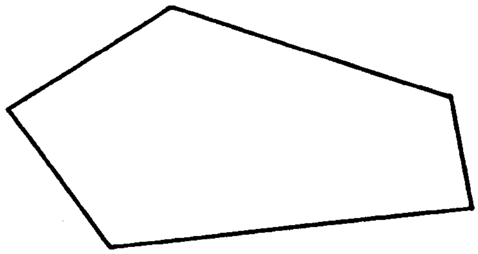
Applying these ideas to the case of numbers is a good way of illustrating the concepts involved. Numbers are in danger of being classed as ineffable private objects; therefore they require to be defined in relation to public objects.

One way this could be done is by defining numbers in relation to physical objects. The choice of physical objects is somewhat important if we are to avoid Frege's objections to viewing number as a property of physical objects (which was Locke's mistake). We cannot say, for example, that my right hand has any number associated with it. For this reason, Frege preferred to say number was a property of concepts rather than of objects. Thus the concept moons of Venus is numbered by zero, though the moons of Venus themselves, since there are none, are not numbered by anything. For more modest ontological purposes, open sentences can be used in place of concepts. Thus zero numbers the open sentence 'moon of Venus x' and four numbers the open sentence 'finger of my right hand x'. Open sentences are to thought of as either events (when uttered) or material objects (when written). Either way they are physical objects in our sense of the word.

Once numbers are defined as objects linked to open sentences in this manner, numbers can be individuated by their relations to open sentences. No two numbers number the same open sentences and so we can test whether S associates the same numbers with the same numerals that H does by asking what numbers S and H take to number various open sentences. For instance, if S and H independently agree that the answers to questions like:-

'What numbers the open sentence 'toe of my right foot x',

'What numbers the open sentence 'side of the figure following x';



is '5', then they attach the same sense to '5'. For the denotation of '5' is defined as just that thing which numbers those sorts of open sentences. The question 'Yes, they both know that it is right to say '5 is the object which numbers these open sentences', but do they really mean the same by '5'?' would be, in Wittgenstein's words, 'a queer reaction which we have no idea what to do with'.

There are a number of salient features of this case which are philosophically of great importance. First, we introduced a domain of public objects by which numbers were to be defined; these were open sentences. Then there were the numbers themselves whose publicity had to be demonstrated. Finally there was the relation of numbering itself which bound the two domains together. Technical vocabulary is required here: call the process by which putative entities of an ontology are proved public, publification (not to be confused with publication); the public objects we invoke to aid publification, discharged objects; the objects whose publicity we seek to demonstrate prototype objects; and the relation that we introduce to bind the two together, the individuating relation. In the specimen publification, open sentences were discharged objects, numbers were prototype objects, and the relation θ numbers δ was the individuating relation.

The essential features of publification can be laid down in numbered form.

(1) The discharged objects must be public objects.

(2) The individuating relation must individuate the prototype objects. It is

the foundation of reference to, and identify criteria for, the prototype objects.

(3) The individuating relation may be metaphysically nonempirical but it must be epistemically empirical.

The discharged objects must be public. We do nothing in the way of publifying a kind K of objects if our reference to them is cashed in terms of objects whose publicity is just as much in doubt as Ks. So for example, if we tried to show that possible worlds are public objects by individuating them in terms of their possible contents, the attempt would be a failure. Possible individuals are as much in need of publification as possible worlds.

To demand that the discharged objects be public objects is not to demand that they be physical objects though of course it they are physical objects they will be public. The discharged objects may be prototype objects of a previous successful publification. For instance, an ontologist may wish to include propositions in his ontology, and also sentence-types. If he publifies sentence-types he can use them as discharged objects to publify propositions. Propositions might then be publified as entities which sentence-types express. It will, however, be true that since physical objects are basic (i.e. public without need of being publified), then the chain of dependancy will end in them.

The individuating relation must individuate the prototype objects. Suppose it did not: then we would have the following scenario. A domain D of discharged objects, a domain P of prototype objects, and an individuating relation R running, say, from the elements of P to those of D. There are at least two distinct elements of P which have the same R-relations to every element of D. Formally:-

$$(\exists x,y;\ x \in P,\ y \in P)(z;\ z \in D)(-(x = y)\ \&\ (Rxz \equiv Ryz))$$

In such a case if speakers S and H identified an element of P in terms of its R-relations to D elements it would still be an open question whether they were referring to the same element or not. For instance, if two distinct numbers could number the same open sentences then it would be no evidence that S and H were referring to the same number that they associated it with the same open sentences. Rejecting this sort of case means rejecting the formula above; or, equivalently, accepting its negation:-

$$(x,y;\ x \varepsilon P,\ y \varepsilon P)(z;\ z \varepsilon D)(Rxz \equiv Ryz) \supset x = y$$

This formula is the schematic form of a criterion of identity, whose importance will become clear in the appendix I.

<u>The individuating relation may be metaphysically nonempirical but it must be epistemically empirical</u>: this remark has certain Kantian overtones that need explanation. Kant at one place in the <u>Critique of Pure Reason</u> remarked that space and time are transcendentally ideal but empirically real. Kant meant that outside of the human mind, space and time did not exist; that is to say, the noumenal world is one where the concepts applicable to spatio-temporal objects do not apply. Nevertheless, space and time are empirically real. They are forms imposed on the raw stuff of the noumenal world by the faculty of intuition, and because the faculty of intuition is the same for everyone, the properties of space and time are objective for us and determinable independent of our fancy.

A relation is metaphysically unempirical when it includes objects which are not objects of experience. We cannot perceive numbers in any way at all, and advocates of the Doctrine of the Mind's Eye have achieved little but obscurity in supposing that we can. The relation $\underline{\theta > \delta}$, interpreted over the domain of positive numbers is metaphysically unempirical since the elements of its domain and range are not objects of experience. The individuating relation

$\underline{\theta \text{ numbers } \delta}$ which holds between numbers and open sentences is also metaphysically unempirical since the elements of its domain are not objects of experience.

But although $\underline{\theta \text{ numbers } \delta}$ is metaphysically unempirical, it is epistemically empirical. For instance, we cannot determine if, say, 5 bears the relation $\underline{\theta \text{ numbers } \delta}$ to 'toe on my right foot x' by empirically inspecting 5 and the open sentence in question to see if this is so. The procedure involved is not at all like standing two men side by side to see who is taller. Nevertheless if $\underline{\theta \text{ numbers } \delta}$ is to be useful as an individuating relation there must be some way of telling when a number bears that relation to an open sentence. This requires that we be capable of carrying out some empirical procedure within the range of our experience to settle if, for example, 5 and 'toe on my right foot x' are respective arguments to $\underline{\theta \text{ numbers } \delta}$. This empirical procedure is the simple one of counting the toes on my right foot. Consequently though $\underline{\theta \text{ numbers } \delta}$ is metaphysically unempirical, the evidence for its holding between two arguments is empirical. Unless $\underline{\theta \text{ numbers } \delta}$ was epistemically empirical, it would not be usable as an individuating relation to test our reference to numbers.

Roughly then, there are four important aspects of a publification; these are;- the prototype objects, the discharged objects, the individuating relation and the empirical techniques used to determine when the individuating relation holds between a prototype object and a discharged object. The table opposite shows some different kinds of prototype and discharged objects and their associated individuating relations. Question marks show where, in my opinion, there are doubts left unsettled.

Diagram 4

Prototype Objects	Individuating Relation	Discharged Objects	Associated Empirical Procedure
numbers	enumerates	open sentences	counting
pain-sensations	causal	accidents & injuries	behavioural observation
sets	ε	various; includes all discharged objects	determination of the identity and/or properties of the discharged objects
word-types	token of	word-tokens	observation of the shape of word-tokens.
sense-data	causal?	stimuli to the body?	observation of stimuli?
propositions	expressed by	sentences	determination of meaning of sentences
possible worlds	?	?	?
facts	?	?	?
spatial points	is part of	volumes of space?	spatial observation
instants	is at?	events?	temporal observation

4.5 The Introduction of non-P variables

The previous section examined the ways in which we could aspire to make sense of terms that purported to refer to non-physical objects without recourse to flights of fancy like the Doctrine of the Mind's Eye. In this section, the task is to adapt the conclusions of the previous section towards specifying the conditions that a non-P-variable has to satisfy in order to be introduced into the canonical language. This is an opportune moment to usher forward another technical concept, one that, hopefully, will come to seem natural and easy to use in context; this is the concept of being <u>well-introduced</u>.

Briefly, a variable is well-introduced when it is <u>demonstrated</u> to have sense. The whole of this chapter is concerned with the conditions of well-introduction for various kinds of logical variable. We have already seen the conditions of well-introduction for a specific class of logical variable, namely those variables which are both P (physicalistic) variables and E variables. These variables are well-introduced when their index of autonomy in a semantic matrix test comes up to a designated minimum. How a semantic matrix test is performed is a matter of record in 4.2. Performing a semantic matrix test for variables which are both non-P variables and E variables is, in many respects, similar to semantic matrix tests for P variables which are also E variables. The difference is that we are not capable of presenting the objects of the variables' extension to the elements of the subject sample; instead we present atomic E sentences and enquire after their truth-value relative to the ontology characterised. Thus in well-introducing, 'x is prime', we do not present 2 and ask if it is prime; we ask instead the question "Is '2 is prime' true in the characterising language of arithmetic?' Agreements in valuations of this kind is the hallmark of such variables being well-introduced.

We observed in 4.2 that the problem with non-P variables lay in showing that non-P terms had an agreement in sense between two speakers as to what they are supposed to signify. In order for any such agreement to be manifest, the objects signified by those terms, the prototype objects, had to be publified. Here what we have to do is translate the requirements of the publification of prototype objects into the requirements of well-introduction for the non-P terms that are supposed to signify them.

Essentially this means a switch from the material to the formal mode of speech. In order to avoid the proliferation of unnecessary jargon, I shall carry over as far as possible, the technical vocabulary used to characterise publification over to well-introduction. Thus instead of publification, I talk of well-introduction. Instead of discharged objects, I talk of well-introduced terms. Instead of prototype objects, I talk of prototype terms; and instead of individuating relations, I talk of individuating predicates.

The extention of a canonical language to include variables which purport to range over a kind K of non-P objects begins with a statement, by the ontologist, of the E variables he is going to use to characterise Ks. These will naturally divide into predicates, function-expressions and names. None of these expressions will be as yet well-introduced and the ontologist's second task will be to show that the names are well-introduced. In order to do this he will draw upon a stock S of well-introduced terms and an individuating 2-place predicate μ. μ will have two important characteristics.

First, μ will be the foundation for the individuation of and reference to, the prototype objects. Translated into the formal mode, this means that the ontologist assents to a formula which says that no two prototype objects have the same μ relations to the discharged objects.

Second, μ must be epistemically empirical. This means that members of M must agree on the truth-value (relative to the assumption of the existence of Ks) of atomic sentences formed out of (i) μ (ii) a prototype name (ii) a member of S. Such agreement is readily quantifiable in a semantic matrix test.

Once μ is demonstrated to have these characteristics it can be held to be well-introduced and the prototype names are well-introduced too. The well-introduction of the rest of the vocabulary used to characterise Ks follows orthodox lines in the form of semantic matrix tests.

4.6 Second Epistemological Interlude: the Problem of Epistemic Access

The problem of Epistemic Access was that of explaining our knowledge of abstract objects given that we are perceptually divorced from them. The problem as stated splits naturally into two parts:-

(1) How do we know (if we do) of the existence of abstract objects?

(2) If we do, how do we know (if we do) of what properties they have?

The answer to the first question was provided by Quine: we are compelled to recognise the existence of a kind K of abstract objects if we are required to quantify over Ks in the formalisation of a true theory. The answer to the second question demands a little more thought than recourse to a cut-and-dried stock response. Once this answer is provided, the stock response to (1) is leavened by a little more freshness.

To begin indirectly on the job of answering (2) it is instructive to start with Locke's remarks in Book IV of <u>An Essay Concerning Human Understanding</u> on the possibility of knowledge of atoms and their properties.

'If a great, nay, for the greatest part of several <u>bodies</u> in the universe escape our notice by their remoteness, there are others that are not less concealed from us by their <u>minuteness</u>. These insensible corpuscles being the active parts of matter and the great instruments of nature on which depend not only all their secondary qualities but also most of their natural operations, our want of precise distinct ideas of their primary qualities keeps us in an incurable ignorance of what we desire to know about them... Whilst we are destitute of senses acute enough to discover the minute particles of bodies and to give us ideas of their mechanical affections, we must be content to be ignorant of their properties and ways of operation; nor can we be assured about them any further than some few trials we are able to reach.... And therefore I am apt to doubt that, how far soever human industry may advance useful and experimental philosophy in physical things, [knowledge] scientifical will still be out of reach...'

Locke [84] (160 - 161)

Locke is expressing a doubt that must be natural to any intelligent commentator who is first confronted with the atomic hypothesis: if atoms are so minute as to be invisible, how can we come to have knowledge of them? Locke thought we could not.

But Locke's prognosis of progress in 'physical things scientifical' turned out to be wrong. What overturned Locke's reasoning in the passage above? In the first instance, Locke could not have foreseen the development of scientific apparatus for the pursuit of atomic physics such as X-ray diffraction, mass spectroscopy and particle accelerators that modern technology has provided. But in the second (and more important) instance, Locke placed too much emphasis on the importance of observation and too little on the importance of the hypothetico-deductive method in advancing the progress of science. It is true that the scientist cannot directly observe the behaviour and properties of atoms; but he can ask himself what properties atoms must have in order to explain his macroscopic observations.

A classic instance of such reasoning in action is Rutherford's theory of atomic structure, derived from his famous gold foil experiment of 1911 in which Rutherford directed a stream of α particles at a piece of gold foil (see diagram 5). Rutherford noticed that whereas most of the particles passed straight through the foil, there were others which were deflected by as much as 180°. In order to explain these observations, Rutherford abandoned the atomic model of J.J. Thomson. Thomson believed that solid objects consisted of atoms joined together. But this model was incapable of explaining how the α particles had managed to pass through the foil.

Rutherford supposed instead that the atoms of which the foil was composed were, in fact, widely spaced, and in consequence most of the α particles passed straight through. The few deflections that occured were explained by supposing that the α particles had collided with the atoms themselves.[4]

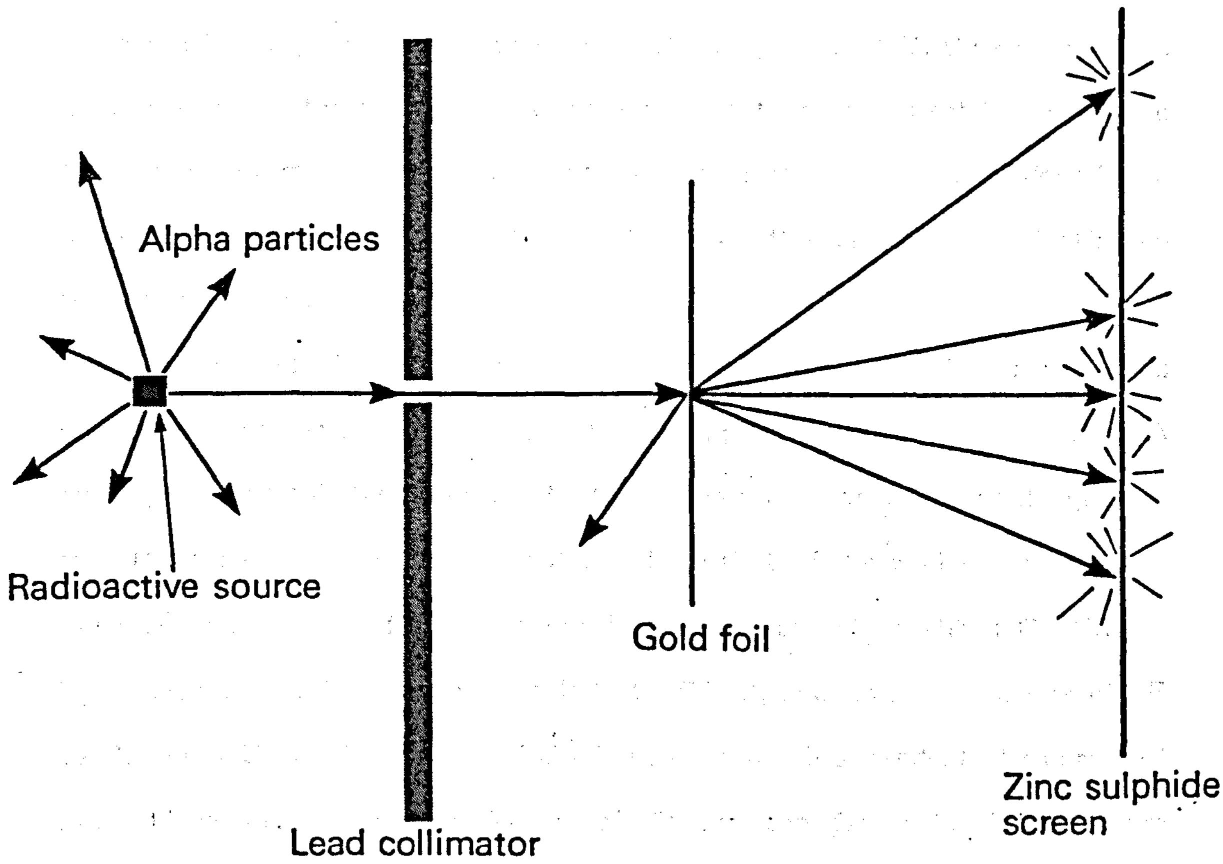
Rutherford's reasoning is a beautiful example of black box reasoning. A black box is a domain of entities which are not directly accessable to human investigations or observation. The methodology of black box reasoning is to hypothesise what might be in the box and the general properties of the same, and to infer what is then to be expected from the black box. In this way the scientist tries to recover the macroscopic output of the black box from his hypothesis.

The means by which an ontologist comes to attribute properties to abstract objects is very similar. Let us take as an example, set theory. Sets are abstract objects and they are not observable. An ontologist who postulates that there is a domain of sets has created a black box, since there is no perceptual access to this domain. The ontologist introduces his hypothesis in order to provide an ontology for pure mathematics.

Approaching the subject in this way, the ontologist quickly discovers that in order that sets play the explanatory role he has assigned them in mathematical reasoning, certain fundamental axioms have to be assumed. For instance, if he

Diagram 5

Taken from Silva & Lochak [132]



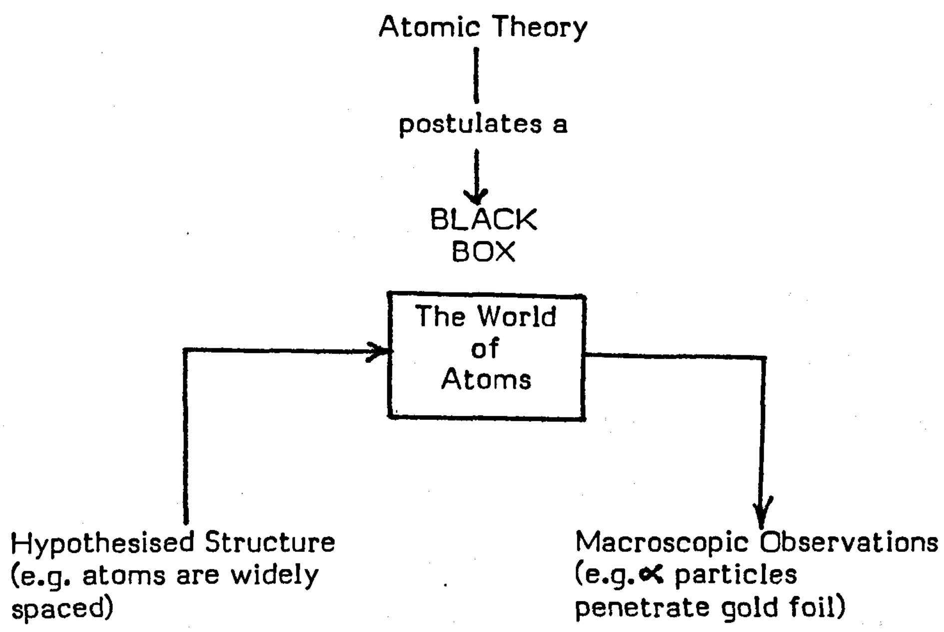
wishes to underpin the mathematical assumption that any countable domain of objects can be well-ordered then he will have to assume the Axiom of Choice or one of its equivalents[5]. Like the scientist, the ontologist will try to adopt the simplest and most economical axiom set to support mathematical thought. The logical structure of this approach is illustrated in diagram 6.

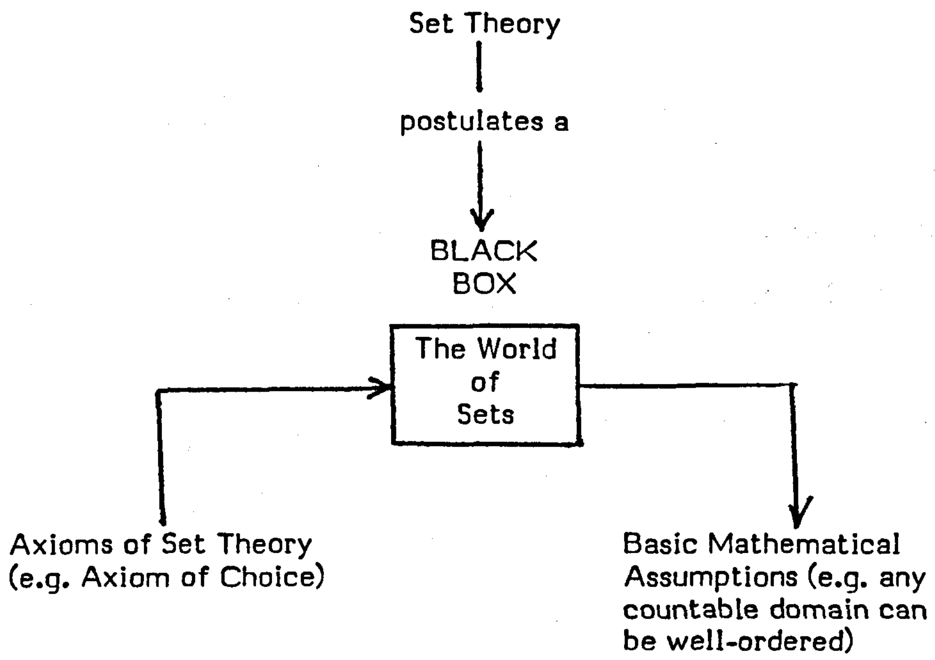
This way of arriving at the axioms of set theory is much the way that mathematical logicians chose to develop the subject; that is, they assumed as axioms all those formulae which were necessary to establish set theory as the foundation for mathematical thought. However this methodology does leave open questions behind it; as to the truth of the Generalised Continuum Hypothesis, for example.

The Generalised Continuum Hypothesis states that there is no set of a cardinality greater than N_n and less than N_{n+1} for any positive integer n. A consequence of this hypothesis is that there is no set of a cardinality greater than the set of natural numbers but less than that of the real numbers, since $\#N = \aleph_0$ and $\#R = \aleph_1$. The consistency of the Generalised Continuum Hypothesis with ZF was proved in 1939 by Godel, and its independance of ZF by Cohen in 1963. The Axiom of Choice is likewise independent of the rest of ZF. However, unlike the Axiom of Choice the Generalised Continuum Hypothesis is not required as an assumption to fund mathematical reasoning. Black box reasoning fails to reach a verdict on the Continuum Hypothesis[6]. There are at least two possible conclusions to be drawn from this.

The first conclusion is that the black box which is the world of sets, cannot be thoroughly explored by black box reasoning. The conclusion will the be that, although the Continuum Hypothesis is either true or false, human reason may never be able to determine which. Parenthetically, it should be said that the present indecision regarding the Continuum Hypothesis need not last forever. Advances in mathematics and, (especially) maths-related disciplines like physics may throw the spotlight of application on the purest of mathematics.

Diagram 6


Atomic Theory
postulates a
BLACK
BOX
The World
of
Atoms
Hypothesised Structure
(e.g. atoms are widely
spaced)
Macroscopic Observations
(e.g. α particles
penetrate gold foil)


Set Theory
postulates a
BLACK
BOX
The World
of
Sets
Axioms of Set Theory
(e.g. Axiom of Choice)
Basic Mathematical
Assumptions (e.g. any
countable domain can
be well-ordered)

Should it ever transpire that transfinite arithmetic becomes useful in this way, it may be that some purpose will be found in assuming the Continuum Hypothesis.

A second possible conclusion is that the very failure of black box reasoning to pronounce on the Continuum Hypothesis shows something about the black box of sets: namely that the Continuum Hypothesis is neither true nor false. The idea is that what obtains in a black box is to be identified with what can be in principle inferred to obtain in the box. What cannot be inferred is not there to be inferred (As Wittgenstein once remarked '..a nothing would serve just as well as a something about which nothing could be said'). Such a conclusion flies in the face of classical logic, with its insistence on LEM and PB. But we shall see in chapter 5, strong philosophical reasons which argue for the existence of just such a kind of ontological indeterminacy.

4.7 Non - E variables

Only E variables have been so far considered. Many variables are not E - variables. The function-expression 'g', here defined, is not an E variable

$$g(n) = \begin{cases} n + 1 & \text{if there are } n \text{ consecutive 7s in the decimal expansion of } \pi \\ n - 1 & \text{otherwise.} \end{cases}$$

g is uncomputable. If there are not n consecutive 7s in the expansion of π, there is no effective method of determining g(n). Nevertheless, we assume that the above definition gives 'g' a sense. But how 'g' acquires a sense cannot be explained by the hypothesis that human beings associate the same values to the same arguments to g. This hypothesis is false: we simply cannot compute such values as the arguments rise in magnitude.

Uncomputable functions may generate one kind of non - E variable. There are others. The predicate 'x is an atom' is a non - E variable. Human beings cannot straightforwardly indicate atoms and it is not expected of a person who claims he grasps the sense that he, per impossible, should be required to pick out individual atoms.

Atomic theory had its earliest sponsors in Leucippus and Democritus who lived in the Golden Age of Greek Civilisation. We can imagine a sophist, Skeptikos, confronting Leucippus with the following argument.

'My dear Leucippus; I find your atomic theory and your cosmogony quite fascinating:- but I must confess that your use of the word 'atoms' (ατομος) perplexes me. You admit that atoms are too small to be seen and so you cannot point them out to me. How then am I to know what sense you attach to this word 'atom'? For all I know, what you have in your mind when you talk of atoms may be quite different from what I have in mind. There really can be no communication between us on this topic because no meaning can be attached to this word 'atom' at all.'

If Leucippus were alert to a reply, then he might offer the following.

'You are right, Skeptikos, in saying that my atoms are too small to be seen. You are right in saying I cannot point them out to you. But you are wrong in insisting we cannot come to appreciate what they are. What I mean by 'atom' is a particle of the shape of a round smooth stone, but unbreakable, and of a thousandth part or less of a grain of sand. I am sure that when I have defined 'atom' for you in this way, that you will appreciate as well as I, what I mean by 'atom'. You know how to pick out a round, smooth stone and you have seen amphorae of wine broken. You have seen moneylenders weighing gold in the market and know what a thousandth part by weight looks like. Well then, you understood everything you need to know in order to understand what I mean by 'atom'.'

The strength of this reply hinges on the ability of Leucippus to define 'atom' in terms which Skeptikos has to agree he understands.

A similar sort of technique can be used to well-introduce 'g'. Suppose that both S and H attach the same sense to 'g'. Whether this is true or not, the supposition cannot be tested by asking them to write out the extension of 'g'. However, it can be concluded that S and H attach the same sense to 'g' if they attach the same sense to the definienda of 'g'; i.e. if S and H mean the same by 'decimal expansion', 'successive', '+' and so on. In short, the ability to test whether a non - E variable like 'g' or 'atom' has the same sense for all speakers depends on being able to give some account of what the non - E variable means in terms of E variables.

In the context of a canonical language an ontologist who introduces a non - E variable ξ, shows that ξ is well-introduced if, and only if, he can offer a set of constructional definitions which allow for the decomposition of ξ into E variables and logical constants alone. Formally, where S, is any sentence of the canonical language containing a non - E variable; D is the set of all constructional definitions for the canonical language are all well-introduced iff:-

$$S_1 \dashv\vdash_D S_2 S'$$

where S_2 is a canonical sentence that contains no non - E variables.

Example: the ontologist lists 'proton x' as a non - E variable. He lists 'qualitatively identical x,y', 'cathode plate x', 'deflected towards x,y, 'numbers the mass in kg x,y', 'x $\geq$ y', 'x $\leq$ y', 'x', '1.8', '1.6', '10^{-27}', as E variables. Within D, his set of constructional definitions, he has the following axioms.

$$(x)\ \text{proton } x \equiv \left[\begin{array}{ll}\text{positive } x\ \&\ (\exists y)(y \leq 1.8 \times 10^{-27}\ \&\ y \geq 1.6 \times 10^{-27} \\ \text{particle} \qquad\qquad \&\ \text{numbers the mass in kg. } (y,x))\end{array}\right]$$

$$\begin{array}{c}(x)\ \text{positive } x \\ \text{particle}\end{array} \equiv \left[\begin{array}{l}(\exists y)(\exists z)(\text{cathode plate } y\ \&\ \text{deflected towards } (z,y) \\ \&\ \text{qualitatively identical } (z,x)\end{array}\right]$$

These two axioms allow for the decomposition of 'proton x' into variables listed as E variables and logical constants. Consequently 'proton x' is well-introduced by the principles that allow for the construction of first-order languages.

Within the context of a canonical language, every non - E variable listed will be formally analysable into E variables with the aid of constructional definitions. However in deciding in what way a given non - E variable is to be introduced in a canonical language, it is not necessary to believe that there is one and only one proper analysis of the variable in question.

As a case in point consider the expression 'hydrogen' (meaning water gas) which has had a use in chemistry since the eighteenth century. There are many properties which can be used to define 'hydrogen'; e.g. hydrogen is the gas evolved at the cathode in the electrolysis of water; hydrogen is the gas evolved by the action of dilute mineral acids on iron, zinc or aluminium; hydrogen is the element with only one proton in the nucleus of the atom. Faced with a multiplicity of ways of fixing the sense of 'hydrogen' there is no reason to believe the word has only one correct analysis. The same holds true of names introduced as non - E variables: there is no reason to suppose that such names can be fixed in sense by only one description. However, in the context of a canonical language, uncertainties in meaning like this are conventionally eliminated by selecting some one analysis. Having made this selection, the analysis then fixes the sense of the sign.

