MICHIGAN STATE UNIVERSITY LIBRARIES
3 1293 00797 1009
LIBRARY
Michigan State
University

THESIS
pt. 1

This is to certify that the

dissertation entitled

CONSTRUCTIVISM AND REFERENCE

presented by

David Brian Boersema

has been accepted towards fulfillment
of the requirements for

PhD degree in Philosophy

Joseph L. Hanna
Major professor

Joseph Hanna

Date July 10, 1985

69

MSU is an Affirmative Action/Equal Opportunity Institution

O-12771

MSU
LIBRARIES

RETURNING MATERIALS:
Place in book drop to remove this checkout from your record. FINES will be charged if book is returned after the date stamped below.

9.27.93
146

CONSTRUCTIVISM AND REFERENCE

by

David Brian Boersema

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

Department of Philosophy

1985

Copyright by

DAVID BRIAN BOERSEMA

1985

ABSTRACT

CONSTRUCTIVISM AND REFERENCE

by

David Brian Boersema

In this book I argue for a constructivist view of reference. By `constructivism´ I mean the view that we socially construct reality (or, minimally, our understanding of reality is only possible through social construction). I argue that the received theories of reference (the cluster theory and the causal theory) are more alike than different, and that they both share fundamental assumptions about privacy, essentialism, truth and realism. I argue against these assumptions and the theories of reference that are based on them. Finally, I offer my alternative, constructivist, view of reference and show how it is superior to the received theories.

For

J.F.H.

B.B.H.

S.A.H.

ACKNOWLEDGEMENTS

I want to thank Barbara Abbott, Richard Hall, Ronald Suter, and Randal Robinson for helpful questions, comments, criticisms and suggestions. I want especially to thank my teacher and friend Joseph Hanna and acknowledge my debt to him. Joe's thoughts, suggestions, examples and challenges permeate this book. Whatever merit this work has and whatever philosophic skills I have are but shadows of him. To Joe as a teacher, a scholar and - most importantly - as a person, I want to express my sincere gratitude. With apologies to Newton, even if I have been unable to see far, I have, nonetheless, stood on the shoulders of a giant. Finally, to Betina Henig and Sally Hiddinga, to whom I owe so much, I want to acknowledge my deepest thanks.

TABLE OF CONTENTS

INTRODUCTION. 1

PART ONE. THE RECEIVED THEORIES..... 8

CHAPTER ONE. THE CLUSTER THEORY..... 12

CHAPTER TWO. THE CAUSAL THEORY..... 50

PART TWO. CRITIQUE OF THE RECEIVED THEORIES..... 79

CHAPTER THREE. CONVERGENCE OF THE RECEIVED THEORIES. 85

CHAPTER FOUR. PRIVATE REFERENCE..... 96

CHAPTER FIVE. INDIVIDUATION AND SIMILARITY..... 121

CHAPTER SIX. ESSENTIALISM..... 169

CHAPTER SEVEN. CORRESPONDENCE AND REALISM..... 201

PART THREE. A CONSTRUCTIVIST ALTERNATIVE..... 239

CHAPTER EIGHT. COHERENCE AND VERIFICATIONISM..... 246

CHAPTER NINE. COHERENCE AND REFERENCE..... 272

CONCLUSION. 311

BIBLIOGRAPHY. 314

INTRODUCTION

I am sitting with a philosopher in the garden; he says again and again `I know that that´s a tree´, pointing to a tree that is near us. Someone else arrives and hears this, and I tell him: `This fellow isn´t insane. We are only doing philosophy.´
(Ludwig Wittgenstein, On Certainty)

INTRODUCTION

The main task in this book is to propose and defend a constructivist (i.e. arealist) position with respect to epistemology and metaphysics and a coherence theory of truth. (I don't like the term `anti-realist' because it connotes to me a sense of a position that is basically a reaction, or perhaps response, to a prior position. In the same sense that `pro-choice' carries a connotation different than that of `anti-abortion', I want a term that is the analogue to `pro-choice' where `anti-realist' is the analogue to `anti-abortionist'. `Constructivism', I think, fills the bill.) What I mean by `constructivism' will, I hope, emerge throughout the book. For now, a simple statement of it is that it is the view that reality is socially constructed.

The vehicle which I use to propose and defend my view is that of the issue of reference. My purpose for this is two-fold. First, I see the issues dealt with and the positions propogated under the rubric of reference as more readily understandable and manageable if the epistemological and metaphysical bases underlying them are exposed. That is, one of my points is to point out that questions about reference rest on other, primarily epistemological, questions which assume and are committed to conceptions of

truth and (a)realism. Second, I want to show the significance of questions about truth and (a)realism by showing how other issues (such as reference) rest on these questions. That is, I want to say that dealing with these questions is prior to and relevant to other philosophical concerns.

The book is laid out as follows:

Part One: The Received Theories

Chapter 1: The cluster theory

Chapter 2: The causal theory

Part Two: Critique of the Received Theories

Chapter 3: Convergence of the received theories

Chapter 4: Private reference

Chapter 5: Individuation and similarity

Chapter 6: Essentialism

Chapter 7: Correspondence and realism

Part Three: A Constructivist Alternative

Chapter 8: Coherence and verificationism

Chapter 9: Coherence and reference

In Part One I lay out the current debate between the two dominant views of reference as presented in the literature, the cluster theory and the causal theory. In Chapter 1 I first present the Searlean version of the cluster theory, summarize the causalist criticisms of this theory and finally respond - for the cluster theory - to those criticisms. The format in Chapter 2 is the same. I first present the theory, primarily the Kripkean and

Devittian versions, present the clusterist criticisms, and finally respond - for the causal theory - to those criticisms. The overall point of Part One is to `set the stage´ in the sense of presenting what I take the current debate about reference to be.

In Part Two I suggest that the two received theories are really two sides of the same theory. I argue that they share many fundamental underlying assumptions and commitments, enough so as to see their dispute as a `family squabble´. In this part of the book I spell out what those underlying assumptions and commitments are and criticize them. In Chapter 3 I argue that even on a `surface´ level the two received theories are more alike than different and suggest that when pushed they begin to converge into a single theory. They both, for example, are committed to the speaker´s intention to refer as an essential element in (successful) reference occurring. In Chapter 4 I argue that one of the fundamental reasons that the received theories converge is that they are both committed to private reference (i.e. reference in a private language). I first lay out what I take private reference to be, then suggest how I see both Searle and Kripke as being committed to this, and finally explain how this commitment is a result of a particular prior conception of individuation and similarity. I flesh out what I mean by this more thoroughly in Chapter 5. Here I present the issues and problems concerning the principle of individuation and the similarity relation. I

explain what these issues are and argue that they are actually two sides of the same problem, that of determining what there is. I argue that individuation and similarity are both language- and interest-dependent. I then suggest that both Searle and Kripke are committed to particular conceptions of individuation and similarity, and which are mistaken because they rest on the assumption that individuation and similarity are not language- and interest-dependent. In Chapter 6 I argue that this mistaken conception of individuation and similarity is exhibited by and presupposed by a commitment to essentialism by both Searle and Kripke. I first discuss the issue of essentialism, examining the distinction between property essences and individual essences, and argue that essentialism - the sort of essentialism which rests on conceptions of individuation and similarity which are not language- and interest-dependent - is wrong. I then explain how Searle and Kripke are committed to this sense of essentialism and claim that to the extent that they are, their positions are weakened. Finally, I remark on a conception of essentialism which I suggest is acceptable (in the sense of resting on the `right´ conception of individuation and similarity). This `non-realist´ essentialism, while perhaps plausible, is, I argue, quite limited in scope. In Chapter 7 I suggest that the received theories of reference are committed to a correspondence theory of truth and to realism. I argue that the

conceptions of individuation and similarity as not being language- and interest-dependent rest on correspondence and realism. In fact, I identify realism with this particular conception of individuation and similarity. I argue that the correspondence theory of truth entails this conception of individuation and similarity. (In the course of this examination, I consider Tarski's semantic theory and argue that if it is committed to correspondence and realism it begs the relevant questions here and if it is not committed to correspondence and realism, but is theory-neutral, it fails to do the job that we want from a theory of truth.) I then argue that Searle and Kripke are committed to a correspondence theory and to realism. I conclude that the two received theories of reference share fundamental assumptions and commitments and the particular assumptions and commitments which they have are mistaken, such that their theories of reference are also mistaken.

In Part Three I suggest an alternative view of reference, a view which is based on the assumption and commitment that individuation and similarity are language- and interest-dependent. This is borne out in the commitment to a coherence theory of truth. In Chapter 8 I argue for a coherence theory. I consider criticisms of coherence and respond to those criticisms. In Chapter 9 I suggest a view of reference based on a coherence theory of truth (with its underlying commitment to a particular conception of individuation and similarity). I first examine what I take

to be a Wittgensteinian view of reference, which I suggest is a version of a coherence-based view of reference. (I also argue that Kripke is mistaken in his portrayal of Wittgenstein as holding a Searlean cluster theory of reference.) Finally I expand on this view by showing how it overcomes the cases which are troublesome for the received theories of reference and by giving examples which display the superiority of a Wittgensteinian view of reference over the received theories.

THE RECEIVED THEORIES OF REFERENCE

"When I use a word," Humpty Dumpty said in a rather scornful tone, "it means just what I choose it to mean - neither more nor less."

"The question is," said Alice, "whether you can make words mean so many different things."

(Lewis Carroll, Through The Looking Glass)

INTRODUCTION

In Naming and Necessity Kripke rejects the Russellian theory of proper names as neither an adequate nor a correct treatment of ordinary (proper) names.[1] Kripke argues (with many others) that the Russellian view fails to account for the significance of the fact that different descriptions may be (and are) used in place of a name to designate an object. So one person might think of Aristotle as `the teacher of Alexander´, another as `the most famous student of Plato´, yet another as `the author of the Metaphysics´, and so on. (Even a single speaker might use these various descriptions at different times when referring to Aristotle.) No one of these descriptions could be the meaning of the name `Aristotle´ or else the meaning of the name would be in constant flux. Additionally, the notion of proper names as disguised or shorthand definite descriptions is faulty, for if `Aristotle´ means `the teacher of Alexander´, then the statement `Aristotle was the teacher of Alexander´ would be a tautology - something it is not. (Indeed, not only is this statement not a tautology, but we could very well discover that it is false.) So, says Kripke, being the teacher of Alexander cannot be part of [the sense of] the name `Aristotle´.

Kripke then goes on to say that the most common way out

of this difficulty with such a view of names is to say that no particular description may be substituted for a name; rather what is needed is a family, or cluster, of descriptions. A good example of this, says Kripke, is found in Wittgenstein's Philosophical Investigations.[2] Kripke quotes the following part of paragraph 79 as introducing the idea of family resemblances:

> Consider this example. If one says `Moses did not exist', this may mean various things. It may mean: the Israelites did not have a single leader when they withdrew from Egypt-or: their leader was not called Moses-or: there cannot have been anyone who accomplished all that the Bible relates of Moses-...But when I make a statement about Moses,-am I always ready to substitute some one of these descriptions for `Moses'? I shall perhaps say: by `Moses' I understand the man who did what the Bible relates of Moses, or at any rate, a good deal of it. But how much? Have I decided how much must be proved false for me to give up my proposition as false? Has the name `Moses' got a fixed and unequivocal use for me in all possible cases?

Kripke then states:

> According to this view, and a locus classicus of it is Searle's article on proper names [Searle (1958)], the referent of a name is determined not by a single description, but by some cluster or family. Whatever in some sense satisfies enough or most of the family is the referent of the name. (1980, p.31).

In this first part I want to present the cluster theory of reference (at least Searle's version of it) in chapter one, as well as present and evaluate Kripke's criticisms of the cluster theory. In chapter two, I want to present the causal theory of reference (represented primarily by Kripke, Devitt and Donnellan), and present and evaluate Searle's criticisms of the causal theory.

NOTE

[1]Russell´s theory of proper names is taken to be a paradigm case of the `descriptivist theory of names´. For Russell, ordinary proper names are disguised definite descriptions. That it, ordinary proper names can be replaced by descriptions (whichs the speaker associates with the name). For example, Russell (1912) states: "Common words, even proper names, are usually really descriptions. That is to say, the thought in the mind of a person using a proper name correctly can generally only be expressed explicitly if we replace the proper name by a description." (1912, p.54). Russell´s view is generally taken to be such that for reference to occur/succeed when using a proper name, the description which actually underlies the name must be true of the object to which reference is made. This results in the problems noted below by both Searle and Kripke. (An important feature of Russell´s theory of proper names is that he discusses it within the context of discussing different kinds of knowledge, knowledge by acquaintance (e.g. immediate sensual knowledge) and knowledge by description (e.g. propositional knowledge). For Russell, the latter is reducible to the former.)

[2]I believe Kripke is mistaken in ascribing the cluster theory to Wittgenstein, but that issue is beyond the immediate concerns here. This point will be taken up later.

CHAPTER ONE

THE CLUSTER THEORY

Searle recognized the difficulties facing the Russellian theory of names as shorthand definite descriptions, and amended it by claiming that a name refers to an object in virtue not of a single description, but rather of a cluster, or disjunctive set, of descriptions. (In this essay I will use `disjunctive set of descriptions´ to indicate the logical sum, or disjunction, of those descriptions associated with a name.) Says Searle:

> Suppose we ask the users of the name "Aristotle" to state what they regard as certain essential and established facts about him. Their answers would constitute a set of identifying descriptions, and I wish to argue that though no single one of them is analytically true of Aristotle, their disjunction is. Put it this way: suppose we have independent means of identifying an object, what then are the conditions under which I could say of the object, "This is Aristotle?" I wish to claim that the conditions, the descriptive power of the statement, is that a sufficient but so far unspecified number of these statements (or descriptions) are true of the object. In short, if none of the identifying descriptions believed to be true of some object by the users of the name of that object proved to be true of some independently located object, then the object could not be identical with the bearer of the name. It is a necessary condition for an object to be Aristotle that it satisfy at least some of these descriptions. (1969, p.169).

So, associated with a name `N´ is a disjunctive set of descriptions (or descriptive predicates), the satisfaction

by an object of some of which is necessary for the object to be the referent of `N´. Clearly, the disjunctive set of descriptions which is associated with a name can vary from speaker to speaker and from occasion to occasion; as new beliefs are accepted about an object, new elements may be added to the set of descriptions, and as old beliefs are rejected, some elements may be deleted from the set of descriptions. It is not clear how many of these descriptions must be true of an object for a name to refer to that object, and it is no oversight on Searle´s part in failing to specify such a sufficient number. However, as Searle says, at least one of the descriptions must be true of an object in order for the name to refer to the object. That is, it couldn´t be possible that all of the elements in the set of descriptions associated with a name turn out false and yet reference successfully occur.

The context within which Searle makes the above claims is that of reference as a speech act (i.e. an action performed by a speaker by the use of rule-governed language). The reason he concentrates his remarks on reference (and philosophy of language in general) within this context is that "all linguistic communication involves linguistic acts. The unit of linguistic communication is...the production or issuance of the symbol or word or sentence in the performance of the speech act." (1969, p.16).[1] In his most recent work Searle places his views of speech acts (and philosophy of language in general) within

the context of intentionality and philosophy of mind. Philosophy of language, he says, is a branch of the philosophy of mind. "The capacity of speech acts to represent objects and states of affairs in the world is an extension of the more biologically fundamental capacities of the mind (or brain) to relate the organism to the world by way of such mental states as belief and desire, and especially through action and perception." (1983, p.vii).