APPENDIX I

No Entity without Identity

Within ontology it is said that to include some novel sort K of entities as amongst the things that exist is to be obliged to provide an identity criterion for Ks. This maxim raises two immediate questions which it is the purpose of this appendix to answer.

(1) Why do we need identity criteria?

(2) If we do, what form should they take?

The first question is logically prior to the second. If we can see exactly why we need identity criteria - what task we need them to perform - then we will also know what constitutes a good criterion of identity and how it should be formed. Understanding what form any human construction should take depends first on understanding its purpose.

Frege is recognised to be the original source of the demand for identity criteria, though what he says is limited to a very short passage.

'.....we have already settled that number words are to be understood as standing for self-subsistent objects. And that is enough to give us a class of propositions which must have a sense, namely those that express our recognition of a number as the same again. If we are to use a symbol a to signify an object, we must have a criterion for deciding in all cases whether b is the same as a, even if it is not always in our power to apply this criterion.'

Frege [46] (62)

The argument, as I understand it, is as follows: if we regard 'a' and 'b' as denoting individuals then we must regard 'a = b' as true or false and hence meaningful. In order to claim 'a = b' is meaningful we have to state the truth-conditions of 'a = b'. Such a statement is a criterion of identity.

Frege's argument leaves very little clue as to how such a criterion should be framed? Would '$a = b \equiv (F)\, Fa \equiv Fb$' count as a criterion? If not, why not? As Frege's alleged leading interpreter it is natural to turn to Dummett to justify and expand Frege's position:-

'If we are to understand an expression as standing for an object, then we must be able, in Frege's vivid phrase, 'to recognise the object as the same again' we must, that is, know under what conditions some other term will stand for the same object. If, for instance, I am told 'This is the River Windrush' and I have no idea of how to determine whether it would be right, at some other place or time or both, to say once more 'This is the River Windrush' then I know nothing about the expression 'the River Windrush' save the bare fact that it was right to say 'This is the River Windrush' at that very time and place. I thus do not know what object was being named, or, indeed that the expression was being employed as a <u>name</u> of an <u>object</u> at all. It could have meant, 'This is beautiful' or anything.... To the extent that I am uncertain of how to 'recognise the object as the same again', not only can I not be said to know what object it is, but I also do not know what is true of it.'

Dummett [42] (73 - 74)

Aside of the historical inaccuracy in interpreting Frege in this manner, what Dummett says here is wrong.

For example, suppose I have a box of qualitatively identical ball-bearings. I pick one ball-bearing out of the box and baptise it 'Henry'. I return Henry to the box and closing the lid shake the box vigourously. Looking at the mass of ball-bearings now at the bottom of the box I have no idea which one is Henry.

Not only have I no idea which one is Henry, but I have no idea how to go about finding which is Henry. But it does not follow the name 'Henry' makes no sense to me and that I cannot understand what is true of Henry. I know Henry is in the box; I know Henry is a ball-bearing; where a,b,c...,n are all the ball-bearings in the box, I know that (a = Henry) v (b = Henry) v (c = Henry)....v (n = Henry). (Dummett chooses to ignore an important rider to Frege's demand for an identity criterion:- namely it need not always be withn our power to apply such a criterion; or even know how it could be applied for every case).

Wiggins in Sameness and Substance also has his own views on why criteria of identity are important.

'The real and abiding interest of Frege's demand for the criterion of identity seems to me to be this: whenever we suppose that entities of kind f exist we are committed to ascribing some point to typical identity questions about particular fs; and, in so far as identity is a puzzling or problematic relation, the first concern of the philosopher of any subject matter must be to enhance our powers of finding the elucidation.... for its disputed identity questions.'

Wiggins [148] (53 - 54)

Much of the vagueness that attached to Frege's original exposition is preserved by Wiggins. Wiggins does not make clear why identity questions are so significant bar saying that we are committed to ascribing some point (what point?) to typical identity qustions. If Wiggins had made clear what this point was then his case would have been considerably advanced. Perhaps the suspicion of a justification lies in his remark that identity is or can be a puzzling relation. Now if 'puzzling' is intended to mean confusing, paradoxical or obscure to men in general then I do not think what Wiggins says is true. If I said 'Caesar was the Roman general who with his army crossed the Rubicon in 55 B.C.' then this statement is neither puzzling, paradoxical or obscure to anybody who grasps plain English. On the other hand if Wiggins means puzzling to those with special interests in philosophy then he may be right. But then, we are entitled to some explanation of what the puzzle or the problem might be, and this is something Wiggins does not provide. Though it would be possible to multiply the instances of philosophers who have thought that criteria of identity are good things to have, without knowing why, such multiplication would not advance our understanding.

Understanding why criteria of identity are necessary starts with realising that a kind K of entity is always introduced into an ontology under a sortal expression. A sortal expression is an expression of which we can ask how many things there are which fall under that expression. 'Table', 'natural number', 'star', 'human beings presently (i.e. at 3.20 p.m. on the 11th July 1984) alive' are all sortal expressions. In each case we can sensibly ask 'how many?':- how many tables are there?; How many natural numbers? How many stars? How many living human beings? In each case the objects that fall under these expressions are rightfully deemed as entities if we accept they exist. In fact we can say that the properties which are essential to an entity qua entity are those inferrable from its falling under a sortal expression.

When a kind K of putative entities is introduced into an ontology the first requirement is to show that Ks have even the right to be considered as entities which could exist; that is to say, we are required to show that, where ψ_K is the expression used to denote Ks, that if anything falls under ψ_K then it is an entity of some sort. Thus if we suppose possible worlds to exist we need to show that, if they did exist, they would be entities of some kind. To be an entity is to either fall under, or be capable of falling under, a sortal expression. Therefore the obligation to show that Ks could ever be entities is discharged when ψ_K is shown to be a sortal expression.

A sortal expression is one to which we can sensibly attach a 'How many?' question. This means it is sensible to suppose that there are a distinct number of things, individuated one from another, which fall under the sortal expression and are therfore countable, if not by ourselves then perhaps by a being of greater powers e.g. God.

There are at least 3 distinct abilities presupposed in counting a collection of entities of kind K. If I wish to count the collection of all Ks then, first, I must know when I am counting in a K rather than a non-K. I must be able to discriminate Ks from non-Ks. Second, I must know when I am assigning a number to one K or more than one K. I have to assign the number 1 to one K, 2 to another, 3 to a third and so on. I must not get confused as to where one K begins and another leaves off. I must be able to distinguish one K from another. Third, and last, I must never count the same K twice. If I came to a K that I have counted in, I must not count it in again. I must be able to recognise the same K I met before. Even if we cannot, for practical reasons, actually count the numbers of entities that fall under a sortal expression, still we can only make sense of the idea that there is a definite number to that collection by analogical extention of our own counting abilities. These abilities presuppose possession of criteria of demarcation (distinguishing Ks from non-Ks), also of differentiation (distinguishing one K from another) and finally of persistance (seeing when one K is numerically the same as a K encountered previously). Thus if we succeed in associating with ψ_K criteria of demarcation, differentiation and persistance then we will have shown ψ_K collects a countable collection and hence that ψ_K is a sortal expression. This is what is also needed to show that Ks could even be entities.

We have established a connection between the concepts entity, sortal expression, and criteria of demarcation, differentiation and persistance. We can add to the family, sets and quantification.

In set theory it is commonly assumed that every set has a definite cardinality (in ZF this is demonstrable from the Axiom for Cardinal Numbers; (see Suppes [140] (111)) and it is also common to denote sets by a set-abstract of the form

'$\{v: F_v\}$' where F_v is an open sentence. The set denoted is the set of all and only those things collected by F_v. Now unless F_v were a sortal expression, the collection that F_v collected would not be countable and '$\{v: F_v\}$' would not have any definite cardinality. Consequently it is a condition on the employment of set abstracts that only sortal expression be used within the braces '$\{$', '$\}$'.

The connection of sortal expressions to set theory leaks over into quantification in two ways. First, we can easily pass over from set-theory into the schema '$(\exists v)F_v \equiv (\exists v_1)\ v_1 \varepsilon \{v: F_v\}$'. The restrictions on the sortal nature of F_v are then transmitted into quantification. But, second, the relation between quantification and sortal predicates lies in the semantics for first-order languages themselves. In order to turn an uninterpreted first-order formula into something that constitutes a declarative sentence, the formula requires an interpretation. Thus we attach a domain D as a range of the bound variables, an element of D to each individual constant of the formula, an n-ary function on D to each n-place function letter and a subset of D^n to each n-place predicate letter. The truth-conditions of the resulting sentence are easily determinable given a grasp of the sense of the logical constants used. Essentially, any sentence which can be generated from a first-order formula can be generated in this way. Even if we should rather choose to replace the predicate-letters by predicates, the individual constants by denoting names and the function letters by function expressions, still we are only doing, in an indirect way, what is done in assigning an interpretation to the logical variables themselves. The only difference is that the symbols we supplant the logical variables of the formula with, carry with them their attendant interpretations. <u>But unless these interpretations were of the appropriate kind, what would result would be not a declarative sentence but nonsense.</u> To demand a predicate ψ carry with it the appropriate interpretation to allow it to substitute for a predicate letter is to demand it carry a <u>set</u> as its extension.

But since every set has a cardinality, it follows that if ψ is appropriate, it must be a sortal term. Only sortal predicates may be attached to objectually bound variables.

(This raises obvious problems if the task is to formalise sentences containing mass predicates. Our reasoning forbids us to formalise 'Water exists' as '($\exists$x) water x'. In order to handle mass predicates with objectual quantifiers, the mass predicates must be converted into sortal predicates. This conversion involves the use of what Griffin [57] calls a SAN (sortalising auxiliary noun). A SAN is a sequence of signs that constitutes a sortal forming operator on mass expressions.

e.g. SAN	Mass Expression
is a molecule of	hydrogen
is the fusion of all	water

Once a mass predicate has been sortalised, then the resulting complex sortal predicate can be formalised in the standard manner. So 'Water exists' might appear as '($\exists$x) x is a molecule of water.')

To sum up then, criteria of demarcation, differentiation (identity) and persistence are what needed to show an expression is a sortal expression. Sortal expressions themselves play a significant part in ontology in at least three ways; (i) to demonstrate that an expression is a sortal expession is to demonstrate that whatever falls under that expression is an entity, (ii) to demonstrate that an expression is a sortal expression is to show it can be used in set-theoretical reasoning; (iii) to demonstrate that an expression is a sortal expression is to legitimise the procedure of quantification. To fail to provide the appropriate criteria and yet to make full use of the machinery of set theory and quantification is 'symbol pushing' of the lowest kind. Readers of the current literature on possible world semantics will know who to number amongst the guilty.

For an ontologist who has already guaranteed that his canonical variables are well-introduced by the standards of chapter 4, the requirement to produce the appropriate criteria of demarcation, identity and persistance will have been largely fulfilled. To take each in turn:-

Criteria of Demarcation

To demonstrate that a variable v has a high index of autonomy is to show that there must be some criterion for demarcating what falls under v and what does not. If there was not such a criterion, then members of M would be lost as when to classify objects under v and when not. Criteria of demarcation are thus subsumed by semantic matrix tests.

Criteria of Identity (Differentiation)

The gap between a criterion of identity and one of differentiation is a marginal one that can be bridged in a few logical steps. Given a criterion of differentiation for Ks of the form:-

$$(x,y; Kx, Ky)\ -(x = y) \equiv \ldots\ldots\ldots\ldots ;$$

elementary logical reasoning allows us to construct a criterion of identity of the form:-

$$(x,y; Kx, Ky)\ \ x = y \equiv \ldots\ldots\ldots\ldots ;$$

and conversely. The process of publification referred to in chapter 4, required, amongst other things, an individuating relation. It was pointed out that one of the most significant aspects of an individuating relation R for a class of prototype objects K was that it permitted the individuation of elements of K by their R-relations to elements of the domain D of discharged objects. In formal terms:

$$(x,y;\ Kx, Ky)\ (z;\ Dz)\ (Rxz \equiv Ryz) \supset x = y.$$

The converse of this formula; i.e:-

$$(x,y;\ Kx, Ky)\ (z;\ Dz)\ \ x = y \supset (Rxz \equiv Ryz);$$

is of course logically true. Putting the two together gives a criterion of identity

$$(x,y;\ Kx, Ky)\ x = y \equiv (z;\ Dz)\ Rxz \equiv Ryz.$$

Criteria of Persistance

To give a criterion of persistance for a kind K is to give some indication of what it is any K-element x at a time t to be identical with a K-element y at a time t + 1. This involves saying what the essential properties of x are i.e. those properties x cannot cease to have without ceasing to exist.

Criteria of persistance are indirectly subsumed under the principles of well-introduction; but the importance of such criteria vary enormously according to the nature of the objects introduced. In the case of abstract objects, criteria of persistance are trivially obtained from criteria of identity. For instance a number n_1, is identical to a number n_2 iff n_1, and n_2 number the same open sentences: this is true irrespective of temporal considerations. In the case of non-abstract objects (e.g. sense data) criteria of persistence are more significant. 'How long may a sense-datum persist?' and 'What are the conditions of its persistance?' are questions that demand a criterion of persistance for sense-data. Criteria of persistance may be conventional in such cases.

APPENDIX II

Substitutional Quantification and Ontology

There is a feeling amongst many philosophical logicians that there is something underhand about substitutional quantification. It is felt that by employing substitutional quantifiers we abjure our responsibilities to talk about the world and end up talking about language instead. Some worried authorities see in substitutional quantification, the downfall of ontology itself. Whenever an ontologist looks in danger of committing himself, he need only escape into substitutional quantification until the danger is past. This is the way Quine sees it.

'Where substitutional quantification serves, ontology lacks point.'

Quine [ ] ( )

Perhaps such a consequence is in itself an acceptable reason for not using substitutional quantification. But other less acceptable reasons are sometimes given. Quine, for instance, claims there are an indenumerably large number of objects (the real numbers between 0 and 1, the elements of the power set of the natural numbers) and there is at best only a denumerable number of names. Since there will be some (nameless) objects whose existence cannot be registered by substitutional quantification, substitutional quantification can never replace objectual quantification. Fortunate indeed that Cantor took an interest in infinity, otherwise philosophers would be faced with a universe containing only names! Henkin [66] (390-397), however, presented a notation for naming real numbers which allowed for infinite decimal expansions as names for real numbers.

Substitutional quantification has even been defined away! The argument is as follows: the substituends for substitutionally bound variables are names. A name is something that can be replaced by an existentially and objectually bound variable. Therefore any true name is something to which the Law of Existential Generalisation applies and so it must denote substitutional existential generalisation implies objectual existential quantification, since substitutional existential quantification claims a name is fit to replace the free variable(s) in the relevant open sentence to create a truth. But reject the authoritarian restriction on the use of 'name' and the argument collapses.

Contrived arguments against substitutional quantification do little to dispel the fear that substitutional quantification is inimical to the serious study of ontology. Understanding the ontological limitations of substitutional quantification first depends on understanding substitutional quantification itself. When this understanding is achieved, the threat of substitutional quantification to ontology is seen to be bogus.

With substitutional quantification as with objectual quantification, a quantified wff has a truth-value only when the wff has a range attached to its bound variables as well as interpretations for all its non-logical constants. In the case of an objectual quantifier, the range is some non-empty set. For a substitutional quantifier the range is some substitution set of meaningful signs. So write the objectual existential quantifier '$\exists$' and the objectual universal quantifier '$\forall$': let D be the range of the bound variables and let 'F' be some dummy predicate, then:-

'$(\exists x)Fx$' is true iff there is some element d, where $d \in D$ and Fd

'$(\forall x)Fx$' is true iff for any element d, if $d \in D$ then Fd.

Let 'Σ' and 'Π' be the substitutional existential and universal quantifiers respectively. Let S be the substitution set assigned to the substitutionally bound variables, then:-

'$(\Sigma x)Fx$' is true iff there is some element s, where $s \varepsilon S$ and $'F'^\frown s$ is true

'$(\Pi x)Fx$' is true iff for any element s, if $s \varepsilon S$ then $'F'^\frown s$ is true.

From now on I adopt the convention of referring to the elements of a substitution set as substituends. The result of replacing all the variables in an open sentence by substituends is a substitution instance.

In first-order substitutional quantification only definite singular terms are substituends. In second-order substitutional quantification the substitutional quantifiers also bind variables which stand where predicates may stand. In regard to first-order substitutional quantification there are two different kinds of case. The substitution set may include only denoting singular terms or it may not. Call the first kind 'denoting substitutional quantification' and the second 'vacuous substitutional quantification'. So we end up with the classification illustrated below

Substitutional Quantification

Second Order

First Order

Substitution set includes predicates; substitutionally bound variables can occur where predicates may stand.

Substitution set includes only singular terms; substitutionally bound variables only occur where terms may stand.

Denoting

Vacuous

Substitution set includes only denoting terms.

Substitution set includes at least one vacuous term.

Denoting substitutional quantification presents no ontological threat; it is unequivocally a medium where ontological commitments occur. Say that '..... is a winged horse' can be completed by a denoting singular term to create a truth and you have said as good as 'There are winged horses'. Second order substitutional quantification might pose a threat to those who see an ontology of universals in sentences like 'Everything has at least one property' and would prefer to see this as grounds for quantifying over universals as in '$(\forall x)((\exists F)(Fx))$' rather than '$(\Pi x)(\Sigma F)(Fx))$'. But here we touch on a specific issue and it is not clear the case for universals is strong enough to bar the employment of second-order substitutional quantification.

Back to generalities. Where the feathers fly is over vacuous substitutional quantification, for there ontological issues look as if they could be avoided. It is said we need only employ vacuous substitutional quantification throughout a canonical language to shirk all ontological commitments. This is true in a superficial way. Given '$(\Sigma x)Fx$' and a substitution set that includes at least one vacuous term, no conclusion can be drawn as to the existence or non-existence of Fs. The stage seems set for some sinister ontological evasion.

But before anything happens the ontologist must say what substitution set is allotted to the substitutionally bound variables. In addition, the ontologist has an obligation to give an explanation of the senses of the substituends. This explanation will explain the truth-conditions of any substitution-instance in which a substitutuend plays a part. Such an explanation must be provided for each and every substituend, whether that substituend is a denoting or a vacuous term.

In the case of denoting terms this is no problem. The result of substituting a denoting term t_d for 'x' in 'Fx' is true just when the object t_d denotes satisfies F. This schematic account provides not only an account of the sense of t_d (how it contributes to the truth-conditions of sentences it occurs in), it also provides the beginning of semantic descent away from talk of truth to talk about the world.

No similar account is available for vacuous terms. Where ϕ is any predicate of our canonical language, we explain nothing by saying that the result of substituting a vacuous term t_V for 'x' in $\phi^\frown$'x' is true iff $\phi^\frown t_V$ is true. What we need is an account of <u>what it is</u> for $\phi^\frown t_V$ to be true. This account should allow us to complete

$$\phi^\frown t_V \text{ is true} \equiv \ldots\ldots\ldots\ldots$$

without mention of truth.

To move from the general to the specific: if we introduce the name 'Pegasus' into our substitution set, we have to explain why we count 'Pegasus is a winged horse' as true but 'Pegasus is a small red cube' as false. Such an account should hopefully be generalisable to provide an explanation of the truth-conditions of 'Pegasus-sentences' as a whole. A formalised explanation of this kind would begin 'For the given predicate Φ, $\Phi^\frown$ 'Pegasus' is true if and only if.....' and then provide a completion which would enable us to eliminate 'true' as a semantic primitive.

Vacuous names in ordinary speech provide the basis for a good example of such an explanation. Vacuous names are of two kinds. There are names of mythology and names of fiction. Names of mythology are vacuous names which originated from a community which believed they denoted. Names of fiction are names that originated from a person or persons who recognised that they were vacuous. 'Zeus' is an example of a mythological name; 'Sherlock Holmes' of a fictitous one. In each case the truth-conditions for atomic sentences containing these names are similar. For mythological names (outside of sentences like 'Zeus was believed to be a god by the Ancient Greeks'), the truth-conditions are dictated by what classical authors and others have written. 'Zeus was the husband of Hera' is true because of what classical authors have recorded of the ancient Greek religion. 'Sherlock Holmes lived at 22b Baker Street' is true because of what Sir Arthur Conan Doyle wrote. These observations could be generalised for a wide class S of predicates.

i.e. For any member ψ of S

'Zeus' is true iff ψ ⌢ 'Zeus' is entailed by the contents of <u>The Greek Myths</u> Vol I - II by Robert Graves

'Sherlock Holmes' is true iff ψ ⌢ 'Sherlock Holmes' is entailed by the written works of Sir Arthur Conan Doyle.

Consider an actual case where vacuous substitutional quantification is used to try to avoid ontological issues: substitutional set theory. Suppose an ontologist wished to quantify using set-theoretic terminology, but at the same time to avoid a commitment to sets. Instead of writing a formula of the kind:-

$$(\exists X)((\exists Y)(X \subseteq Y));$$

where 'X' and 'Y' range over sets; he writes:-

$$(\Sigma X)((\Sigma Y)(X \subseteq Y)).$$

But he is nonetheless obliged to say what substitution set S of singular terms is allotted to 'X' and 'Y' in the formula immediately above. Presumably the ontologist will define S to be the totality of all set-abstracts, with certain restrictions, perhaps, to prevent the inclusion of set-abstracts which may have paradoxical consequences (e.g. '$\{X: X \notin X\}$', which is the foundation of Russell's Paradox). Having done so, the ontologist is then faced with the task of explaining the senses of the substituends. Such an explanation should provide us with an explanation of the truth-conditions of any atomic sentence which is also a substitution-instance; i.e. an atomic sentence derived by removing all the binding variables from a substitutionally quantified wff and replacing any free variables so obtained by elements from S.

If the ontologist is successfully to continue his program of practising set theory but avoiding sets, his explanation of the truth-conditions of these atomic sentences cannot refer back to sets. This means that a homophonic explanation of the truth-conditions of these atomic sentences is ruled out immediately. The ontologist cannot, for example, complete:-

''$\Lambda \subseteq \{x: x = x\}$' is true iff ....,'

by

'$\Lambda \subseteq \{x: x = x\}$'

without admitting the existence of the empty set and the universe set.

Dr. J.E. Tiles has suggested in correspondence to me, a non-homophonic explanation of these atomic sentences. Tiles' suggestion is to anchor the predicate 'is true' by 'is provable in ZF', he points out that ZF set theory is written in a formal language which determines whether a given expression is a well-formed singular term (wfst). When these terms are used to close open sentences expressed in the language of ZF, one can tell in a finite number of steps whether any finite sentence of wffs constitutes a proof of the sequence thus formed. Tiles then interprets the truth-conditions of the substitutionally quantified formulae thus.

'$(\Sigma x)\ \psi x$' is true iff for some wfst, t, ψt is provable in ZF

'$(\Pi x)\ \psi x$' is true iff for any wfst, t, ψt is provable in ZF

Where this suggestion falters, I believe, is in application to set-theoretical sentences <u>outside</u> the scope of ZF. Consider the set-theoretical assertion that the power-set of the set of all philosophers has a least two elements:-

$$(\exists x)(\exists y)(x \neq y \ \& \ x \in P\{z\text{: philosopher } z\} \ \& \ y \in P\{z\text{: philosopher } z\})$$

A Platonistic and entirely natural interpretation of the above formula takes it to report on the existence of at least two abstract objects, sets, (e.g. {Quine}, {Peirce}) which are both members of P{z: philosopher z}. Exchanging 'Σ' for '$\exists$' allows the ontological commitments to be postponed; but at the cost of specifying a substitution-set and giving an account of the truth-conditions of the substitution instances. Following Tiles, we might give as the substitution set, the set of all wfsts and substitute 'provability in ZF' for 'true'. But then the formula:-

$$(\Sigma x)(\Sigma y)(x \neq y \ \& \ x \in P\{z\text{: philosopher } z\} \ \& \ y \in P\{z\text{: philosopher } z\})$$

turns out to be false. The language of ZF does not permit us to construct wfsts which will substitute for the bound variables in this formula to create a truth. Even if we increase the stock of wfsts to allow for, e.g. ' Quine ' and ' Peirce ', the formula will still be false; for it is not provable in ZF that {Quine} $\neq$ {Pierce} & {Quine} $\in$ P{z: philosopher z} & {Peirce} $\in$ P{z: philosopher z}. The price of relativising the truth-conditions of the sentences of substitutional set theory to one of the standard axiomatised set theories, is that we find no place for substitutional assertions outside of these axiomatisations. Only from the view of the purest of pure mathematicians, could a set theory which offered no scope for application outside of mathematics seem a tenable option.

What began as a simple and expedient way of practising set-theory without sets, turns into a major operation. Whatever device the substitutional set theorist finally seizes on to anchor the truth-conditions of his substitution-instances, it is clear he has a formidable task in front of him. He must:-

(a) explain the truth-conditions of the substitution instances. The concept 'true' as applied to these instances must not remain a semantic primitive.

(b) Do (a) without appealing to the existence of sets and yet ...

(c) ...succeed in preserving the truth-values not only of those set-theoretical assertions that receive valuations in pure mathematics, but also preserve the truth-value of those which are the result of applying the set-theory to the physical world.

If the substitutional set theorist succeeds in (a), (b) and (c) I expect that he will have accomplished something very like an ontological reduction of sets and not just an ontological evasion.

The point can be generalised. Introduction of vacuous substitutional quantification introduces an obligation to explain the senses of the vacuous terms used. This obligation is discharged when the concept of a sentence which contains any such vacuous name being true is shown not to be semantically primitive. The ontologist has to explain this concept by reference to the world alone. This is the point when deferred ontological responsibilities come home to roost. Substitutional quantification is not an easy way out: it is hard work.

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[1] Actually, to be ontologically neutral the question should rather be 'Does this sentence count as true-in-the-characterising language?' In the case of physical objects however, the ontological evidence for their existence seems so overwhelming that it seems futile to maintain the distinction between 'true' and 'true in any ontology of physical objects'. See 4.4 for a defence of the existence of physical objects and an attack on Idealism.

[2] Compare Strawson [136] (13)

[3] To run through those arguments would take more space than can be spared here, though I do not wish to give the impression that the issues are too trite to worth mention. Russell [124], and Price [106] are good sources of these arguments. Austin [4] conducts a very able attack on them.

[4] Rutherford's hypothesis had been anticipated in 1901 by Jean Perrin, who supposed the atom to be a miniature Solar System in which electrons revolved round a positive nucleus. See Silva and Lochak [132] (82).

[5] See Suppes [140] for a statement of the Axiom of Choice and its equivalents. Makinson [86] contains a brief survey of the important axioms of set theory.'

[6] See Suppes [140].

[7] For a good review of how mass predicates have been handled see Pelletier [97]. Some of Pelletier's conclusions are suspect, however, since he presumes formalisation preserves meaning.

CHAPTER FIVE

Ontological Elasticity

Ontological elasticity is the property that a theory displays when the domain of discourse of that theory can be identified with different domains. Competing, but equally satisfactory recursive reductions of a single axiomatised theory (such as the axioms for number theory) offer the best known and most spectacular examples of ontological elasticity. The phenomenon can, however, be extended to natural language theories through the graces of formal frameworks. In chapter three, we saw a limited instance of ontological elasticity in respect of sentences of measure. This elasticity was proven by alternative formalisations of sentences of measure: one which represented these sentences as concerned with impure numbers, the other which represented them as concerned with pure numbers.

The usefulness of ontological elasticity to the working ontologist is that it enables him to refute unwanted charges of ontological commitment. For instance, faced with the charge that he is committed to impure numbers in accepting statements of measure, the ontologist can appeal to a formalisation which makes no use of an ontology of impure numbers. Ontological elasticity is as fundamental to ontology as energy is to physics or valency to chemistry. But like many such philosophically significant concepts, the proper analysis of ontological elasticity is frought with problems, to which we now turn.

5.1 The Analysis of Ontological Elasticity

I wish to consider the question 'What is it for a theory to be ontologically elastic?' and the answers that will be considered fall roughly into three broad groups: (a) answers which attempt to define ontological elasticity in proof-theoretic or syntactic terms; (b) answers which attempt to define ontological elasticity in model-theoretic terms; (c) answers which define ontological elasticity in intensional terms. Of each sort of answer, only the last group has, I believe, any chance of real success. At the same time, recognising ontological elasticity as intensional has the depressing consequence that our philosophical understanding of ontological elasticity must wait on the elucidation of disputed concepts in the theory of meaning.

Since formal frameworks are the modus operandi of ontology, it is both natural and legitimate to try to exploit them in defining ontological elasticity. An initial attempt to bring such a definition to fruit is this one. A theory T (where T can be written in either formal or natural language) is ontologically elastic just when, if T is incorporated into a target language, there is more than one extensionally adequate formal framework capable of accommodating the language in which T is expressed.

The definition raises two problems. The first arises out of the syntactic nature of the definition offered. Formal frameworks are a species of first-order theory, and like all first-order theories, are individuated according to the formulae deducible from them. Trivial syntactical changes in the choice of signs used to express a chosen ontology will, strictly speaking, generate a new formal framework just as much as a more fundamental change in ontology itself will. This weakness besets another definition of ontological elasticity in respect of formal theories; that is, the definition that a first-order theory is ontologically elastic if and only if it is recursively reducible to more than one other theory. Syntax, uninformed with any commentary about the meanings of the signs used, simply fails to separate the noise from the pure signal.

A second, rather deeper problem, lies in the nature of formal frameworks themselves. That different formalisations of the same target language obtain, even supposing that these formalisations are not merely syntactical variants, does not entail that a given target language theory T is itself ontologically elastic. To reason thus would be to reason as a mechanic who erroneously infers that since the engine is faulty and the ignition is part of the engine, then the ignition is faulty as well. The ability to construct significantly distinct formal frameworks show that ontological elasticity is at work <u>somewhere</u>, but does not help to localise it.

A thought is that perhaps the solution to this lack of localisation is to be more modest in the choice of a medium of formalisation, or else to try to construct some test whereby, like diligent mechanics, we can try to trace the ontological elasticity can be traced down to some one component in the machinary of a formal framework. As for the first alternative we might try to determine the ontological elasticity of a piece of theory by constructing a mini-framework whose target language was only just rich enough to express it. The presence of many non-trivially different mini-frameworks with a common target language would prove the elasticity of our piece of theory. But this modest way of formalising is inaccurate in principle. It simply does not follow from being able successfully to formalise a piece of theory that that formalisation will be sustained when it is incorporated into the wider perspective. A good formalisation of pure mathematics should also be extendable to sentences of applied mathematics and from thence to the sciences in general. There are simply no self-contained language-games to be found short of that language-game within which all our moves can be made. Mini-frameworks can reveal an illusory range of possible trails that peter out when viewed from a height of a formal framework.

Can formal frameworks themselves be used to localise the elasticity? It may, perhaps seem that they can. Merely by contrasting different formal frameworks and seeing <u>where</u> they differ (i.e. to what target language sentences they assign different formal sentences to) surely one could see where the elasticity fell? But sadly, this test is inaccurate too, for any theory can be rendered ontologically elastic in this way. Thus let F be an extensionally adequate formal framework to which T is a target language theory; to construct a new extensionally adequate framework F', increment each sentence used to formalise any true sentence S of T by p, where p is true in the canonical language of F, or -p if S is false. T will now be formalised differently whatever value for T. Only by binding formal frameworks by tighter intensional conditions like synonymy, can such evasions be ruled out. But to introduce these restrictions is to subvert the very purpose of formal frameworks themselves: formalisation need not preserve meaning. Perhaps the fault is not so much in the formal frameworks' failing to localise accurately ontological commitment, but in the fact that the very idea of localising ontological elasticity may be fundamentally misconceived.

The ontological elasticity which is credited to arithmetic is sometimes put down to the plenitude of its non-standard models. Is ontological elasticity, then, merely a matter of a theory having more than one model? This would be conveniently simple if it were true. But it would also have alarming consequences if it did obtain, for then ontological elasticity would be commonplace in practically every consistent theory. Ontological elasticity on this scale would discharge practically every theory of its ontological commitments, since for whatever model we found to convict a theory of certain ontological commitments, there would be another close by to pardon it.

Consequences like this have been seen in the Lowenheim - Skolem theorem by various philosophers and mathematical logicians. This theorem states that every consistent theory has a model with a denumerable domain. This theorem is the basis of Skolem's Paradox. The paradox is that there seem to exist consistent first-order theories which claim the existence of indenumerable sets. (Cantor's proof of the indenumerability of P(N), the power set of the natural numbers, being one example); but nevertheless the Lowenheim -Skolem theorem proves that these theories have models which do not require the existence of indenumerable domains.[1]

Skolem's Paradox is not a paradox in the true sense of the word: it is not centred (unlike Russell's Paradox) on a statement which is true if and only if it is false. The natural question is to ask why 'Skolem's Paradox' is felt to be a paradox at all.

I believe that one reason for this is that mathematical logicians have tended to harbour a suppressed premiss by which ontological commitment is defined model-theoretically. If this premiss is brought to the surface, and expanded, it runs somewhat as follows. If every model $J = \langle D,i \rangle$ of a first-order theory T is such that $(\exists x)Kx \ \& \ x \in D$, then T must be treated as ontologically committed to Ks. Conversely if there is a model $K = \langle D',i' \rangle$, where $-(\exists x)Kx \ \& \ x \in D'$, and K is a model of T, then T is not ontologically committed to Ks. This reasoning issues in a definition of ontological commitment somewhat as follows:-

for any theory T and sort K, and where $n \geq 1$;

$T \ oc \ K$ iff $(D)(i) \ \langle D,i \rangle$ is a model of $T \supset [(\exists x)(Kx \ \& \ x \in D) \vee (\exists x)Kx \ \& \ x \subseteq D^n)]$

Now, formally, it might appear as if there is a genuine contradiction to be derived from Skolem's Paradox, because (a) we want to claim that there are consistent theories with commitments to indenumerable domains (b) that nonetheless any consistent first-order theory has a model in a denumerable domain, and also (c) that a theory has commitments of the kind mentioned in (b) just when every model of that theory contains an indenumerable domain.

There are several things wrong with this line of thinking, not the least of which is the model-theoretic definition of ontological commitment given above. But to begin modestly, it is perhaps useful to note that the phrase 'model contains a denumerable domain' is ambiguos: and the ambiguity centres about the use of 'contains' which can mean, in context, either one of two things. (a) It can mean (and is generally used to mean) that where J = $\langle D,i \rangle, \# D = \aleph_0$; (b) it can mean that where J = $\langle D,i \rangle$, $(\exists x)\ x \in D \ \& \ \# x = \aleph_0$. It may be that if T is consistent, then there is some model of T with a denumerable domain in sense (a): which is what the Lowenheim - Skolem theorem asserts. But this is quite consistent with every model of T being 'indenumerable' either in the sense that the domain of that model has an indenumerable number of elements or else in the sense that some element of the domain of that model is indenumerable. In such a case T will be ontologically committed to the existence of an indenumerable item, even by the model-theoretic definition of ontological commitment given previously.

One way of restoring Skolem's paradox is to invoke a stronger form of the Lowenheim - Skolem theorem. A stronger theorem states that every consistent first-order theory has a model in the domain of natural numbers. Treating natural numbers as unreduced (i.e. non-set theoretical) objects it follows that for any consistent first-order theory there is a model which does not contain an indenumerable set either in sense (a) or (b) of 'contain'. This restores Skolem's Paradox.

It also brings in a lot more: specifically the spectre of Pythagoreanism - the view that only numbers exist. If other things than numbers exist, then there must be some true first-order theory T which is ontologically committed to non-numbers. Let us suppose there is some such theory T, where T is committed to something which is neither a number or a set containing numbers ('$(\exists x)$ mountain x' would be such a theory). From the definition of ontological commitment given, we derive

'$(\exists x)$ mountain x' oc mountains iff

$(D)(i)\ \langle D,i \rangle$ is a model of '$(\exists x)$ mountain x'

$[(\exists x)(\text{mountain } x \ \& \ x \in D) \vee (\exists x)(\text{mountain } x \ \& \ x \subseteq D^n)]$

Plainly since mountains are not sets, $-(\exists x)$ mountain x & $x \subseteq D^n$ for all D.[n]

Therefore it follows from the above that:-

'$(\exists x)$ mountain x' oc mountains iff $(D)(i)$ $\langle D,i \rangle$ is a model of '$(\exists x)$ mountain x'

$\supset (\exists x)$(mountain x & $x \varepsilon D$).

But by the strong Lowenheim - Skolem theorem, $(\exists D)(\exists i)$ $\langle D,i \rangle$ is a model of '$(\exists x)$ mountain x' & $(x)x \varepsilon D \supset$ natural number x. Since mountains are not natural numbers, there is a model of '$(\exists x)$ mountain x' which shows it not to be committed to mountains!

There is an atmosphere of absurdity about this conclusion which surely must show that the model-theoretic definition of ontological commitment given previously is wrong. Indeed it is wrong; for it can be formally proved from the definition as given that no consistent theory is committed to something which does not exist! The proof is as follows:-

1.	T oc K	Hyp
2.	$-(\exists x)$ Kx	Hyp
3.	T oc K $\equiv (D)(i)$ $\langle D,i \rangle$ is a model of T $[(\exists x)(Kx \,\&\, x \varepsilon D) \vee (\exists x)(Kx \,\&\, x \varepsilon D^n)]$	Hyp
4.	T is consistent $\supset (\exists D)(\exists i)$ $\langle D,i \rangle$ is a model of T	Hyp (Completeness Theorem)
5.	T is consistent	Hyp
6.	$(\exists D)(\exists i)$ $\langle D,i \rangle$ is a model of T	4,5 Taut
7.	$(D)(i)$ $\langle D,i \rangle$ is a model of T $\supset [(\exists x)(Kx \,\&\, x \varepsilon D) \vee (\exists x)(Kx \,\&\, x \varepsilon D^n)]$	1,3 Taut

By 6, for some D and i

8.	$\langle D,i \rangle$ is a model of T	
9.	$\langle D,i \rangle$ is a model of T $\supset [(\exists x)(Kx \,\&\, x \varepsilon D) \vee (\exists x)(Kx \,\&\, x \subseteq D^n)$	7 UI
10.	$(\exists x)(Kx \,\&\, x \varepsilon D) \vee (\exists x)(Kx \,\&\, x \subseteq D^n)$	8,9 Taut

From 10 it follows by elementary first-order reasoning that:-

11. $(\exists x)Kx$

12. $(\exists x)Kx$ & $-(\exists x)Kx$ 2,11 Taut

therefore by reductio ad absurdum

13. - (T is consistent)

The conclusion of this argument rests on T oc K, $-(\exists x)Kx$, the definition of ontological commitment given, and the Completeness Theorem for first-order logic. Since the first two assumptions are true by hypothesis and the last is a well-established theorem of mathematical logic, the culprit must be the definition of ontological commitment given. Ontological commitment is not a model-theoretic concept.

Granted this, then the ability to prove the existence of non-standard models of a theory does not reflect on the ontological commitments of that theory. Nor does the existence of non-standard models prove that theory to be ontologically elastic: more is needed. What that more is, can be seen by reflecting on the strong Lowenheim - Skolem theorem and why it fails to reduce our ontology to a Pythagorean ontology.

For let T be any true (and hence consistent) first-order theory written in a language L. Suppose T carries prima facie ontological commitments to entities of a non-Pythagorean nature. In order to reduce T, by our standards of a recursive reduction, to the terms of a non-Pythagorean ontology, a recursive function would have to be given from the domain of L sentences into the domain of L_1 sentences, where L_1 is some arithmetical language. Moreover this recursive function would not only have to map T into a body of arithmetical truths: we should also be certain that T was an exhaustive description of the entities of its own domain of discourse. The strong Lowenheim - Skolem theorem falls a long way short of supplying the materials for such a project. The theorem itself guarantees only the existence of an arithmetical model for each consistent theory: it supposes no effective means for finding that model.[2]

If model-theoretic and proof-theoretic accounts of ontological elasticity do not work, then the prospects for a formally rigourous account of the concept look dim indeed. A return to the idiom of formal frameworks is needed.

As observed formal frameworks do not successfully localise ontological elasticity. Perhaps the lesson to be learnt from this is that ontological commitment is, sui generis, to be counted as a holistic concept, applying to nothing less than the totality of human science. If so, then perhaps formal frameworks do offer some ingress into the concept of ontological elasticity, if the enquiry is conducted at the highest level of generality. At this level ontological elasticity can be seen in the presence of alternative model worlds; each capable of representing the results of the empirical sciences.

Formally, a model world is a deductively closed set of formulae generated from the set of all formulae, any of which is a formal image of some true target language sentence. Philosophically a model world is a representation of the contents of the universe and the principles that describe their behaviour, as developed from an initial ontological hypothesis. Ontological elasticity reveals itself when there is a model world μ and a model world μ, where, for some kind K, μ quantifies over Ks but μ does not.

The use of 'quantification over' serves to hide an intensionality implicit in this account. Quantification over Ks cannot be identified proof-theoretically, with '$(\exists x)$ Kx' being a theorem or an element of the theory. For example, quantification over numbers does not require '$(\exists x)$ number x' as a theorem. Any synonym such as '$(\exists x)$ nombre x' will do. Nor does the variable 'number' or any synonym actually have to appear in a theory, for that theory to be committed to numbers. '$(\exists x)\ x = 3$' and '$(\exists x)$ prime x' both quantify over numbers without specifically mentioning numbers. Quantification over Ks can only be explained by a theory entailing $(\exists x)\ Kx$ and entailment is an intensional concept belonging to the theory of meaning. In this respect, ontological commitment and ontological elasticity are much alike. The possibility of giving a formally precise, extensional definition of either concept depends on the how well philosophers of language succeed in absorbing the theory of meaning into the theory of reference.

This is a depressing conclusion for some; but it is not altogether unsurprising. Frequently it turns out that the most important and central concepts in a subject turn out to be resistant to formal definition. It is altogether perverse to reject these types of concepts merely because they resist systematic treatment, for their employment and examination can be extremely rewarding. Similarly, with ontological elasticity, the importance of this concept is too great for it to be bypassed without further ado.

In what follows, the concept of ontological elasticity is taken for granted, and the consequences of accepting the phenomenon of ontological elasticity are examined. Three such areas of consequence are here examined; they are as follows.

(i) The relation of ontological elasticity to philosophical taxonomy:- that area of philosophy which enquires after the proper classification of fundamental kinds of object.

(ii) The relation of ontological elasticity to the concept of existence. In particular, the consequences for classical logic of the influence of ontological elasticity on our ideas of what the universe must be like.

(iii) The relation of ontological elasticity to the concept of truth. In particular, the consequences for the Correspondence Theory of Truth and the Principle of Bivalence.

5.2 Ontological Elasticity and Philosophical Taxonomy

One of the most popular forms of philosophical interrogation after questions beginning 'What is' are questions beginning 'What are'. What completes the first kind of question is a nominalised adjective; (e.g. 'What is truth?', 'What is justice?', 'What is goodness?', 'What is beauty?') What completes the second is some sortal term; (e.g. 'What are numbers?', 'What are truths (or falsehoods)?', 'What are thoughts?').

Questions of the second kind are the concern of what can be called philosophical taxonomy, which attempts to provide some species and general classification of the fundamental kinds of being.

Frege's The Foundations of Arithmetic is a classic work of philosophical taxonomy. At the introduction of his book, Frege defines the topic of enquiry as the nature of numbers.

'When we ask ourselves what the number one is, or what the symbol '1' means, we get as a rule the answer 'Why a thing'. And if we go on to point out that the proposition

'the number one is a thing'

is not a definition because it has the definite article on one side and the indefinite article on the other.... then we shall likely be invited to select something for ourselves - anything we please - to call one ....

Questions like these catch even mathematicians for that matter, or most of them, unprepared with any satisfactory answer. Yet is it not a scandal that our science should be so unclear about the first and foremost among its objects, and one which is apparently so simple? Small hope, then, that we shall be able to say what number is.'

Frege [4] (i - ii)

Debates in philosophical taxonomy have continued up to contemporary times. Strawson and Austin, in a Tweedledum - Tweedledee sort of fashion, argued over the nature of facts for fifteen years.[3] Questions of philosophical taxonomy are capable of maintaining long-standing disagreements - a fact that suggests such questions may have been wrongly posed in a first place.

A question of philosophical taxonomy is generally posed in the material mode of speech: that is, it is posed as a question about the status of a particular kind of object under some genus. It is not a question that presents itself in any essential way as connected to language: that is, it is usually not posed as a question in the formal mode. On the face of it, a question like 'What are the facts?' is no more concerned with language than the questions 'What are aardvarks?' or 'What are Drysophila?'. In fact, appearances here are truly deceptive and because taxonomic questions in philosophy are usually wrongly posed in the material mode, the result is fruitless wrangling.

A question of philosophical taxonomy like 'What are numbers?' is better rephrased as 'What are we talking about (if anything) when we practice arithmetic, algebra or number theory?'. At first glance, changing the style of questioning brings the desired solutions no closer. Perhaps. But the change in style does serve to bring out certain presuppositions of interrogation in the material mode.

The first of these presuppositions is concerned with the way that Ks are identified in questions of the form 'What are Ks?'. If an interrogator should enquire 'What are numbers?', we are entitled to respond 'What do you mean by 'numbers'?' If he should respond that he has no idea what he means by 'numbers' then we can reply that if he attaches no sense to 'numbers' in his question then how can we be expected to give a sensible answer? On the other hand, if he should respond by giving a precise account of what he means by 'number' then we can reply that he has already answered his own question, so why is he bothering us? To the harassed questioner there is only one way of phrasing his question so as to get it across. He can define numbers as the elements of the domain of discourse of a particular style of language, for example, of arithmetic, and then enquire as to the status of numbers. In this way he avoids the dilemma of either admitting he does not understand his own question or else admitting he already has the answer. Treating questions of philosophical taxonomy in this manner means that important domains of items are defined relative to certain kinds of discourse. Numbers are the elements of the domain of discourse of arithmetic, facts are the elements of the domain of discourse of ordinary language sentences that mention the word 'fact' or some synonym. These are <u>discourse-relative definitions</u>. Here is the principal difference between questions of <u>philosophical taxonomy</u> and questions of say, <u>biological taxonomy</u>. If I enquire 'What are penguins?' then it is always open to me or another to define penguins ostensively. This means that the concept of being a penguin need not been defined in a discourse-relative fashion.

Defining numbers, facts, thoughts etc discourse-relatively throws up the second presupposition. To define numbers as the elements of <u>the</u> domain of arithmetic is to presuppose the truth of a uniqueness claim: namely there is one and only one domain of elements to which arithmetic can be interpreted. This presupposition is, of course, false. Therefore the questions 'What are numbers?' and its cash-value equivalent 'What is the domain of discourse of arithmetic?' are both ill-conceived: they both commit the Fallacy of Many Questions . Frege's opening questions of the <u>Foundations</u> were improperly formulated. In order to reset Frege's enquiry along modern lines we should have to ask 'How may arithmetic be formalised?'; and the answer comes back 'In various ways: such as that of Zermelo, Von Neumann .... and, of course, Frege himself.'

What this all amounts to is that questions of philosophical taxonomy cannot be properly asked in the material mode in the presence of ontological elasticity. To pose a question of philosophical taxonomy in the material mode, we should be justifiably certain that our discourse-relative definition does not falsely presuppose a lack of ontological elasticity. Alternatively, a simpler and altogether better solution would be to abandon the material mode altogether! This departure from tradition would be a major conceptual revolution in the way human beings have so far pursued their research of the universe and the long-term consequences of adopting this method of enquiry would have to be pursued by the experts in their various fields. What I have to say, therefore, is general in the extreme and is somewhat akin to the authority with which amateur meteorologists deliver long-range weather forecasts by gazing at the clouds.

The most immediate, and I think, merciful effects of rephrasing all questions of philosophical taxonomy in the formal mode (i.e. of asking 'How may K-talk be formalised?' rather than 'What are Ks?') is an end to sterile and interminable arguments about the nature of Ks. A case in point is the debate on the nature of truth-bearers: truth bearers have been identified variously with propositions, statements, ordered n-tuples of word-tokens, sets of possible worlds, equivalence sets of synonymous declarative sentences etc... It is quite possible, given time and acumen, that at least some of these suggestions could be made to work in the context of formalisation. Asking 'How may talk of what is true or false be formalised?' helps to dispel the idea that there is only one uniquely right answer to the question. Asking 'What are truth-bearers?' merely entrenches the discussion in a muddy (and roughly circular) rut in which the disputants reason that since truth-bearers are determinate somethings of some kind, at most only one answer to the question can be right.

Of all the natural sciences, modern physics offers perhaps the greatest scope for formalisation and ontological elasticity. The progression of physics has been, for over a hundred years, towards the study of systems which are more and more remote from human experience or even human imagination, probability waves and electromagnetic fields being two examples. Despite the enormous increase in sophistication of the new physics over what little theory prevailed in ancient times, the philosophical tenor of the modern research physicist's enquiries is still transmitted from the mental outlook that characterised the Pre-Socratics. This outlook insists that the physical universe is determinate in the way it is constructed, and hence that, of any two representations of one and the same part, at most only one will be right. Thus the modern physicist would still approach a question about the internal architecture of atoms with the same presupposition of investigating a determinate something that motivated the early Greek cosmologists in their speculations about the nature of matter. But if atoms and electrons are defined in a discourse-relative fashion, that is to say, as the elements of the domain of discourse of our most currently successful atomic theories, then it is at least conceptually possible that such theories might enjoy different representations as to the elements of their domains. In such a case, physicists might well ponder whether it is not as mistaken to ask after the nature and identity of atoms as it was for mathematicians like Frege to ask after the nature and identity of numbers. Since (both) Russell and Whitehead each succeeded in finding different domains to represent all that men wished to say about instants of time, the possibility of ontological elasticity in modern physics seems worth bearing in mind.

5.3 Ontological Elasticity and Existence

'To be is to be required to be by some true theory' is about the shortest account of existence short of total triviality that is is possible to attain. The reply comes in different flavours, 'To be is to be required to be existentially quantified over in the formalisation of some true theory' is another: but only a variant of the first. It is, at first sight, the most innocuous of all accounts of existence; but joined to the doctrine of ontological elasticity, together they rebound with some considerable force into some classical ideas about how the universe must be.

The doctrine of ontological elasticity states that for at least one theory T (and probably for many), T can be formalised in various different ways, each equally valid, but each requiring assent to the existence of (and quantification over) the elements of a different domain. In such a case, T is ontologically elastic and there is no sense in enquiring after the elements of <u>the</u> domain of T. Thus suppose that T can be formalised so as to be concerned with K_1s, K_2s or K_3s but nothing else. It makes no sense to ask which domain T is concerned even though it would be right to say in the absence of all of K_1, K_2 or K_3 , T would be false. Accepting the truth of T compels, by the definition of existence given previously, the recognition of the existence of all that T requires to be true. But we cannot say that because T is true, that the universe <u>must</u> contain K_1s, or it <u>must</u> contain K_2s, or it <u>must</u> contain K_3s for T can be legitimately asserted in the absence of <u>any</u> one (or indeed two) of them.

But here is the nub. If we can accept that a single true theory can display ontological elasticity, then it must also be accepted that this ontological elasticity will be maintained even if this theory is incorporated into a wider corpus of truths. Taking this reasoning to its logical conclusion it follows that different formalisations will be possible even of a completed science in which nothing more remains to be discovered. By our definition of existence, what exists will be what is required to exist by this completed science and nothing else. <u>But given this completed science is ontologically elastic, the objects that are to be counted into the universe will be as under-determined as the objects which are to be counted into 'the' domain of objects with which such a completed science is concerned</u>. In fact, the two kinds of underdeterminacy are really the same.

It is important to grasp that this underdeterminacy is not a underdeterminacy in human knowledge, but an underdeterminacy as to the things that are. The radical conclusion is not that it may well be that human beings can never arrive at a determinate knowledge of just what is, but rather that because what is is not determined, there is no such determinate knowledge to be had.

A metaontology of this kind reflects on many philosophical arguments of lasting duration. For example, the differences between the opinions of Platonists in the philosophy of language who insist that we should recognise the existence of universals and those of their nominalist opponents who say philosophers should stay clear of such commitments, may, in the end, be a reflection of differing ways of legitimately interpreting the domains of discourse of various natural language sentences. There may be no answer to the question 'Who is right?' and no side to choose. One casualty of this thinking is classical logic with its insistance on the exclusive rightness of either a statement or its denial.

The conclusions of the foregoing argument can be conveniently restated in the jargon of chapter three. In that chapter, a model world was introduced as a set of formulae each of which was a formal image of some true object sentence of science. Philosophically, a model world is a detailed representation of the way the world stands which is compatible with scientific truth defined relative to the object language. Ontological elasticity requires that different model worlds can be constructed to fit all the evidence. What exists will be what is common to all those model worlds so that Ks exist if and only if each model world quantifies over Ks and Ks do not exist, if and only if no model world quantifies over Ks. Where model worlds disagree, there lies uncovered the ontological underdeterminacy.

As observed, classical logic kicks hard against this sort of definition. Classical logic insists that each kind either exists or it does not, and nothing in between. So much the worse for classical logic is one reply; and the next question must be what logic is most appropriate to the doctrine of ontological underdeterminacy.

Given the large range of deviant logics currently extant, I can see no easy answer to this problem. The questionis, to my mind, not so much the formal one of whether a logic can be found to accommodate philosophical intuition, but whether philosophical intuition can be sharpened and justified so as to select just one logic. There are various logics for example, Heyting's and Johansson's intuitionist propositional calculi, and Lukasiewiez' and Kleene's 3-valued calculi, which lack LEM and are compatible with ontological underdeterminacy in that respect.

A promising line of pursuit seems to me to originate from a Hintikka [61] model set approach, and ancestrally from Carnap's [22] use of state-descriptions. The idea of Carnap's approach is to specify sets of formulae (state-descriptions), each set sharing a common language L, and each bound in conformance with certain rules of membership. Intuitively each such state-description represents a possible world and the formulae common to all such state-descriptions are the formulae recognised as valid. Borrowing this technique from Carnap, what would be required would be a domain of sets representing model worlds, each set having a membership bound in conformance with certain rules.

Of a range of such systems investigated in the course of this research one seems to me of sufficient interest to be mentioned. This system I call 'System 4' (because is was the fourth of ten different systems investigated). In System 4, model worlds are represented by sets of formulae written in a language L. Each element of every set is either (a) an atomic formula or (b) the negation of an atomic formula. Each such set is consistent; i.e. where p is any formula and S any such set, if $p \in S$ then $-p \notin S$. However completeness is not a requirement and so $p \notin S$ and $-p \notin S$ is permissible. Each such set is expanded to a <u>conforming set</u> by means of the following rules. Where μ is any conforming set generated in this way, and p and q any formulae of L; if $p \varepsilon \mu$ then $-p \varepsilon \mu$ and:-

(i) $--p \varepsilon \mu$ iff $p \varepsilon \mu$

(ii) $p \vee q \varepsilon \mu$ iff $p \varepsilon \mu$ or $q \varepsilon \mu$

(iii) $p \,\&\, q \varepsilon \mu$ iff $p \varepsilon \mu$ and $q \varepsilon \mu$

(iv) $p \supset q \varepsilon \mu$ iff $p \not\varepsilon \mu$ or $q \varepsilon \mu$

(v) $-(p \vee q) \varepsilon \mu$ iff $-p \varepsilon \mu$ and $-q \varepsilon \mu$

(vi) $-(p \,\&\, q) \varepsilon \mu$ iff $-p \varepsilon \mu$ or $-q \varepsilon \mu$

(vii) $-(p \supset q) \varepsilon \mu$ iff $p \varepsilon \mu$ and $-q \varepsilon \mu$

The set of conforming sets is just the set of sets generated in this manner. A formula is <u>valid</u> if and only if it is an element of every conforming set and a rule of inference is <u>validity - preserving</u> if and only if it derives only valid formulae from valid formulae.

It turns out that System 4 characterises a logic in which <u>modus ponens</u> is validity preserving. It also turns out that the Deduction Theorem is provable of this logic since modus ponens is admitted and so are the formulae:-

$(p \supset (q \supset p))$;

$(p \supset (q \supset r)) \supset ((p \supset q) \supset (p \supset r))$.

By a proof by Herbrand (see Mendelson [90] (32)), any logic admitting these two formulae, plus <u>modus ponens</u>, has the Deduction Theorem true of it. Unlike intuitionist logics, the Law of Double Negation is admitted. But like intuitionist logics, LEM is not a valid formula since it will fail to hold of conforming sets generated from incomplete sets. Since the Law of Double Negation is reckoned to be instrumental in the generation of LEM, how exactly does System 4 retain DN and reject LEM?

The answer is that there is an important classical principle that is not retained under System 4: that of reductio ad absurdum or indirect proof. Neither of the following formulae:-

$$((p \supset -q) \,\&\, (p \supset q)) \supset -p$$

$$(p \supset (q \,\&\, -q)) \supset -p$$

turns out to be valid. I would conjecture that if either of these formulae were added to the valid formulae of System 4 and modus ponens, the resulting theorems would be wholly classical. If so, then there is an interesting parallel with Heyting's propositional calculus. Heyting's calculus turns out to collapse into the classical propositional calculus with the addition of $--p \supset p$ as an axiom.

5.4 Ontological Elasticity and Truth

It is part of the conventional wisdom attached to set theory that ordered pairs are mathematically reducible to sets. Weiner suggested one way: treat $\langle x,y \rangle$ as $\{x, \{\Lambda, y\}\}$. Kuratowski suggested another: treat $\langle x,y \rangle$ as $\{x, \{x,y\}\}$. Both these fulfill the identity criteria for ordered pairs

(i) $\langle x,y \rangle = \langle w,z \rangle$ iff $x = w$ and $y = z$

As far as mathematics goes, both Wiener's and Kuratowski's approaches are successful. Quine puts the case for the mathematical status quo, commenting:-

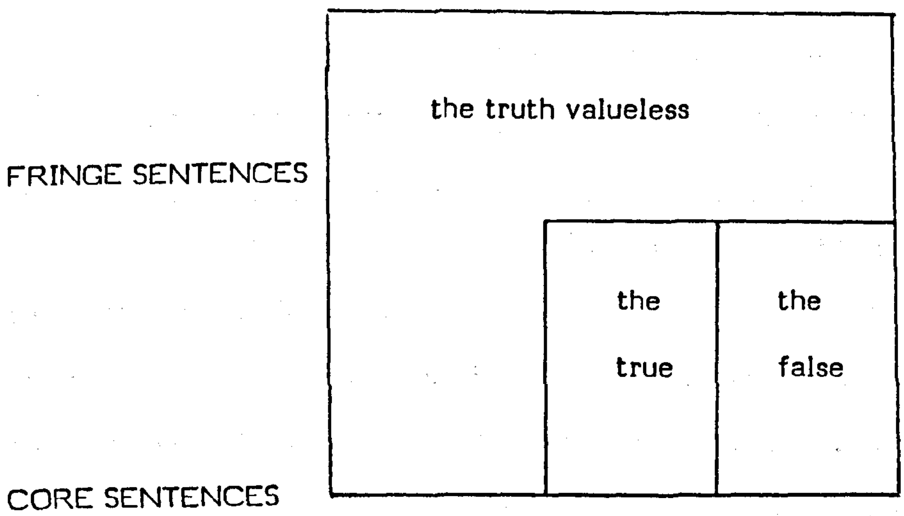
'Which is right? All are; all fulfil (i), and conflict with one another only out amongst the don't cares. Any air of paradox comes only of supposing that there is a unique right analysis - a mistake that is encouraged by the practice, otherwise convenient, of using the term 'ordered pair' for each version. On this and other points, the nature of explication as illustrated by the ordered pair may be made wholly evident by retelling the story of Wiener, Kuratowski and the ordered pair in a modified terminology. In the beginning there was the notion of the ordered pair, defective and perplexing but serviceable. Then men found that whatever good had been accomplished by talking of the ordered pair $\langle x,y \rangle$ could be acomplished instead by talking of the class $\{x, \{y, \Lambda\}\}$ - or, for that matter, of $\{x, \{x,y\}\}$.