Restricting his analysis to singular definite referring expressions (i.e. proper names, definite descriptions and pronouns), Searle claims that these referring expressions pick out or idenitfy one object, or `particular´, apart from other objects or `particulars´ and then go on to say something about that object or `particular´. In discussing the success of a referring expression, he makes the distinction between a fully consumated reference and a successful reference. A fully consumated reference is one in which an object is identified unambiguously for the hearer; a successful reference (in the sense that we could not accuse the speaker of having failed to refer) is one in which an object could be, on demand, identified unambiguously for the hearer. A question the theory of reference must answer then becomes: What conditions are necessary for the utterance of an expression to be sufficient to identify for the hearer an object intended by the speaker?

The theory of reference which Searle proposes in order

to answer this question is capsulized in seven `rules of reference´ (listed below). These rules presuppose two `axioms of reference´ and a `principle of identification.´ The axioms are:

> A1. The axiom of existence: There must exist one and only one object to which the speaker´s utterance of the expression applies.
>
> A2. The axiom of identification: The hearer must be given sufficient means to identify the object from the speaker´s utterance of the expression. (1969, p.82).

The principle of identification is:

> P1. A necessary condition for the successful performance of a definite reference in the utterance of an expression is that either the expression must be an identifying description or the speaker must be able to produce an identifying description on demand. (1969, p.88).

Given these three conditions, we can now state Searle´s seven rules of reference and consider an example to illustrate the theory. Searle states:

> Given that S utters an expression R in the presence of H in a context C then in the literal utterance of R, S successfully and non-defectively performs the speech act of singular identifying reference if and only if the following conditions 1-7 obtain:
>
> 1. Normal input and output conditions obtain.[2]
>
> 2. The utterance of R occurs as part of the utterance of some sentence (or similar stretch of discourse) T.
>
> 3. The utterance of T is the (purported) performance of an illocutionary act.[3]
>
> 4. There exists some object X such that either R contains an identifying description of X or S is able to supplement R with an identifying description of X.[4]
>
> 5. S intends that the utterance of R will pick

out or identify X to H.

6. S intends that the utterance of R will identify X to H by means of H's recognition of S's intention to identify X and he intends this recognition to be achieved by means of H's knowledge of the rules governing R and his awareness of C.[5]

7. The semantical rules governing R are such that it is correctly uttered in T in C if and only if conditions 1-6 obtain.

These rules can be exemplified with the following sentence:

(T) Venus is hidden from view by thick cloud cover.

Taking `Venus' as R, it is evident that the rules are satisfied. `Venus' occurs as part of the utterance of T, where the utterance of T performs as illocutionary act (say, that of informing H). Venus exists and an identifying description (e.g. the planet at such-and-such a place in the sky at such-and-such a time) could be offered if needed. Additionally, it is intended that `Venus' picks out Venus (and H knows this).

On the other hand, if not all of the rules are satisfied, then reference has not occurred. For example, if S uttered `Venus' simply as part of a rhyming game (e.g. sounds that rhyme with `wean us'), then rule 5 (and perhaps rule 2) would not be fulfilled and reference would not have taken place.

Having laid out these rules of reference as a proposed theory of reference, Searle turns directly to the problem of definite descriptions and proper names. As noted above, Searle amends Russell's view by claiming that a singular

referring expression refers to an object in virtue of not a single description, but rather of a cluster of descriptions associated with a name. Again, Searle claims: It is a necessary condition for an object to be Aristotle that it satisfy at least some of these descriptions.[6] However, Searle's theory is more complex than that. There are problems, he says, with the view that names have no senses. If names have no senses, then, as Frege pointed out, there would be no cognitive difference between a=a and a=b. In addition, negative existential statements (e.g. `Cerberus does not exist') are meaningful (and, in this case, true), but they couldn't be if names have no senses.

On the other hand, says Searle, strong arguments militate against the view that names do have senses. If names have senses, at least in the form of being shorthand descriptions, then "descriptions should be available as definitional equivalents for proper names" (1969, p.166), but they are not. In addition, if names have senses, then if we substitute descriptions for names, then the following (non-intuitive) result would ensue: some non-analytically true statements about an object using the name as subject would be analytic (e.g. `Aristotle was the teacher of Alexander'). (This, as noted above, is one of the criticisms levelled against Russell.) Also, the meaning of the name, and perhaps the identity of the object, would change every time there was a change at all in the object, and the name would have different meanings for different

speakers. Surely this is not the case.

With strong arguments available both for and against the hypothesis that names have senses, Searle reinterprets the question `Do proper names have senses?´ as having two forms (which he labels `weaker´ and `stronger´). The weaker form is: `Are any statements where the subject is a proper name and the predicate a descriptive expression analytic?´. The stronger form is: `Are any statements where the subject is a proper name and the predicate an identifying description analytic?´. In answering these questions, Searle states:

> My answer, then, to the question, "Do proper names have senses?" - if this asks whether or not proper names are used to describe or specify characteristics of objects - is "No". But if it asks whether or not proper names are logically connected with characteristics of the object to which they refer, the answer is "Yes, in a loose sort of way". (1969, p.170).

This "loose sort of way" is the necessity that for an object to be X (e.g. Aristotle), it must satisfy the logical sum of the properties attributed to X (i.e. at least one description of the cluster must be true). Searle summarizes his position:

> What I have said is a sort of compromise between Mill and Frege. Mill was right in thinking that proper names do not entail any description, that they do not have definitions, but Frege was correct in assuming that any singular term must have a mode of presentation and hence, in a way, a sense. His mistake was in taking the identifying description which we can substitute for the name as a definition. (1969, p.170).

Criticisms of the Cluster Theory

Kripke suggests that the cluster theory contains the following six theses:

> (1) To every name or designating expression `X´, there corresponds a cluster of properties, namely the family of those properties Ø such that A [the speaker (or hearer?)] believes `ØX´.
>
> (2) One of the properties,or some conjointly, are believed by A to pick out some individual uniquely.
>
> (3) If most, or a weighted most, of the Ø´s are satisfied by one unique object Y, then Y is the referent of `X´.
>
> (4) If the vote yields no unique object, `X´ does not refer.
>
> (5) The statement, `If X exists, then X has most of the Ø´s´ is known a priori by the speaker.
>
> (6) The statement, `If X exists, then X has most of the Ø´s´ expresses a necessary truth (in the idiolect of the speaker). (1980, p.71).[7]

Kripke then presents a detailed critical analysis of these theses.

Thesis (1): This, Kripke tells us, is a definition. The import and legitimacy of this definition is to be borne out by theses (2)-(6) and so he does not offer a critical analysis of this thesis. Rather, an analysis of the subsequent theses will, if they are shown to be incorrect, yield the incorrectness (or irrelevance) of thesis (1) as well.

Thesis (2): Kripke offers two counter-examples to demonstrate that thesis (2) is incorrect. First, he gives a case to show that the thesis fails to be satisfied. If we consider the name `Feynman´, we note that many people who

know very little about Feynman are nonetheless able to refer to Feynman when using the name `Feynman´. When asked about Feynman, a person might say: well, he´s a physicist or something. The person may not think that this picks out anyone uniquely (and in this case, probably wouldn´t think that it does). Yet, says Kripke, it seems that such a person is still using the name `Feynman´ as a name for Feynman. Second, to show that the thesis is simply false, Kripke offers the following case. We can uniquely pick out Einstein as `the man who discovered the theory of relativity´. However, many people can only say of the theory of relativity that it is `Einstein´s theory.´ We are led, then, "into the most straightforward sort of vicious circle." (1980, p.82). The problem here is that one property is believed to pick out Einstein uniquely, but only at the cost of circularity, for in this case the property which picks out Einstein contains reference to Einstein within it.

Thesis (3): Kripke asks, "Suppose that most of the Ø´s are in fact satisfied by a unique object. Is that object necessarily the referent of `X´ for A?" (1980, p.83). His answer is: no. To support this he presents the following example. Suppose someone says that Gödel is the man who proved the incompleteness of arithmetic. Suppose further that Gödel was not in fact the author of this theorem, but rather a man named `Schmidt´ was. On the cluster view, says Kripke, when the `ordinary´ speaker uses the name `Gödel´,

"he really means to refer to Schmidt, because Schmidt is the unique person satisfying the description `the man who discovered the incompleteness of arithmetic´." (1980, p.84). So when the speaker talks of Gödel, he is in fact referring to Schmidt.[8] Thesis (3) "seems simply to be false." (1980, p.85).

Thesis (4): Concerning this thesis, Kripke states that his previous examples show it to be incorrect. Suppose, he says, that nothing satisfies most, or even any substantial number, of the Ø´s. Does that mean that the name doesn´t refer? Kripke says: no, it does not mean that, for just as one may have false beliefs about X which are in fact true of Y, so one may have false beliefs about X which are in fact true of no one - and these false beliefs might constitute the totality of one´s beliefs about X. For example, Einstein might be referred to as `the inventor of the atomic bomb´. However, possibly no one really deserves to be called the inventor of the device (or, at least, no single person was the inventor). Yet, even if `the inventor of the atomic bomb´ were our only belief about Einstein, we would still be referring to Einstein by `Einstein´. So, this thesis, too, is incorrect.

Thesis (5): About this, Kripke says that it is simply false. Even if theses (3) and (4) happen to be true, this hardly constitutes <u>a priori</u> knowledge that they are true. We certainly believe that Einstein was the man who discovered the theory of relativity, but this belief is

hardly a priori.[9]

Thesis (6): This thesis, according to Kripke, "need not be a thesis of the theory if someone doesn't think that the cluster is part of the meaning of the name." (1980, p.65).[10] This thesis, along with thesis (5), seems primarily to say that a sufficiently reflective speaker grasps this theory of proper names. Kripke's attitude toward this `necessity' thesis is the same as toward the `a prioricity' thesis (5), namely, it is obviously false. He states: "It would seem that it is a contingent fact that Aristotle ever did any of the things commonly attributed to him today, any of the great achievements that we so much admire." (1980, p.75).

Having investigated each of the theses (1)-(6) above, Kripke concludes:

> What I think the examples I've given show is not simply that there's some technical error here or some mistake there, but that the whole picture given by this theory of how reference is determined seems to be wrong from the fundamentals. It seems to be wrong to think that we give ourselves some properties which somehow qualitatively uniquely pick out an object and determine our reference in that manner. (1980, pp.93-94).

Now, we need to ask: are Kripke's criticisms legitimate (i.e. are they fair criticisms of what Searle's theory is committed to) and, if so, are they debilitating (i.e. can Searle's theory be defended or must it be abandoned)? In answering these questions each of the theses that Kripke attributes to the cluster theory will be investigated in turn.[11]

Thesis (1): "To every name or designating expression `X´, there corresponds a cluster of properties, namely the family of those properties Ø such that A believes `ØX´." Kripke regards this, as noted earlier, as a definition, the legitimacy of which hinges on (the fate of) the other theses. Granting this assumption, we will turn to the remaining theses.

Thesis (2): "One of the properties, or some conjointly, are believed by A to pick out some individual uniquely." The motivation for asserting this as a thesis of the cluster theory is statements such as the following: "In short, if none of the identifying descriptions believed to be true of some object by the users of the name of that object proved to be true of some independently located object, then the object could not be identical with the bearer of the name." (1969, p.169).

As we saw, Kripke offers his Feynman example to show that one can refer even though one does not believe that an object has been uniquely picked out. Such a counter-example does indeed seem to violate the thesis (which does seem to be implied by the Searle quote above). However, in other places, Searle seems to allow for such cases. Before considering these cases, though, a preliminary distinction that Searle makes between the primary and secondary aspects of reference must be explicated.

Donnellan (1966) distinguishes between the referential and the attributive uses of definite descriptions. A

speaker who uses a definite description referentially uses the description to enable the hearer to pick out whom or what the speaker is talking about and states something about that person or thing. A speaker who uses a definite description attributively uses the description to state something about whomever or whatever is so-and-so. For example, if a speaker says "Smith´s murderer is insane," meaning that particular person over there, Jones, the speaker would be using the term `Smith´s murderer´ referentially. On the other hand, if a speaker says "Smith´s murderer is insane," meaning not any particular person, but whoever it was who murdered Smith, the speaker would be using the term `Smith´s murderer´ attributively.

Now, in arguing that the referential-attributive distinction is bogus, Searle distinguishes what he calls the primary and secondary aspects of reference (or, under which reference is made).[12] Searle says:

> Sometimes when one refers to an object one is in possession of a whole lot of aspects under which or in virtue of which one could have referred to that object, but one picks out one aspect under which one refers to the object. Usually the aspect one picks out will be one that the speaker supposes will enable the hearer to pick out the same object. In such cases...one means what one says but one means something more as well. In these cases any aspect will do, provided it enables the hearer to pick out the object. (It may even be something which both the hearer and the speaker believe to be false of the object... (1979, p.144).
>
> ...provided that the speaker´s intentions are clear enough so that we can say that he really knew what he meant, then even though the aspect expressed by the expression he utters may not be satisfied by the object he "has in mind" or may

> not be satisfied by anything, still there must be some aspect (or collection of aspects) such that if nothing satisfies it (or them) the statement cannot be true and if some one thing satisfies it the statement will be true or false depending on whether or not the thing that satisfies it has the property ascribed to it. (1979, p.145).

The primary aspect under which reference is made is that aspect which, if not satisfied, would yield a statement that cannot be true. The secondary aspect is any aspect which the speaker expresses such that the speaker utters it in an attempt to secure reference to the object which satisfies the primary aspect, but which is not intended as part of the truth conditions of the statement the speaker is attempting to make. For example, the speaker, looking at someone in the room, says, "Smith´s murderer is insane." Now, the speaker and the hearer might agree that the speaker has referred to, and made a true statement about, that particular person being looked at even though that person (and perhaps everyone) fails to satisfy the expression `Smith´s murderer´. The speaker could, on demand, fall back on another aspect, say, one expressed by `the person I am looking at´. If it turns out that there is no person being looked at, only a hologram perhaps, then the speaker could fall back on another aspect, say, one expressed by `the person arrested by the police and accused by the District Attorney as Smith´s murderer´. If it turns out that there is no such person, then the speaker could fall back on another aspect. Eventually, however, an aspect must be reached such that if no one satisfied it, then the statement

could not be true. (And, though Searle does not say so explicitly, we must assume that in such a case no one has been referred to.) The primary aspect of reference is this `last´ aspect, the aspect which either works or results in a statement which is false. The other aspects (e.g. the one expressed by `the person I am looking at´) are secondary.

As mentioned earlier, Searle believes that the referential-attributive distinction is bogus. Having introduced his primary-secondary aspect distinction, Searle explains why. According to Searle, all of Donnellan´s cases are cases where the definite description is used to refer. The difference in the cases is that in the so-called referential cases the reference is made under a secondary aspect, and in the so-called attributive cases it is made under a primary aspect. Furthermore, since every statement containing a reference must have a primary aspect, then in the so-called referential use the speaker may still have referred to something that satisfies the primary aspect even though the expression uttered, which expresses a secondary aspect, is not true of that object and may not be true of anything.