Quine [119] (260)

The point of epistemological interest is the 'don't cares'. The facts of logical life are that Wiener's and Kuratowski's competing formalisations demand incompatible valuations of certain set theoretical sentences. For instance the sentence '$\{x,y\} \varepsilon \langle x,y \rangle$' is recognised as <u>true</u> under the formalisation of Kuratowski since it is equivalent to '$\{x,y\} \varepsilon \{x, \{x,y\}\}$'. But under Wiener's formalisation '$\{x,y\} \varepsilon \langle x,y \rangle$' emerges as '$\{x,y\} \varepsilon \{x, \{y, \Lambda\}\}$' which is <u>false</u>. If we accept that '$\{x,y\} \varepsilon \langle x,y \rangle$' is determinately true or false for each x and for each y, then we must also accept that either Wiener or Kuratowski is wrong: and that is contrary to the council of mathematical tolerance preached by Quine. By <u>modus tollens</u>, accepting the tolerant attitude means rejecting the determinacy of truth-values of sentences like '$\{x,y\} \varepsilon \langle x,y \rangle$'.

The rejection of the view that every well-formed declarative sentence is either true or false, is, as I understand it, a rejection of the Principle of Bivalence, and this is what is recognition of ontological elasticity demands. The Principle of Bivalence is historically, but not inevitably, associated with Correspondence theories of truth. A Correspondence theorist, archetypically, believes there is a completely determinate collection of states of affairs or <u>facts</u>. There is also a range of truth-bearers, sometimes presented as <u>propositions</u>, which either <u>correspond</u> or do not <u>correspond</u> to the facts. If a proposition corresponds to the facts, it is true: if not, false. The epistemological problem is that theories of truth which follow this general pattern (theories handed to us by such imposing authorities as Aristotle, Russell, the early Wittgenstein, and Tarski) leave no room for ontological elasticity or its philosophical consequences. Ontological elasticity teaches us not to think of the world as a collection of facts with determinate relations to what we surmise. On the contrary, ontological elasticity enjoins that we accept that there are domains in which it is drastically underdetermined just what exactly the properties of the elements of these domains are. Ontological elasticity teaches us that certain questions about the nature of various kinds are radically misplaced and that in certain areas the very <u>existence</u> of certain would be objects may be left unsettled. For ontological elasticity to be accommodated, the Correspondence picture of the world as a collection of given facts has to go in favour of something more flexible. We are forced to recognise that if statements about K_0s are legitimately formalised as statements about K_1s, or K_2s, then liberality must be used as regards the valuation of sentences that attribute to K_0s, properties reserved for K_1s, or which attribute to K_0s, properties reserved for K_2s. Just as ontological elasticity demands its own logic, so it also demands its own theory of truth. The detailed exegesis of such a theory of truth belongs by right to a substantial work in epistemology. This is not such a work and so what follows is not a detailed exegesis, but rather the elements for the construction of such an exegesis.

Within any language there is a solid core of sentences which are either determinately true or else determinately false. The domain of such sentences has been the domain of classical logic and within this domain the canons of classical logic hold good. Such declarative sentences I call <u>core sentences.</u> Outside the solid core is a domain of <u>fringe sentences</u>. Fringe sentences are characterised by their resistance to any conceivable means of allocating them a truth-value and constitute the 'don't cares' of Quine's quoted comment. Examples of such fringe sentences include '$\{1,2\} \in \langle 1,2 \rangle$', 'the cardinality of a point is $\aleph_0$', and possibly 'My thoughts are in my brain'. In formalisation an ontologist is expected to take note of the truth-values of core-sentences, for these are the sentences which place constraints of his formalisation. The fringe sentence are the 'don't cares' which can be played fast and loose with: these sentences lack truth-value, but can be conventionally given a truth-value if the formalisation requires it. So we get a diagrammatic picture like this:-



The fringe-core distinction raises two questions:-

(i) What, more precisely, places a sentence in the fringe rather than the core?

(2) Can a sentence move from one to the other?

I shall take these questions in turn.

In a sense, the first question has received something of an answer in saying that a fringe sentence is a sentence that reveals no significant consequence whatever its valuation. But the answer can, I think, be pressed a little further and I shall try to do so.

The pursuit of truth or knowledge, can be thought of in terms of a model in which sentences are processed through what can be called an epistemic filter. An epistemic filter embodies our principal modes of epistemic inquiry: that is to say, our important ways of assessing the truth or likely truth, falsehood or likely falsehood, of various declarative sentences. The elements of our cultural epistemic filter range from the simplest decision-effective procedures (like deciding the truth of a sentence of elementary arithmetic) to the most abstract methodologies such as the Popperian scheme for the pursuit of science. Epistemic filters are not fixed in nature; that is to say an epistemic filter is a growth system rather than a pre-wired system incapable of change. Some parts (e.g. logic and mathematics) of an epistemic filter do, by reason of their recurrent usefulness in many cases, acquire the status of being fixed and unassailable necessary truths. But in general, the cultural epistemic filter changes and evolves from generation to generation. This is simply because an epistemic filter is being constantly modified by its own output. To make the point simply, as an epistemic filter is used to separate out truths from falsehoods, so certain declarative sentences that become thus well-established and become part of the epistemic filter itself. Thus Newton's equation for gravitational attraction $F = G\, m_1 m_2/d^2$, where F is the gravitational force, m_1 and m_2 the masses of the bodies, d the distance between the bodies and G the gravitational constant, was once the object of investigation to the epistemic filter of seventeenth-century physics. Subject to its corroboration, it became in due course, part of the epistemic filter of physics itself and was used to evaluate reports on the masses of the planets.

In describing an epistemic filter as a growth system rather than a prewired system, it does not follow that epistemic filters do not tend, as most growth systems do, to a stable state where growth ceases or becomes slowed to a minimum and the system becomes in all essential respects a fixed-wire system.

An example of such a growth system would be that of a computer programmed to play chess, but also programmed to change its programme as its experience of the game grew. Such a computer would never play the same losing game twice. Over a period of time the computer would lose fewer and fewer games and consequently modifications to its programme would grow less too. The stable state to which the computer would gravitate would be one where it played the best possible move in any chess situation. In this way the concept of 'the best chess move' could be defined as the move the computer would make in its ideal stable state. Though the rate of the computer towards its stable state would be, to an extent unpredictable, depending in part on the quality of the opposition, the stable state would itself be determinate.

In a similar fashion an epistemic filter would move towards a stable state and the concept of truth could be defined in relation to such a stable state: a sentence is true if and only if it is classified as such when input to a stable-state epistemic filter and false when it is classified as such by the same filter. Sentences left unclassified would be fringe sentences. Apart from the use of the terminology of cybernetics, the spirit of this account of truth was captured by Peirce in his definition of truth: 'The opinion which is fated to be ultimately agreed to by all who investigate is what we mean by the truth...'

Are fringe sentences incapable of changing their status or can they become core sentences? The answer to this question is that they can become core sentences because of the human passion for interfering with their own epistemic filters and altering the course of their development. Acts of intellectual deus ex machina frequently result in incorporating whole tracts of fringe sentences into the core and the epistemic filter, so enriched, then begins to progress towards a stable state which it would have not attained had it been left alone. These acts of deus ex machina are conventions which fix the truth-values of certain crucial fringe sentences, on the promise of thus evolving a new area of research and eliminating doubt or fog over fundamental issues.

Cantor's foundational work on infinity was an example of such a deus ex machina intervention in the epistemic filter of nineteenth century mathematics. Prior to Cantor's work, most discussion of the concept of infinity took place within philosophy and the concept of infinity resisted incorporation into mathematics. For this reason, statements about infinity (e.g. 'All infinite collections have the same number of items') were fringe sentences relegated to philosophers. Had the procedures and conceptual equipment of the mathematician circa 1850 been allowed to develop, no means of evaluating these sentences would have emerged.

Cantor's achievements lay in altering the epistemic filter of mathematics in such a way as to reclaim fringe sentences relegated to philosophy, for mathematics. In order to do so, Cantor had to propose certain conventions for the valuation of fringe sentences concerned with infinite collections. Principally Cantor chose to adopt a definition of infinity (that a set is infinite iff it is equivalent to a proper subset of itself) which reversed the classical conception that the whole is always greater than a proper part. By use of such conventions for fixing the values of fringe sentences concerned with infinity Cantor created the study of transfinite cardinal arithmetic in which hitherto fringe sentences became core sentences capable of proof or disproof. A similar example, though less enthusiastically endorsed than Cantor's work, was Whitehead's [147] ontological reduction of points and instants of time to set-theoretical constructions of events. Accepting the benefits of Whitehead's approach involves admitting to the domain of the core sentences, sentences reporting on the cardinality of space-time points that previously would have had no valuation. In this way, truth can evolve with the evolution of human ideas.

[1] See Grandy [56] for a proof. The strong Lowenheim - Skolem theorem was first proved by Bemays.

[2] See section 6 on recursive reduction. Gottlieb [55] has pointed out that even if human intellect was so powerful as to be able to specify some arithmetical model on the presentation of a consistent theory, this would still not establish Pythagoreanism. Thus suppose T to be a true first-order theory written in L, whose domain of discourse was non-arithmetical. By the strong Lowenheim - Skolem theorem there is an arithmetical model $\langle D,i \rangle$ of T where D = N, the set of natural numbers. Suppose we could isolate this model. We might be able to specify a language L' with the following properties. Where v is any element of L and i(v) the interpretation of v under i, there would be a variable v' of L' where the extension of v' = i(v). By replacing each such variable v in T by v' of L', a true arithmetical theory T' would be created, i.e. one whose variables ranged only over numbers. T would be reduced to T'.
However it does not follow that if L' is an arithmetical language whose variables range only over numbers, that every theory formulated in L' is Pythagorean (i.e. committed only to numbers). For suppose that the predicate 'x is greater than the number of members of the House of Representatives' is an element of L'. '($\exists$ x) x is greater than the number of members of the House of Representatives' is both a theory of L' and a theory whose ontological commitments are not totally Pythagorean. Arithmetical languages need not generate arithmetical ontologies.

[3] See Austin [1] [8] and Stawson [138].

CHAPTER SIX

Ontological Reductions

Ontological reduction is both a generic and a dialectical procedure: generic, in that there is not simply one kind of procedure called 'ontological reduction' to be examined; dialectical, in that the procedure of ontological reduction is often executed within a human exchange. Dialectical procedures (such as demonstration or giving evidence) cannot be analysed outside contexts where on human being reasons with another.

Having said that ontological reduction is a generic concept, it is then natural to enquire what binds the various species of ontological reduction together and brings them together under one rubric or heading. It would be easy - and somewhat disappointing - to hide behind the ink-cloud of Family Resemblance and deny this question significance. Here, at any rate, it is not necessary to do so. What binds the various species of reduction together is not how they are performed but why. It may arise that a person wishes to demonstrate of a subject (possibly himself) that the discredit of a particular ontological commitment attributed to that subject can be avoided in some manner. Ontological reduction is simply a name given to any philosophically interesting procedure that can bring this about.

In this chapter, four kinds of ontological reduction will be examined; recursive reduction, reduction by logic-shift, reduction by limitation of the target language, and identity reduction. There is no claim to completeness in this list; they are merely those reductions that strike me as most pervasive or philosophically interesting. Anybody who can add to the list is welcome to do so. It should be noted that one species of reduction has already been dealt with: that which takes advantage of ontological elasticity by reformalising to avoid an unwanted existential quantification. I shall not cover this ground again. Finally the chapter will end in an examination of the development of Quine's views on ontological reduction.

6.1 Recursive Reduction

Recursive reduction originated in mathematics where the technique was used ancestrally in Riemann's relative consistency proof of Riemannian to Euclidean geometry. (Indeed, there are several striking similarities between a recursive reduction and a relative consistency proof). Although derived from mathematics, recursive reduction is applicable to other areas of human research. Appendix III at the end of this chapter contains a detailed expansion of a recursive reduction of instants of time by Russell, and is worth consulting by anybody interested in seeing the technique in action.

The procedure of recursive reduction is irreducibly dialectical. Consequently the procedure will be illustrated between two imaginary parties, Yodelstein and Zollicoffer, arguing over the existence of a species K of entity, which for the sake of generality, shall be left unspecified. The position - pre-recursive reduction - is that Yodelstein is committed to the existence of Ks, but is willing to abandon his commitment to Ks should anything better offer. Zollicoffer is determinedly opposed to recognising the existence of Ks and hopes to persuade Yodelstein to his position. The game is set.

The first move is Yodelstein's. Yodelstein isolates a formal language L_K, which can be assumed to be first order. L_K is selected on the basis that Yodelstein believes that anything significant that he wishes to assert of Ks can be asserted in this language.

The second move is also Yodelstein's: the isolation of a set T_k of axioms written in the language L_k. Yodelstein takes the elements of T_k to be true and ideally T_k will also be complete. However completeness being a property laid up more in heaven than found in terrestrial theories, it will not matter that T_k is incomplete. It is important, however, that Yodelstein be happy to identify the seminal truths about Ks with the theorems of T_k, and hence that, in respect of the important verities about Ks, Yodelstein should agree T_k is 'complete'.

The first two moves define Yodelstein's position, and from thereon in, most of the moves are made by Zollicoffer. Zollicoffer has to specify a domain of Js, within which Ks are not included and Zollicoffer has to get Yodelstein agreement that there is such a domain. Without Yodelstein's agreement on this, the game terminates without Zollicoffer arriving at a winning position.

Having secured Yodelstein's agreement, Zollicoffer then procures a formal language L_j, (whose variables range only over the items of J) which Zollicoffer considers is rich enough to say what he wishes of the elements of J. Zollicoffer produces a theory T_j written in the language L_j; and Zollicoffer must persuade Yodelstein that T_j is true. Again without Yodelstein agreeing, Zollicoffer fails to arrive at a winning position.

If he has got this far, Zollicoffer is in a position to make his crucial winning-move - the application of a recursive reduction. What Zollicoffer endeavours to do is to produce a computable or <u>recursive</u> function r, where r takes as its domain the set of L_k sentences and has as its range a subdomain of the domain of L_j sentences. Zollicoffer attempts to prove, <u>on the assumptions granted to him by Yodelstein</u>, that r preserves truth-values; i.e. $s \equiv r(s)$ for all s of L_k. If Zollicoffer suceeds in doing this, then he has provided Yodelstein with what Yodelstein must agree is a mechanical truth-preserving means of translating (and hence eliminating) all talk about Ks in favour of talk about Js. He has effectively shown that Yodelstein no longer has any good reason to cling to an ontology of unreduced Ks.

How may Zollicoffer constructively prove the existence of such a recursive function? In fact, granted the assumptions provided by Yodelstein, Zollicoffer proves his point if he proves the following:-

$$\vdash_{T_k} s \supset \vdash_{T_j} r(s)$$
$$\vdash_{T_k} - s \supset \vdash_{T_j} - r(s)$$

The reasoning is quite simple. Let s be any sentence of L_k. Now suppose v(s) = 1; on the assumption granted by Yodelstein the idea of truth-in-L_k can be conveniently approximated to derivability from T_k. So Zollicoffer has been given licence to treat v(s) = 1 and $\vdash_{T_k} s$ interchangably; let him do so. Since Yodelstein has allowed that T_j is true, by mapping s to a theorem r(s) of T_j, Zollicoffer shows that, according to Yodelstein's own thinking, whenever v(s) = 1, r preserves truth-value.

Now suppose v(s) = 0; by the same assumptions as before Zollicoffer is entitled to treat this as equivalent to $\vdash_{T_k} - s$, and since T_j is assumed true, if s is mapped to r(s) where $\vdash_{T_j} - r(s)$, then again Yodelstein must agree r preserves truth-value. Game to Zollicoffer.

There are a few points to note about this little scenario. Yodelstein can have a reply if he wishes. He can, for instance, change his mind about T_k and enlarge its repertoire of home truths about Ks, even if such an indulgence smacks of gamesmanship. Zollicoffer, for his part, is restricted by the rules of the game; especially by the fact that r must be a recursive function. The reasons for the emphasis on recursiveness is that Yodelstein cannot eschew his commitment to Ks unless he is provided, whenever he is tempted to quantify over Ks, with an effective means of avoiding this quantification. To be told that talk of Ks is dispensable, but to have no means to hand to dispense with it is no use to Yodelstein. There are two corollaries to this demand for recursiveness.

Corollary one is that since r is recursive, Zollicoffer must define r purely syntactically, without reference to any semantic concepts like truth or meaning. r must be defined purely in relation to the order and appearance of the signs used in the sentences of L_k and L_j.

Corollary one contains its own justification for the requirement that r be recursive. For imagine that r need not be recursive; then we could easily find a truth-preserving non-recursive function r*; where s is any sentence of L_k, r* is defined as follows:-

$$r^*(s) = \begin{cases} \text{'}(x)x = x\text{' if } v(s) = 1 \\ \text{'}-(x)x = x\text{' if } v(s) = 0 \end{cases}$$

In this way the ontology of every theory could be dispensed with altogether. Recursiveness blocks out this option. However, where truth in L_k is <u>decidable</u>, then a function such as r* can be defined recursively. It follows that the ontology of any decidable theory is dispensable.

Corollary two is that if the domain of r (the set of L_k sentences) is denumerable, then r cannot be specified by enumeration. Thus defining, for the sake of illustration, a recursive function in terms of Turing computability, to define r by enumeration for each element of L_k would require a program of infinite length, and programs of this kind (as well as being humanly impossible to write) are defined as inadmissable for Turing machines. In such a case r must be defined from the elements of L_k itself, and r(s) is defined for each s by the components from which s is constructed.

In closing, it is worth remarking on the close parallels between a recursive reduction and a relative consistency proof. The techniques are basically the same. In both cases the means is a procedure which maps one theory into a subtheory of another theory. In fact, the recursive reduction of T_j to T_k is <u>at the same time</u> a relative consistency proof of T_j to T_k. The difference between a recursive reduction and a relative consistency proof lies mostly in the different purposes for which the same technique is adopted.

6.2 Reduction by Logic Shift

Haack [59] defines an extended logic as a logic which includes as a proper part both the symbolism and axioms/rules of inference of either the classical propositional or predicate calculi. Where the study of extended logics intersects with metaontology, is when an ontologist shifts to the use of an extended logic to avoid a quantification that he finds distasteful. To illustrate.

The sentence 'Tom moved' is a simple action sentence. If asked to formalise 'Tom moved' then the atomic sentence:-

moved (Tom)

is close at hand. Complexities begin with the introduction of adverbs. Suppose the task is to formalise not 'Tom moved' but 'Tom moved swiftly'. Plodding along the lines of previous thought we suggest:-

moved swiftly (Tom)

as a good formalisation.

Davidson [38] disagrees. Davidson criticises formalisations like this one on two counts.

First, the proposal, if generalised as a way of treating adverbial sentences and placed in the context of a Davidsonian theory of meaning, stands in danger of violating the constraint of finite axiomatisation. The string of modifying adjectives and adverbs can be extended indefinitely, thus:-

Tom moved ....

Tom moved swiftly....

Tom moved swiftly and with stealth....

Tom moved swiftly and with stealth towards the pantry....

Tom moved swiftly and with stealth towards the pantry on all fours......................

If each of these sentences is formalised as an atomic sentence then the predicates 'moved', 'moved swiftly', 'moved swiftly and with stealth', 'moved swiftly and with stealth towards the pantry', 'moved swiftly and with stealth towards the pantry on all fours' become semantic primitives each of which requires its own satisfaction conditions to be axiomatised. Since there is no guarantee that an infinite number of such predicates cannot be formed, then there is no guarantee that an infinite number of axioms will not be required.[1] Second, the species of formalisation advocated fails to legislate for inferences like:-

$$\frac{\text{Tom moved swiftly}}{\text{Tom moved;}}$$

which becomes formalised as the logically invalid:-

$$\frac{\text{moved swiftly (Tom)}}{\text{moved (Tom)}}$$

of the form $Fa \vdash Ga$.

Both these disadvantages are avoided on Davidson's proposal. This proposal would formalise 'Tom moved swiftly' as:-

'$(\exists e)$ movement (e) & swift (e) & of (e, Tom)'

Here complex adverbial constructions are teased apart into predicates true of events (or ordered n-tuples containing events). The threat of using infinite axioms is avoided. As a bonus, if 'Tom moved' is formalised as:-

'$(\exists e)$ movement (e) & of (e, Tom)'

then the inference from 'Tom moved swiftly' to 'Tom moved' becomes represented by the logically valid argument:-

$$\frac{(\exists e)\ \text{movement (e) \& swift (e) \& of (e, Tom)}}{(\exists e)\ \text{movement (e) \& of (e, Tom)}}$$

Davidson reasonably identifies the values for which these sentences are true as being events. Consequently Davidson recognises an ontological commitment to events in adverbial sentences.

Romane Clark [30] is not happy about admitting the existence of events. He complains that Davidson fails to supply identity criteria for events.[2] Clark follows a course of formalising adverbial sentences unlike Davidson's. Clark's approach, he believes 'reflects the grammar of our native language' as regards adverbial sentences. Clark's idea is to see 'swiftly' in 'Tom moved swiftly' not as predicating of an event, but as attaching to and modifying the sense of 'moved'. 'Moved' becomes what Clark calls 'a core predicate' and 'swiftly' a predicate modifier. So 'Tom moved swiftly' is formalised in Clark's predicate modifier logic as:-

'swiftly (moved (Tom))'

In a logic of predicate modifiers there is a primitive rule of inference that allows predicate modifiers to be peeled off from the outside of formulae. Formally, where M is any predicate modifier, $\emptyset$ any concatanation of n ($n \geq 0$) predicate modifiers ending with an m-place ($m \geq 1$) predicate, and $t_1,\ldots, t_m$ are terms; then all inferences of the following schema are allowed.

$$\frac{M\,(\emptyset(t_1,\ldots t_m))}{\emptyset\,(t_1,\ldots t_m)}$$

If 'Tom moved swiftly' is formalised as 'swiftly (moved (Tom))' and 'Tom moved' by 'moved (Tom)' then the argument 'Tom moved swiftly therefore Tom moved' becomes represented as:-

$$\frac{\text{swiftly (moved (Tom))}}{\text{moved (Tom)}}$$

which is an instance of the rule of predicate modifier detachment given above. Clark opens his paper by stating what he takes to be the difference between stating what he takes to be the difference between Davidson and a 'radical' like himself on issues of this kind.

'The conservative philosopher [Davidson] attributes to the English sentence a hidden logical form which does not coincide with its apparent logical form. Radicals [Clark], by contrast, will be inclined to take the English sentence at face value. Instead they will tinker with standard logic hoping to accommodate those recalcitrant inferences in an enriched formal structure'.

Clark [30] (311)

This summarises the differences quite well. The different patterns of Clark's and Davidson's arguments can be brought out if their positions are simplified and idealised to draw attention to the salient features of each; thus:-

<u>Davidson</u>

P	The first-order predicate calculus is the correct logic for the purposes of formalisation.
Q	'Tom moved swiftly' is true.
R	In order to properly formalise 'Tom moved swiftly' in first order logic, events have to be quantified over.
((P & Q) & R S	If the first-order predicate calculus is the correct logic for the purposes of formalisation and 'Tom moved swiftly' is true and in order to properly formalise 'Tom moved swiftly' in first-order logic events have to be quantified over then events exist.

S	Events exist.

<u>Clark</u>

- S	Events do not exist.
Q	'Tom moved swiftly' is true.
R	In order to properly formalise 'Tom moved swiftly' in first order logic events have to be quantified over.
((P & Q) & R) ⊃ S	If the first-order predicate calculus is the correct logic for the purposes of formalisation and 'Tom moved swiftly' is true and in order to properly formalise 'Tom moved swiftly' in first-order logic logic events have to be quantified over then events exist.

- P	The first-order predicate calculus is not the correct logic for the purposes of formalisation.

Both arguments are tautologically valid. The Davidsonian argument is of the form $P,Q,R,((P \& Q) \& R) \supset S \vdash S$. Clark's argument is of the form $-S,Q,R,((P \& Q) \& R) \supset S \vdash -P$.

The problem facing the ontologist here is this: does he accept the correctness of the first-order predicate calculus (thus accepting P) and then acknowledge the existence of events (accepting S); or does he reject the existence of events (thus affirming - S) and then revise his opinion of the correctness of the predicate calculus (asserting - P)?

Each course has its own prima facie advantages. Accepting (P & S) allows the ontologist to keep his logic simple; but at the cost of inflating his ontology with events. Accepting (- P & - S) allows the ontologist to keep his ontology free from events - at the cost of inflating his logic. The metaontological problem is to try to discern some superordinate principles by which such contests can be decided.

These contests are by no means restricted only to the formalisation of action sentences. For instance:-

Quantification over periods of time in formalising tensed sentences and retaining first-order logic	vs	Rejecting first-order logic in favour of tense logic and dispensing with moments in time
Quine [119]		Geach [49]
Quantification over possible worlds in formalising strong conditionals and counterfactuals and retaining first-order logic	vs	Using a special notation for strong conditionals and counterfactuals thus removing the need to quantify over possible worlds.
Lewis [83] Stalnaker [134]		Pollock [102]

Quantification over attributes in formalising sentences of propositional attitude, but retaining first-order logic.	vs	Dispensing with attributes but using epistemic logic.
Quine [117]		Ackermann [1] Hintikka [67]

Haack recognises the generality and importance of this kind of logico-ontological disagreement. Her conclusion is that there may well be no question-begging way of deciding these contests.

'For myself I concede the desirability both of austerity of symbolism ... and of simplicity of paraphrase ....; I fear it is just a fact of logical life that these are competing desiderata.'

Haack [60] (161)

Gilbert Harman [63] is less pessimistic than Haack - but also less than cautious. Harman lays down a set of rules which are designed to filter out unwanted formalisations leaving only one survivor: 'the logical form' of the sentence under formalisation. In summary form, Harman's rules are:-

(1) A mode of formalisation must formalise sentences in such a way as to obey the constraint of finite axiomatisation.

(2) A mode of formalisation should limit itself, as far as possible, to first-order logic.

(3) A mode of formalisation should minimise the axioms that would be needed in a good (Davidsonian) theory of meaning for a language if that formalisation were incorporated in such a theory.

(4) A mode of formalisation should avoid ascribing unnecessary ontological commitments to the sentences formalised.

(5) A theory of formalisation should assign forms which would be assigned by a good transformational grammar.

Some of these rules are quite obscure (what for instance counts as a 'good' transformational grammar?) others are lacking justification (why the bias towards first-order logic?). The most obviously relevant rules to our discussion are (2) and (4); on those Harman has this to say.

On rule (2)

'A theory of logical form should minimise rules of logic. In practice this means that rules of logical implication should be kept as close as possible to the rules of ordinary (first-order) quantificational logic; for example, one might suppose that a three-valued logic yields a better account of the language than a two-valued logic does; or one might take quantification in the language to be 'restricted' in one or another way. But, other things equal, one account is better than another, the closer its logical rules are to those of ordinary quantificational logic'.

Harman [63] (291)

On rule (4)

'A theory of logical form would avoid ascribing unnecessary ontological commitments to sentences of the language. Other things being equal, one theory is better than another to the extent that it interprets sentences as implying the existence of more ordinary sorts of things.'

Harman [63] (299)

I do not know what Harman means by 'ordinary sorts of things': but rules (2) and (4) are incapable, as Harman states them, of arbitrating the sorts of contest previously examined. It is over these contests that an ontologist has to choose between (2) and (4). What Harman needs is some hierarchic arrangement whereby his rules can be placed in order of importance.

Harman does have some idea of the relative importance of rules (2) and (4). In considering predicate modifiers vs quantification over events, Harman chooses the latter. Harman remarks that one should only change one's logic 'as a last resort'. But the game is given away at the end of the article where Harman's logical conservatism is defended on the grounds that it goes to 'help to narrow down possibilities'.

This is true. But a series of ad hoc and unsupported conditions does not rationally limit possibilities: nor does it give confidence to the idea that every sentence has a unique logical form. Harman needs to show that his possibility-limiting rules have some other justification apart form limiting possibilities - otherwise why not just pick straws? This problem will be resumed in chapter seven.

6.3 Reduction by Limitation of the Target Language

A major 'discovery' of post-war Oxford philosophers was made by Austin [9] who found that not all indicative sentences performed or could perform in a statement-making role. Austin's case study specialised much in promises and other avowals which Austin called 'performatives'. In ontology the fact that an indicative sentence may not be declarative is sufficient condition to bar it from being formalised or from having ontological commitments attributed to it. Such sentences can be limited from target language.

An instance of a philosopher taking this line, though not admittedly for ontological reasons, is Campbell [17]. After discussing the well-known problems involved in formalising the subjunctive and counterfactual conditionals in terms of the weak material conditional, Campbell chooses

Mackie's analysis of both. According to Mackie [87], strong and counterfactual conditionals are not really declarative sentences at all. They are really very telescoped arguments with many suppressed premises (enthymemes). If strong and counterfactual conditionals are emthymemes, then they are not statements and so they need not be formalised. Campbell concludes:-

'....since on Mackie's view conditionals are not assertions, they need not (and indeed cannot) figure in a complete schedule of assertions....[Therefore]... there is no good purpose served by including these reasonings among the assertions on which reasoning rests. So such conditionals should not figure in the canonical schedule of assertions that claims to be complete in principle.'

Campbell [17] (173)

Mathematicians often use the non-declarative response in defending mathematics (and themselves) from the charge of Platonism. Mathematical sentences, it is claimed, do not make statements at all; they are simply collections of marks on paper which are manipulated according to certain rules to give certain results. The applicability of mathematics to the physical sciences is explained by the mathematical signs being capable of interpretations which relate them to physical and measurable properties and operations. But these interpretations are not essential to pure mathematics, and properly speaking mathematical sentences are senseless. Mathematics has no ontology. This is the philosophy of mathematics which is modern formalism as espoused by Haskell Curry [34] and others.

Whether an invocation of the non-declarative response is correct obviously depends on whether the sentences so classified are declarative or not. How does one tell? Given Austin labelled as the 'descriptive fallacy', that of assuming that an indicative sentence can always be used to make a statement, syntax can give no help. The best approach is to consider what it is a declarative can be used to do, that a non-declarative sentence cannot.

One of the commonest uses of statement-making sentences is to justify, defend, or explain a position; or else to refute or attack somebody else's position. It is a peculiar feature of declarative sentences that, when put together to form a set of assumptions or premisses, it often happens that it is then possible to infer a completely new decarative sentence, and this peculiar feature is the basis of reason and argument in general. What defines a sentence as declarative, is that it can be added to a set of declarative sentences to derive a declarative sentence not previously derivable.[3] Thus from 'Charles I was beheaded or Charles I escaped to France' we can infer neither 'Charles I was beheaded' or 'Charles I escaped to France'. But add 'Charles I did not escape to France' and it is immediately deducible that 'Charles I was beheaded'.

Applied to strong conditionals the criterion disagrees with the Mackie-Campbell view of their non-declarative status. Consider the sentences:-

(a) 'This solution of hydrogen peroxide has bleach added to it at time t.'

(b) 'This solution of hydrogen peroxide evolves oxygen at time t + 5 seconds.'

Suppose the indexical elements of (a) and (b) are anchored to the appropriate particulars. (a) does not entail (b). However if to (a) we add (c)

(c) 'If this solution of hydrogen peroxide has bleach added to it at time t then it must be that this solution of hydrogen peroxide evolves oxygen at time t + 5 seconds.'

then from (a) and (c), (b) is derivable. Consequently (c) is an information bearing sentence and counts as declarative.

Applied to formalism as a philosophy of mathematics, the criterion propels discussion into the area of mathematical reasoning in applied mathematics.

The issues here are too ramified to be treated fully in a short section, and Lehman [79] contains an examination of Curry's modern formalism which can be consulted by those interested in the issues. I will only mark out where I think the important issues are.

Bearing in mind the criterion for declarativeness suggested, there is a prima facie case for saying that mathematical sentences are declarative or information-bearing. Mathematics plays an important part in inferences in the physical sciences. For instance, calculus is required in order to derive a report on the position and velocity of a shell at time t given a complete account of the forces acting on the shell from the moment of firing. Without calculus such ballistic calculations would be impossible. This suggests that calculus must consist of declarative sentences.

The strangest counter to this argument I can think of is to deny what is presumed in the argument: that calculus is required to derive results in physics. This seems patently wrong, but the position is quite defendable in fact. The formalist argues that calculus can be used in applied mathematics as a set of transformation rules for deriving physical sentences from other physical sentences, much as the schema '$((p \supset q) \& p) \supset q$' can be used as a rule of reasoning in argument. Physicalistic sentences are actually derivable without mention of calculus, but calculus is mentioned because it makes the reasoning easier to follow. The fact that the sentences of calculus appear in the same calculation as evidently declarative reports on the velocity of the shell, should not, according to the formalist, mislead us into thinking they have the same logical status.

As a tentative realist I must register my disquiet over two aspects of this reply. The first, and possibly the least justified flutter of suspicion is that when inferences in applied mathematics are formalised in first-order idiom, that the sentences of pure mathematics have to be entered in the same manner as sentences reporting on the physical position, velocity etc., of any mass body, if the argument is to be represented as logically valid. The formalist will retort that formalisation in first-order logic distorts the nature of mathematical reasoning by requiring the misrepresentation of the logical status of mathematical sentences. (Since logic is equipped fundamentally only to handle declarative sentences, my first disquiet is perhaps too much of a petitio anyway). The second grumble is perhaps more serious. The formalist owes us an account of the logical status of sentences which combine terminology of pure mathematics with that of reference to the physical world (e.g. reports on standard deviations, numbers of items etc.). These sentences are obviously declarative and the challenge to the formalist is to supply an account which is fully satisfactory and yet involves no recantation of his formalist ideals.

6.4 Identity Reduction

An identity reduction occurs where a sort K of entities is identified with another sort J. In philosophy, identity reductions cause the maximum of argument and the minimum of reasoned agreement; why this is will be seen shortly. First, it is useful to see why identity reductions are ever attempted at all.

Frequently, in ontology, an ontologist ambitious enough to formulate an ontological hypothesis, is challenged to take a position in respect of a kind K, which (a) there is good reason to think instances exist; but (b) does not apparently fit into any category of objects admitted by his hypothesis. The ontologist may then choose to challenge (b) by claiming that Ks are in fact identical with a particular species of entity J recognised by his hypothesis.

An example of this technique is provided by Berkeley [14]. Berkeley's ontology consisted of minds and their ideas. This ontology collides with the view that there are also objects of everyday experience which are non-mental. Rather than deny the existence of such objects, Berkeley identified them with perceptions in the mind of God. A more modern identity reduction is attempted by Smart [133]. Smart believes in a physicalist ontology. What then of thoughts and sensations? Rather than deny their existence, Smart identifies them with brain-processes. Since this example is the most currently absorbing of all identity reductions, I will use it as the centrepiece in examining identity reductions in general.

Identity claims come in two kinds. The first, most familiar kind, are where two terms are equated via an identity sign (eg. 'Cicero = Tully', 'Abraham Lincoln = America's greatest president'). Identity statements of this kind I call <u>token-token identity statements</u>, since they relate specific particulars. <u>Type-type identity statements</u> are statements which claim the identity of a particular kind with that of another e.g. 'Numbers are identical with a species of set', 'Thoughts are brain-processes (of an unspecified kind)'.[4]

One way of coming to grips with the problems of identity reductions is to enquire how type-type identity statements can be verified or falsified.

Consider the case of the mind-brain identity theory. Might we say that this theory is <u>falsified</u> if it became known that there was a lack of significant correlation between mental processes and brain processes? Such an empirical discovery need not falsify the mind-brain identity theory. The mind-brain identity theorist can then retreat to a position that Davidson [40] calls 'anomalous monism' in which mental events are identical to brain processes for each <u>token</u>, but there is a lack of correlation in the form of psycho-physical laws relating the occurence of a particular <u>type</u> of brain-process to a particular <u>type</u> of thought. Anomalous monism is by no means merely an <u>ad hoc</u> evasion of a potentially falsifying situation (though it does, I think, deprive the mind-brain identity theory of most of its empirical interest). It is quite possible that given genetic and environmental differences that the same type of thought in X as in Y is not encoded as the same type of brain-state. (An analogy exists in computing: the same piece of software can be stored at different addresses by different computers).

Would a correlation between mental processes and brain processes verify (or at least render more probable) the mind-brain identity theory? Alas, once again, no. There is a group of traditional dualist views which insist both on such a correlation and yet deny any identity between brain-processes and mental processes: parallelism, epiphenomenalism, and interactionism are the leading three versions of dualism. At this point, it becomes clear that there is no possible empirical resolution of this particular philosophical dispute. When the hard core empirical question about mind-brain process correlation is subtracted from the mind-brain identity theory, what is left over is a philosophical pseudo-problem concerned with the valuation of fringe sentences which could only be settled by convention. But not all type-type identity claims are pseudo-claims; indeed Smart [133] groups 'Mental processes are brain processes' with 'Lightning is a form of electrical discharge' as being both genuine empirical theories. This is not so, and it is worth looking at the difference.

For the sake of a simple illustration I prefer to use 'Macroscopic organisms are collections of cells' rather than 'Lightning is an electrical discharge'. It is obvious that the claim 'Macroscopic organisms are collections of cells' is a substantive empirical claim. How would it be verified (or for Popperians, corroborated)? The obvious way to endorse this claim (in fact, the way it was endorsed and the study of ontology founded) is by microscopic examination of thin slices of plant and animal tissue. Under the resolution of a good optical microscope it is quite easy to make out the cellular structure of an appropriately prepared and mounted specimen.

Looking down the microscope the cytologist is presented with a structure of remarkable diversity in appearance from the structure that presents itself to the naked eye on the platform of his microscope. What he sees down the microscope looking at a piece of plant tissue is a brick-wall arrangement of cells each with a cellulose wall framing a cytoplasm and nucleus and an internal vacuole filled with fluid. What he sees on the platform is a thin green translucent piece of plant tissue. At very high magnifications, e.g. under an electron microscope, the cytologist is presented with a world even more bizarre and diverse from the macroscopic world of his senses.

Yet ontology has never been plagued by philosophical arguments. There are no cellular interactionists who insist that the domain of cellular structures and living organisms are radically diverse and distinct and yet mysteriously interactive. There are no cellular epiphenomenalists who insist that what happens to the body influences the cells, that nothing that happens to the cells influences the body. The main reason for this happy agreement is that cytologists have agreed that the same identity criteria apply to both collections of cells and parts of living organisms and living organisms themselves. Living organisms and collections of cells belong to the same category. A cytologist who looks down a microscope is presented with a structure at space-time coordinates x,y,z,t which are the same coordinates as the tissue mounted on his slide. Any material object x and any material object y are identical just when their space-time coordinates are the same: therefore the cytologist of a philosophical cast of mind rightly and logically concludes an identity.

Identity claims (whether token-token or type-type) are significant, cognitive claims just when they equate items of the same category. To put the matter in the formal mode, as long as there is an agreement on the criterion of identity appropriate to evaluating an identity claim, so that the terms/expressions of the claim are assessed relative to the same criterion then there is a significant claim. Identity claims of this kind I call subcategory identity statements. In contrast identity claims that relate expressions where there is no such agreed criterion are intercategory identity claims, which belong to the domain of fringe sentences as do their denials. If two disputants of the truth of an identity claim are not willing to abide by the same identity criterion then there is no way in principle of settling their differences. Contests like the mind-brain identity theory vs dualism are incapable of resolution, because neither the identity theorist, nor his dualist opponents, can concede that mental processes or brain processes are governed by the same identity criteria: to do so would be either to beg the question in favour of their own position or to give it away to their opponents.

As practised by scientists identity reductions are an important part of the progress of science. In philosophy, however, identity reductions are generally intercategoric operations frought with dissent, and ontologists are advised to avoid them.

6.5 Quine on Ontological Reduction

Quine views on ontological reduction are required reading for anybody anxious to get to grips with the topic of ontological reduction. This chapter would be incomplete without an examination of his writings on the subject. Since Quine's views evolve starting from 'On What There Is' in 1948 and continuing to 'Ontological Relativity' in 1970, I have chosen to examine his thought chronologically.

'On What There Is' (1948)

Quine's remarks on ontological reduction are limited to part of one paragraph.

'......when we say that some zoological species are cross fertile we are committing ourselves to recognising as entities the several species themselves, abstract though they are. We remain so committed at least until we devise some way of paraphrasing the statement as to show that the seeming reference to species on the part of our bound variable was an avoidable manner of speaking.'

Quine [116] (13)

Despite the brevity of this passage, there are two points here to note of special interest.

First, when Quine talks here about banishing ontological commitments, he has in mind banishing the ontological commitments of <u>people</u> rather than <u>theories</u> (hence '<u>We</u> remain so committed....'). Quine consequently avoids a mistake attributed to him by Alston [2], and an attribution endorsed by Searle [131], of believing that it is possible to change the ontological commitments of a theory through formalisation or paraphrase. Alston's argument is in essence a direct one. Let S be any sentence with an ontological commitment to Ks; an ontological commitment the ontologist O does not want to share. O tries to reduce this commitment by paraphrasing S as S_1. In S_1, reference to Ks is avoided and an ontological reduction effected. What is wrong with this method, argues Alston, is that either S and S_1 make the same statement or they do not. If they do, then their ontological commitments are the same and no reduction has taken place. If they do not, then S_1 is no true paraphrase of S since they are used to make different claims.

Applied to Quine; this criticism misses the point of his theory of ontological reduction as Chihara [26] has observed. The idea of paraphrasing a theory T to remove an unwanted reference to species is not to show that T was not so committed to species but to provide an ontologically <u>superior</u> theory T_1 which does not have such a commitment and which we can adopt in place of T. The presupposition of this approach is that although whatever useful purpose T served can be served by T_1, T and T_1 do not make the same ontological claims, and hence do not make the same overall statement.

This point raises the second and related point about the nature of paraphrase. It follows directly from Quine's position that paraphrase need not preserve meaning: if it did, then it would be impossible to reduce a theory T_1s by paraphrase alone. Quine's disavowal, in Word and Object, of the relevance of synonymy to paraphrase, was legislated by his position in 'On What There Is'.[5]

Word and Object (1960)

Word and Object is rich in ontological speculation concerning propositions, numbers, sets and universals. However metaontological developments are more restricted. I shall concentrate on a theme that Quine raises: that of the distinction (if any) between explicative and eliminative reduction.

Quine's exposition of the distinction is marred by some amount of the equivocation and unclarity. At the simplest level the distinction between eliminative and explicative reductions is the distinction between (a) a reduction where a kind K is eliminated from our ontology and (b) where a kind K_1 is explicated as a kind K_2.

However Quine fogs the distinction by declaring that '...explication is elimination, and ... conversely elimination can often be allowed the air of explication' (Quine [116] (265)). Quine also refers to explication as 'philosophical analysis' (259), and hastens to add he does not understand by 'analysis' what the Oxford School of linguistic analysis understood by 'analysis', but something quite different that does not depend on replacing expressions by synonymous expressions. What that something is, is left in the air, but Quine's passing remarks incline to the judgement that what Quine means by 'explication' is what he means by 'paraphrase' since what he says about them is much the same.

Quine's 'explication is elimination', which he italicises (260), comes clearer in respect ot Wiener's and Kuratowski's treatment of ordered pairs, which Quine offers as an example of 'explication'. Quine's point is that this 'explication' (=paraphrase) allowed mathematicians to achieve a kind of elimination - whether of ordered pairs from their ontology or the language of ordered pairs from the language of mathematics is not made clear - which they could not have achieved before. This is an uncontroversial stand which can only be condemned on the grounds that the terminology used to put it across is confusing.

Quine's '...conversely elimination can often be allowed the air of explication.' is even more confusing; principally because Quine is here equivocating again of 'explication' using it in the original sense as applied to reduction. Decoded, Quine is saying that, in certain cases, when T is paraphrased as a theory T_1, the reduction of the entities of T to those of T_1 can be treated as an explicative reduction as easily as an eliminative reduction.

It becomes apparent on later reading that Quine is ambivalent about the value of his own distinction between explicative and eliminative reduction. Thus in contrasting eliminative and explicative physicalism, Quine remarks:-

'Is physicalism a repudiation of mental objects after all, or a theory of them? Does it repudiate the mental state of pain of anger in favour of its physical concomitant [eliminative physicalism], or does it identify the mental state with a state of the physical organism [explicative physicalism] (and so a state of the physical organism with the mental state)?.... Some may... final comfort in reflecting that the distinction between an eliminative and explicative physicalism is unreal.'

Quine [116] (265)

Immediately afterwards, Quine openly rejects the eliminative-explicative distinction.

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Some attempt on these questions can be made through the services of System 4 which was designed specifically to deal with reasoning of this kind in which classical principles do not apply. Let P = 'There are only physical objects and processes', Q = 'Mental processes do exist' and R = 'Mental processes are physical processes'. We have (P & Q)   R. Since R has been classified as a fringe sentence neither R nor - R is true i.e. - (R v - R). The question as to whether P or Q being fringe hypotheses follows from $(P \& Q) \supset R$ and - (Rv - R) can be formalised in System 4 s the query as to whether

$$[((P \& Q) \supset R) \& -(R \vee -R)] \supset [-(P \vee -P) \vee -(Q \vee -Q)]$$

is valid in System 4 or not. The answer is that it is valid; for it can be proved that this formula is a member of any conforming set.[6]

The conclusion, based on System 4 reasoning, is that either physicalism (P) and/or the view that mental processes exist is a fringe hypothesis. Since the existence of mental processes seems fairly established, the weight of suspicion would seem to fall on P.

As regards Quine's second example, Eddington vs Stebbing, the issues are quite different. Swarms of molecules and solid tables belong to the same category of being, since, as spatio-temporal objects, they share the same identity-conditions. If a swarm of molecules occupies the same space-time coordinates as a wardrobe or table, then the swarm and the table are one. There is nothing problematic in this identification and in this Stebbing is in the right; and Quine in the wrong for classifying the identity claim as unreal.

'Ontological Reduction and the World of Numbers' (1964)

In this article Quine specifically sets out to examine the nature of ontological reduction as applied to first-order theories. The goal is to specify the nature of the reduction relation (R) as it holds between two theories $\emptyset$ and $\emptyset_1$. 'ROO_1' can be read as '$\emptyset$ reduces to $\emptyset_1$'. Quine's definition of R is as follows:-

'The standard of reduction of a theory $\emptyset$ to a theory $\emptyset_1$ can now be put as follows. We specify a function, not necessarily in the notation of $\emptyset$ or $\emptyset_1$, which admits as arguments all objects in the universe of $\emptyset$ and takes values in the universe of $\emptyset_1$. This is the proxy function. Then to each n-place primitive predicate of $\emptyset$, for each n, we effectively associate an open sentence of $\emptyset_1$ with n free variables, in such a way that the predicate is fulfilled by an n-tuple of arguments of the proxy function always and only when the open sentence is fulfilled by the corresponding n-tuple of values.'

Quine [116] (218)

The idea, I take it, is that if $R\emptyset\emptyset_1$ then theory $\emptyset_1$ is capable of subsuming the position that $\emptyset$ held and can be freely used in place of $\emptyset_1$. Quine does not mention constants or function-letters in his definition of R. I conjecture that a plausible extention of Quine's account would be as follows. Let p be the proxy function from the domain D of $\emptyset$ to the domain D_1 of $\emptyset_1$, then for each n-ary primitive function-letter f of $\emptyset$ there is effectively associated an n-ary function-expression f_1 of $\emptyset_1$ such that where $d_1,\ldots, d_{n+1}$ are any elements of D, $\langle d_1,\ldots d_{n+1}\rangle$ is an element of the extension of f iff $\langle p(d_1)\ldots, p(d_{n+1})\rangle$ is an element of the extension of f_1. Where c is a primitive constant of $\emptyset$ which denotes d, there is associated a closed term c_1 which denotes p(d). I also take it that the formulae of $\emptyset$ are written out in primitive notation. Some logical observations on R.

First, an unfortunate feature of Quine's definition of R has been isolated by Tharp [143]. It turns out that unless the proxy function is an onto function from the domain D of $\emptyset$ to the domain D_1 of $\emptyset_1$, that truths in $\emptyset$ do not necessarily go into truths of ϕ_1'. Tharp suggests adding this extra condition into Quine's definition and I shall suppose this done.

R is reflexive. Given theory ϕ' with domain D, we give the identity function on D as proxy function and the identity function on the set of primitive variables of ϕ as Quine's second function (since Quine does not give it a name call it the symbol function). In this case $\phi' = \phi_1'$.

Is R transitive? Certainly it should be and intuition suggests that, as Quine defines it, it is. Certainty would demand a proof in mathematical logic of considerable bulk and complexity. In view of the fact that, as we shall shortly see, Quine's criterion is at any rate seriously wrong, the labour involved would not be rewarded by a result of sufficient philosophical interest to repay it.

Is R symmetrical? In the general case, no, since to demonstrate R is symmetrical we should have to suppose two things. First that the proxy function was always 1 - 1 and hence always had an inverse. Second, that the values that the symbol function gave for its arguments were all and only the primitive variables of ϕ_1.

In Ontological Relativity, Quine denied that the proxy function need always be 1 - 1.

'The proxy function used in reducing one theory to another need not, like Godel numbering, be one-to-one ....[for] the fragment of economic theory lately noted...[we] would happily reduce its ontology of persons to a less numerous one of incomes. The proxy function would assign to each person his income. It is not one - to - one; distinct persons give way to identical incomes. The reason such a reduction is acceptable is that it merges the images of only such individuals as never had been distinguished by the old theory.'

Quine [113] (56)

However Quine goes on to argue that given any 1 - 1 function f from the domain D of a theory ϕ, the ontology of ϕ can be reduced to that of the range of f.

'One ontology is <u>always</u> reducible to another when we are given a proxy function f that is one - one. The essential reasoning is as follows. Where P is any predicate of the old system, its work can be done in the new system by a new predicate which we interpret as true of just the correlates fx of the old objects x that P was true of.'

Quine [113] (59)

It seems that Quine's standards of ontological reduction are too liberal. For let the domain D of $\emptyset$ be countable; then a 1 - 1 function exists from the domain D into the set N of natural numbers. Would this show that we could dispense with all countable ontologies bar that of numbers? Quine recognises a similar sort of danger from a strong form of the Lowenheim-Skolem theorem which says that every first-order theory that has a model in the domain of natural numbers. Remarking on this theorem, in 'Ontological Reduction and the World of Numbers', Quine says

'Reduction of a theory $\emptyset$ to natural numbers - true reduction by our new standard, and not mere modeling - means determining a proxy function that actually assigns numbers to all the objects of $\emptyset$ and maps the predicates of $\emptyset$ into open sentences of the numerical model. Where this can be done, with preservation of truth-values of closed sentences, we may well speak of reduction to natural numbers. But the Lowenheim-Skolem argument determines, in the general case, no proxy function. It does not determine which numbers are to go proxy for the respective standards of $\emptyset$. Therein it falls short of our standard of ontological reduction.'

Quine [110] (219)

Quine is, in effect, adding a rider to his previous conditions; this rider is that the proxy function be <u>constructively</u> demonstrated to exist from the domain of

$\emptyset$ to that of $\emptyset_1$ i.e. that the reductionist first give an individuating description of the proxy function before proving it to be a proxy function. This requirement does banish the spectre of a wholesale Pythagoreanism blanking out our ontology. However, Quine's criterion of reduction still produces some peculiar results.

One of these results is that whenever a relation R is discovered to well-order the domain D of a theory $\emptyset$, where D has countably many items, (by Quine's criterion) the ontology of $\emptyset$ is reducible to that of natural numbers. It is quite simple in such as case to specify a proxy function that takes the form of an order isomorphism from the set D into a subset S of the set N of natural numbers. Thus supposing that no two Oxford philosophers are born at exactly the same time, it would be possible to enumerate them by their ages. Any statement about these philosophers could thus be effectively associated in the manner Quine suggests, with a remark about natural numbers.

What is wrong in principle with Quine's suggestion that one ontology is always reducible to another given a 1 - 1 proxy function? The answer is that we cannot implement the suggestion unless we introduce numerical predicates whose sense is fixed by reference to the objects of the old ontology. Thus i may attempt to reduce the elements of Oxford's philosophy department by enumerating them. I then proceed to replace each predicate P applied to the unreduced dons by a numerical predicate P_N true of just those numbers correlated to the objects P is true of. But then to fix the sense of P_N, an essential reference is required to the Oxford philosophy department and I am forced to reacknowledge my old commitment. There are other things wrong with Quine's criterion however.

A serious deficiency of the whole of Quine's use of proxy functions is that it seems to require an acknowledgement of the objects of the domain of the old theory $\emptyset$ as well as those of the new theory $\emptyset_1$. How can a proxy function be defined at all unless we acknowledge the existence of the objects of the old theory as unreduced objects? Quine recognises he has a difficult problem here:-

'I must admit that my formulation suffers from a conspicuos element of make-believe.... My formulation belongs, by its nature, in an inclusive theory that admits the objects of $\emptyset$ as unreduced, and the objects of $\emptyset_1$ on an equal footing.'

Quine [110] (219)

But Quine shrugs off the problem adding:-

'But the formulation seems, if we overlook this imperfection [!], to mark the boundary we want.'

In 'Ontological Relativity', Quine takes this issue more seriously.

<u>'Ontological Relativity' (1969)</u>

Quine tackles the problem posed by the proxy function directly.

'.......we cannot declare our new ontological economies without having recourse to the uneconomical old ontology.
This sounds, perhaps, like a predicament: as if no ontological economy is justifiable unless it is false economy and the repudiated objects exist after all. But actually this is wrong; there is no more cause for worry here than there is

in _reductio ad absurdum_. If what we want to show is that the universe U is excessive and that only a part exists, or need exist, then we are quite within our rights to assume all of U for the space of the argument. We show thereby that if all of U were needed then not all of U would be needed, and so our ontological reduction is sealed by _reductio ad absurdum_.'

Quine [113] (58)

Is this a fair analogy? Reflection suggests it is not. Thus supposing one wishes to prove p; the method of _reductio ad absurdum_ invites the assumption of -p. From -p and a pool of assumptions S, p is derived. Thus a contradiction is evolved from $S \cup \{-p\}$ and --p (=p by Double Negation) is derived on the strength of S. Though an inconsistency is evolved during the course of the proof, consistency in regard to assumptions is restored at the end of the proof. But in Quine's procedure, the _intitial_ position is one of _consistency_ and the _terminal_ position is one of _inconsistency_ . It is consistent to suppose that K_1s exist and K_2s exist and a proxy function f exists from the domain of K_1s to that of K_2s. It is not consistent then to conclude that one has shown that K_1s need not be acknowledged to exist on the strength of such a proxy function, since to acknowledge f is, _ex hypothesi_, to acknowledge K_1s.

Quine suggests an alternative formulation of ontological reduction in 'Ontological Relativity'.

'We may picture the vocabulary of a theory as comprising logical signs such as quantifiers and the signs for the truth functions and identity, and in addition descriptive or nonlogical signs, which, typically are singular terms, or names, and general terms, or predicates. Suppose next that in the statements which comprise the theory, that is, are true according to the theory, we abstract from the meanings of the nonlogical vocabulary and range of the variables. We are left with the logical form of the theory, or, as I shall say, the _theory_ form. Now we may interpret this theory from anew by picking a new universe for its variables of quantification to range over and assigning objects from this universe to the names, and choosing subsets of this universe as extensions of the one-place predicates and so on.'

Quine [113] (33 - 54)

Chihara [26] (127 - 128) does not regard this model-theoretic account of reduction as successful. Essentially Quine is arguing that we can ontologically reduce a commitment to Ks incurred by a theory O if we can define a model for O in which Ks do not occur. Chihara takes T as his theory form.

T: $(\exists x)(\exists y)(\exists z)(x \neq y \ \& \ y \neq z \ \& \ x \neq z \ \& \ (w)(w = x \vee w = y \vee w = z))$

$(x) -Rx$

$(\exists x)(y)(Ty \ \& \ Oy) \equiv x = y$

The interpretations given to T are as follows:-

Td:	Domain:	dogs that live in my neighbourhood.
R:	____	has retractable claws.
T:	____	has only three legs.
O:	____	is owned by Mr. Jones.

Tc:	Domain:	cats that live in my neighbourhood.
R:	____	has non-retractable claws.
T:	____	has only three legs.
O:	____	is owned by Mr. Smith.

Assuming that Tc and Td are both models for T, by Quine's criterion Tc can be reduced to Td or vice-versa. But does the ability to define alternative models for small slices of theory prove a reduction? Chihara does not think so and argues convincingly that since the key ontological concepts of a theory can be used outside that theory, reduction is only successful when it applies across the board to all occurrences of the concepts. Therefore the proper unit of reduction is not the theory but the language in which all relevant theories are expressed. In reducing a language ipso facto the theories expressed in that language are reduced as well.

The main focus of 'Ontological Relativity' is on the phenomenon I have called 'ontological elasticity', and the consequences of accepting its presence. That I have no chosen to follow Quine's terminology of 'ontological relativity' is an indicator that our conclusions are not always in sympathy. Nevertheless there is much of first-rate importance with which I thoroughly agree with Quine and there is no doubt that Quine was the first to set the right level of importance on these fundamental issues. It is best to lay down the elements of Quine's position and demarcate the areas of agreement from disagreement. These elements are as follows:-

(a) There are often alternative representations of the domain of a theory which are equally acceptable on the basis of all evidence. None of them can be identified as the unique right representation.

'Each.... interpretation of ..[a]...theory form is called a model of it if it makes it come out true. Which of these models is meant in a given actual theory cannot, of course, be guessed from the theory form... It is thus meaningless within the theory to say which of the various possible models of our theory form is our real or intended model.'

Quine [113] (54)

(b) In such cases we cannot enquire after the identity of the elements of the domain of the theory.

'Numbers..... are known only by their laws, the laws of arithmetic, so that any constructs obeying those laws - certain sets for instance - are eligible in turn as explications of number. Sets in turn are known only by their laws, the laws of set theory.... The subtle point is that any progression will serve as a version of number so long and only so long as we stick to one and the same progression. Arithmetic is, in this sense, all there is to number: there is no saying absolutely what the numbers are; there is only arithmetic.'

Quine [113] (44 - 45)

(c) Therefore we cannot enquire after the ontology of a theory or its ontological commitments, unless our enquiry is relativised to a particular interpretation of the theory itself.

'What makes sense is to say not what the objects of a theory are, absolutely speaking, but how one theory of objects is interpretable or reinterpretable in another.'

Quine [113] (50)

I agree with Quine on elements (a) and (b); but I am in some doubt about element (c), which Quine seems to treat as a corollary of (a) and (b).
First, a general observation. The phenomenon of ontological elasticity is a phenomenon which has been proven to extend only to <u>some</u> theories and <u>not</u> to <u>all</u>. Therefore for Quine to draw a conclusion in respect to <u>all</u> theories is an overstatement of what may reasonably be inferred from his assumptions. It seems wise then, to restrict the domain of (c) to all those and only those theories of which (a) and (b) hold true.

Does (c) follow from (a) and (b)? An informal argument which suggests it does not is this one. A theory $\emptyset$ may be ontologically elastic in that $\emptyset$ can be formalised to a variety of domains $D_1, \ldots, D_n$. But all of these domains may share a general feature F in common. In such as case, we can argue that although $\emptyset$ does not require a universe which contains specifically one of $D_1, \ldots D_n$ in order for $\emptyset$ to be true, nonetheless $\emptyset$ does require a universe which displays the general property F. For example, it is true to say with Quine that any progression can be made to serve as the domain of Peano arithmetic. But a general feature of any such domain must be that it contains a denumerable number of elements. Therefore we can say, ontological elasticity notwithstanding, that Peano's axioms are committed to the existence of a denumerable domain.

Had Quine adopted this course, it would have fitted neatly in with one of his earlier definitions of ontological commitment which was quoted in chapter one and which I quote here once again.

'If a theory implies '$(\exists x)$ (x is a dog)' it will not tolerate an empty universe; still the theory might be fulfilled by a universe that contained Collies to the exclusion of Spaniels and also vice-versa. So there is more to be said of a theory, ontologically, than just saying what objects, if any, that the theory requires; we can also ask what various universes would be severally sufficient. The specific objects required; if any, are the objects common to all these universes.'