Having Searle´s primary-secondary aspect distinction in hand, we are now ready to return to Kripke´s criticism of thesis (2). It seems that with respect to secondary aspects under which reference is made, Kripke´s thesis (2) is not a thesis of the cluster theory; however, with respect to primary aspects under which reference is made, Kripke´s

thesis (2) - and his criticisms of it - are on target, at least for the cluster theory as Searle has fleshed it out. One might be able to slightly amend Searle's theory, though, and handle the counter-examples which Kripke proposed. Both the Feynman and the Einstein-as-discoverer-of-relativity cases work because the speaker has (apparently) no primary aspect under which to refer to Feynman and Einstein respectively. However, if we allow that a speaker in such a situation could appropriately appeal to another speaker or source to supply other aspects by which to refer, then the counter-examples would fail. That is, I may only know Feynman as `a physicist or something' and fully acknowledge that I have not uniquely picked out Feynman, but believe that reference has occurred successfully because I can add something like, "I don't know anything more about Feynman, but Keith does. He can tell you all about Feynman." I can recognize that the set of properties which I associate with `Feynman' do not uniquely pick out Feynman, but also recognize that someone else could amend the set such that Feynman could be uniquely picked out. (It would not be necessary that Keith be the person from whom I heard about Feynman. I might simply rely on the fact that Keith is a physicist friend of mine who I know, or have good reason to believe, is familiar with other physicists.) As noted before, though, even if such an amendment to Searle's theory is legitimate, the theory is still committed to the primary aspect under which reference is made as having to be

satisfied in order for reference to occur. While this would entail that thesis (2) is indeed a thesis of the cluster theory, Kripke's particular counter-examples would no longer be lethal to that thesis. (Whether or not amended counter-examples to the cluster theory - in which this move to appealing to other speakers to secure reference is blocked - are possible or can work will be considered later.)

Thesis (3): "If most, or a weighted most, of the Ø's are satisfied by one unique object Y, then Y is the referent of `X'." This thesis, Kripke tells us, says that the speaker's belief noted in thesis (2) - that `ØX' - is correct. That is, thesis (2) is purely doxastic, it states only that some property (or enough of them) is <u>believed</u> by the speaker to uniquely pick out some object, whereas thesis (3) states that some property (or enough of them) in fact does uniquely pick out some object.[13] The motivation for making this a thesis of the cluster theory is statements such as those noted above for thesis (2): if none of the identifying descriptions believed to be true of some object by the users of the name of that object proved to be true of some independently located object, then the object could not be identical with the bearer of the name. This statement clearly implies that if an object is identical with the bearer of a given name (i.e. if Y is (identical with) the referent of `X'), then at least one of the identifying descriptions believed to be true of the object by the users of the name of that object must be true of that

(independently located) object (i.e. some of the Ø's are satisfied by Y). As Searle says: "Since the speaker is identifying an object to the hearer, there must, in order for this to be successful, exist an object which the speaker is attempting to identify, and the utterance of the expression by the speaker must be sufficient to identify it." (1969, p.82).

Kripke's counter-example to this thesis was Gödel being identified as the man who proved the incompleteness of arithmetic, though, unbeknownst to the speaker, Schmidt was actually the author of the proof. Since the (only) descriptions associated with `Gödel' are in fact satisfied by Schmidt, under the cluster theory, the speaker must be referring to Schmidt by `Gödel'.

Searle's (explicit) response to this proposed counter-example is that depending upon our intention in a particular context of using the name `Gödel', the referent of `Gödel' could "go in either direction." (1983, p.251). Suppose, he says, that Jones proclaims, "On line 17 of his proof, Gödel makes what seems to me a fallacious inference." If we query Jones as to who is meant by (his use of) `Gödel', Jones might respond, "I mean the author of the famous incompleteness theorem." If then informed that Schmidt was the author, what would Jones say? Says Searle:

> It seems to me that he might well say that by "Gödel" he just means the author of the incompleteness proof regardless of what he is, in fact, called. Kripke concedes that there could be such uses. They involve what I have called secondary aspect uses of proper names. (1983,

p.251).

On the other hand, if Jones says, "Kurt Gödel lived in Princeton" and we query Jones as to who is meant by `Gödel´, Jones will likely be referring to Gödel and not Schmidt, and will associate a different set of secondary aspect uses than in the first case (and as well a different primary aspect use). In any case, for Searle, it is not a singular, given use of a name that determines reference, nor even the particular description associated with that singular, given use, but rather the underlying intentional content which is attached to the name. With the name `Gödel´, different intentional contents and primary (and secondary) aspects might be attached to the name for any given use of the name. It seems to me that Searle would say that the reason Kripke´s intuitions are so strong that when we use the name `Gödel´ we mean Gödel and not Schmidt is because in most cases the intentional content attached to our use of a name in fact allows us to pick out the `correct´ (i.e. intended) object. The fact is that we usually pick out the right object when we use a name; the primary aspect under which reference is made (and usually the secondary aspect) does the job. This should work for `Gödel´, too. If it doesn´t (i.e. if the intentional content is `incorrect´, if the Ø´s associated with `Gödel´, or at least the primary associated with `Gödel´, turns out to identify Schmidt), then, for Searle, we have referred to Schmidt. Now, it seems to me that the motivation underlying Searle´s position

here is clear and very intuitive. It is clear to see why Searle would say that in the cases above where all of the Ø´s (or the primary Ø, or the intentional content) associated with `Gödel´ turn out to identify Schmidt, then we obviously have referred to Schmidt. On the other hand, Kripke´s insistance that we refer to Gödel by our (every?) use of `Gödel´ seems to be based on our belief that we pick out the `correct´ object when we use names.

Once again: if `the author of the incompleteness theorem´ is the primary aspect under which reference is made to Gödel, then, a la Searle, we haven´t referred to Gödel, but to Schmidt. But why think that in such a case we have referred to Gödel? What is underlying Kripke´s claim that even in this situation we are in fact referring to Gödel? It is not clear to me, unless it is the belief that usually when we use `X´ we mean, and correctly pick out, X rather than Y. However, this hardly runs counter to Searle´s theory; indeed, he agrees completely.

It is noteworthy that in a footnote Kripke makes some remarks that sound rather conciliatory with regard to his Gödel-Schmidt case and with regard to the cluster theory in general. The note reads:

> The cluster-of-descriptions theory of naming would make `Peano discovered the axioms for number theory´ express a trivial truth, not a misconception, and similarly for other misconceptions about the history of science. Some who have conceded such cases to me have argued that there are <u>other</u> uses of the same proper names satisfying the cluster theory. For example, it is argued, if we say, `Gödel proved the incompleteness of arithmetic,´ we are, of course,

> referring to Gödel, not to Schmidt. But, if we say, `Gödel relied on a diagonal argument in this step of the proof,´ don´t we here, perhaps, refer to <u>whoever proved the theorem</u>? Similarly, if someone asks, `What did Aristotle (or Shakespeare) have in mind here?´, isn´t he talking about the author of the passage in question, whoever he is? By analogy to Donnellan´s usage for descriptions, this might be called an `attributive´ use of proper names. If this is so, then assuming the Gödel-Schmidt story, the sentence `Gödel proved the incompleteness theorem´ is false, but `Gödel used a diagonal argument in the proof´ is (at least in some contexts) true, and the reference of the name `Gödel´ is ambiguous. Since some counter-examples remain, the cluster-of-descriptions theory would still, in general, be false, which was my main point in the text; but it would be applicable in a wider class of cases than I thought. I think, however, that no such ambiguity need be postulated. It is, perhaps, true that sometimes when someone uses the name `Gödel´, his main interest is in whoever proved the theorem, and, <u>perhaps</u>, in some sense, he `refers´ to him. I do not think that this case is different from the case of Smith and Jones... . If I mistake Jones for Smith, I may <u>refer</u> (in an appropriate sense) to Jones when I say that Smith is raking the leaves; nevertheless I do not use `Smith´ ambiguously, as a name sometimes of Smith and sometimes of Jones, but univocally as a name of Smith. Similarly, if I erroneously think that Aristotle wrote such-and-such passage, I may perhaps use `Aristotle´ to <u>refer</u> to the actual author of the passage, even though there is no ambiguity in my use of the name. In both cases, I will withdraw my original statement, and my original use of the name, if apprised of the facts. Recall that, in these lectures, `referent´ is used in the technical sense of the thing named by a name (or uniquely satsifying a description), and there should be no confusion. (1980, pp.85-86, note 36).

Several points need to be made here. First, although Kripke is obviously going to great lengths to put qualifiers on his remarks (e.g. "perhaps," "in some sense," "<u>refer</u>" as opposed to "refer"), he clearly concedes that not every use of the name `X´ results in X being referred to, and, in

fact, a speaker might refer to Schmidt even when saying `Gödel´. While Kripke admits that the cluster theory might be applicable in a wider class of cases than he originally thought, he states that other counter-examples remain to nonetheless prove the theory false in general. Part of the purpose of this chapter is to suggest that none of the counter-examples which Kripke has given do the job, and I still see Kripke as bearing the onus to show that they do the job. Second, Kripke tries to overcome his concessions to the cluster theory by implying that the cluster theory works (or might work) in these Gödel-Schmidt cases because they imply that the names are ambiguous. However, Searle never makes any claim to that effect, nor is it necessary that he do so. Searle claims that we have different primary and secondary uses under which reference is made that result in the variation in reference. For Searle, a name `X´ has no meaning at all, so it certainly doesn´t have an ambiguous meaning. Rather, we intend to refer to a given object by using a given name and we associate different descriptions with the name. Because a given description might express a given primary or secondary aspect under which reference is made, the same name `X´ might be used now to refer to X and later to refer to Y. Kripke´s charge of ambiguity here is spurious. Third, in dismissing even his concessions to the cluster theory, Kripke emphasizes that we might refer to Y with `X´, but, of course, there´s no confusion of reference here (since `referent´ is used in the "technical sense" of

the thing named by a name), and happily we still refer to X with `X´. It is hard to believe that Kripke thinks that he has explained anything by saying: well, `X´ refers to Y, but `X´ refers to X. The unhelpful, non-explicatory, question-begging nature of this sort of claim needs no further comment.

Perhaps a better candidate as a counter-example (because it is a more `natural´ example than the Gödel case) is one constructed by Donnellan (1972). Consider a case, he says, in which a young child is awakened during a party given by his parents. At this time the child encounters and speaks with one of the party guests and learns the name of the guest to be `Tom´. Later, reflecting on the event, the child remarks, `Tom is a nice man´. This is the child´s only description associated with `Tom´, but it does not identify for the parents who Tom is, as they know many Toms. It might even be that Tom is not a nice man, but the child believes him to be so. Now, suppose that another man was at the party and this man, whose name is also `Tom´, is nice. However, the child did not meet this Tom. It seems that if thesis (3) is true, then the child referred to the Tom he did not meet, as this man was the unique object which satisfied (a weighted) most - in this case all - of the Ø´s. But this is surely incorrect. Surely the child was referring to the Tom he did meet, even though he did not satisfy the Ø´s and another man did.

Again, it seems that Searle´s response would be that as

discussed above: the aspect expressed by `is a nice man´ is a secondary one in this case and could be replaced by the aspect `is the man I met´. Probably this aspect would serve as the primary aspect under which reference is made, but if not, then eventually one could be found which would - or else reference would not have occurred. And, again, appeal could (theoretically) be made to other speakers to provide the requisite expression of the primary aspect.

Thesis (4): "If the vote yields no unique object, `X´ does not refer." The motivation for asserting this as a thesis of the cluster theory is statements such as the following: "There must exist not more than one object to which the speaker´s utterance of the expression applies." (1969, p.83). As noted above, Kripke believes his previous counter-examples show this thesis to be incorrect - just as one can have false beliefs about X which are in fact true of Y, so one can have false beliefs about X which are in fact true of no one or of more than one. So even if `the inventor of the atomic bomb´ is the only description which a speaker associates with `Einstein´, the speaker nonetheless refers to Einstein by `Einstein´.

Now, as we saw above Searle explicitly admits that we sometimes use a description to identify an object even though no unique object is picked out by that description. The reason, for Searle, that a description can work in making reference to an object even though the description may be true of more than one object or true of no one is

that the description might express a secondary aspect under which reference is made. The particular description may work in identifying to the hearer the object to which the speaker is referring even though the description does not uniquely pick out the intended object. With respect to secondary aspects under which reference is made, then, it seems that thesis (4) is not a thesis of the cluster theory.

With respect to the primary aspect under which reference is made, it seems that if no object is uniquely picked out, then `X´ indeed does not refer, and thesis (4) is a thesis of the cluster theory. But in such a case, why think that reference has been made? If `the inventor of the atomic bomb´ is in fact the primary aspect under which a speaker (supposedly) refers to Einstein, then it is not clear why one should insist, with Kripke, that the speaker nonetheless referred to Einstein. As with the Gödel example, it seems that the motivation for claiming that the speaker has still referred to Einstein is that usually we successfully refer to the object to which we intend to refer; and again, this is hardly contrary to Searle´s position. So, as with theses (2) and (3), thesis (4) seems to be a thesis of Searle´s theory only if it is restricted to the primary aspects under which reference is made (in which case, it is not at all obvious that it is false), and even then only if it is restricted such that reference can be made only through the descriptions which the given speaker associates with a name. These restrictions are

certainly subject to challange.

Thesis (5): "The statement, `If X exists, then X has most of the Ø´s´ is known a priori by the speaker." Kripke says only that this is wrong - even if the above theses happen to be true, a typical speaker hardly knows a priori that they are.

Now, Searle says nothing explicitly to the effect that the statement `If X exists, then X has most of the Ø´s´ is known a priori by the speaker. An initial (minor) amendment to this supposed thesis is in order. The statement needs to be changed to `If X exists, then X has some of (or the `primary´) Ø´s´ to be a thesis of Searle´s theory. It remains to be seen if Searle´s theory is committed to this amended thesis. In investigating this, several points need to be made. First, Kripke´s claim makes sense only if Searle is proposing his theory as giving the meaning of a name. That is, only if `Aristotle´ means ´the most famous student of Plato´ or `the teacher of Alexander´ or ..., can this proposed thesis be a thesis of the theory. However, Searle does not see the disjunctive set of descriptions as giving the meaning of a name, but rather as being an identifying mode of presentation of a name. The disjunctive set of descriptions associated with a name is the means of identifying an intended object, not the means of defining a name. (With respect to thesis (6), Kripke notes that if one doesn´t think that the descriptions are part of the meaning of a name, but only that they fix the reference, then the

thesis need not be a thesis of the cluster theory. The same point, it seems, would hold for thesis (5), and it seems that this could well be the case for Searle, i.e. the descriptions only fix the reference.)

What if Searle's theory were committed to the view that the disjunctive set of descriptions associated with a name did give (in some sense) the meaning of the name? Would this thesis then be a thesis of his theory, and, if so, would this show a serious weakness in the theory? In answering these questions, we need to get clearer on exactly what Kripke is claiming. It is clearly not a thesis of Searle's theory if the statement means that the speaker knows a priori that some particular Ø is true of X. Searle definitely denies that this must be the case. Searle does, however, say that although no single description is analytically true of a given name, the disjunctive set of descriptions associated with the name is analytically true. The doxastic corollary of this would be that the speaker knows this analytic truth a priori. This reading of Kripke's claim, then, seems not unlikely to be a thesis of Searle's theory, for it is not unreasonable to suggest, as Kripke does, that a sufficiently reflective speaker knows (or could come to know) this aspect of names and their uses. However, even if this is a thesis of Searle's theory, it is not obvious that this fact carries the unwelcome stigma that Kripke attaches to it. The reason is that this reading of Kripke's claim seems to say nothing more controversial than

that the speaker knows `ahead of time´ (a priori, as it were) that some description (but no particular one) is true of X. This is to say very little. In addition, it seems to be a statement not of our knowledge about the object or name in question, but rather of our knowledge of the language and how names are used. That is, this reading seems to say nothing more than what Searle readily asserts: if I am to refer by using the name `Aristotle´, then I must believe that some of the descriptions which I associate with the name are true of the intended object (and I know this fact about names even before I use `Aristotle´). Under this reading of thesis (5), Searle is clearly committed to it, but then the apparent ruinous significance of this commitment is lost.

There is another reason to suggest that while thesis (5) may indeed be a thesis of Searle´s theory, that he would be glad to accept it as such (and in fact this could be seen by Kripke as not a point against Searle´s theory). It is this: if one believes that names have essences (or rather, that objects named have essences), and if one believes that at least one of the descriptions in the disjunctive set of descriptions associated with a name "picks out" this essence, then one might be much more inclined to accept the claim that the disjunctive set is analytically true of the object, and, as a corollary, that the sufficiently reflective speaker knows this analytic truth a priori.

Thesis (6): "The statement, `If X exists, then X has

most of the Ø's' expresses a necessary truth (in the idiolect of the speaker)." As noted earlier, Kripke holds that this `necessity' thesis, like the `a prioricity' thesis (5), is false. He says: "It would seem that it is a contingent fact that Aristotle ever did <u>any</u> of the things commonly attributed to him today, <u>any</u> of the great achievements that we so much admire." (1980, p.75). Again, making the amendment from `most of the Ø's' to `some of the Ø's,' such a statement is true of Searle's theory, but it is not obvious that this is so objectionable or unintuitive. If Kripke is saying that for the cluster theory it must be the case that if X exists, then some description must be believed to be true of X (and is true of X), then indeed Searle is committed to this thesis (assuming Searle is committed to names having meanings). But again, as with thesis (5), this seems to be a commitment to a fact about language and the use of names, not a commitment to facts about any object. Furthermore, if one believes that names have essences (or that objects named have essences) and if one believes that at least one description of the disjunctive set of descriptions associated with a name `picks out' this essence, then this thesis may be not only acceptable, but desirable.

<u>Conclusion</u>

What can be culled from this presentation and analysis of Searle's cluster theory of reference? It seems that his cluster theory is more sophisticated and defensible than it

has been given credit for being. Searle's primary-secondary distinction staves off (or at least offers some promise at staving off) many of Kripke's objections. In addition, this distinction does not seem an ad hoc device; it has definite intuitive appeal. However, the distinction does not immunize the theory from the criticisms that Kripke has raised. Searle's notion of secondary aspects under which reference is made does disarm much of Kripke's attack, but the necessity of bringing in a primary aspect rearms Kripke's objections, for it still seems that much of the basic theory which Kripke claims as the cluster theory is indeed the view that is held by Searle. Of course, as I have tried to show, even to the extent that Searle holds a version of the theory that Kripke attributes to him, it is not obvious that such a theory is "wrong from the fundamentals." To those theses to which Searle is committed, it is not so obvious that they are false or implausible or even unintuitive. Minimally, something other than Kripke's proposed counter-examples is needed to show that Searle's theory should be rejected.

The most evident response to these claims is that even if Searle could successfully defend his theory from these particular counter-examples, others could be offered which can do the trick. All that is needed is one example where no descriptions (or at least no true descriptions) are associated by the speaker to a given name being used. For instance, the speaker might really know nothing at all about

Gödel. So, my attentive neighbor might remark to her friend, "My weird neighbor kept me up last night yelling something about `that damn turtle´ or `Gödel´ or something like that. I had to pound on the wall to get him to quiet down."

Has my neighbor referred to Gödel? The causal theory devoid of descriptivist assumptions we will see later must say: yes. Searle, it seems, would say: no. Why think that reference has <u>not</u> occurred? For Searle, the reason is that rule 5 of his theory (and perhaps rule 6) has been violated. [Rule 5: S intends that the utterance of R will pick out or identify X to H. Rule 6: S intends that the utterance of R will identify X to H by means of H´s recognition of S´s intention to identify X and he intends this recognition to be achieved by means of H´s knowledge of the rules governing R and his awareness of C.] Why think reference <u>has</u> occurred? For the causal theory, as we will see later, the reason is that there is a causal chain linking `Gödel´ to Gödel. Apart from or prior to these theories, however, why think that reference has or hasn´t occurred? It is clear to see why one would claim that reference hasn´t taken place: the speaker didn´t intend to refer to Gödel; she didn´t even know that `Gödel´ is a name. On the other hand, it is not clear why one would say that reference has taken place.

Perhaps another example would be fairer to the causal theory: Suppose my obsession with Gödel has gotten to the point that I often blurt out my innermost thoughts

Gödel. So, my attentive neighbor might remark to her friend, "My weird neighbor kept me up last night yelling something about `that damn turtle' or `Gödel' or something like that. I had to pound on the wall to get him to quiet down."

Has my neighbor referred to Gödel? The causal theory devoid of descriptivist assumptions we will see later must say: yes. Searle, it seems, would say: no. Why think that reference has <u>not</u> occurred? For Searle, the reason is that rule 5 of his theory (and perhaps rule 6) has been violated. [Rule 5: S intends that the utterance of R will pick out or identify X to H. Rule 6: S intends that the utterance of R will identify X to H by means of H's recognition of S's intention to identify X and he intends this recognition to be achieved by means of H's knowledge of the rules governing R and his awareness of C.] Why think reference <u>has</u> occurred? For the causal theory, as we will see later, the reason is that there is a causal chain linking `Gödel' to Gödel. Apart from or prior to these theories, however, why think that reference has or hasn't occurred? It is clear to see why one would claim that reference hasn't taken place: the speaker didn't intend to refer to Gödel; she didn't even know that `Gödel' is a name. On the other hand, it is not clear why one would say that reference has taken place.

Perhaps another example would be fairer to the causal theory: Suppose my obsession with Gödel has gotten to the point that I often blurt out my innermost thoughts

concerning him. On one such occasion, while standing at a bus stop, I mutter, "Ha! Gödel couldn't hold a candle to Cantor. Now, there was a man!" Upon hearing this, a bemused and intrigued stranger next to me asks, "Who is Gödel?" Has the stranger referred to Gödel? The causal theory says: yes, because there is a causal link from the stranger's utterance of `Gödel' to Gödel. Searle would most likely say: no, because it does not seem that the stranger intended that the utterance of `Gödel' would pick out or identify Gödel (i.e. Searle's rule 5 is violated).[14] The theories, then, differ in the answer to whether the stranger has referred or not to Gödel. Which theory best matches our pre-theoretic intuitions here? It seems to me that it is not clear, but I must admit that Searle's theory comes closer to my intuitions. It seems to me that the very question, `Who is Gödel?' shows that the speaker has no clear referent associated with the name, and in fact the question can be paraphrased with no loss of meaning with a different question, `Whom are you talking about?' or `Whom are you referring to when you say "Gödel"?' For the present purposes, I think the following (minimally) can be said: there is no clear counter-example in which reference occurs to Searle's theory of a name for which the speaker has no descriptions at all associated with a name.

These examples illustrate a general point to which Searle responds (properly, I think) in connection with the variety of proposed counter-examples to his theory. The

point is this: the cases generally offered as inconsistent with Searle´s theory are singular, private instances of supposed reference. That is, these cases where reference supposedly takes place are cases of singular utterances of names, or singular statements in which the name occurs. All of the examples in which the speaker has at best (apparently) one description associated with a name are of this sort (e.g. `Feynman is a physicist or something,´ ´Einstein is the inventor of the atomic bomb´). But it seems that such cases (if possible) are far from paradigmatic for reference. We usually have an assortment of descriptions associated with a given name, even if some of them are rather unilluminating in terms of identifying the object being referred to. In those cases (if any) where we have one or no descriptions associated with a name, it seems more plausible to either appeal to other speakers as a source of reference or to simply admit that reference has not occurred. It is in connection with the former alternative (appeal to other speakers as a source of reference) that I say that the usual counter-examples to Searle´s theory are cases of private instances of supposed reference. By that I mean the following: generally the proposed counter-examples to Searle´s theory are ones in which reference borrowing is not considered or allowed. Yet, as noted above in response to Kripke´s examples, reference borrowing is a natural and common move. To forbid such a move would be to make reference a `private´ matter,

i.e. possible in a private language. This issue will become significant in subsequent discussion, so I only mention it here with the caveat that I see such a condition as untenable.

While Kripke and others have said little or nothing with respect to the private-public underpinnings of reference (at least in the sense that I mean it - which will be discussed later), they have dealt explicitly with the issue of reference borrowing. Kripke, for instance, briefly discusses Strawson's (1959) comments on reference borrowing (cf. 1980, pp.90-92, 160-161) in connection with his remarks on his non-circularity condition (C). However, the notion of reference borrowing discussed there is that of appealing to another speaker as the historical source for the given speaker's ability to refer by using a name (e.g. `By `Gödel', I mean the man that Joe thinks proved the incompleteness theorem.'). This is not the sense of reference borrowing which I am considering here, and while Kripke's condition (C) may well forbid cases of reference borrowing in the Strawsonian sense, it is irrelevant to the sense which I am using. That sense of reference borrowing is this: we can appeal to other speakers to provide requisite information or descriptions in order to secure reference, not because they stand in the position of being the historical source of my acquisition or acquaintance of a name, but because they can stand in the position of providing the needed identifying descriptions (which I lack)

in order to pick out the intended object. I don´t necessarily get my acquaintance with a name from these other speakers, but I get identifying descriptions to help pick out the intended object. The other speakers who provide me with these identifying descriptions could well be different speakers than those from whom I first gained acquaintance of a name. So, for example, I could have first heard of Gödel from Alice, but appeal to Betty to provide me with an identifying description so that I (and Carol) can pick out Gödel. Such appeal to other speakers is clearly not in violation of Kripke´s condition (C) (or Searle´s non-circularity constraints). Objections to this sense of reference borrowing, it seems to me, only make sense if one presupposes a view of reference as a `private´ matter, and, as mentioned above, this issue will be dealt with later.

Finally, in connection with Searle´s theory of reference, I want to remark that I have not tried to say that Searle is right and Kripke is wrong. Rather my point so far has been to give a somewhat detailed - and hopefully fair - treatment of the theory by presenting it and then considering criticisms of it from the perspective of someone external to the theory. I now want to turn to the causal theory of reference and do the same. Later I will suggest how both the cluster and the causal theories can be seen to be more similar than different, and having shown the shared bases for that similarity, I will in the final section sketch out an alternative, third, theory of reference.

NOTES

[1]A few pages later Searle re-emphasizes this point of reference as a speech act: "The term `referring expression´ is not meant to imply that expressions refer. On the contrary, as previously emphasized, reference is a speech act, and speech acts are performed by speakers in uttering words, not by words. To say that an expression refers (predicates, asserts, etc.) in my terminology is either senseless or is shorthand for saying that the expression is used by speakers to refer (predicate, assert, etc.); this is a shorthand I shall frequently employ." (1969, p.28).

[2]By "normal input and output" Searle simply means to cover such things as: both the speaker and hearer understand the language, there are no physical impediments (such as laryngitis or deafness), both are aware of what they are doing, they are not acting or telling a joke, etc.

[3]An illocutionary act is the performance of an action (such as referring, promising, requesting) by the utterance of a sentence. Searle´s point here is to rule out gibberish.

[4]This condition is meant to capture the axioms and principle mentioned above.

[5]This condition, says Searle, enables us to distinguish referring to an object from other ways of calling attention to it (such as hitting the hearer over the head with it).

[6]A few pages later he reiterates this: it is a necessary truth that Aristotle has the logical sum (inclusive disjunction) of the properties commonly attributed to him.

[7]Kripke adds a condition (C): For any successful theory, the account must not be circular. The properties which are used in the vote must not themselves involve the notion of reference in such a way that it is ultimately impossible to eliminate. That is, among the properties associated with the reference of a term `X´ cannot be the property `is called "X"´. The motivation for this condition is obvious - to prevent a theory which does not lead to any independent determination of a reference. Searle makes the same point: "...we are only justified in calling [an object] `Everest´ if we can give a reason for supposing it to be identical with what we used to call `Everest´ and to give as that reason that it is called `Everest´ would be circular." (1969, p.167).

[8]While the Gödel-Schmidt example might seem a bit `cooked up´, Kripke provides other, more natural, examples. For instance, it is believed by some people that Columbus

was the first man to realize that the earth is round and that he was the first European to land in the western hemisphere. Probably neither of these beliefs is true. If thesis (3) is correct, then when people use the term `Columbus´ they really refer to some Greek if they use the roundness of the earth, or to some Norseman if they use the `discovery of America´. But they don´t, says Kripke; they refer to Columbus.

[9]Kripke never states in Naming and Necessity just what a priori knowledge is. He argues against its conflation with necessity. He says that "the notion of a prioricity is a concept of epistemology." (1980, p.34). Beyond that, however, he says only what it is not (e.g. it is not the metaphysical concept of necessity). Since he is reticent about meanings and argues against a prioricity as knowledge that can be known prior to or independent of experience (because such a definition includes the modal notion of possibility, which is metaphysical rather than epistemic), it is not clear to me what Kripke takes a prioricity to be.

[10]Kripke claims that there are two ways in which the cluster theory (or even the single description theory) can be viewed. One is that of saying that the cluster (or single description) actually gives the meaning of the name (e.g. `Aristotle´ means `the most famous student of Plato´ or `the teacher of Alexander´, etc.). Another is that of saying that the cluster (or single description) determines the reference (e.g. `the teacher of Alexander´ is used to determine to whom someone is referring when someone says `Aristotle´). Apparently thesis (6) is a thesis of the cluster theory only if the descriptive referring expressions are used to determine the reference of `X´.

[11]Michael McKinsey (1978) has done a somewhat similar critique and found Kripke´s arguments lacking credibility. Other attempts have been made to defend the cluster theory, e.g. Boer (1972) and Ingber (1979).

[12]Whether or not Searle succeeds in showing Donnellan´s distinction to be bogus is not the main concern here. It should also be noted that Kripke (1977) criticizes Donnellan´s distinction.

[13]It is not clear if Kripke thinks that for the cluster theory the property (or set of properties) which the speaker believes uniquely picks out the object being referred to must be the same property (or set of properties) which in fact uniquely picks out the object being referred to. This is a minor point, however.