Quine [112] (96)

Quine's actual course, as remarked, differs sharply from that which might be projected from the above quotation, and it brings him into collision with some of his earlier views on ontological commitment. Thus the earlier Quine would have said that a theory which contained '$(\exists x)$ number x' was committed to the existence of numbers. But the Quine of 'Ontological Relativity' would disagree. Whether '$(\exists x)$ number x' was committed to numbers or not depends, according to the later Quine, on how we interpret the 1 - place predicate 'number'. Thus let O be any theory having '$(\exists x)$ number x' as a theorem. Suppose that O is reducible to a range of theories $\emptyset_1, ..., \emptyset_n$ i.e. $R\emptyset\emptyset_1$, $R\emptyset\emptyset_2$, ... $R\emptyset\emptyset_n$. In some of $\emptyset_1,..., \emptyset_n$, '$(\exists x)$ number x' is a theorem but in others it is not; the predicate 'number' being taken over by some other predicate.

What prevents Quine from taking the course suggested; of examining each reduction and arriving at the ontological commitments of $\emptyset$ by distilling off the common elements of $\emptyset_1, ..., \emptyset_n$? I think the reason may be that Quine feels that the range of any reducing theories is not fixed, and therefore that any such distillation will be made from an arbitrarily limited sample. Thus the ontologies of $\emptyset_1,...., \emptyset_n$ are subject to the same relativism that $\emptyset$ is, in that for any $\emptyset_i$, $(1 \leq i \leq n)$, there is another reducing series $\emptyset'_1,..., \emptyset'_m$ (where $RO_i\emptyset'_1,..., RO_i\emptyset'_m$) and for any $\emptyset'_j$ $(1 \leq j \leq m)$ there is another reducing series $O''_1, ... O''_L$ and so on in an infinite regress. Two remarks about this possibility.

The first is that there is a distinctness assumption buried in this idea of infinite regress. This is that $\emptyset_1, ..., \emptyset_n$ and $\emptyset'_1,..., \emptyset'_m$ and $\emptyset''_1,..., \emptyset''_L$ are all disjoint series. This assumption is false if R is transitive. For if $R\emptyset\emptyset_i$ and $R\emptyset_i\emptyset'_j$ then $R\emptyset\emptyset'_j$ and $\emptyset'_j$ is an element of the series $\emptyset_1..., \emptyset_n$. Hence the generation of a new <u>series</u> need not generate any new <u>elements.</u> It seems intuitively true that R is or should be transitive, in which case the distinctness assumption fails. So much for the regress.

The second remark concerns the infinite part of the regress. It may be that $\emptyset$ has an infinite number of reducing theories in which case $\emptyset_1, \emptyset_2, \emptyset_3.....$ is a series without end. But would this stop the distillation process from being practicable or at least comprehensible? It seems it should not, for Peano's axioms are ontologically committed to a denumerable domain as remarked, but they also have a denumerable number of reducing theories with different domains. Whether the concept of infinity ultimately proves bothersome here, will depend, I think, on two factors. First, on whether a series like $\phi_1, \phi_2, \phi_3, \ldots$ has a definable well-ordering. Secondly on whether one adopts a constructivist or anti-realist attitude to statements about infinite collections themselves.

If $\phi_1, \phi_2, \phi_3 \ldots$ proves to have a definable well-ordering relation then it becomes possible to use inductive techniques to establish universal quantifier statements about them. In such a case I can see nothing problematic about a claim that all $\phi_1, \phi_2, \phi_3, \ldots$ share F, where this claim is backed by an inductive proof.

What if $\phi_1, \phi_2, \phi_3, \ldots$ lacks the appropriate well-ordering relation? In this case the dissent between the realist mathematician and the anti-realist and constructivists becomes important. The realist will still credit sense to universally quantified claim about $\phi_1, \phi_2, \phi_3, \ldots$ The constructivists and anti-realists will see it differently. The constructivist denies the existence of completed infinite collections: for him, to say that there are (e.g.) an infinite number of numbers is just a bad way of saying that for any finite collection of numbers it is always possible to think up (construct) a number not found in that collection. The constructivist prefers to see a universally quantified statement to the effect that all numbers have property F as being equivalent in cash-value terms to a statement about the impossibility of constructing a number which has -F. The dubious idiom of modal logic, 'impossibility', can be conveniently exchanged by saying that from the assumption that Fn for some arbitrary number n, a contradiction can be derived. This proof-theoretic interpretation of claims about infinite totalities is shared by the anti-realist mathematician. The difference is that whereas the constructivist bases his claim on an excursion into the metaphysics of infinity, the anti-realist trots towards the theory of meaning to gain his support. Both sorts of mathematician might be reasonably suspicious of the sense of an infinity-claim when the domain in question had no useful well-ordering and hence no means of proving the claim.

APPENDIX III

Russell's Recursive Reduction of Instants of Time

Russell's recursive reduction of instants of time is a good example of recursive reduction, if only because it dispels the idea that recursive reduction is necessarily limited to mathematics. The reduction is taken from Russell [123]. Russell observes that the three main conceptions of physics are space, time and the objects therein. As regards time, we are only aware of events of measurable duration and not of instants of time. Instants of time are not objects of acquiantance, and for Russell, this meant their existence as unreduced entities was in doubt. According to Russell logical constructions were required to be substituted for instants of time. The process of recursive reduction begins with isolation of an axiom set describing the important properties of instants. Russell retails:-

'What are the properties we expect of instants? First, they must form a series: of any two, one must be before the one; if one is before another, and the other before a third, the first must be before the third. Secondly, every event must be at a certain number of instants; two events are simultaneous if they are at the same instant, and one is before the other if there is an instant, at which the one is, which is earlier than some [? any] instant at which the other is. Thirdly, if we assume that there is always some change going on somewhere during the time when any given event persists, the series of instants ought to be compact, i.e. given any two instants, there ought to be other instants between them.'

Russell [123] (95)

Some of the expressions of this passage require explanation.

In Russell's terminology, two events are *simultaneous* *not* only if they begin and end at the same time, but simply when there is a time at which they are both going on. One event is *before* another just when that event begins before the other event. An event is *wholly* before another when it begins *and* ends before the other event. The relations of being after and being wholly after, are just the converses of the relations of being before and being wholly before. (See diagram 5).

Russell's remarks about the properties we expect of moments of time can be codified in a language L_1.

$$L_1 = \{\text{'EV, IN, AT, } \leq \text{, SIM, } <\text{'}\}$$

EV $=_{df}$ is an event

IN $=_{df}$ is an instant

AT $=_{df}$ at

$\leq$ $=_{df}$ before

SIM $=_{df}$ simultaneous

$<$ $=_{df}$ wholly before

Axiomatised in L_1, Russell's statements about instants emerge as an axiom set A, whose elements are just the following

$A_1(1)$ $(x)(y)(\text{IN } x \mathbin{\&} \text{IN } y \mathbin{\&} -x = y) \supset (x < y \vee y < x)$

$A_2(2)$ $(x)(y)(\text{IN } x \mathbin{\&} \text{IN } y) \supset (x < y \supset - y < x)$

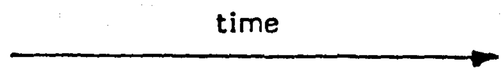
$A_1(3)$ $(x)(y)(z)(\text{IN } x \mathbin{\&} \text{IN } y \mathbin{\&} \text{IN } z) \supset ((x < y \mathbin{\&} y < z) \supset x < z)$

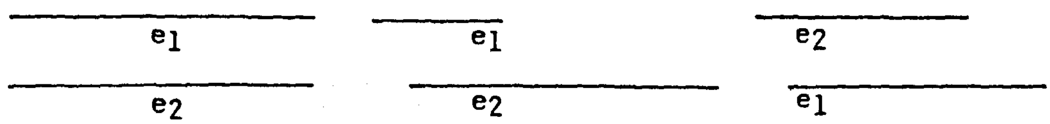
$A_1(4)$ $(x)\ \text{EV } x \supset (\exists y)\ \text{IN } y \mathbin{\&} \text{AT } x,y$

$A_1(5)$ $(x)(y)(\text{EV } x \mathbin{\&} \text{EV } y) \supset (\text{SIM } x,y \equiv (\exists z)\ \text{IN } z \mathbin{\&} \text{AT } x,z \mathbin{\&} \text{AT } y,z)$

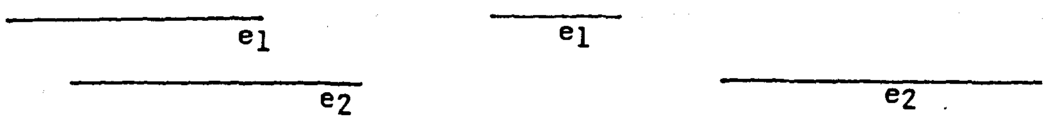
$A_1(6)$ $(x)(y)(\text{EV } x \mathbin{\&} \text{EV } y) \supset (x \leq y \equiv ((\exists z)\ \text{IN } z \mathbin{\&} \text{AT } x,z \mathbin{\&} (w)(\text{IN} w \mathbin{\&} \text{AT } y,w$
$\supset z < w))$

Diagram 7



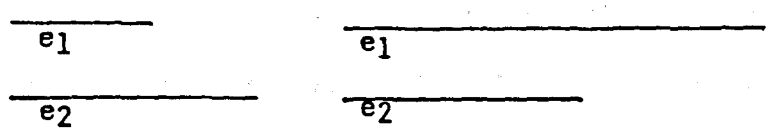


e_1 and e_2 are simultaneous, SIM e_1 e_2

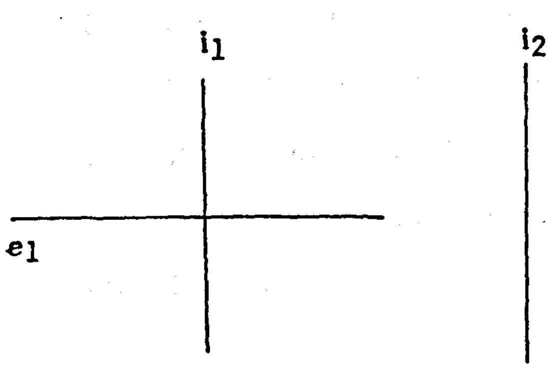


e_1 is before e_2, e_2 is after e_1, $e_1 \leq e_2$

e_1 is wholly before e_2, e_2 is wholly after e_1



e_1 is an initial contemporary of e_2, ICON e_1 e_2



e_1 is at instant i_1,

e_1 is not at instant i_2,

i_1 is before i_2.

AT e_1 i_1 & - AT e_1 i_2 &

$i_1 < i_2$

$A_1(7)\ (x)(y)(IN\ x\ \&\ IN\ y\ \&\ x < y) \supset (\exists z)\ IN\ z\ \&\ x < z\ \&\ z < y.$

In order to recursively reduce unreduced instants, Russell begins by identifying the nature of the logical construction he is to offer in place of an instant, which I call a CIN or <u>constructed instant.</u>

'Let us take a group of events of which any two overlap, so that there is some time, however short, when they all exist. If there is any other event which is simultaneous with all of these, let us add it to the group; let us go on until we have constructed a group such that no event outside the group is simultaneous with all of them, but all the events inside the group are simultaneous with each other. Let us define this whole group as an instant of time. It remains to show that it has the properties we expect of an instant.'

Russell [123] (95)

To guarantee that CINs, as he has constructed them, have the properties expected of <u>unreduced</u> instants, Russell makes some auxiliary assumptions about the nature of SIM, $\leq$ < and EV. Russell apparently believed (wrongly as it turns out) that his auxiliary assumptions suffice to guarantee that constructed instants have the properties of unreduced instants. Russell tabulates these assumptions in a footnote.

'In order to secure that instants form a series, we assume:

(a) No event wholly precedes itself. (An 'event' is defined as whatever is simultaneous with something or other.)

(b) If one event wholly precedes another, and the other wholly precedes a third, then the first wholly precedes the third.

(c) If one event wholly precedes another, it is not simultaneous with it.

(d) Of two events which are not simultaneous, one must wholly precede the other.'

Russell [123] (96)

Together with his definition of a constructed instant these assumptions are axiomatised in language L_2.

$$L_2 = \{ \text{'EV, CIN, } \varepsilon, \leq, \text{SIM, } <, \text{ICON'} \}$$

EV $=_{df}$ is an event

CIN $=_{df}$ is a constructed instant

ε $=_{df}$ is a member of

$\leq$ $=_{df}$ is before

SIM $=_{df}$ is simultaneous with

$<$ $=_{df}$ is wholly before

ICON $=_{df}$ is an initial contemporary of

To be added to these assumptions is Russell's definition of temporal beforeness for CINs.

'.....we shall say one [constructed] instant is before another if the group which is the one instant contains an event which is earlier than, but not simultaneous with, some event in the group which is the other instant.'

Russell [123] (95)

It turns out that, of the assumptions Russell makes to prove CINs have the properties of unreduced instants, some that are included are not needed and some that are needed are not included. If the necessary revisions are made, then the resultant axioms are encapsulated in $A_2(1)$ to $A_2(7)$.

$A_2(1)$ (w) CIN $w = [(\exists x)(x \varepsilon w)$ & $(x)(x \varepsilon w \supset EVx)$ & $(x)(y)$ $((x \varepsilon w$ & $y \varepsilon w)$

$\supset$ SIM $xy)$ & $(x)(-x \varepsilon w \supset (\exists y)y \varepsilon w$ & -SIM $xy)]$

$A_2(2)$ $(x)(y)$ (CIN x & CIN$y) \supset (x = y \equiv ((z)\ z \varepsilon x \equiv z \varepsilon y))$

$A_2(3)$ $(x)(y)$ (EV x & EV $y) \supset$ (SIM $xy \supset$ SIM $yx)$.

$A_2(4)$ $(x)(y)$ (EV x & EV y & $x < y) \supset$ - SIM xy.

$A_2(5)$ $(x)(y)$ (EV x & EV y & - SIM $xy) \supset (x < y \vee y < x)$.

$A_2(6)$ $(x)(y)$ (CIN x & CIN $y) \supset (x < y \equiv ((\exists w)(\exists z)(w \varepsilon x$ & $z \varepsilon y$ & $w < z))$.

$A_2(7)$ $(w)(x)(y)(z)$ (EV w & EV x & EV y & EV$z) \supset$ ((SIM xy & $x < z$ & $w < y)$

$\supset w < z)$.

We now require a recursive function r, where the domain of r is the set of L_1 sentences, and the range of r is a subset of the set of L_2 sentences. Moreover r, should be truth-preserving. In order for this recursive reduction to be judged successful, it must be assumed that; (i) the true sentences formulable in the language L_1 are sufficient to state comprehensively what is taken as true of instants of time; (ii) that the set of theorems deducible from axiom set A_1 coincides fairly exactly, with the set of significant truths in L_1.

Under these suppositions it is sufficient, to effect a recursive reduction, to specify a function r that maps the theorems of A_1 into the set of theorems of A_2. Here it suffices to show that if α is an axiom of A_1, then $\vdash_{A_2} r(\alpha)$. r itself is specified thus:-

where s is any sentence of L_1, r(s) = the result of substituting 'CIN' for 'IN' and 'ε' for 'AT' throughout s.

From the definition of r, together with $A_2(1)$ - $A_2(7)$, it is provable, where s = $A_1(1)$ or s = $A_1(2)$ or s = $A_1(3)$, that $\vdash_{A_2} r(s)$

Theorem 1 $\vdash_{A_2} r(A_1(1))$

Proof: $r(A_1(1))$ = '(x)(y) (CIN x & CIN y & - x = y)⊃(x < y v y < x)'

Let A and B be CINs where - A = B. It is required to prove from A_2 that

A < B v B < A;

that is, - (A < B) ⊃ B < A. Assume - A < B. By $A_2(6)$ this assumption is equivalent to:-

- ((∃w)(∃z) w ε A & z ε B & w < z);

that is:-

(w)(z) (w ε A & z ε B)⊃ - w < z

Now since - A = B, by $A_2(2)$ either there is some x such that x ε A & - x ε B or there is some y such that - y ε A & y ε B.

Lemma 1: there is some x such that x ε A & - x ε B. Let x = a. By $A_2(1)$ there is some b such that b ε B & - SIM a,b. Since (w)(z)(w ε A & z ε B)⊃ - w < z then (a ε A & b ε B)⊃ - a < b. Thus we have:-

- SIM a,b & - a < b

for EVs a and b. By $A_2(5)$ it follows that b < a. Therefore:-

b ε B & a ε A & b < a

By Existential Generalisation.

(∃w)(∃z) w ε B &, z ε A & w < z.

This, by $A_2(6)$, entails B < A.

Lemma 2: there is some y such that - y ε A & y ε B. Let y = b. By $A_2(1)$ there is some a such that a ε A & - SIM ba. But by $A_2(3)$ - SIM b,a ⊃ - SIM a,b; so - SIM a,b. Thus we are returned to the same assumptions that were used in lemma 1 to derive B < A.

Theorem 2 $\vdash_{A_2} r(A_1(2))$

Proof: $r(A_1(2))$ = '(x)(y)(CIN x & CIN y) ⊃ (x < y ⊃ - y < x)'

Let A and B be CINs. To prove A < B ⊃ - B < A, we will prove -(A < B & B > A) by indirect proof.

Assume A > B and B < A. Since A > B; by $A_2(6)$ it follows that:-

(∃w)(∃z)(w ∈ A & z ∈ B & w < z)

Since B < A; by $A_2(6)$ it follows that:-

(∃w)(∃z)(w ∈ B & z ∈ A & w < z)

So for some a,b,c,d:-

a ∈ A & b ∈ B & a < b & c ∈ B & d ∈ A & c < d.

By $A_2(1)$ it follows:-

SIM ad & SIM bc

By $A_2(7)$:-

(SIM ad & a < b & c < d) ⊃ c < b

Therefore c < b. But by $A_2(4)$, c < b ⊃ - SIM cb and by $A_2(3)$, - SIM cb ⊃ - SIM bc. Therefore - SIM bc & SIM bc. This establishes - (A > B & B > A) by indirect proof.

Theorem 3 $\vdash_{A_2} r(A_1(3))$

Proof: $r(A_1(3))$ = '(x)(y)(z) (CIN x & CIN y & CIN z) ⊃ ((x < y & y < z) ⊃ x < z)'

Let A,B,C be CINs. Assume A < B & B < C. By $A_2(6)$ for some a,b,c,d.

a ∈ A & b ∈ B & a < b

c ∈ B & d ∈ C & c < d.

By $A_2(1)$, SIM cb, and so by $A_2($ $)$:-

(SIM cb & c < d & a < b) a < d.

Hence a < d. Therefore

a ∈ A & d ∈ C & a < d.

By Existential Generalisation

$$(\exists w)(\exists z)\ w \varepsilon A \ \&\ z \varepsilon C \ \&\ w < z$$

Hence by $A_2(6)$, $A < C$.

In order to accommodate the other axioms of A_1, Russell defines the concept of an initial contemporary (ICON).

'We have next to show that every event is 'at' at least one instant, i.e. that, given any event, there is at least one class, such as we used in defining instants, of which it is a member. For this purpose, consider all the events which are simultaneous with a given event, and do not begin later, i.e. not wholly after anything simultaneous with it. We will call these the 'initial contemporaries' of the given event. It will be found that this class of events is the first instant at which the given event exists, provided every event wholly after some contemporary of the given event is wholly after some <u>initial</u> contemporary of it.'

Russell [123] (96)

Russell's definition of an initial contemporary is contained in $A_2(8)$

$$A_2(8)\ (x)(y)\ (EV\,x \ \&\ EV\ y) \supset (ICON\ xy \equiv (SIM\ xy \ \&\ (z)\ (SIM\ y\ z \supset - z < x))$$

His assumption about ICONs of any given event is expressed in $A_2(9)$

$$A_2(9)\ (x)(y)(z)\ (EV\ x \ \&\ EV\ y \ \&\ EV\ z) \supset ((y < x \ \&\ SIM\ yz) \supset (\exists w)\ ICON\ wz \ \& \ w < x)$$

Again, supplementary axioms are needed relating to ICONs. First, that for every event, there is a set containing all and only those ICONs of that event.

Second of any two things, if they are ICONs of each other then they are events. Third, that any event is simultaneous with itself. Fourth, that if a and b are ICONs of each other and c is wholly before a, then c is wholly before b. These assumptions are expressed in axioms $A_2(10)$ - $A_2(13)$.

$A_2(10)$ $(x)\ EV\ x \supset (\exists y)(z)\ z \in y \equiv ICON\ zx.$

$A_2(11)$ $(x)(y)\ ICON\ xy \supset (EV\ x\ \&\ EV\ y).$

$A_2(12)$ $(x)\ EV\ x \supset SIM\ xx.$

$A_2(13)$ $(x)(y)(z)\ (EV\ x\ \&\ EV\ y\ \&\ EV\ z) \supset ((ICON\ xy\ \&\ z < x) \supset z < y)$

To prove that every event is 'at' an instant, it suffices to prove that every event is a member of an ICON set (a set of all ICONs of a given event) and any ICON set is a CIN. Theorems 4 and 5 prove just that.

Theorem 4 $(x)\ EV\ x \supset (\exists y)(z)((z \in y \equiv ICON\ z\ x)\ \&\ x \in y)$

Proof: Let a be any EV. By $A_2(10)$ there is some A such that $(z)\ z \in A \equiv$ ICON za. Thus if ICON aa then $a \in A$.

To prove ICON aa, it is required to prove that for any EV b, SIM aa & (SIM ab $\supset$ -b < a). (see $A_2(8)$). By $A_2(12)$, SIM aa. Assume SIM ab, by $A_2(3)$ SIM ba, and by $A_2(4)$, -b < a. Hence SIM ab $\supset$ - b < a.

Therefore any event is a member of an ICON set.

Theorem 5 Every ICON set is a CIN.

Let A be any ICON set. To establish A is a CIN it is required to prove that

(i) $(\exists x)\ x \in A$

(ii) $(x)\ x \in A \supset EV\ x$

(iii) $(x)(y)\ (x \in A\ \&\ y \in A) \supset SIM\ xy.$

(iv) $(x) - x \in A \supset ((\exists y)\ y \in A\ \&\ -\ \text{SIM}\ xy)$

Lemma 1 Since A is an ICON set then

$(\exists x)(y)\ (y \in A \equiv \text{ICON}\ yx)$

Since by theorem 4, ICON xx for all x, then $(\exists x)\ x \in A$. This proves condition (i)

Lemma 2 Let a and b be any members of A, then there is some $c \in A$ where ICON ac and ICON bc. By $A_2(11)$, a and b are both EVs. This proves condition (ii).

Lemma 3 Let a and b be any members of A. There is some $c \in A$ where ICON ac & ICON bc. By $A_2(8)$, SIM ac & SIM bc. By $A_2(8)$ again, $(z)\ \text{SIM}\ cz \supset - z < a$ and $(z)\ \text{SIM}\ cz \supset - z < b$. By universal elimination, $\text{SIM}\ cb \supset - b < a$ and $\text{SIM}\ ca \supset - a < b$. Given SIM ac & SIM bc, by $A_2(3)$, SIM ca & SIM cb. Therefore $- b < a$ and $- a < b$. By $A_2(5)$, SIM ab.

Lemma 4 Let a be any EV not belonging to A. Then for some $c \in A$, - ICON ac. By $A_2(8)$

$- \text{SIM}\ ac\ \ v -(z)\ \text{SIM}\ cz \supset - z < a)$

Assume - SIM ac; then $(\exists y)\ y \in A\ \&\ -\ \text{SIM}\ ay$. This proves condition (iv).

Assume $- (z)\ \text{SIM}\ cz \supset - z < a$; then $(\exists z)\ \text{SIM}\ cz\ \&\ z < a$ and hence by $A_2(9)$ $(\exists w)$ ICON wc & $w < a$. But then $(\exists y)\ y \in A\ \&\ y < a$, by the definition of A. By $A_2(4)$, $(\exists y)\ y \in A\ \&\ -\ \text{SIM}\ ya$ and by $A_2(3)$, $(\exists y)\ y \in A\ \&\ -\ \text{SIM}\ ay$. This proves condition (iv).

Theorem 6 $A_2\ r(A_1(4))$

Proof: $r(A_1(4)) =$ '(x) EV x (y) CIN y & x y'.

By theorem 4 every EV is a member of an ICON set and by theorem 5, every ICON set is a CIN.

In order to prove $r(A_1(5))$, it is required to assume $A_2(14)$

$A_2(14)$ $(x)(y)(EV\ x\ \&\ EV\ y) \supset (SIM\ xy \supset (\exists z)\ CIN\ z\ \&\ x \in z\ \&\ y \in z)$

$A_2(14)$ is a particularly powerful axiom since, with $A_2(14)$, theorem 6 can be derived very simply without using Russell's ICONs (proof: let a be any event; SIM aa by A_2 (12); by $A_2(14)$, $(\exists z)$ CIN z & a $\in$ z). However Russell's ICONs have useful services to perform in deriving other theorems.

As for the truth of $A_2(14)$, this can be justified by the following informal proof. Let a and b be any events where SIM a b, (a and b need not be distinct). The series $a_0, a_1, a_2, a_3, \ldots$ is an ordering of the set of all events.

$\Delta_0, \Delta_1, \Delta_2, \Delta_3, \ldots$ is a series of sets defined as follows; $\Delta_0 = \{a,b\}$. For any Δ_i, where $i \geq 1$, define $\Delta_i = \Delta_{i-1} \cup \{a_i\}$ if a_i is simultaneous with all members of a_{i-1}, otherwise $\Delta_i = \Delta_{i-1}$. Let $\Delta = \cup\{\Delta_0, \ldots\}$: then Δ is a CIN since all elements in Δ are simultaneous with each other and no element simultaneous with all members of Δ is to be found outside Δ. By hypothesis, a $\in \Delta$ and so is b.

<u>Theorem 7</u> $\vdash_{A_2} r(A_1(5))$

Proof: $r(A_1(5))$ = '$(x)(y)(EV\ x\ \&\ EV\ y) \supset (SIM\ xy \equiv (\exists z)\ CIN\ z\ \&\ x \in z\ \&\ y \in z)$'

That $(x)(y)(EV\ x\ \&\ EV\ y) \supset (SIM\ xy \supset (\exists z)\ CIN\ z\ \&\ x \in z\ \&\ y \in z)$ is axiom $A_2(14)$.

Let a and b be such that $(\exists z)$ CIN z & x $\in$ z & y $\in$ z. By $A_2(1)$, SIM ab.

To prove theorem 8 it is required to prove that any event a is before an event b if and only if there is a CIN which a is 'at' which is before any CIN b is 'at'. This theorem requires two axioms. The first, $A_2(15)$, defines beforeness or $\leq$.

$A_2(15)$ $(x)(y)(EV\ x \ \&\ EV\ y) \supset (x \leq y \equiv (\exists z)\ SIM\ xz \ \&\ z < y)$.

$A_2(15)$ suffices to show that if $a \leq b$, then there is an instant at which a is a member which is before any instant at which b is a member. To demonstrate the converse, and hence to complete the equivalence, $A_2(16)$ is needed.

$A_2(16)$ $(w)(x)(y)(z)\ (EV\ w \ \&\ EV\ x \ \&\ EV\ y \ \&\ EV\ z) \supset ((w < x \ \&\ SIM\ wy \ \&\ ICON\ xz) \supset y \leq z)$

Theorem 8 $\vdash_{A_2} r(A_1(6))$

Proof: $r(A_1(6))$ = '$(x)(y)(EV\ x \ \&\ EV\ y) \supset (x \leq y \equiv (\exists z)\ CIN\ z \ \&\ x \in z \ \&\ (w)(CIN\ w \ \&\ y \in w) \supset z < w)$'

Assume $a \leq b$ where EV a and EV b. By $A_2(15)$, $(\exists z)\ SIM\ az \ \&\ z < b$. Let $z = c$; then SIM ac & $c < b$. By $A_2(14)$ there is some A such that A is a CIN and $a \in A$ & $c \in A$. Let B be any CIN where $b \in B$, then we have:-

$c \in A \ \&\ b \in B \ \&\ c < b$

from whence, $(\exists w)(\exists z)\ w \in A \ \&\ z \in B \ \&\ w < z$, which entails $A < B$ by $A_2(6)$. Thus there is some CIN of which a is a member which is before any CIN of which B is a member.

Assume $(\exists z)\ CIN\ z \ \&\ a \in z \ \&\ (w)\ (CIN\ w \ \&\ b \in w) \supset z < w$. By theorems 6 and 7 there is an ICON set B such that:-

$(x)\ x \in B \equiv ICON\ xb$.

and B is a CIN. There is some CIN A, where $a \in A$ and $A < B$. By $A_2(6)$ there is some c and d, where $c \in A$ & d B $c < d$. Hence by $A_2(1)$.

$c < d$ & SIM ca & ICON db

Therefore by $A_2(16)$, $a \leq b$.

Theorem 9 establishes that CINs constitute a compact series. Russell remarks on the assumption necessary to derive theorem 9.

'Finally, the series of instants will be compact if, given any two events of which one wholly precedes the other, there are events wholly after the one and simultaneous with something wholly before the other. Whether this is the case or not, is an empirical question; but if it is not, there is no reason to expect the time-series to be compact.'

Russell [123] (96)

$A_2(17)$ $(x)(y)(EV\ x\ \&\ EV\ y\ \&\ x < y) \supset (\exists z)(x < z\ \&\ (\exists w)\ SIM\ zw\ \&\ w < y)$

$A_2(17)$ is used to demonstrate theorem 9.

<u>Theorem 9</u> $\vdash_{A_2} r(A_1(7))$

Proof: $r(A_1(7))$ = '$(x)(y)(CIN\ x\ \&\ CIN\ y\ \&\ x < y) \supset (\exists z)\ CIN\ z\ \&\ x < z\ \&\ z < y$'
Let A and B be CINs where A < B. By $A_2(6)$, there is some a and b where:-

$a \varepsilon A\ \&\ b \varepsilon B\ \&\ a < b$

By $A_2(17)$ there is some c and d where

$a < c\ \&\ SIM\ cd\ \&\ d < b$

Since SIM cd, by $A_2(14)$, there is a CIN C of which $c \varepsilon C$ and $d \varepsilon C$. Since a < c by $A_2(6)$, A < C and since d < b, by $A_2(6)$, C < B.