[14]Actually, for reasons seen later, I will clarify this claim and assert that in the second case Searle thinks that reference does occur. For now, though, I want to portray

Searle as a `pure´ cluster theorist, such that having the appropriate associated description is necessary and sufficient for reference to occur.

CHAPTER TWO

THE CAUSAL THEORY

Having analyzed the cluster theory of reference, and finding it "wrong from the fundamentals," Kripke proposes to present a "better picture" of how reference takes place. "In general," says Kripke, "our reference depends not just on what we think ourselves, but on other people in the community, the history of how the name reached one, and things like that. It is by following such a history that one gets to the reference." (1980, p.95). This "better picture," which Kripke says is "not a theory" (1980, p.96), has nonetheless served as the nascency and kernal of what has come to be called the `causal theory of reference´. Kripke gives a "rough statement" of such a theory:

> A rough statement of a theory might be the following: An initial `baptism´ takes place. Here the object may be named by ostension, or the reference of the name may be fixed by a description. When the name is `passed from link to link´, the receiver of the name must, I think, intend when he learns it to use it with the same reference as the man from whom he heard it. (1980, p.96).[1]

The sense in which this is a causal theory of reference is that the passage of a name from link to link is said to secure a causal connection between the name of an object and the object. The initial baptismal act of naming the object (by ostension, perhaps) establishes the causal connection in

the first place. Later uses of the name must be connected to the object in some sort of causal chain stretching back to the original naming act. As noted above, Kripke does not explicitly propose a theory of reference. However, others, e.g. Devitt (1981), have attempted to forge a causal theory of reference based on Kripke's picture. According to Devitt:

> The central idea of a causal theory of names is that our present uses of a name, say `Aristotle', designate the famous Greek philosopher Aristotle, not in virtue of the various things we (rightly) believe true of him, but in virtue of a causal network stretching back from our uses to the first uses of the name to designate Aristotle. It is in this way that our present uses of a name "borrow their reference" from earlier uses. It is this social mechanism that enables us all to designate the same thing by a name. (1981, p.25).

This simple statement of the causal theory indicates what Devitt takes to be the basic elements of the theory. Reference, or designation, is explained in terms of d-chains (short for `designating chains'). There are three types of links in a d-chain: (1) groundings, which link the chain to an object, (2) abilities to designate, and (3) communication situations in which abilities are passed on or reinforced (i.e. reference borrowing).

In a grounding, Devitt tells us, a person perceives an object (preferably face to face), correctly believing it to be an object of a certain very general category. The grounding consists in the person coming to have `grounding thoughts' about that object as a result of the act of perceiving the object. A grounding thought about an object,

according to Devitt, "includes a mental representation" of that object brought about by an act of perception. The thought is one which a speaker would express using a demonstrative from that language. To use one of Devitt's examples: "Consider the case of our late cat. We acquired her as a kitten. My wife said, `Let's call her "Nana" after Zola's courtesan.' I agree. Thus Nana was named." (1981, p.26). In this instance, Devitt perceived the cat and correctly believed it to be a cat. Furthermore, it was paramount that Devitt saw it as a member of the very general category which contains cats. (I must admit, I'm not sure what this category would be: cat? animal? living animal? domesticated animal? house pet? single-headed entity?)[2] Having perceived it, Devitt was causally affected by it. As a result of seeing the object, and hence being causally affected by it, Devitt comes to have `grounding thoughts' about it, i.e. he comes to have mental representations of the object (and of perceiving the object?). For Devitt, having the appropriate mental representations is a necessary condition for having grounding thoughts.

The name, having been grounded, can now be used by speakers to refer to the object because a causal link has been established between the initial use of the name and the object, and a causal link can be established between later uses of the name and the initial use. For instance, having named the cat `Nana', Devitt could then exercise his new ability (to refer to Nana) by saying, `Nana is hungry.' He

could speak to his friends about Nana, thus enabling them, in turn, to refer to Nana. These friends (and their uses of `Nana´) are causally linked to Nana by perceiving (hearing) Devitt´s utterance, in which mention of Nana is made. Since to perceive is to be causally affected, they are now causally affected (in a sense) by Nana. These friends now have the ability to refer to Nana <u>because of this causal linkage</u>. The nature of the causal hook-up in this chain seems to be based on identity beliefs, says Devitt (i.e. beliefs we would express using identity statements). According to Devitt, non-initial uses of a name become grounded in objects by taking advantage of grounding thoughts. This process of taking advantage can be either direct or indirect, but in both cases advantage is taken by coming to hold identity beliefs. Direct advantage of grounding thoughts is taken by an identity belief containing a demonstrative representation like those in the grounding thoughts (e.g. `<u>that</u> cat is Nana´ or by definite descriptions). Indirect advantage of grounding thoughts is taken when a speaker has nondemonstrative representations of an object, i.e. by an identity belief that includes an already grounded nondemonstrative representation (e.g. `the F´, knowing that Nana is the F). In both cases of advantage being taken, it is the identity beliefs that lead to the representational thoughts and these identity beliefs link the thoughts associated with `Nana´ to the grounding thoughts and thus to Nana.

Closely connected with the notion of grounding and the uses of a designating term is the ability to designate. Devitt claims that "we should see a person's ability to designate with a term as, basically, his having thoughts that include mental representations which are both of the object and associated with the term" (1981, p.130). The ability to designate can be divided into the ability to designate *in thought* and the ability to designate *in speech* (or writing and so forth), with the former being basic. Says Devitt:

> For a person to have an ability in thought to designate an object, an ability associated with a physical type, is for him to have a set of thoughts including tokens which are grounded in the object and which dispose him to use the type. (1981, p.132).
>
> ...for a person to have an ability to designate an object with a sound type is for him to have (a) the power of speaking that type, and (b) a set of thoughts including tokens which are grounded in the object and which dispose him to speak the type. (1981, p.131).

So, Devitt's ability to designate his cat - if it is a speech ability - is the ability to utter certain sound tokens. The set of thoughts associated with that type (viz. sound) dispose him to speak it. Likewise, *mutatis mutandis*, for the ability to designate if it is a thought ability.

We have, then, the first two types of links in a designating chain. The third type of link is that of communication situations in which abilities to designate are passed on or reinforced (i.e. reference borrowing). In reference borrowing, says Devitt, the act of perceiving a

designation of the object by the term plays the role for the borrower that the earlier act of perceiving the object played for a person present at a grounding; it grounds the term in the object (1981, p.137). So, when Devitt comments to his friends that Nana is hungry, `Nana´ is a designational term, since it had underlying it a representation grounded in the object. On the strength of this utterance, Devitt tells us, the friends come to have certain thoughts and gain the benefit of that grounding. The friends have mental representations which are of the object in virtue of being causally linked to the object via the act of borrowing. Underlying these representations are designating chains grounded in the object in virtue of appropriate grounding thoughts and identity beliefs.

Criticisms of the Causal Theory

In response to the causal theory of reference, Searle has said little with respect to Kripke and almost nothing at all with respect to Devitt. He has, however, directed criticisms against aspects of the causal theory (e.g. proper names do not have senses) as far back as his 1958 essay and against the causal theory and Kripke in his most recent works. Among his criticisms are:

(1) The view that proper names have no senses is, minimally, counter-intuitive.

(2) Kripke´s distinction between speaker reference and semantic reference is bogus (as is Donnellan´s distinction between referential uses and attributive uses of definite descriptions).

(3) There are intuitive counter-examples showing that the causal theory of reference fails to give

necessary conditions for successful reference and fails to give sufficient conditions for successful reference.

(4) Kripke's (and Donnellan's) position is really a descriptivist one.

These criticisms need to be looked at in detail.

Criticism (1): The view that proper names have no senses is, minimally, counter-intuitive. In the context of existential statements such as `Princeton exists' or `Cerberus does not exist' proper names cannot be said to refer. If they did, the precondition of such statements having a truth value would guarantee its truth (in the case of affirmative existential statements) or its falsity (in the case of negative existential statements). This is because, says Searle, every existential statement states that a certain predicate is instantiated. An existential statement does not refer to an object and state that it exists, rather it expresses a concept and states that the concept is instantiated. If Cerberus does not exist and if `Cerberus' has no sense, then how can we make sense of `Cerberus does not exist'? Surely, at least intuitively, the statement `Cerberus does not exist' is true. But this can't be if `Cerberus' has no sense.

Another problem with the view that proper names have no senses is that such a view cannot explain the cognitive difference between identity statements of the form a=b and of the form a=a. That is, if proper names have no senses, but at best only referents, then the assertion `Everest is Chomolungma' could convey no more information than the

assertion `Everest is Everest.´ However, this is counter-intuitive, at least.

Criticism (2): Kripke´s distinction between speaker reference and semantic reference is bogus (as is Donnellan´s distinction between referential uses and attributive uses of definite descriptions). According to Searle, Kripke (1977) rejects - or reinterprets - Donnellan´s referential-attributive distinction as rather a distinction between speaker reference and semantic reference. For Kripke, if a speaker has a designator in his idiolect, then certain conventions of his idiolect determine the referent in his idiolect. This is the semantic referent of the designator. On the other hand, the object which the speaker wishes to talk about, on a given occasion, and which the speaker believes fulfills the conditions for being the semantic referent of the designator is the speaker´s referent of the designator. In Donnellan´s attributive cases, speaker reference and semantic reference coincide because the speaker´s intention is just to refer to the semantic reference. In the referential cases, however, the speaker´s reference and the semantic reference would be different if the speaker is mistaken in believing that they both determine the same object. For example, two speakers see Smith in the distance and mistake him for Jones. One speaker asks, `What is Jones doing´? The other speaker answers, `Raking the leaves´. Now, `Jones´ is the name of Jones, not of Smith. The semantic referent of `Jones´ is

Jones, but the speaker referent of `Jones´ in this case is Smith.

Kripke´s account couldn´t quite be right as it stands, says Searle, because in the `referential´ use the speaker need not even believe that the object referred to satisfies the expression he uses. For example, to avoid political persecution one might refer to the usurper of a throne as `the King´ even though one does not believe the usurper to be the king. Where Kripke goes wrong is in his attempt to analyze both speaker´s reference and semantic reference in terms of different kinds of intentions. Kripke states that in a given idiolect "the semantic reference of a designator (without indexicals) is given by a general intention of the speaker to refer to a certain object whenever the designator is used. The speaker´s referent is given by a specific intention, on a given occasion, to refer to a certain object." (1977, p.264; Kripke´s emphasis). Searle responds to this attempt:

> This is where the account bogs down. In the sense in which I really do have both general and specific intentions (e.g. I have a specific intention to drive to Berkeley tomorrow, and a general intention to drive on the right hand side of the rode, ceteris paribus, whenever I drive in the United States) I have no such general intentions about definite descriptions. If my use of definite descriptions required such general intentions I would have to form an infinite number of them since there are an infinite number of definite descriptions I am able to use and understand in my language. (1979, p.156).

Rather than speaking of speaker´s reference and semantic reference, Searle suggests that the real distinction to be

noted with respect to reference is that between primary and secondary aspects under which reference is made (which was discussed in chapter 1 above).

Criticism (3): There are intuitive counter-examples showing that the causal theory of reference fails to give necessary conditions for successful reference and fails to give sufficient conditions for successful reference.[3] Searle presents several counter-examples. `Madagascar´ was originally the name of part of Africa. Mistakenly, Marco Polo used the name to refer to an island off the coast of Africa. So, says Searle, the present uses of the name `Madagascar´ (by which we now mean to refer to the island, not the mainland) satisfy a causal connection that connects it with the mainland, but that is not sufficient to enable it to refer to the mainland. The use of names of abstract entities (e.g. `two´) or of `future´ entities (e.g. `the K car´ while still in the planning stage) are examples of reference in which no causal connection is necessary between an object and (the use of) a name.[4]

Criticism (4): Kripke´s (and Donnellan´s) position is really a descriptivist one. Says Searle:

> From the point of view of the descriptivist theory, what the causal analysis amounts to is the following: the "causal chain of communication" is simply a characterization of the parasitic cases seen from an external point of view. Let me try to make this clear. Kripke says that at each link in the chain of communication the speaker must have the intention, "when I utter `N´ I mean to refer to the same object as the person from whom I got the name `N´". The descriptivist says that one sort of identifying description that one can attach to a name `N´ is "the person referred to by

> others in my linguistic community as `N´". Both sides agree that this is not enough by itself: Kripke insists that the chain must terminate in an initial baptism; the descriptivist allows for a variety of ways in which it can terminate, of which an initial baptism is one. Where is the difference? As far as the issue between descriptivism and the causal theory is concerned there is no difference: Kripke´s theory is just a variant form of descriptivism. (1983, p.244).

Now, as with the earlier criticisms of Searle´s cluster theory, we need to ask: are Searle´s criticisms of the causal theory legitimate, and, if so, are they debilitating?[5]

Criticism (1): "The view that proper names have no senses is, minimally, counter-intuitive." As stated, the first counter-intuitive result of denying senses to proper names is to make (certain) existential statements nonsensical or paradoxically false. The second counter-intuitive result is that the apparent cognitive difference between a=a and a=b cannot be explained.

Donnellan (1974) and Devitt (1981) have responded to the first counter-intuitive result. Donnellan makes use of an "admittedly not well-defined" notion of a `block´ to sketch a means of dealing with the problem of negative existentials. For Donnellan, there is a `block´ in the history of the use of a name when events preclude any referent being identified. An example of a block would be the following: a child who once believed in Santa Claus comes to abandon this belief. What has the child learned? Says Donnellan:

> Our account is that he has learned that when in the past he believed something, for example, "Santa Claus comes tonight," and would have thought himself when saying this to be referring to someone, the historical explanation of this belief does not involve any individual who could count as the referent of "Santa Claus"; rather it ends in a story given him by his parents, a story told to him as factual. (1974, p.23).

The block, then, is the introduction of the name into the child's speech via a fiction told to him by his parents. Another example is the following: the Homeric poems, suppose, were written not by a single author, but by many people. Perhaps the poems were combined from a patchwork of many writings over time. Suppose further that someone later attributed these poems to a single author, Homer. If this were the historical explanation of our saying, for example, `Homer wrote the Iliad', then the block occurs at the point at which the person attributed the poems to Homer.

Armed with the notion of a `block', Donnellan proposes the following rule for giving truth conditions of existential statements containing a name:

> (R) If N is a proper name that has been used in predicative statements with the intention to refer to some individual, then `N does not exist' is true if and only if the history of those uses ends in a block.[6]

So we can assign truth values to negative existentials such as `Cerberus does not exist' even though we deny that `Cerberus' has a sense.

Devitt responds to the Searlean objection of negative existentials by proposing the following rule:

> If e_1 is a singular term, then $\ulcorner e_1$ exists$\urcorner$ is true_s if and only if there is an object a to which

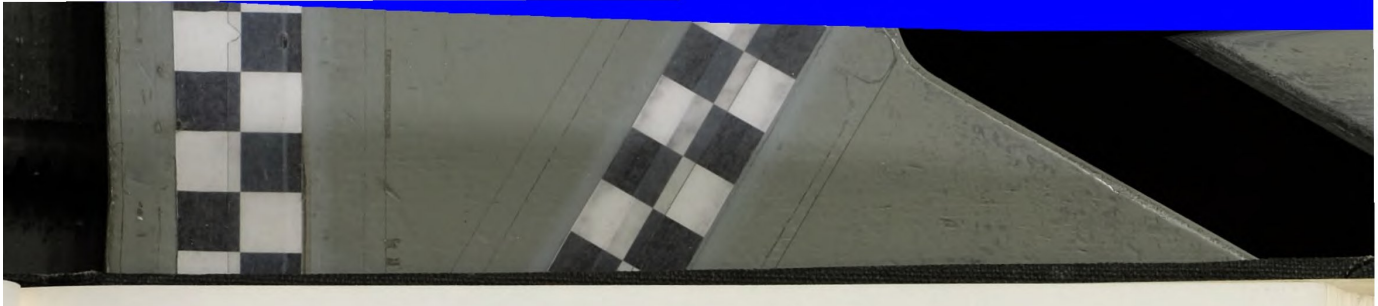
> e_1 m-refers$_s$ (where `true$_s$´ means "true relative to the assignment of sequence s," `refers$_s$´ means "refers relative to the assignment of sequence s," `m-true$_s$´ means "true$_s$ relative to a structure m which maps terms onto the world," and `m-refers$_s$´ means "refers$_s$ relative to a structure m which maps terms onto the world").[7]

Negative existentials are handled by the addition of a negation rule:

> If e_1 is a formula and e_2 is a negation symbol, then $\ulcorner e_2(e_1) \urcorner$ is m-true$_s$ if and only if e_1 is not m-true$_s$.

As with Donnellan´s rule (R), these rules do not analyze negative existentials in the sense of giving a synonym or translation of them, but they do offer a method of determining the truth conditions of such statements.

Kripke more directly addresses the second counter-intuitive result of names having no senses (viz. a=a and a=b would then have no cognitive difference, but they do). In laying out his argument for why he thinks that names do not have senses, he first attempts to clarify several notions and distinctions which he sees as important. Kripke argues that, contrary to popular usage, a prioricity is not interchangeable with necessity. Both a prioricity and a posterioricity are epistemic concepts, whereas both necessity and possibility are metaphysical concepts. To see that a prioricity is not interchangeable with necessity, consider the following example: Goldbach´s conjecture that every even number greater than two is the sum of two prime numbers is either true or false. Furthermore, if it is true (false), then it is presumably necessarily true (false).



However, none of us can be said to have any a priori knowledge about the answer to this question. As Kripke puts it, we don't know whether the conjecture is true or false, so we certainly don't know a priori whether it is true or false.

Having given some intuitive appeal for distinguishing a prioricity and necessity, Kripke goes on to suggest that some statements (such as Goldbach's conjecture) are necessary and a posteriori, while others (such as the horsepower example below) are contingent and a priori. In giving examples he brings up discussion of possible worlds and transworld identity. Possible worlds are stipulated by descriptive conditions we associate with them (and are, in a sense, says Kripke, no more than counterfactual situations). Transworld identity is the identity of an object across possible worlds. So, if we say that Nixon might have lost the 1968 Presidential election, we will be asserting a necessary or contingent statement depending upon what singular term is used and what is meant to be referred to. That is, some referring terms (for Kripke, all names, but not all definite descriptions) are what Kripke calls <u>rigid designators</u>, i.e. terms that designate the same object in every possible world in which that object exists. `Nixon' is a rigid designator because it refers to the same object in all worlds in which that object exists, but `the winner of the 1968 Presidential election' is not a rigid designator. Clearly, Nixon could not be anyone other than



Nixon, but the winner of the 1968 Presidential election could have been (and in some possible worlds, is) someone other than Nixon.

Now, this notion of rigid designation and the earlier distinction between a prioricity and necessity come together in the following way. Some statements, such as identity statements involving names, are necessary and a posteriori, while other statements, such as ones involving (some) definite descriptions, are (or could be) contingent and a priori. For example, to say `Siddhartha is Gautama Buddha´ is to make a statement that is known a posteriori. That is, we come to discover that `Siddhartha´ refers to the same object as does `Gautama Buddha´. (Or, `Hesperus´ and `Phospherus´, we find out, refer to the same object, even though we might believe them to refer to different objects.) However, if names are rigid designators, then the same object is picked out in all possible worlds in which that object exists. So `Siddhartha´ picks out the same object in all possible worlds and `Gautama Buddha´ picks out the same object in all possible worlds. Consequently, if the identity is true, then (since the same object is picked out in all possible worlds for each name) the identity is true in all possible worlds (i.e. the identity is necessarily true). The truth of the statement `Siddhartna is Gautama Buddha´ is (epistemically) a posteriori and (metaphysically) necessary.

On the other hand, the statement `One horsepower equals

the power required by one horse to displace 150 pounds by 220 feet in one minute´ is contingent and a priori. For Kripke, such a statement is contingent because it simply fixes the reference of `horsepower´ (and given a different horse in different possible worlds, the actual value might have been different than the value - 550 foot-pounds/second - in this world). The value in the actual world was picked out by a particular accidental property (the distance a certain mass could be displaced in a certain amount of time by a certain horse), and so the statement is contingent. Nonetheless, the statement is (now) known a priori. This is so because once the reference has been fixed, the definition holds. We don´t (need to) appeal to any evidence to know the truth of the statement.

Returning, then, to Searle´s criticism that the cognitive difference between a=a and a=b cannot be accounted for unless names have senses, we see that Kripke disagrees. Both identities, if true, are necessarily true - assuming we are dealing with names (i.e. rigid designators). On the other hand, while a=a is known a priori, a=b is known a posteriori, accounting for the apparent cognitive difference. However, Kripke has not introduced senses in accounting for this cognitive difference, hence Searle´s criticism fails.

Criticism (2): "Kripke´s distinction between speaker reference and semantic reference is bogus." Again, the reason Searle makes this claim is that he sees it as

assuming that in referring we have both a general intention to refer to a certain object (whenever the designator is used) and a specific intention to refer to a certain object (on a given occasion). Searle finds this assumption questionable, because, he says, we have no such intentions for definite descriptions.

Kripke anticipates this Searlean objection and responds (to some extent, anyway) in a footnote (1977, p.273, note 22). What he is trying to show is that reference is not subjective. That is, it is true that we as speakers mean to refer to a certain object when we use a referring term, but obviously we cannot refer to whatever we want when we use a referring term. A referring term has a history and is objective in the sense that within a language a given speaker's use of the term must conform to the other speakers's uses of that term. For Kripke, the notion of what words can mean and refer to is given by the conventions of our language and what words mean and refer to on a given occasion is determined by these conventions together with the intentions of the speaker and various contextual features. (This issue of objectivity versus subjectivity of reference will be dealt with in some detail later, so I will only say at this point that I think is Kripke is right. It is not clear to me, however, that Searle is lobbying for, or is committed to, subjectivity of reference. His objection seems to be more that on a given occasion we do not have any general intentions to refer in Kripke's sense.)

Kripke continues explicating his distinction by considering just the sort of cases that Searle mentions (e.g. the `the King´ case). Fearing the secret police, people may well use `the King´ to refer to a usurper or, humoring a lunatic, call Smith `Napolean´. Further, such cases shade into ironic or `inverted commas´ cases, such as `He´s a "fine friend"´ (spoken ironically) or `"Napolean" has gone to bed´ (spoken of Smith). The point of all these cases is that the more contextual phenomena one includes in the notion of speaker´s reference (such as irony, linguistic errors, deliberate misuses of language), the further one gets from any connection of the notion with semantical matters. Kripke does not deny, then, that many of these Searlean features are present, and important, in ordinary discourse, but he does deny that they play the role in semantic analysis that Searle seems to think they do.

Additionally, it seems to me, we could construct intuitive counter-examples to Searle´s objection. It seems we often `mistakenly´ refer to someone (e.g. call someone by the wrong name, or believe someone´s name is Smith when it is in fact Jones). In such cases we are often corrected (and even correct ourselves): `You mean Jones, don´t you? Smith was in Cleveland on that day´. Our response often exhibits our acceptance of a speaker reference-semantic reference distinction (e.g. `I´m sorry, I meant to say "Jones", not "Smith"´ or `I didn´t know her name was "Jones"; I thought it was "Smith"´). Such examples seem to

me to indicate that to whom I believe I am referring and to whom the (use of the) name refers is not always identical. (I should add that while I think Kripke is right on this point, I don´t think he has said anything to refute Searle´s `infinity of descriptions´ objection. The force of Kripke´s point and of Searle´s objection lies on their respective notions of intention. Kripke makes use of this notion, but, I think, does not flesh it out. A major difference between Searle and Kripke, it seems to me, is that Searle places far more emphasis on the role of intention in reference than does Kripke.)

Criticism (3): "There are intuitive counter-examples showing that the causal theory of reference fails to give necessary conditions for successful reference and fails to give sufficient conditions for successful reference." The case of reference to abstract or to `future´ objects is said to demonstrate the failure to give necessary conditions for reference and the case of Madagascar is said to demonstrate the failure to give sufficient conditions for reference.

As noted before, Kripke (at least) does not see himself as offering a theory wherein necessary and sufficient conditions are stipulated. To criticize him for not having done what he didn´t attempt to do seems puzzling. Of course, a Searlean reply could be that not only has Kripke not given necessary and sufficient conditions for reference, but - as the examples above illustrate - he can´t, and this is clearly a signal that the causal theory is weak.

In the addenda to Naming and Necessity Kripke explicitly responds to the Madagascar case. Says Kripke:

> ...a present intention to refer to a given entity (or to refer fictionally) overrides the original intention to preserve reference in the historical chain of transmission. The matter deserves extended discussion. But the phenomenon is perhaps explicable in terms of the predominantly social character of the use of proper names emphasized in the text: we use names to communicate with other speakers in a common language. This character dictates ordinarily that a speaker intend to use a name the same way as it was transmitted to him; but in the `Madagascar´ case this social character dictates that the present intention to refer to an island overrides the distant link to native usage. (1980, p.163).

Making this all clear, says Kripke, is a problem for further work, but minimally a present intention to use a name for an object and a present belief that the object is the only one having a certain property must be distinguished. Certainly, however, to say that the problem has not been thoroughly worked out does not entail, contra Searle, that it can´t be.

With respect to the objection that reference to abstract entities illustrates the failure of the causal theory to provide necessary conditions for reference, Kripke (and others) have said practically nothing. There are a variety of responses that could be made. One could deny that the supposed names of abstract entities are `really´ names. For example, one could deny that `two´ is a name. Such a denial might take the form of claiming that all such `names´ are really disguised descriptions. So, one might insist that `two´ can be translated into set-theoretic notation and defined in such a way as to eliminate the

`name´. Even if this were to work for numbers (and it´s not obvious that it would), it seems much less intuitive that this move could succeed for reference to `future´ entities. For example, a recent groom might exclaim, `The curtains in Larry´s room have got to go!´ where Larry is, as yet, an unborn (unconceived?) son. There is, at the time of the utterance, no Larry in which `Larry´ is grounded. Or, in the initial planning stages of the marketing and manufacturing of the K car, someone might say, `Good idea, Noseworthy! The K car could save this company!´ The K car might be, at the time of the utterance, nothing more than an idea in Noseworthy´s mind or the K car might be only in the initial blueprint stage. In any case, it is not at all clear that `Larry´ or `the K car´ (where I take `the K car´ to function as a name, not as a definite description) can be dismissed as disguised descriptions. A causal theorist might respond to such cases of supposed reference by taking a nominalistic stance with respect to the entity in question. That is, one could deny that such entities are real and so treat reference to them in the same manner as to other fictional entities, say, by appealing to Donnellan´s notion of a `block´. So, just as supposed reference to Santa Claus resulted in a block, supposed reference to the number two or to the K car might result in a block. In each case, statements containing the `names´ can still be assigned truth values even if there are not really objects being referred to. (This tack could apply not only to

`future´ entities, but to numbers as well, by insisting that what we refer to are not numbers, but numerals (or to specific tokens).) The cost of taking such a nominalistic position, however, might be too high (because of other difficulties) such that this sort of move would be unsatisfactory.

Another response to Searle´s objection might be found in Salmon (1981), though his comments were not directed to this issue. In looking at the notion of rigid designation, Salmon despairs that there are several distinct notions of rigid designation that can be found in the literature. The most familiar notion is that a designator is rigid if it designates the same thing with respect to every possible world in which that thing exists. Salmon suggests that this notion can be taken in two ways. One interpretation of a rigid designator Salmon dubs a <u>persistently rigid designator</u>. A designator is persistently rigid if it designates the same thing with respect to every possible world in which that thing exists and which designates nothing with respect to possible worlds in which that thing does not exist. Another interpretation of a rigid designator Salmon dubs an <u>obstinately rigid designator</u>. A designator is obstinately rigid if it designates the same thing with respect to every possible world, period. Obstinate designators, Salmon tells us, "may be insensitive to the question of whether their designata exist in a given possible world; they designate the same thing with respect

to every possible world, whether that thing exists or not." (1981, p.34). Further, Salmon contends that Kripke may very well intend for his notion of rigid designation to be that of obstinate rigid designation, and points to the following note in Naming and Necessity as evidence:

> Concerning rigidity: In many places, both in this preface and in the text of this monograph, I deliberately ignore delicate questions arising from the possible nonexistence of an object. ...I say that a proper name rigidly designates its referent even when we speak of counterfactual situations where that referent would not have existed. Thus the issues about nonexistence are affected. (1980, p.21, note 21).

The point of this, with respect to the Searlean objection of reference to abstract or `future´ entities, is that if we take names (of numbers, say) as obstinate designators, then there is no problem for the causal theory. Reference can be made whether or not the name refers to anything which exists in the world, if the name designates obstinately. Since the actual world is obviously a possible world, and if obstinate designators designate their objects in all possible worlds whether or not the object exists in those worlds, then, if names of numbers are obstinate designators, they designate their objects even if they don´t exist. For Salmon, names of numbers (or of numerals) do designate obstinately (at least for the causal theory). He states:

> As is most obvious in the clearest case of a nondescriptional singular term - the free variable whose denotation is fully and directly determined by an assignment of values to variables - there is no reason to suppose that the denotation of a nondescriptional term should change with respect

> to different possible worlds. ...Proper names do not denote by, so to speak, rummaging through the individuals in a possible world seeking anyone or anything uniquely meeting certain conditions. Hence, there remains no reason to expect denotation to vary from one world to the next. Instead, one may think of the denotation as being fixed for all time and for all possibilities by an initial dubbing ceremony, or an assignment of values to variables, etc. (1981, p.33, note 35).

Since the actual world is a possible world, there is also no reason to think that proper names denote in it by "rummaging through individuals...seeking anyone or anything uniquely meeting certain conditions." If this is so, then reference to abstract or `future´ entities should be no problem for the causal theory of reference.

Another response to Searle´s objection might be - and this seems to be Kripke´s response - that the initial grounding of the causal chain may well be achieved by means of a description which fixes the reference of a name. Once the reference of a name has been fixed by the use of a description, then future uses of the name will link up to that baptismal event. I say that this seems to be Kripke´s response because of his comments like the following:

> Neptune was hypothesized as the planet which caused such and such discrepancies in the orbits of certain other planets. If Leverrier indeed gave the name `Neptune´ to the planet before it was ever seen, then he fixed the reference of Neptune by means of the description just mentioned. At that time he was unable to see the planet even through a telescope. (1980, p.79, note 33).