Theorem 9 concludes the theorems. The axiom set A_2 which recursively reduces instants of time is not uniquely privileged in that respect. Indeed, Russell suggests another logical construction which can serve as the basis for a recursive reduction of instants of time, (space prevents <u>the</u> investigation of this parallel axiom set). The ability of a theory to have its ontology reorganised into different axiom sets is, of course, simply more evidence of ontological elasticity at large.

[1] See Davidson [38] (107 - 108). Davidson admits he has no 'knock down' argument to show FAC would be violated by this proposal. A knock-down argument would prove that adverbial modifiers could be extended to any finite length.

[2] Clark does not mention Davidson's criterion for event identity which appeared in Davidson [39]. Since Clark's article appeared less than a year after Davidson's article on event identity, it is likely that Clark had not read Davidson [39] when Clark was writing. Tiles [144], writing six years after Davidson [39], does offer independent criticism of Davidson's criterion of event identity.

[3] Put formally, the definition of a declarative sentence is:-

(1) If S is a true sentence or S is a false sentence then S is a declarative sentence.

(2) If B is a set of declarative sentences and S_1 is a declarative sentence then if S_1 is deducible from B$\cup\{S\}$ but not from B then S is a declarative sentence.

(3) Nothing else is a declarative sentence.

[4] 'All' statements can, in general be seen as covert type-type identity claims of a kind. Thus 'All men are mortal' is equivalent to 'Every thing that is a man is identical to a thing which is mortal'. In first order logic this is given by the theorem; $(x)(Fx \supset Gx) \equiv (x)(Fx \supset (\exists y)\ Gy \;\&\; x = y)$.

[5] See Quine [ ] (53, 54)

[6] The proof runs as follows. Let μ be any conforming set:-

$$[((P \& Q) \supset R) \& -(R v -R)] \supset [-(P v -P) v -(Q v -Q)] \in \mu$$

iff

$$((P \& Q) \supset R \quad \& -(R v -R)) \notin \mu) \text{ or } (-(P v -P) v -(Q v - Q)) \in \mu$$

iff

$$(P \& Q) \supset R \notin \mu \text{ or } -(R v -R) \notin \mu \text{ or } -(P v -P) \in \mu \text{ or } -(Q v -Q) \in \mu$$

iff

$$(P \& Q \in \mu \text{ and } R \notin \mu) \text{ or } (-R \notin \mu \text{ or } --R \notin \mu)$$
$$\text{or } (-P \in \mu \text{ and } --P \in \mu) \text{ or } (-Q \in \mu \text{ and } --Q \in \mu)$$

iff

$$(P \in \mu \text{ and } Q \in \mu \text{ and } R \notin \mu) \text{ or } -R \notin \mu \text{ or } R \notin \mu$$
$$\text{or } (-P \in \mu \text{ and } P \in \mu) \text{ or } (-Q \in \mu \text{ and } Q \in \mu)$$

Since conforming sets are consistent, then both ($-P \in \mu$ and $P \in \mu$) and ($-Q \in \mu$ and $Q \in \mu$) are ruled out: therefore the final formula above is equivalent to the following:-

$$(P \in \mu \text{ and } Q \in \mu \text{ and } R \notin \mu) \text{ or } -R \notin \mu \text{ or } R \in \mu$$

This formula is true. Assume $R \in \mu$, then since μ is consistent, then $-R \notin \mu$; in which case the formula is true. Assume $R \notin \mu$, then the formula is again true.

CHAPTER SEVEN

Logics and Ontology

7.1 Preliminary Remarks

Section 6.2 closed on an intransigent problem in contemporary philosophical logic: that of determining the proper choice (if any) of a notation for the formalisation of natural languages. The choice is by no means a purely cosmetic one, since, as became clear, important ontological issues concerning the existence of events, instants of time, possible worlds etc., were predicated on the choice of an appropriate notation. Thus the choice of a logic or canonical notation can play as great a part in the elimination or selection of an ontological hypothesis as the current state of empirical science. The philosophical problem posed by extended logics is that, whereas there is a definite rationale in the selection of competing scientific theories, there is none in the selection of competing logics. Yet the results of each kind of choice can be equally important for ontology.

If inroads are to be made on the problem of choosing logics, it has to begin with a very precise idea about what a logic is and how it is applied. For this reason, this chapter begins with the elucidation of logics as formal structures, open to logical or mathematical investigation as are other formal structures like Abelian groups or fields. The second part of this chapter is devoted to the investigation of the applications of these same formal structures or logics. Again the treatment of this topic is formal in that the application of a logic is taken as a formal structure incorporating the formal structure which is the logic of which it is the application. If the reader comes to find the formal exposition here unduly wearing, then he is referred to Haack [60] to whom is owed a debt of philosophical inspiration in writing this chapter.

7.2 What a Logic is

A logic is an ordered pair $\langle F,S \rangle$ where F is a formal system and S is a formal semantics for F.

A formal system F is an ordered quadruple $\langle A,W,P,R \rangle$. A is a non-empty set called the alphabet of F, and whose elements are called signs. W is a non-empty subset of the set M(A) of all finite sequences of elements of A. P is a subset of W and R is a set of finitary relations over W (a finitary relation over W is some subset of W^n where $n \geq 1$ is some positive integer). Elements of M(A) are strings of F; elements of W are called well-formed formulae, abbreviated as wffs, or just formulae of F; elements of P are called axioms of F; and elements of R are called primitive rules of inference or just rules of inference of F.

The above definition is the standard one of a formal system.[1] It is also useful to partition the alphabet of F into a set of logical variables and a set of logical constants, and to require that each wff of F contain at least one logical variable. I shall make this requirement.

Given a formal system F and a set of wffs of F, we say y is immediately inferred from the set Δ of wffs just when there exists a primitive rule of inference R^n of degree n and a finite sequence $b_1,\ldots, b_{n-1}$ of elements of such that $R^n(b_1,\ldots b_{n-1},y)$ holds or y is an axiom (thus an axiom is derivable from any set of wffs). We say y is deducible from the hypotheses Δ if there exists a finite sequence $c_1,\ldots,c_n$ such that $y = c_n$ and such that every member of the sequence is either (i) an element of Δ or (ii) immediately inferred from a set of prior members of the sequence.

The finite sequence $c_1,\ldots,c_n$ itself is called a formal proof, formal deduction or derivation from the hypotheses . If Δ is empty then the sequence $c_1,\ldots,c_n$

is called simply a formal deduction, formal derivation or formal proof in F. In this case y is said to be a provable wff or theorem of F. We write ' $\Delta \vdash_f y$' to show y is deducible from the hypotheses Δ, and '$\vdash_F y$' to show y is a theorem of F.

Example of a Formal System F

F = <A,W,P,R>

A is the alphabet of F and A = V ∪ C, where V is the set of logical variables of F and C is the set of logical constants of F. V and C are defined as follows:-

'a' ε V

If x ε V, so is x'.

Nothing else is a member of V

C = 'b'

W is the set of wffs of F defined as follows:-

If x ε V and y ε V, then $x^\frown{}'b'^\frown y$ ε W

Nothing else is a member of W.

P is the set of axioms of F defined as follows:-

If x ε V, then $x^\frown{}'b'^\frown x$ ε P

Nothing else is a member of P.

R is the set $\{r_1,r_2\}$ of primitive rules of inference of F defined for any wffs w_1,w_2,w_3 of F as follows:-

$r_1(w_1,w_2)$ iff where x ε V and y ε V and given $w_1 = x^\frown{}'b'^\frown y$; $w_2 = y^\frown{}'b'^\frown x$

r_2 (w_1, w_2, w_3) iff where $x \in V$, $y \in V$ and $z \in V$, given $w_1 = x ^\frown$ 'b' $^\frown y$ and $w_2 = y ^\frown$ 'b' $^\frown z$; then $w_3 = x ^\frown$ 'b' $^\frown z$.

Given the above definition of F, elementary formal proofs of F include the following.

a' b a" $\vdash_F$ a" b a'

1.	a' b a"	Hypothesis
2.	a" b a'	By r_1

a' b a", a" b a''' $\vdash_F$ a''' b a'

1.	a' b a"	Hypothesis
2.	a" b a'''	Hypothesis
3.	a' b a'''	By r_2
4.	a''' b a'	By r_1

$\vdash_F$ a b a

1.	a b a	Axiom

We now turn to the definition of a formal semantics of a formal system. A <u>formal semantics</u> for a formal system F is an ordered triple S = <T,I,v>. T is a set of <u>truth-values</u> and $T \geq 2$. At least one element of T shall be designated the <u>model truth-value</u> or the truth-value <u>true</u>. I is a non-empty set whose elements are <u>interpretations</u> and v is the <u>valuation function in S</u> which takes ordered pairs of the form <w,j>, where w is a wff of F and j an interpretation and gives an element of T as a value. An interpretation j is said to <u>satisfy</u> or be a <u>model</u> of a wff w just when v<w, j> = t, where t is a model truth-value. An interpretation j satisfies or is a model of a set Δ of wffs if, and only if j is a model of every element in Δ. Δ logically implies w if and only if every interpretation that is a model of Δ is also a model of w. Wffs w_1 and w_2 are logically equivalent if, and only if there is no model of one that is not a model of the other. A wff is valid or logically valid if and only if every interpretation is a model of it. If Δ logically implies w, we write '$\Delta \models_S w$' and if w is valid, we write '$\models w$'. The logic L = <F,S> is sound if $\vdash_F w$ implies $\models_S w$, and complete if $\models_S w$ implies $\vdash_F w$. S is said to characterise F just when L is sound and complete.

Example of a Formal Semantics S

F is as given in the previous example.

S = <T,I,v>

T is the set of all interpretations. I is the set of all functions whose arguments are just the logical variables of F.

v is the valuation function. v is defined as follows:

Where x 'b' y is any wff of F (x and y being logical variables of F) and $j \in I$:-

$v \langle x^\frown 'b'^\frown y, j\rangle = 1$ iff $j(x) = j(y)$

$v \langle x^\frown 'b'^\frown y, j\rangle = 0$ iff $j(x) = j(y)$

The soundness of the logic L = <F,S> under the above semantics follows from the equivalence of identity (namely $(x)\ x = x$; $(x)(y)\ x = y \supset y = x$; $(x)(y)(z)\ (x = y \ \&\ y = z) \supset x = z$). To prove completeness it is required to prove $\models_S w \supset \vdash_F w$ for all wffs w; or equivalently, $-\vdash_F w \supset -\models_S w$. Assume $-\vdash_F w$; then since all theorems of F are of the form $x^\frown$'b'$^\frown x$, then for some logical variables x and y, $w = x^\frown$'b'$^\frown y$ and $x \neq y$. Let j be any interpretation where j(x) = j(y); then v <w,j> = 0 and hence - $\models_S$w. The logic L = <F,S> is somewhat simpler, and less useful, than the logics recognised as the classical propositional and predicate calculi. The difference serves to remind us that the word 'logics' serves to define a great many formal structures of greater or lesser application.

It may be said that the formal definition of a logic given fails to distinguish between a formalised theory and a logic. An immediate reply is that are a number of pertinent differences between the two. A formalised theory consists of statements which make significant claims and are (at lease in some cases) true or false. A set of formulae of a logic is not a theory, but a set of meaningless strings. We may, of course, wish to stipulate that, for the purposes of a particular occasion, a given variable shall be given to have meaning: such variables are revealingly referred to as dummy variables. But dummy variables play a part in the use of a logic rather than in the logic itself. 'Every statement or its denial is true' makes an assertion: 'pv-p' does not. Logics make no assertions.

It may be charged that the definition of 'logic' supplied fails to discriminate set theory from logic. Again a fair reply is that set theory consists of assertions about sets whereas a logic consists of no assertions at all.

Set theory could be reconstructed so that it contained no assertions. The axioms of ZF would become uninterpreted first-order formulae. The trouble then becomes finding a semantics for characterising ZF; first-order semantics will not do since the axioms of ZF are not valid in first-order logic. Even if 'ε' is treated as a logical constant, the results are still unsatisfactory. For let A and B be any two distinct sets, and let C be any domain in which the elements belonging to A and B are one and the same. Now assign C as the range of the variable 'z' in the formula '$A = B \equiv (z)(z \in A \equiv z \in B)$', and the Axiom of Extensionality comes out as false. Perhaps more complex contrivances are possible, but it is simpler and more natural to demarcate set theory from logic.

7.3 Applications of Logics: Readings

So far we have treated logics as purely formal structures open to investigation in the same way that other mathematical theories are in abstract algebra. It is of the greatest importance to distinguish those purely formal investigations of a logic as a mathematical structure from those investigations and questions which arise when a logic is used as a guide for reasoning.

The situation that has obtained in contemporary logics is similar, in many ways to the situation that obtains with Euclidean and non-Euclidean geometries. Mathematicians have come to separate pure geometry as a mathematical discipline from applied geometry as it is used in physics. Within mathematics there is no sense to the question as to which geometry is correct. All that is important from the mathematical point of view is that Euclidean geometry and non-Euclidean geometries are all self-consistent: all can be practised without allotting any meaning to the primitive expressions used. Questions of correctness only arise when the primitive expressions of each geometry are given some interpretation. For instance, if 'straight line' is interpreted to mean 'path described by a beam of light', then Riemannian geometry becomes a true description of the behaviour of beams of light in gravitational fields. Hence physicists talk of Riemannian geometry as being 'correct' (and of space as being Riemannian) only under the presumption that the expressions of Riemannian geometry are given the appropriate physicalistic interpretations: any attempt to judge the correctness of Riemannian geometry *in vacuo* is misguided. Consequently modern mathematics has long since outgrown the ancient definition of its status as the science of space and quantity.

Modern logic has outgrown its ancient definition as the science of reasoning. The development of multitudinous systems of logic in place of Aristotelean syllogistic logic, and the divorce of logic, as formal discipline akin to mathematics, from philosophy, makes it apppropriate to develop the same attitude to logics as to mathematics. This attitude makes a clear separation between logics and their applications. Unlike mathematics, however, this distinction is not so universally rooted, or taken for granted, in the minds of those involved in logic, as it is in mathematics. Mathematical logicians understand well how an uninterpreted 'theory' gains an interpretation, and in model theory the nature of and relations between uninterpreted theories and their models are disseminated in depth. This area requires no development here. But the way in which a logic acquires an 'interpretation' and thereby ceases to be a more formal structure on which to perform symbolic gymnastics is nowhere near as well understood. Until a clear view of how a logic acquires an interpretation is gained, discussions of logics must necessarily lack the desired precision. A consequence of this lack of precision will be a loss of philosophical penetration about the relation of logics to natural language reasoning.

As an entry into the area of applications of logics, Heyting's Intuitionist Logic furnishes a useful illustration. As a formal system, Heyting's logic consists of 11 axioms and a rule of modus ponens. The axioms are as follows; where A B, C are any wffs of Heyting's calculus:-

1. $A \supset (A \& A)$
2. $(A \& B) \supset (B \& A)$
3. $(A \supset B) \supset ((A \& C) \supset (B \& C))$

4. $((A \supset B) \& (B \supset C)) \supset (A \supset C)$

5. $B \supset (A \supset B)$

6. $(A \& (A \supset B)) \supset B$

7. $A \supset (A \vee B)$

8. $(A \vee B) \supset (B \vee A)$

9. $((A \supset C) \& (B \supset C)) \supset ((A \vee B) \supset C)$

10. $-A \supset (A \supset B)$

11. $((A \supset B) \& (A \supset -B)) \supset -A$

MP. From A and $A \supset B$, derive B.

Heyting's calculus was devised to incorporate just those canons of reasoning which intuitionists believed were validly employed in mathematics. For philosophical reasons to do with their views on infinite domains, intuitionists rejected the Law of the Excluded Middle and consequently 'pv-p' is not a theorem of Heyting's calculus (nor is '$--A \supset A$).

Heyting's calculus is a single identifiable formal structure created with a view to its application to mathematical reasoning. But this is not the only application which can be found for the calculus. Under another application, Heyting's calculus could be treated as a logic of <u>verification</u> and <u>falsification</u> as follows:-

A .... It is verified that A

-A .... It is falsified that A

A v B .... It is verified that A or it is verified that B.

A & B .... It is verified that A and it is verified that B.

$A \supset B$ .... If it is verified that A then it is verified that B.

The lack of 'A v - A' as a theorem, which seemed so radical previously, now seems eminently in accord with commonsense: it is not true to say, if any statement A, that A is either verified or falsified, since patently there are many statements (e.g. Goldbach's Conjecture) which are neither. Similarly the lack of '$- - A \supset A$' as a theorem is right too, since it does not follow from it being falsified that A is falsified, that A is therefore verified. The fact that Heyting devised his logic in response to the case for intuitionism in mathematics is, so as to speak, accidental to the logic he evolved. Heyting's original application is just one application amongst many. To revert to the geometrical analogy used earlier; Euclidean geometry was originally evolved as a deductive set of assertions dealing with what the ancients reasonably supposed were the fundamental properties of space. But in its modern form, it is best viewed as an uninterpreted theory with a genealogy deriving from, and an application to, the nature of space.

There are two aspects to an application of a logic; or as a mathematician would say, an application A is an ordered pair $\langle R,D \rangle$ where R is a reading of a logic L and D is a depraved semantics for R.

A reading R is a quintuple $\langle L,t,S,f,E \rangle$.

L is the logic of which the rest of R is a reading. The formal structure of a logic has already been defined in the previous section, so no comment is required here.

t is a bijective computable function called the transcription function. The domain of t is the set of W of wffs of L and the range of t is the set of S of sentence-frames of R. Typically a sentence-frame results from a wff W by the replacement of the logical constants in W by some fragment of a natural language which we shall call a depraved constant or natural language constant.

Example of a Transcription Function t

Let L be Heyting's calculus. Let R be the reading that interprets Heyting's calculus as a logic of verification and falsification. t is defined thus.

For any atomic wff A

$t(A) = $ 'It is verified that' $\frown$ A

For any wffs B, C

$t(-B) = $ 'It is falsified that' $\frown t(B)$

$t(B \supset C) = $ 'If' $\frown t(B)$ - 'then' $\frown t(C)$

$t(B \vee C) = t(B) \frown$ 'or' $\frown t(C)$

$t(B \,\&\, C) = t(B) \frown$ 'and' $\frown t(C)$

The depraved constants are 'It is verified that', 'It is falsified that', 'If' 'then', 'or' and 'and'.

f is a substitution function that assigns to each logical variable v of L a non-empty substitution set of possible substitutions for v. For Heyting's calculus treated as a logic of verification and falsification: f would assign to the logical variables (sentence letters) of the calculus, in each case, the set of all declarative sentences of some natural language (e.g. English).

Finally E is the set of recognised sentences of R. A recognised sentence of R arises by the uniform substitution of each logical variable v in a sentence-frame s, of an element of the set f(v). Every element of E shall be a sentence and if every element of E is a declarative sentence ~~for~~ then R is a declarative reading. *(Not all logics are designed for declarative readings e.g. erotetic and* imperative logics).

A sentence-frame s is depravedly deducible from a set Δ of sentence frames, in a reading R <L,t,s,f,E> if, and only if $\Delta_{t-1} \vdash_L t^{-1}(s)$ where Δ_{t-1} is defined as follows:-

$$(x)(x \varepsilon \Delta_{t}\text{-}1 \equiv (\exists y)\ y \varepsilon \Delta \ \&\ t^{-1}(y) = x)$$

A recognised sentence s* is depravedly deducible from a set Δ* of recognised sentences if, and only if there is some set Δ of sentence-frames, and some sentence-frame s, such that s is depravedly deducible from Δ; and s* and Δ* result from Δ and s by some uniform substitution of the logical variables in s and in the elements of Δ by possible substitutions.

When a sentence-frame s is depravedly deducible from a set Δ of sentence frames in R we write '$\Delta \vdash_R$ s'. If s is such that $\vdash_L t^{-1}(s)$ then s is a depraved theorem-frame of R and we write '$\vdash_R$s'. A depraved theorem of R is any substitution-instance of a depraved theorem-frame.

An argument-frame is a pair <Δ,s> where Δ is a non-empty set of sentence-frames and s is a sentence-frame. <Δ,s> is a recognised argument-frame in R if and only if $\Delta \vdash_R$ s. An argument in R is a substitution-instance of an argument frame and a recognised argument is the substitution instance of a recognised argument frame.

Declarative readings are far more important than non-declarative readings; so in what follows, attention is given solely to declarative readings.

What is it that is to be required of a declarative reading? The most important requirement of a declarative reading is that traditionally made of logic: that the patterns of inference laid down be reliable, in the sense that they never lead from a collection of truths to a falsehood. Call an argument <Δ*,s*> materially correct when it is not the case that:-

(1) (x)(x ε Δ* ⊃ x is true).

(2) s* is false.

To demand that a reading R be fully reliable is to demand that every recognised argument in R is materially correct. Material correctness is a weak property for an argument to have, since, it is satisfied by all and only arguments whose premises materially imply their conclusions. However, we might expect that any useful logic which was fully reliable in our sense would also legislate for only valid arguments; i.e. arguments whose premises strictly imply their conclusions. A weak property, distributed over a very large number of cases, puts strong constraints on things as a whole.

Our definition of reliability in respect of readings can be defined equivalently thus:-

(S) If R is reliable then for any recognised argument frame $\langle \Delta, s \rangle$ of R, every substitution-instance of $\langle \Delta, s \rangle$ should be materially correct.

(S) suggests a natural complement.

(C) It is desirable that, if an argument-frame $\langle \Delta, s \rangle$ is such that every substitution-instance of $\langle \Delta, s \rangle$ is true, that $\langle \Delta, s \rangle$ be recognised by R.

In an unfarfetched way, (S) and (C) are suggestive of the formal soundness and completeness conditions of logic. The analogies will be explored in the next section.

7.4 Applications of Logics: Depraved Semantics

Logicians go to much trouble to provide formal systems with formal semantics, even though only professionals seem interested in the results. The secondary interest that formal semantics poses for those who use logic can be seen in the history of modern logic. Frege provided the modern apparatus of quantification in the *Begriffschrift* in 1879. Russell and Whitehead used and adopted form of Frege's 'conceptual notation' in *Principia Mathematica* circa 1910. It was only in 1930, over half a century after Frege's innovation, that Godel offered the first completeness proof of first-order logic. The ability of logic to expand and establish itself, was not notably affected by the retarded development of formal semantics: a fact which must inevitably cast doubt on its relevance.

Even today, with another half century or so separating us from Gödel's work, the case for the philosophical relevance of formal semantics is still not fully established. Formal semantics, has not, for instance, helped significantly in the task of selecting the right (if any) modal logic. It is easy, given the formal ability, to show a modal logic is 'right' by concocting a formal semantics which characterises it. But no philosophical skeptic of modal logic is convinced by this sort of chicanery, since he knows that the correctness of a given formal semantics has to be itself justified by philosophical argument as much as the system it characterises.

A logician, if asked to justify the existence of formal semantics, will most probably reply that without formal semantics, formal soundness and completeness proofs are not possible. But what of it? Why are formal soundness and completeness proofs important anyhow? If pressed on this point, a logician is apt to reply that a soundness proof proves that everything that <u>is</u> a theorem, <u>ought</u> to be a theorem, and a completeness proof proves that everything that ought to be a theorem <u>is</u> one; so proving the identity of 'is' and 'ought' of a logic. But what sense can be made of the prescriptive 'ought' in this context? The expression 'ought to be a theorem' is not a standard expression in logic textbooks and it has no given analysis; yet it seems indispensable in the philosophical justification of the logicians' work. Applied to a formal structure like a logic it seems to have no purchase. 'Ought' makes no sense as applied to logics in isolation, any more than such a teleological concept would as applied to atoms, cosmic rays, electricity or rocks. What for instance would we make of a pure mathematician who, having <u>proved</u> that a group was non-Abelian then said that is <u>ought</u> to be Abelian?

<u>'Ought' in this context assumes its proper perspective when a logic is put to use or given an application.</u> Only in regard to what has purpose can we find fault or praise, and if we wish to justify the significance of soundness or completeness proofs in pure logic, we have to see it in the application for

which that logic is designed. A useful place to begin is with a depraved interpretation.

A depraved interpretation $\mathfrak{G}$ of a reading R = $\langle L,t,s,f,E \rangle$ is a function that assigns to each variable v of L an element of the substitution-set f(v). $\mathfrak{G}$ is said to be a depraved model of a sentence-frame s just when the result of replacing each variable v in s by $\mathfrak{G}$(v) is a true sentence. We write '$\mathfrak{G} \vDash_A$ s' to show that in application A, $\mathfrak{G}$ is a depraved model of s. $\mathfrak{G}$ is a depraved model of a set Δ of sentence-frames if $\mathfrak{G}$ is a depraved model of every element of Δ. A set Δ of sentence-frames depravedly imples a sentence-frame s just when every depraved model of Δ is a depraved model of s; or '$\Delta \vDash_A$ s'. A sentence-frame s is depravedly valid in A if every depraved interpretation $\mathfrak{G}$ is a depraved model of s or '$\vDash_A$ s'.

Having to hand the concepts of a depraved theorem-frame and depraved validity, it is natural to go on to define depraved soundness and depraved completeness. An application A is depravedly sound, where A = $\langle R,D \rangle$, if where any sentence-frame s is a depraved theorem in R, s is depravedly valid in A. A is depravedly complete if, where s is any depravedly valid sentence-frame, s is a depraved theorem of R.

i.e. A is depravedly sound iff $\vdash_A s \supset \vDash_A s$

A is depravedly complete iff $\vDash_A s \supset \vdash_A s$

where A = $\langle R,D \rangle$ and s is any sentence-frame in R.

Depraved soundness and depraved completeness subsume, to a great extent, the philosophical purpose of conditions (S) and (C) of the previous section. It is not hard to see that depraved soundness (especially) and depraved completeness are two very desirable properties of any application of a logic. Therein lies the only possible point of any formal soundness and completeness proof. The only philosophical point to formal soundness and completeness proofs lie in their contribution to proofs of depraved soundness and completeness.

Having made this statement we are still not clear of the woods, for it is not clear what exactly the relations between formal soundness and completeness proofs and their depraved counterparts are. A formal soundness/completeness proof is not a depraved soundness/completeness proof. What has to be added to a proof of formal soundness/completeness before it becomes a proof of depraved soundness/completeness? The answer is: a depraved semantics.

A depraved semantics is essentially a means of establishing a connection between a formal interpretation and a depraved interpretation. More formally a depraved semantics $\mathcal{D}$ is an ordered triple $\langle I,J,h \rangle$ where I is the set of formal interpretations of a logic L, in an application A, J is the set of depraved interpretations in A, and h is a function from J into I. The role of h is to demonstrate that the elements of I simulate, in all important respects, the elements of J; for this reason I call h the simulation function in A.

Let $A = \langle R, \mathcal{D} \rangle$ be any application where $R = \langle L,t,S,f,E \rangle$ and $\mathcal{D} = \langle I,J,h \rangle$. Let ϕ be any depraved interpretation (member of J) and s any sentence-frame (member of S). It is provable that:-

If L is sound and $h(\phi) \models_L t^{-1}(s)$ implies $\phi \models_A s$; then A is depravedly sound.

If L is complete, h is onto, and $\phi \models_A s$ implies $h(\phi) \vdash_L t^{-1}(s)$; then A is depravedly complete.

These two theorems establish a vital connection between the formal and applied sides of logic and are worthy of proof.

Theorem 1 If L is sound and $h(\phi) \models_L t^{-1}(s)$ implies $\phi \models_A s$; then A is depravedly sound.

Proof: Assume L is sound and $h(\mathfrak{G}) \models_L t^{-1}(s)$ implies $\mathfrak{G} \vDash_A s$. Assume $\vdash_A s$; then $\vdash_L t^{-1}(s)$ and, since L is sound, $\models_L t^{-1}(s)$. Let $\mathfrak{G}$ be any depraved interpretation, $h(\mathfrak{G}) \models_L t^{-1}(s)$ implies $\mathfrak{G} \vDash_A s$ and since $\models_L t^{-1}(s)$, then $h(G) \models_L t^{-1}(s)$ and so $\mathfrak{G} \vDash_A s$ for any $\mathfrak{G}$. Therefore $\vDash_A s$.

Theorem 2 If L is complete, h is onto, and $\mathfrak{G} \vDash_A s$ implies $h(\mathfrak{G}) \models_L t^{-1}(s)$; then A is depravedly complete.

Proof: Assume L is complete, h is onto, and $\mathfrak{G} \vDash_A s$ implies $h(\mathfrak{G}) \models_L t^{-1}(s)$. Assume $\vDash_A s$. Let $\mathfrak{J}$ be any formal interpretation. Since h is onto, then for some depraved interpretation $\mathfrak{G}$, $h(\mathfrak{G}) = \mathfrak{J}$. $\mathfrak{G} \vDash_A s$ implies $h(\mathfrak{G}) \models_L t^{-1}(s)$, and since $\vDash_A s$ then $\mathfrak{G} \vDash_A s$ and so $h(\mathfrak{G}) \models_L t^{-1}(s)$ i.e. $\mathfrak{J} \models_L t^{-1}(s)$ for all $\mathfrak{J}$. Therefore $\models_L t^{-1}(s)$. Since L is complete, $\vdash_L t^{-1}(s)$ and so $\vdash_A s$.

Where A satisfies the antecedent of theorem 1, h is said to simulate the depraved soundness of A. Where A satisfies the antecedent of theorem 2, h is said to simulate the depraved completeness of A. If both these conditions hold, then h is said to be a good simulator in A. Demonstrating that h is agood simulator and hence that an application is depravedly sound and complete will take us into the very heart of applied logic: the point where logic, philosophy, and ontology meet.

7.5 Monism, Pluralism, Instrumentalism

The previous section completes our analysis of the formal structure of logics and their applications. In this section, the emphasis will be less on the formal aspects of logics and more on the philosophical aspects. Nevertheless the clarity of perspective gained in our formal investigations will be invaluable in later stages.

Haack [60] (221), in her section on the metaphysical and epistemological aspects of logic, distinguishes between three metaphysical attitudes to logics.

Monism: there is just one correct system of logic.

Pluralism: there is more than one correct system of logic.

Instrumentalism: there is no 'correct' logic; the notion of correctness is inappropriate.

If a logic is identified as a formal structure in the manner of this chapter, then there is no doubt that the instrumentalist is right in saying that there is no 'correct' logic; since a logic is only a set of rules for manipulating meaningless signs. But Haack's trichotomy can be restored to life if we replace 'system of logic' and 'logic' by 'application' in the above. The problem then remains in seeing what 'correct' could mean in this context.

If 'correct' means 'depravedly sound and complete' then the pluralist is in the right. There is no question but that there are many applications of different logics which are depravedly sound and complete. In these terms the context is decided immediately. Let us see if there is a more interesting sense to 'correct'.

Another interpretation of Haack's 'correct', which is, perhaps, closer to her intentions (and brings the debate closer to our own interests) is that to say an application of a logic is 'correct' is to say that it is suitable for reasoning in general. The concept of suitability for general reasoning can be conveniently exchanged for the idea that this concept is manifested by an application just when that application is suitable to the task of formalising the language of science. Derivatively, a logic is correct when it has an application of this sort. In this roundabout manner, Haack's original distinctions begin to make sense.

Instrumentalism is one casualty of this line of definition. There are logics which, in virtue of their own structural limitations, are simply not rich enough to provide the basis for 'correct' applications. A case in point is the elementary logic of 7.2 which was designed specifically for reasoning about identity. This logic was far too simple a formal structure to use in formalisation. Just as there are mathematical structures which are too limited in detail for research into them to be interesting or for them to have useful physical applications, so there are logics which are too far removed from the syntactical structures of Indo-European languages for them to subsume the job of reasoning in those languages. These more primitive logics may, like the propositional calculus, find a use in regard to limited areas of thought; but they will never be serious candidates for formalising all precise thought.

This leaves monism and pluralism; and both positions have their attractions. The rest of this section will be concerned with the case for pluralism developed from the issues of 6.2.

The immediate difficulty about the Davidson - Clark dispute was that both their arguments were valid, but there were no obvious superordinate principles to check the truth of their assumptions. We can either follow Davidson in keeping our logic first-order (and include events) or follow Clark in making our logic more complicated (and exclude events): but how to choose!

The pluralist is ready to move into the vacuum created by those philosophical uncertainties. First, he roundly asserts that there are no principles by which contests of the sort between Davidson and Clark can be decided. The trouble with this kind of dispute, the pluralist continues, is that there are <u>two</u> disagreements being conducted simultaneously: (a) a disagreement about <u>logic</u>, (b) a disagreement about <u>ontology</u>. The resolution of either of these disagreements depends on how the other is resolved. The situation is analogous to that of a linear equation with two unknowns: there is no <u>unique</u> pair of solutions to be found; but any given solution will help determine the other. The pluralist insists that a choice of logic is conditional on a choice of ontology and science. Thus given an ontology containing events and recognising the truth of some adverbial sentence, it is rational to prefer first-order logic as the basis for formalisation. On the other hand, assuming there are no events, but still recognising the truth of some adverbial sentences, it is rational to prefer a predicate-modifier logic. But there is no reason to prefer one logic to another in vacuo.

The pluralist has the makings of a very strong case: but his reasoning would still be beside the point if the pluralist accepted that every kind K existed or it did not exist, and every declarative sentence was either true or false. Let us call the former position ontological objectivism. If ontological objectivism is true, then either events exist or they do not. Assume they do: then Davidson is right (if events exist, why not quantify over them?). Assume they do not: then Clark is right (if events do not exist, we need to use a logic which avoids them).

But the pluralist does not have to accept ontological objectivism. He can assert that not only do answers to ontological questions are hard to capture, but that answers do not exist to be discovered even in principle. The position that the pluralist is adopting towards logico-ontological questions is akin to that of the quantum physicist, who, when confronted with the impossibility of simultaneously determining the position and momentum of an electron, prefers to argue that determinate twin values for these variables just don't obtain, rather than that they do obtain but are experimentally undiscoverable.

There is a good deal of theoretical support for the pluralist's attack on ontological objectivism to be found in the preceding chapter: particularly in respect of ontological elasticity. One consequence of ontological elasticity is that there may be some radical indeterminacy as regards the objects counted to exist. It is possible for the universe to accommodate conflicting ontologies, each inconsistent with each other, but each internally self-consistent. Ontological elasticity demands that we interpret this rivalry seriously as revealing ontological gaps in the universe, where conflicts break out.

The pluralists position thus has far-reaching consequences that overflow the boundaries of the original dispute as to whether there is one 'correct' logic or many. The pluralist sees logical, ontological and even scientific decisions as all interrelated, each affecting the other in that, e.g., a choice of ontology, if rigidly adhered to, may force logical and scientific reevaluations. The particular complex of a choice of logic, ontology and science, may fail to be free from internal conflict. It may be impossible to accept first-order logic, an event-free ontology and the truth of certain adverbial sentences: but it is not determined which element has to be revised in order to bring harmony. The pluralist suggests that a better conception of ontology subsumes that subject under a wider conception: that of comparing world-pictures. A world picture is determined by a choice of logic, science and ontology. Fruitful and resolvable disagreement about any element of a world-picture between any two respondents who agree on two elements of a world-picture. External disagreement takes place between respondents who disagree on more than one element of a world-picture and hence have no common ground on which to argue. External disagreements are unsolvable pseudo-arguments. Buttressed and developed in this way, the pluralist cannot be easily put down. Nevertheless if the pluralist cannot be crushed, he may yet be evaded. It has to be seen what arguments can be put up by the monist to defend his own cause.

7.6 The Case for Monism

It has been taken as gospel so far, that extended logics do offer alternative means for formalisation, with concomitant ontological savings to compensate for their added symbolism. The pluralist built his case with this much taken for granted. But there are reasons why the monist might wish to deny the pluralist free use of this assumption.

Closer study of many extended logics reveals something both faintly surprising and disturbing about extended logics. Though extended logics are put forward as superior alternatives to classical (first-order) logic, examination of them reveals that their formal semantics are often expressed in a first-order fashion. A sort of strange implicit contradiction is to be found in these extended logics, for while their promotors often pretend that one of these logics should supercede classical logic; it is classical logic that they turn to in the course of giving a formal semantics to their logics.

Modal logic is the clearest example of an extended logic with this sort of background. Modal logics are generally explained by recourse to possible world semantics. A formula of the form $\Box p$ is said to be true if, and ony if p is true in all possible worlds and $\Diamond p$ is true if, and only if p is true in some possible world. Realists in regard to possible worlds and construe the reference to 'possible worlds' straightforwardly: possible worlds exist and everything that is possible occurs in at least one of them.

Realism in regard to possible worlds and their use to provide semantics for modal operators forces a recognition of the superfluity of modal operators except as a notational shorthand. Chellas [25] (13) in his book on modal propositional logics observes that the formulae of S5 can be effectively mapped into a first-order logic in which modal operators are eliminated in favour of first-order formulae and quantifiers ranging over possible worlds. Given a commitment to realist possible world semantics, it is ontologically more honest to register this commitment by employing first-order notation and quantifying over possible worlds, than by hiding this commitment behind modal operators which are explained, eventually, by employing the same first-order notation and making the same commitments.

The same criticisms apply to Clark's sponsorship of predicate modifier logic. Although Clark is wary of including events in his ontology, he is ontologically prodigal in equipping his predicate modifier logic with a semantics that includes entities called 'states of affairs' amongst its ontology. This same semantics appears recognisably first-order in appearance. Therefore, on

Clark's own showing, whatever good can be derived from predicate modifer logic can be derived from first-order logic employing quantification over states of affairs.

The monist has a stick to belabour the pluralist in all this. Extended logics, so the line goes, offer no ontological savings after all, only ontological camouflage. We can camouflage our commitment to possible worlds or states of affairs by extending first-order logic to include modal operators or predicate operators; but we regain those same commitments in providing these trendy logics with semantics. The ontological dilemmas which the pluralist sees as revolving on a choice of logics, are in the end sham. There is no real alternative to first-order logic on display.

There is a counter to this attack, as there often is to general philosophical arguments. Although it is possible to interpret the references to possible worlds, in possible world semantics, as references to the elements of a Leibnitzean ontology, it is not necessary to do so. The modal logician can insist that any structure that satisfies his description of the domain of possible worlds can be used to foot the ontological bill. Moreover as long as the modal logician retains his grip on first-order notation then he has a very powerful ally: the completeness theorem for first-order logic. This theorem assures us that any consistent first-order theory has a model. The modal logician can thus follow up his protestations of ontological innocence by bullishly insisting that since his frolic with symbols was at least a consistent frolic, then the completeness theorem underwrites all ontological debts.

This is an ingenious counter which goes a good distance to clearing possible world semantics of ontological doubt: but it does so at an extremely high price. What has been lost in throwing overboard all ontological inclinations towards possible entities, is also the very raison d'etre of formal semantics. We may have good reason to doubt the realist's ontology of possible worlds and their possible contents, but at least we can see how, if we accept his

Leibnitzean vision, where this all comes into reasoning involving modal logic. But if we reject the philosophical story that accompanies the symbol-pushing, what remains may be formally impeccable, but it is philosophically with purpose: rather like an engine robbed of its flywheel.

To see this clearly it is necessary to relate the picture built up of logics in the previous sections to a specific example. For our purposes Kripke's semantics for the logic S5 will do. Kripke [75] [76] is amongst these logicians who are most free in their use of the dialect of possible worlds. But unlike Lewis [83] or Plantinga [98], Kripke is careful to disown any ontological commitment to possible worlds. Whether Kripke's remarks about rigid designators can be made sense of without such an ontology remains an open question. But that question is not the relevant one here. The relevant question is this. Can Kripke's semantics for modal logic (e.g. S5) justify the employment of S5 as a basis for modal reasoning without presupposing the existence of possible worlds?

This question can be formally sharpened. Suppose we determine a reading R for S5 in which 'O' is mapped to the depraved constant 'It is necessary that ...' and the other constants their usual readings (we will be more precise later). Does Kripke's semantics for S5 show anything about whether R is depravedly sound and/or complete? If the answer is <u>no</u>, then Kripke's semantics is philosophically useless. If the answer is <u>yes</u>, then some bridge is required between Kripke's formal semantics and the informal reading given to the formulae of S 5. Such a bridge was found to be a <u>depraved semantics.</u> Therefore, even if an ontological commitment to possible worlds is not admitted in Kripke's semantics, it may still be that within the depraved semantics that gives the formal semantics purpose, a Leibnitzean ontology may lie concealed. Only by close examination of the formal structures involved, can it be seen how matters stand.

S5 is given by the axiom set A0 - A4.

A0 Truth-functional tautologies using '$\supset$' and/or '-'.

A1. $\Box A \supset A$

A2. $\Box(A \supset B) \supset (\Box A \supset \Box B)$

A3. $\Box A \supset \Box\Box A$

A4. $\Diamond A \supset \Box\Diamond A$

and the rules R1 and R2

R1. $\vdash A \supset \Box A$

R2. $A, A \supset B \rightarrow B$

In Kripke's semantics for S5 a model structure <G,K,R> is an ordered triple where $G \in K$ and R is an equivalence relation on K. (Informally G is supposed to represent the actual world; K the set of possible worlds; and R is the relation of relative possibility on K. We shall self-conciously try to forget this informal picture for a moment). A Kripke interpretation is a 2-place function Φ whose arguments are of the form <A,H> where A is a wff of S5 and $H \in K$ and whose range is the set $\{T,F\}$ where T is the model truth-value. For any Kripke interpretation ϕ, ϕ is defined for all atomic formulae and all possible worlds. In other cases ϕ is defined by induction.

$\phi(-A, H) = T$ iff $\phi(A,H) = F$

$\phi(A \supset B, H) = T$ iff $\phi(A,H) = F$ or $\phi(B,H) = T$

$\phi(\Diamond A, H) = T$ iff for some H' where RH'H, $\phi(A,H') = T$

$\phi(\Box A, H) = T$ iff for any H', if RH'H then $\phi(A,H') = T$

A formula A is valid if, and only if $\phi(A, G) = T$ for all Kripke interpretations ϕ. We write '$\models A$' to show A is valid. Kripke [77] has proved that $\vdash_{S5} A$ iff $\models A$. In the reading R = <S5, t, S, f, E>, t is defined thus:-

$t(A) = A$ for all atomic sentences A.

$t(A \supset B) = t(A)^\frown$'materially implies'$^\frown t(B)$

$t(-A) =$ 'It is not the case that'$^\frown t(A)$

$t(\Diamond A) =$ 'It is possible that'$^\frown t(A)$

$t(\Box A) =$ 'It is necessary that'$^\frown t(A)$

S is the set of resultant sentence frames and f is a function assigning to each sentence-letter in S5 the set of declarative English sentences. E is the resultant set of recognised sentences.

To prove the depraved soundness or the depraved completeness of R, a depraved semantics <I,J,h> is required, where h is a good simulator. For our modest purposes, to demonstrate soundness is sufficient. We shall look at <u>two</u> depraved semantics. The first is ontologically free of a commitment to possible worlds, but turns out to be <u>inadequate</u> to guarantee the depraved soundness of R. The second is ontologically committed to possible worlds and is sufficient to give a guarantee of depraved soundness.

<u>The First Depraved Semantics:</u> is an ordered triple <I,J,h> where I is the set all Kripke interpretations and J is the set of all depraved interpretations, where a depraved interpretation $\mathfrak{G}$ is a function that assigns to each sentence letter A of S5, a declarative English sentence (member of f(A)). h is an <u>into</u> function from J into I.

This completes our description of the minimal apparatus of our first depraved semantics. In order to attempt a depraved soundness proof, it is required to introduce some auxiliary assumptions. The first is as follows.

<u>Assumption One:</u> that there is some function i from J into I, such that where $\mathfrak{G} \in J$ and A is any atomic wff of S5, $G \models A$ iff $i(G) \models A$.

Assumption one says that for any assignment of declarative sentences to the sentence letters of S5, given some sentence-letters will be represented as truths, there will be a Kripke interpretation that assigns the value T to just those sentence letters in question. Let us define h to be a function with the properties of i.

Two more assumptions are needed.

<u>Assumption Two</u>: that for any $\mathcal{G} \varepsilon$ J, and for any wff A of S5, $\mathcal{G} \underline{\in}$'It is not the case that'$^\frown$t(A) iff- $\mathcal{G} \underline{\in}$ t(A).

Assumption two says that the result of placing a declarative sentence after 'It is not the case that', is true if, and only if the declarative sentence itself is not true.

<u>Assumption Three:</u> that for any $\mathcal{G} \varepsilon$ J, and for any wffs B and C of S5, $\mathcal{G} \underline{\in}$ t(B)$^\frown$'materially implies that'$^\frown$t(C) iff - $\mathcal{G} \underline{\in}$ t(B) or $\mathcal{G} \underline{\in}$ t(C)

Assumption three states that any sentence materially implies another just when the first is not true or the second is.

To try to prove depraved soundness we proceed by induction on the number n of natural language constants in any sentence frame t(A), to prove $\mathcal{G} \underline{\in}$ t(A) iff h (G) $\models$ A for all $\mathcal{G} \varepsilon$ J.

n = 0: then A is an atomic sentence and so t(A) = A. By the definition of h, for any $\mathcal{G} \varepsilon$ J, $\mathcal{G} \underline{\in}$ A iff h(G) $\underline{\in}$ A and so $\mathcal{G} \underline{\in}$ t(A) iff h($\mathcal{G}$) $\models$ A.

Assume $\mathcal{G} \underline{\in}$ t(M) iff h($\mathcal{G}$) $\models$ M for any sentence frame M with any number j, j < n, natural language constants: then t(A) is either of the form (i) It is not the case that t(B) (ii) t(B) materially implies t(C) (iii) It is necessary that t(B) (iv) It is possible that t(B)

<u>Case 1</u> $t(A) =$ 'It is not the case that'$^\frown t(B)$

We have the following series of equivalences:-

$G \mathrel{\underline{\in}} t(A)$ iff $h(G) \models A$,

is equivalent to

$G \mathrel{\underline{\in}} t(A)$ iff $h(G) \langle A,G \rangle = T$,

is equivalent to,

$G \mathrel{\underline{\in}}$ 'It is not the case that'$^\frown t(B)$ iff $h(G) \langle -B,G \rangle = T$,

is equivalent to,

$G \mathrel{\underline{\in}}$ 'It is not the case that'$^\frown t(B)$ iff $h(G) \langle B,G \rangle = F$.

By assumption two, $G \mathrel{\underline{\in}}$ 'It is not the case that' $t(B)$ iff $- G \mathrel{\underline{\in}} t(B)$. Hence the formula, '$G \mathrel{\underline{\in}}$ 'It is not the case that' $t(B)$ iff $h(G) \langle B,G \rangle = F$' is equivalent to:-

$- G \mathrel{\underline{\in}} t(B)$ iff $h(G) \langle B,G \rangle = F$;

which is equivalent to

$G \mathrel{\underline{\in}} t(B)$ iff $h(G) \langle B,G \rangle = T$,

which is equivalent to

$G \mathrel{\underline{\in}} t(B)$ iff $h(G) \models B$;

Where B has n - 1 natural language constants. This last formula is true by the inductive hypothesis, and so $G \mathrel{\underline{\in}} t(A)$ iff $h(G) \models A$ is established for case 1.

<u>Case 2</u> $t(A) = t(B)^\frown$ 'materially implies that'$^\frown t(C)$

We have the following series of equivalences:-

$G \mathrel{\underline{\in}} t(A)$ iff $h(G) \models A$

is equivalent to

$G \mathrel{\underline{\in}} t(B)^\frown$ 'materially implies that'$^\frown t(C)$ iff $h(G) (B \supset C, G) = T$

is equivalent to

$₲ \in t(B) \frown$ 'materially implies that' $\frown t(C)$ iff $(h(₲)\langle B,G\rangle = F$ or $h(₲)(C,G) = T)$

By assumption three, the last formula is equivalent to:-

$(-₲ \in t(B)$ or $₲ \in t(C))$ iff $(h(₲) \langle B,G\rangle = F$ or $h (₲) \langle C,G\rangle = T)$

To prove the above formula it is sufficient to prove $- ₲ \in t(B)$ iff $h(₲)\langle B,G\rangle = F$ and $₲ \in t(C)$ iff $h(₲)\langle C,G\rangle = T$.

$-₲ \in t(B)$ iff $h(₲)\langle B,G\rangle = F$

is equivalent to

$₲ \in t(B)$ iff $h(₲)\langle B,G\rangle = T$

is equivalent to

$₲ \in t(B)$ iff $h(₲) \models B$

where B has n - 1 natural language constants. This last formula is true by the inductive hypothesis. Similarly C has n - 1 natural language constants and so

$₲ \in t(C)$ iff $h(G)\langle C,G\rangle = T$

is equivalent to

$₲ \in t(C)$ iff $h(G) \models C$

which is true. Consequently $₲ \in t(A)$ iff $h(G) \models A$ is established for case 2. And here the proof stops. Quite simply, the depraved semantics suggested is not ontologically or structurally rich enough to provide a depraved soundness proof. Cases 3 and 4 involving the modal constants cannot be established. Thus without invoking a depraved semantics richer than the one suggested here; it is only possible to validate that part of S5 which it shares in common with the classical propositional calculus. However, as we shall see, by invoking a Leibnitzean ontology of which Kripke's model structure is a representation, cases 3 and 4 can be completed.

The Second Depraved Semantics: is explicitly committed to a Leibnitzean ontology of possible worlds. There is a domain W of possible worlds in which $\alpha \in W$ and α is the actual world. There is an equivalence relation P on W of relative possibility where $P\langle\beta,\alpha\rangle$ iff β is possible relative to α. Kripke's model structure $\langle G,K,R\rangle$ is thought of as representative of the structure of W in the following sense. There is an isomorphism i from W onto K such that $i(\alpha) = G$ and $P(\beta,\alpha)$ iff $R(i(\beta),i(\alpha))$

To this ontological picture has to be added a number of ancillary assumptions. The first is that, given there are a number of possible worlds, a depraved interpretation can be a model of a sentence-frame in one world and not in another. To say $\mathcal{G}$ is a depraved model of t(A) is to say that in the actual world (i.e. α), $\mathcal{G}$ is a depraved model of t(A).

Assumption One: $\mathcal{G} \models t(A)$ iff $\mathcal{G} \models t(A)$ in α.

The next two assumptions relate to the depraved constants 'It is not the case that' and 'materially implies that'. These assumptions apply assumptions two and three of the previous depraved semantics to all possible worlds.

Assumption Two: for any possible world β, $\mathcal{G} \models$ 'It is not the case that'$^\frown$t(A) in β iff $-\mathcal{G} \models t(A)$ in β.

Assumption Three: for any possible world β, $\mathcal{G} \models$ t(B)$^\frown$'materially implies that'$^\frown$t(C) in β, iff $-\mathcal{G} \models t(B)$ in β or $\mathcal{G} \models t(C)$ in β.

Case 1: $t(A) =$ 'It is not the case that'$^\frown t(B)$.

$\mathfrak{G} \models t(A)$ in β iff $h(\mathfrak{G})\langle A, i(\beta)\rangle = T$

is equivalent to

$\mathfrak{G} \models$ 'It is not the case that'$^\frown t(B)$ in β iff $h(\mathfrak{G})\langle -B, i(\beta)\rangle = T$

is equivalent to

$\mathfrak{G} \models$ 'It is not the case that'$^\frown t(B)$ in β iff $h(\mathfrak{G})\langle B, i(\beta)\rangle = F$

By assumption two, this last formula is equivalent to:-

$-(\mathfrak{G}$ $t(B)$ in β iff $h(\mathfrak{G})\langle B, i(\beta)\rangle = F$

is equivalent to

$\mathfrak{G}$ $t(B)$ in β iff $h(\mathfrak{G})\langle B, i(\beta)\rangle = T$

where has $n - 1$ depraved constants. This last formula is true by the inductive hypothesis, so the hypothesis is established for case (i).

Case 2 $t(A) = t(B)^\frown$'materially implies that'$^\frown t(C)$

$\mathfrak{G} \models t(A)$ in β iff $h(\mathfrak{G})\langle A, i(\beta)\rangle = T$

is equivalent to

$\mathfrak{G} \models t(B)^\frown$'materially implies that'$^\frown t(C)$ in β iff $h(\mathfrak{G})\langle B \supset C, i(\beta)\rangle = T$

is equivalent to

$\mathfrak{G}$ $t(B)^\frown$'materially implies that'$^\frown t(C)$ in β iff $h(\mathfrak{G})\langle B, i(\beta)\rangle = F$ or $h(\mathfrak{G})\langle C, i(\beta)\rangle = T$

By assumption two this last formula is equivalent to:-

$(-(\mathfrak{G} \models t(B)$ in $\beta)$ or $\mathfrak{G} \models t(C)$ in $\beta)$ iff $h(\mathfrak{G})\langle B, i(\beta)\rangle = F$ or $h(\mathfrak{G})\langle C, i(\beta)\rangle = T$

To prove this formula it is sufficient to prove $-(\mathfrak{G} \models t(B)$ in $\beta)$ iff $h(\mathfrak{G})\langle B,i(\beta)\rangle = F$ and $\mathfrak{G} \models t(C)$ in β iff $h(\mathfrak{G})\langle C,i(\beta)\rangle = T$.

First $-(\mathfrak{G} \models t(B)$ in $\beta)$ iff $h(\mathfrak{G})\langle B,i(\beta)\rangle = F$ is equivalent to $\mathfrak{G} \models t(B)$ in β iff $\langle B,i(\beta)\rangle = T$ which is true by the inductive hypothesis since B has n - 1 depraved constants. $\mathfrak{G} \models t(C)$ in β iff $h(\mathfrak{G})\langle C,i(\beta)\rangle = T$ also holds by the inductive hypothesis, since C has n - 1 depraved constants.

Thus the hypothesis is established for case 2.

<u>Case 3</u> $\quad t(A) =$ 'It is possible that' $t(B)$

$t(A)$ in iff $h(\mathfrak{G})\langle A, i(\beta)\rangle = T$

is equivalent to

'It is possible that'$\frown t(B)$ in β iff $h(\mathfrak{G})\langle \Diamond B, i(\beta)\rangle = T$

is equivalent to

'It is possible that'$\frown t(B)$ in β iff for some H, $R(H, i(\beta))$ &

$h(\mathfrak{G})\langle B,H\rangle = T$

To prove the formula immediately above it is required to prove (i) If 'It is possible that'$\frown t(B)$ in β then for some H, $R(H,i(\beta))$ & $h(\mathfrak{G})\langle B,H\rangle = T$ and (ii) If for some H, $R(H,i(\beta))$ & $h(\mathfrak{G})\langle B,H\rangle = T$ then $\mathfrak{G} \models$'It is possible that'$\frown t(B)$ in β.

Assume for some H, $R(H,i(\beta))$ & $h(\mathfrak{G})\langle B,H\rangle = T$. Then by the inductive hypothesis applied to $t(B)$, $\mathfrak{G} \models t(B)$ in $i^{-1}(H)$ and by the definition of i, $P(i^{-1}(H), \beta)$.

Thus by assumption four, $\mathfrak{G} \models$'It is possible that '$\frown t(B)$ in β.

This proves the hypothesis for case 3.

<u>Case 4</u> $\quad t(A) =$ 'It is necessary that'$\frown t(B)$

$\mathfrak{G} \models t(A)$ in β iff $h(\mathfrak{G})\langle A,i(\beta)\rangle = T$

is equivalent to

'It is necessary that' t(B) in β iff $h(G)\langle OB, i(\beta)\rangle = T$

is equivalent to

'It is necessary that' t(B) in β iff for any H, if $R(H,i(\;))$ then

$h(G)\langle B,H\rangle = T$

To prove the formula immediately above it is necessary to prove (i) that if $\mathfrak{G} \vDash$ 'It is necessary that'$^\frown$ t(B) in β then for any H, if $R(H,i(\beta))$ then $h(G)\langle B,H\rangle = T$ (ii) if R for any H, if $R(H,i(\beta))$ then $h(G)\langle B,H\rangle = T$ then $\mathfrak{G} \vDash$ 'It is necessary that'$^\frown$ t(B) in β.

Assume $\mathfrak{G} \vDash$ 'It is necessary that'$^\frown$ t(B) in β. Assume for some given H, $R(H,i(\beta))$. By the definition of i, $P(i^{-1}(H),\beta)$. By assumption five, since $P(i^{-1}(H),\beta)$ and $\mathfrak{G} \vDash$ 'It is necessary that'$^\frown$ t(B) in β, then $\mathfrak{G} \vDash$ t(B) in $i^{-1}(H)$, Since t(B) has n - 1 constants, by the inductive hypothesis applied to t(B), since $\mathfrak{G} \vDash$ t(B) in $i^{-1}(H)$, then $h(\mathfrak{G})\langle B,H\rangle = T$.

Assume for any H, if $R(H,i(\beta))$ then $h(G)\langle B,H\rangle = T$. Assume for some given γ, $P(\gamma,\beta)$. By the definition of i $R(i(\gamma), i(\beta))$ and so $h(G)\langle B,i(\gamma)\rangle = T$. By the inductive hypothesis applied to t(B), $\mathfrak{G} \vDash$ t(B) in γ. Hence if $P(\gamma, \beta)$ then $\mathfrak{G} \vDash$ t(B) in γ. By assumption 5, $\mathfrak{G} \vDash$ 'It is necessary that'$^\frown$ t(B) in β.

This proves the hypothesis for case 4 and for all cases.

Granted $\mathfrak{G} \vDash$ t(A) in β iff $h(G)\langle A,i(\beta)\rangle = T$ for all β, then $\mathfrak{G} \vDash$ t(A) in α iff $h(G)\langle A,G\rangle = T$ and, by assumption one, this is equivalent to $\mathfrak{G} \vDash$ t(A) iff $h(G) \vDash A$.

Since S5 is formally sound by Kripke's semantics for S5, then by theorem 1 of 6.3.1, the reading R is depravedly sound.

What philosophical conclusions are to be drawn from the preceding proof? In essence, that the elements of Kripke's model structure $\langle G,K,R\rangle$ have been thought of as isomorphic to, or standing for, elements of some sort of ontology with relevance to modal reasoning. If assumptions like this are not made, then a proof of depraved soundness reaches no further than the classical propositional calculus. Ontological decisions are deferred, rather than eliminated, in steadfastly refusing to consider what significance the signs have, that are used in formal soundness and completeness proofs. In a proof of depraved soundness and completeness, those same decisions are forced back upon us.

Modal logic is perhaps the most thoroughly known and best researched of all branches of extended logic; but the same principles emerge in other extended logics. For example, it was remarked in the previous chapter that tense logic might be preferred, on ontological grounds, to first-order logic plus quantification over moments of time. McArthur [90] in his lucid work on tense logics provides the syntax and the formal semantics for a wide variety of tense logics. The formal semantics of these logics are based on the idea of indexed sets which are maximally consistent, called historical moments. Inspection of the semantics for tense logics invites the idea that these historical moments are formal proxies for temporal moments in time in the most real sense. It is difficult to see how the formal semantics could give any assurance to tense logics unless that were so. Therefore to bridge the gap between formal and depraved soundness/completeness for tense logics, some commitment to instants of time seems needed.

So the monist's defence is a powerful one: essentially a defence of first-order notation against its rivals, The defence is that where extended logics have been invoked, their formal semantics reveals, either directly (as with the realist interpretation of possible world semantics) or indirectly (as with Kripke's treatment of possible world semantics), that their useful purpose could be subsumed by first-order quantification over the elements ontologically presupposed in the formal semantics.

On balance, it seems fair to award the contest to the monist rather than the pluralist. But to draw this conclusion is not to adopt a neo-Kantian complacency that no improvement on first-order logic is possible. The conclusions of the monist's defence were, after all, based on an inductive inspection of certain researched extended logics. No apriori reason was offered to show that any conceivable extended logic had to fall prey to the monist's attack. Monism, is, simply speaking, the best bet as matters currently stand in logic; but this is not to say it cannot be overthrown. In logic, as in ontology and science, hypothesis must substitute for certainty.

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