A page later Kripke notes that the reference could be fixed even if the description used turned out to be false, as when `Phospherus´ had its reference fixed by the description `the

morning star´ (which is false because Phospherus (=Venus) is not a star at all). By allowing that the reference of a name can be fixed by a description, then, there is no problem for the causal theory to refer to abstract or `future´ objects. Since these were the sort of cases which Searle proposed as counter-examples to the causal theory as being incapable of providing necessary conditions for successful reference, it seems that the criticism fails.[8]

Criticism (4): "Kripke´s (and Donnellan´s) position is really a descriptivist one." Searle claims that the `causal chain of communication´ is simply a characterization of parasitic cases of reference seen from an external point of view. That is, for Searle, the causal (or historical) chain plays no explanatory role in an account of reference (or, in Kripke´s account of reference). What does matter in an explanatory role is the transfer of intentional content from one speaker or speech act to another. Reference is secured, says Searle, under Kripke´s account in virtue of the intentional content in the mind of the speaker who uses a particular designating expression. After an initial baptism, subsequent `intentional contents´ are parasitic on the prior ones for achieving reference. Of course, an omniscient observer could give an historical (causal) account of the chain of reference, and hence describe historically (causally) how a later use of the expression refers, what secures reference is the descriptive, intentional content.

Now, even if Searle were correct that this is `really' what is going on with Kripke's account, it is a funny sort of criticism. That is, he seems to be saying that Kripke's account is fine as it stands, but that Kripke is mistaken in thinking he has offered anything new, anything contrary to a descriptivist theory. Rather, he has wrongly identified or overemphasized one aspect of his theory as unique and essential.[9] However, it does not seem that Kripke would agree with Searle's assessment of Kripke's view. While admitting that intentionality plays a role in reference and even that the descriptivist view might suffice for explaining reference in limited contexts (such as an initial baptism), Kripke clearly denies that the descriptivist account is adequate. His view is not descriptivist because he insists that in general "our reference depends not just on what we think ourselves, but on other people in the community" (1980, p.95). It is simply false that for Kripke reference is secured in virtue of the intentional content in the mind of the speaker who uses a particular designating expression. Indeed, one of the points of making the speaker reference-semantic reference distinction is to show that reference is not dependent upon the intentional content in the mind of the speaker who uses a particular designating expression.[10]

Conclusion

Just as in chapter 1 above I was not trying to claim that Searle is right and Kripke wrong when it comes to

explicating reference, so now I am not trying to claim that Kripke is right and Searle wrong. What I have tried to do is to fairly present both the cluster theory and the causal theory, criticize each from the other's perspective and respond to those criticisms from within the two theories. That is, I have tried to capture (in an admittedly glossing manner) the debate between the received theories of reference as I see it and I have tried to let the authors of those theories speak for themselves as fully as possible (given what they have said in print).

I see the debate between the two theories as a family squabble. It seems to me that both theories are right in some respects and wrong in others. The important issues to me, though, are the shared assumptions and commitments underlying the two theories. It strikes me that both theories are committed (in a sense I hope to explain) to essentialism, realism and a correspondence theory of truth. In the remainder of this work I want to address just what these underlying positions are and to show how both the cluster theory and the causal theory are committed to them. I will then suggest a theory of reference (incorporating, among other things, what I see as being right about the cluster and causal theories) which does not share these underlying commitments.

NOTES

[1]It is noteworthy that at the `initial baptism´ stage the description theory, for Kripke, might be feasible. He states in a footnote: "...the primary applicability of the description theory is to cases of initial baptism." (1980, p.96, note 42). Two things, however, must be emphasized. First, the description used is not synonymous with the name it introduces but rather fixes its reference. Second, most cases of initial baptism are not of the sort which originally inspired the description theory. Usually a baptizer is acquainted in some sense, according to Kripke, with the object he names and is able to name it ostensively. (In a later addendum, Kripke remarks that these brief comments on initial baptism give a somewhat oversimplified picture. He stresses here that "there need not always be an identifiable initial baptism" (1980, p.162), though cases of such a lack of an identifiable initial baptism are rarer for proper names than for other cases, such as natural kind terms.)

[2]Devitt is somewhat vague here, perhaps unavoidably. He states: "...we insist only that the object be in the same very general category as it is taken to be. There is an element of arbitrariness in our determination of these categories." (1981, p.63). I find this quite unhelpful. Devitt claims that if we believe we are referring to a cat, but the `cat´ is in fact a mongoose, then we have referred to a mongoose, whereas if the `cat´ is a shadow, then we have referred to nothing. Devitt strikes me as rather cavalier about this issue, and it is one that I see as important. I will return to it later in discussing the underlying commitments of the alternative theories of reference.

[3]Others, e.g. Evans (1973) and Steinman (1982), have made similar criticisms. Kripke himself acknowledges that in his `better picture´ one might never reach a set of necessary and sufficient conditions. In fact he states: "I want to present a better picture without giving a set of necessary and sufficient conditions for reference." (1980, p.94). However, other proponents of the causal theory, e.g. Devitt, seem more enamored with establishing such conditions.

[4]Searle does not need abstract or `future´ objects to show that the necessity condition is faulty. He gives another example: "I can, for example, refer to M Street in Washington [D.C.] simply because I know that there is in that city an alphabetical sequence of street names, `A´, `B´, `C´, etc. I needn´t have any causal connection with M Street to do that." (1983, p.239). (Of course, we must also assume that Searle knows the alphabet and that he knows that the street names go up to, at least, M Street, say, by

knowing that there is an N Street.)

[5]To my knowledge no one has responded in print directly to Searle, though, as noted below, causal theorists have considered some of the criticisms in some form or other.

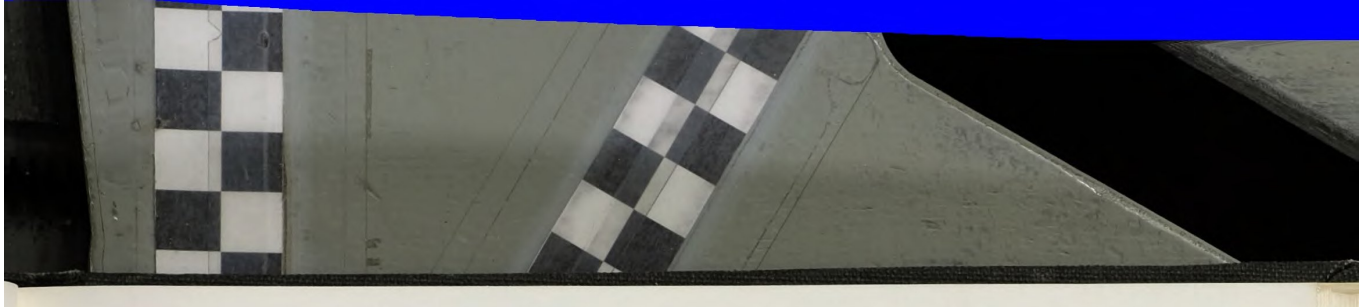
[6]Donnellan considers several objections to rule (R), which are not essential to the present point, which is to show how Donnellan could respond to Searle's criticism. Suffice it to say that Donnellan considers himself to have successfully defended rule (R) and his position from the objections that he himself raises.

[7]For Devitt, a sentence is true if and only if it is m-true for every structure that corresponds to it (i.e. for every structure in which each of the sentence's designational terms partially designates the object it m-designates and applies to the objects it m-applies to).

[8]There are many cases of reference being fixed by a description, especially for `theoretical' entities. For example, only very recently have scientists at CERN claimed to have `seen' the `top' quark, although it had been dubbed as such, predicted to exist and sought for years.

[9]This statement should be tempered somewhat. Searle does see differences between the descriptivist view and the causal view. To the extent that Kripke's view works, says Searle, it works because it is descriptivist. Among the differences, Searle lists three that he sees as noteworthy: (1) for the causalists the transfer of intentionality in the reference chain is essential for proper names, for the descriptivists it is incidental; (2) for the causalists the only intentionality which secures reference is that each speaker intends to refer to the same object as the previous speaker, for the descriptivists this is not the case, other `intentionalities' are important; (3) for the causalists parasitic identifying descriptions are paramount, for the descriptivists they are not. Whether or not Searle is correct in his assessment here (and I don't think he is) is not important for the present concerns.

[10]This point, just as some earlier ones related to criticisms of Searle's view, involves the issue of private reference versus public reference, which will be discussed later.



Language is a kind of code dependent upon the life rhythms of the species which originated the language. Unless you learn those rhythms, the code remains mostly unintelligible. (Frank Herbert, Whipping Star)



INTRODUCTION

In this second part I want to expose and analyze underlying commitments of the received theories of reference. In particular I want to argue that both the Searlean cluster theory and the Kripkean causal theory are committed to - or, at least, presuppose - certain views with respect to the similarity relation and a principle of individuation. Commitments to certain views there are exhibited in subsequent commitments to essentialism, realism and a correspondence theory of truth. However, before turning to the task of laying out what I take these various notions to be and showing how the received theories are committed to them, I want to look more at the received theories as they have been presented in the first two chapters. I want to argue that even prior to considering any underlying commitments, there is reason to believe that the two theories are not nearly so far apart as the debate in the literature would seem to indicate. I also want to make some initial observations and criticisms about the received theories before going into the more basic observations and criticisms which I believe are lethal to those theories.

First a review. Minimally, much of the focus and force of the criticisms by the causal theorists of the cluster

theory of reference (and of the descriptive theory in general) is that it seems to allow a speaker's intentions to play too key a role in the business of reference. As Kripke says, "It seems to be wrong to think that we give ourselves some properties which somehow qualitatively uniquely pick out an object and determine our reference in that manner." (1980, pp. 93-94). Kripke's speaker reference-semantic reference distinction and his proposed counter-examples to Searle's position are designed to address this very point. Reference is not the sort of thing, say the causalists, that a speaker secures simply by uttering an expression and intending to pick out some unique object. Rather, reference is a public, historical phenomenon. Merely intending to pick out X while saying `Y' is not enough to refer to X (especially if other speakers have used `Y' to pick out Y and `X' to pick out X). Whether we like it or not, `Gödel' has a history which is intimately tied up with Gödel and not with Schmidt.

On the other hand, Searle has claimed that the causal theory is nothing more than a descriptivist theory in disguise. The key feature of the theory - that which secures reference for the causal theory - is the intention to refer to the same object as that from whom the speaker learned or acquired the name. Granted, one could describe an historical chain of the uses of a name, and so this is a public phenomenon, nonetheless, a speaker's ability to refer to an object by (the use of) a name depends upon the

speaker's intention to in fact designate the same object by that name as did the person from whom the speaker learned or acquired the name[1] (otherwise the speaker's present use of the name would not become part of the historical chain, but would be part of some other historical chain stretching back to a different object or to nothing at all).

On the surface, then, the two theories seem clearly at odds. The Kripkean counter-examples show - or are intended to show - that the descriptions which a speaker associates with a name may be totally inadequate for or even irrelevant to picking out the actual referent of a name. Indeed, it is held that a speaker could legitimately use a name to refer to an object and yet have no (true) descriptions associated with that name which would uniquely pick out that object. The Searlean counter-examples show - or are intended to show - that an historical chain linking a speaker's use of a name and an object is inadequate as an explanation or theory of successful reference. It is held that there are legitimate cases of successful reference (such as to abstract objects) for which historical chains are irrelevant (and even nonsensical). For Searle it is the case that at least one member of the disjunctive set of descriptions associated with the use of a name with an object must be true of the object for reference to successfully occur; for Kripke this is not the case.

Despite the apparent fundamental differences between these two theories, I want to claim that they are much

closer in agreement than they at first appear. Much of the dispute seems to be centered (at least implicitly) on the issue of public versus private reference. That is, as noted earlier, Kripke has accused Searle of, in effect, seeing reference as a private affair for any given speaker. I think that given much of what Searle has said Kripke is right, but I want also to accuse Kripke of the same thing. In spite of Kripke's insistence that reference depends "on other people in the community, the history of how the name reached one, and things like that" (1980, p.95), the causal theory is just as compatible with, and in the manner in which it has been presented by its adherents, just as committed to private reference as is the cluster theory. Before directly supporting this claim, however, I want to present some initial observations and criticisms of the received theories, which I hope will give a better background for a discussion of public and private reference.

NOTE

[1]Or, in the case of baptisms, a speaker's ability to refer to an object by a name depends upon the speaker's intention to designate that object by (the use of) the name (e.g. for Devitt, by having the mental representation of the object).

CHAPTER THREE

CONVERGENCE OF THE RECEIVED THEORIES

Are the cluster and causal theories as divergent as they are usually portrayed as being? I think not. As I will argue later, they share fundamental underlying commitments, but even on a more `surface´ level, the theories do not seem so far apart. What has been said to be unique to the cluster theory is that speakers have a set of identifying descriptions associated with a name, the disjunction of those descriptions being analytically true of the object referred to by the name. However, in several places Searle remarks that, as far as he is concerned "the issue is not really about analyzing proper names in words at all." (1983, p.233). Some cases, for example reference for children (who often learn names before other expressions), are such that the only `identifying description´ a speaker might have that he associates with the name is simply the ability to recognize the object. A young child, saying `Mama´ might well have no descriptions at all associated with the name, and indeed `Mama´ might have no significance to the child except when Mama is in sight and can be pointed to. For Searle, "pointing is precisely an example that fits [the descriptivist´s] thesis, since pointing succeeds only in virtue of the intentions of the pointer." (1983, p.233).



That is, for Searle, what is fundamental for reference is that the speaker intends to pick out an object uniquely by the use of a name, and, apparently, as long as the intention is there, the `description´ used to pick out an object may not have to be verbal. For Searle, ostension seems to count as a means of picking out an object. This sounds very much like Kripke´s view that descriptions can be used to fix the reference of a name and that, a la Devitt, in the presence of an object we can name an object by having the appropriate `mental representations´ while uttering the name. In fact, ostension has seemed in some ways as much a paradigmatic example of the strength of the causal theory as of the weakness of the cluster theory. Naming by ostension has been seen as a clear case in which a name is used but no description (or disjunction of descriptions) associated with the name is either necessary or sufficient for reference to occur. Now, we see Searle arguing that naming by ostension is paradigmatic of the cluster (descriptivist) theory! This issue of ostension points to a focus of commonality for the cluster and the causal theories: intentionality.

Two of Searle´s seven rules of reference employ intention in successful reference: Rule 5 [S intends that the utterance of R will pick out or identify X to H] and Rule 6 [S intends that the utterance of R will identify X to H by means of H´s recognition of S´s intention to identify X and he intends this recognition to be achieved by means of H´s knowledge of the rules governing R and his awareness of

C]. A key element of Searle's theory is that a speaker intend to pick out an object by (the use of) a name. (Kripke's objection is that this is not sufficient to guarantee successful reference, because the speaker might intend to pick out one object by a name but in fact might pick out a different object by that name.) As just noted, we see that for Searle this intention to refer to an object by (the use of) a name does not entail that any verbal descriptions must be employed by the speaker; pointing is fine. So, cases where the speaker supposedly associates no descriptions with the name (as, for example, the cases in chapter one) are no trouble for Searle because in those cases rules 5 and 6 of his theory are violated (i.e. the intentional element of the speech act of referring is missing). My neighbor, then, does not refer to Gödel simply because she uttered the name `Gödel'. The fact that the name `Gödel' has an historical chain stretching back to an initial use of the name (to refer to Gödel) is irrelevant. Furthermore, just as for Kripke, for Searle, intending to refer to an object by (the use of) a name is not sufficient to actually refer to that object. If none of the descriptions in the disjunctive set of descriptions which a speaker associates with a name uniquely pick out an (otherwise independently located) object, then merely intending to pick out that object by (the use of) a name does not secure reference. I do not and can not, for Searle, refer to Aristotle by uttering `Aristotle' simply

because I intend to refer to him. I must have at least one description (albeit not necessarily a verbal one) which is true of Aristotle in order to pick him out (i.e. the primary aspect under which reference is made must succeed).

In chapter one I presented two cases for which I said that the cluster theory denies (but the causal theory asserts) reference takes place. The first case was my neighbor complaining to her friend, "My weird neighbor kept me up last night yelling something about `that damn turtle´ or `Gödel´ or something like that. I had to pound on the wall to get him to quiet down." The second case was a stranger asking me, "Who is Gödel?" after hearing me mention Gödel´s name. For both cases I claimed that under Searle´s theory reference does not occur (and the reason why is that in both cases the speaker did not intend to refer). I also claimed that under the causal theory `devoid of descriptivist assumptions´ reference does occur. I now want clarify those claims. First, under what I will call a `pure´ cluster theory of reference (i.e. a theory in which a speaker´s intention to uniquely pick out an object by a name is a necessary and sufficient condition for reference to that object to occur), both my neighbor, in the first case, and the stranger, in the second case, would not have referred (for the reasons I stated in chapter one). However, Searle is not a `pure´ cluster theorist in that sense. While I do think that he would claim that in the first case, my neighbor did not refer to Gödel, I think in

the second case, he would allow (insist?) that the stranger did refer to Gödel. This is borne out by his claim that "there are uses of names where one is almost totally dependent on other people's prior usage to secure reference." (1983, p.261). In such a case, the stranger could secure reference through my use of the name `Gödel' to refer to Gödel. (This move by Searle brings up two important points. First, it makes his position sound more and more like the causalist view. Second, it points to the issue of the publicity of reference borrowing and reference in general, an issue which will be explicitly addressed in chapter four.)

Besides clarifying my initial strong claim about the cluster theory on the two cases (viz. my neighbor and the stranger), I want to clarify my claims about the causal theory with regard to these cases. Under what I will call a `pure' causal theory of reference (i.e. a theory in which a causal connection between an object and (the utterance of) a name is both necessary and sufficient for successful reference), both my neighbor, in the first case, and the stranger, in the second case, would have referred (because the historical, causal chain linking their use of the name `Gödel' with Gödel would have been in effect). However, neither Kripke nor Devitt are `pure' causal theorists in this sense, since both, it seems, would deny (at least for the first case) that reference has occurred. Kripke explicitly states (in his "rough statement" of a "better

picture" of reference): "When the name is `passed from link to link´, the receiver of the name must, I think, intend when he learns it to use it with the same reference as the man from whom he heard it." (1980, p.96). Devitt is not nearly so straightforward as Kripke about admitting intention into the causal theory. In fact, Devitt seems to go to great lengths to avoid committing the causal theory to intentions. He states:

> It is tempting, and I think correct, to say that when a person uses a name he (mostly) intends to designate whatever was designated by the person from whom he borrows his reference. ...Such intentions seem as much in need of explanation as designation itself. I do not talk of these intentions but capture the intuitions in another way: the tokens of the name a person produces are tied to the token from which he borrowed his reference by causal links (of the sort discussed) between his mental representations of the object. (1981, p.138).

So, even though not actually using intentionalistic language, Devitt nonetheless wants and needs to capture the intuitions of such language in order to guarantee that different speakers are in fact referring to the same object when using a given name.[1] My point here is not to debate whether talk of intentions is necessary in dealing with reference, but to indicate that advocates of both the cluster theory and the causal theory invoke it (or, at least, invoke the same intuitions that names are not used helter skelter in referring).

Not only does Kripke seem to indicate that a necessary condition for successful reference is the employment of the `proper´ intention (to refer to the same object by a name as

the person from whom one acquires the name), but he also seems to see the cluster theory as appropriate in certain contexts, such as in the fixing of reference (say, during a baptismal event). In Kripke's own example of the naming of Neptune he acknowledges that there may be cases where the cluster-of-descriptions view seems to make sense.

Both of these considerations (the requirement for Kripke of the `proper' intention to refer and the admission of the possibility of using a name to fix the reference a la Searle) indicate that the causal theory and cluster theory might be closer in agreement than initially thought. But there is more. As noted in chapter one, Kripke concedes that there may be cases in which `X' is used to refer to Y. (For example, if someone asks, `What did Aristotle have in mind here?', the speaker is referring (or, for Kripke, referring) to whomever wrote the passage even if it turns out not to have been Aristotle.) With that concession, Kripke remarks that the cluster theory would be "applicable in a wider class of cases" than he originally thought (even though he still believes it to be ultimately false). Whether or not the cluster theory is false, the point here is that again Kripke seems to be softening his blanket opposition to the cluster theory.

Another sign of the convergence of the cluster and causal theories is Kripke's response to the Searlean counter-examples to the causal theory involving reference to abstract and `future' objects. Recall, Kripke's apparent

way out of the difficulty of referring to such objects for the causal theory is to allow that descriptions can fix the reference of a name even if there is no causal hook-up to an object. Reference is secured, then, but not in a `purely´ causal way, and indeed Kripke´s response seems to fit in quite easily with Searle´s theory.

Devitt, too, in a brief discussion of reference to `future´ objects sounds like Kripke (and like Searle). Says Devitt:

> Consider, for example, the situation where there exists a plan or blueprint for a ship or building. We could introduce a name for that future object and it would seem to function like an ordinary designational name. This requires a small modification in our theory: the network for a name may be grounded initially not in the object itself but in some plan to produce the object according to certain specifications. Later, of course, the network becomes grounded in the object so produced.
>
> Those who make these claims about naming future objects [viz. that the causal theory can´t account for reference to such objects] are struck by the oddity of now naming, say, the first child born in the twenty-first century. The oddity arises, I suggest, because we could not have any particular object in mind using the names so introduced. This is striking because we mostly do have an object in mind in using a name. However, sometimes we don´t... . (1981, pp.59-60).

This "small modification" of the theory sounds more like a major concession to the cluster theory than a minor adjustment in the causal theory.

Now, what is the point of all of this? The point is that even at the level of what the cluster and causal theories present explicitly as their positions, they are not so far apart. Both Searle and Kripke assert that intending

to refer to a given object by the use of a name (and indeed intending to refer to the same object as the person from whom one acquires the name) is a necessary condition for successful reference. Both deny that such intention is a sufficient condition for successful reference. Both see ostension as paradigmatic of a correct theory of reference. Both see the use of reference fixing descriptions as legitimate modes of referring. Kripke´s insistence that we "use names to communicate with other speakers in a common language" (1980, p.163) sounds like Searle´s claim that we "need to make repeated references to the same object, even when the object is not present, so we give the object a name" (1983, p.231). Both deny (as in the `Madagascar´ case) that an historical link between an object and a name is sufficient to secure reference. Both deny (as in the reference to abstract objects) that a causal link between an object and a name is necessary to secure reference.

With all of these similarities between the two theories, we end up getting comments like the following:

> Since the point of having proper names is just to refer to objects, not to describe them, it often doesn´t really matter to us much what descriptive content is used to identify the object as long as it identifies the right object, where the "right object" is just the one that other people use the name to refer to. (1983, p.250).

> There may be cases...where some man really gives a name by going into the privacy of his room and saying that the referent is to be the unique thing with certain identifying properties. (1980, p.94).

The first, `anti-descriptivist´, quote is, of course, Searle´s and the second, `pro-descriptivist´, quote is, of

course, Kripke's. But if these two theories actually share many common features, what, then, is the difference between them? In the way it has been presented in the literature (and primarily from the causalist perspective) the difference is that the cluster theory is supposedly committed to private reference and fails to account for the historical, public nature of reference. I now want to turn to this issue and I want to claim that even here the two theories agree; they both at bottom allow private reference. To the extent that they do, the distinction between the theories is blurred even more and the received theories of reference look more and more like a received theory of reference.

NOTE

[1]Notice the philosophical gymnastics in which Devitt engages to avoid talk of intentions. Designation involves three types of links in the designating-chain: (1) groundings, (2) abilities to designate, and (3) communication situations in which abilities are passed on or reinforced. The grounding consists in the person coming to have "grounding thoughts" about that object as a result of the act of perceiving the object. "A grounding thought about an object includes a mental representation of that object brought about by an act of perception. The thought is one which a speaker of a public language would express using a demonstrative from that language... ." (1981, p.133). About the ability to designate, Devitt states that his strategy is to tie an ability to an object and a term in virtue of their role in bringing about the relevant mental representations. We have (at least) two abilities: abilities in thought (which are basic) and abilities in speech (or writing, etc.). An ability to designate an object in thought is to have thoughts including tokens which are grounded in the object. Now, if, as Devitt claims, the notion of intention is in as much need of explanation as is the notion of designation, then I want to say likewise for mental representation and his conception of thoughts. While he never gives an analysis of what a mental representation is, he does say the following about thoughts:

> A thought consists of an attitude to a sentence in the language of thought. I continue my former practice of ignoring the fact that most of the thoughts we have we never entertain [?!]; also of using the word `thought´ to refer to the sentence only... . Now a thought token will be `about our cat´ in the appropriate sense if it includes a token - a mental representation - that designates her. That token in thought will designate her if she is in fact the object in which the token is grounded. (1981, p.131).

I, for one, do not find this a great improvement over talk of intentions. (Devitt´s prohibition against talk of intentions is later relaxed, or forgotten, in his discussion of fictional discourse. He introduces an `F operator´ to indicate sentences that "can be true even though the contained singular terms are empty" (1981, p.130). What determines whether a given sentence token is or is not an F-sentence (i.e. one with an F operator)? Says Devitt: "That is determined by what the speaker had in mind, meant, or intended." (1981, p.181).)

CHAPTER FOUR

PRIVATE REFERENCE

What I mean by `private reference´ will, I hope, become clear in the ensuing discussion. Initially, what is meant here by the notion of private reference is that a speaker can refer to an object without any necessary conceptual or linguistic link to other speakers, i.e. that a speaker can refer in a private language. So, a speaker can refer to X (e.g. Neptune) simply by, say, uttering a designating expression (e.g. `the object which caused such and such discrepancies in the orbits of certain planets´) and having the intent to pick out X. Before showing that both the cluster theory of reference and the causal theory of reference accommodate (and, in the way they are presented, presuppose) private reference, more needs to be said about the notion of a private language.

There is a vast literature on the notion of a private language[1] and along with this literature has come various conceptions of what a private language is (or could be). One conception is that a private language is one that, as a matter of fact, only one person speaks, even though it could be taught to others. (Many of us as children have `invented´ our own private languages - which we quickly



taught to and conversed in with only our best friends.) Another conception of a private language is one that no other speaker could understand and that could not be taught; in principle, only the speaker can understand this language. A third conception of a private language is one that is (and can be) understood only by the speaker because it is a language used to talk about `private experiences´ (e.g. the speaker´s particular feelings, moods, etc.). The first conception of a private language is, I think, not philosophically interesting, and is not the sense of a private language which I claim is incorporated with the cluster and causal theories of reference. The second conception of a private language also is, I think, not philosophically interesting because it seems to be simply a glossing over of the third conception. I say this because when one investigates why a language is (would be) in principle unteachable, it turns out that it is because the terms in the language (are meant to) refer to private experiences. This is the notion which Wittgenstein has when he asks:

> But could we also imagine a language in which a person could write down or give vocal expression to his inner experiences - his feelings, moods, and the rest - for his private use? - Well, can´t we do so in our ordinary language? - But that is not what I mean. The individual words of this language are to refer to what can only be known to the person speaking; to his immediate private sensations. So another person cannot understand the language. (1953, #243).

Now, I want to extend this notion of a private language which refers to private experiences to include referring in

general. That is, I want to argue that the primary private element in such a language isn't so much the objects referred to (e.g. feelings, moods, etc.), but is the act of referring (and of naming). It is the act of, or, perhaps, the experience of, referring (naming) that is private, in the sense of being unique to the speaker. What is private with respect to my talk about my pain is not so much that it is talk about my pain, but that it is talk about my pain. When Wittgenstein says: "The individual words of this language are to refer to what can only be known to the person speaking; to his immediate private sensations," the emphasis (at least for my notion of privacy) is on `refer' not on `sensations.' (This will hopefully become clearer below, as I criticize the notion of private reference.) There does not seem to be any clear significant difference between a speaker who "gives a name by going into the privacy of his room and saying that the referent is to be the unique thing with certain identifying properties" when that unique thing is a headache or when that unique thing is Neptune. The point is that a speaker, outside of a linguistic community, can refer to objects. This is the sense of private reference which I want to claim is not only part of the cluster theory of reference, but is also part of the causal theory of reference.[2]

I want to suggest two arguments showing that the Searle and Kripke are both committed to private reference. The first argument involves the nature of the examples and

counterexamples which they employ; the second argument involves the inclusion of intentionality as a (necessary) component of reference for both of them.

The very examples and counterexamples which both Searle and Kripke employ, I want to claim, show their commitment to private reference. Consider Searle's discussion of reference to Aristotle and his insistence that at least one description in the disjunctive set of descriptions associated by the speaker must be true of an object for the name to refer to that object. As Kripke correctly points out, this view is clearly mistaken, for there are many cases in which the descriptions in the disjunctive set all seem to fail, yet our intuitions are that reference has successfully occurred. While Searle's primary-secondary aspect distinction may well work as a defense in some cases, I think that it, too, fails because the problem is simply moved back one step. Reference at the level of the primary aspect can still suffer the Kripkesque problems. The important point here for my concerns is that Searle's original position stresses the descriptions associated by a single, given speaker and his counter-move to Kripke's objection is still at the level of reference made by a single speaker without any apparent need to appeal to the language community. While this is not a definitive argument showing that Searle is committed to private reference, it is, I think, a fairly reliable indication.

On the other hand, for all his talk of the publicity of

reference (e.g. semantic reference), Kripke, I think, also shows an affinity to private reference. Consider his Feynman example. The example works against Searle only to the extent that Searle is not allowed to appeal to other speakers in the community for reference to successfully occur, as I mentioned in chapter one. That is, Kripke thinks that a given speaker might still refer to Feynman even though none of the descriptions associated by that speaker with the name `Feynman´ are true of Feynman. This strikes me as a clear indication that Kripke intuitively sees reference as something that can be done by a single, given speaker independent of a language community. While his example works against Searle, it fails if we allow the speaker to appeal to the language community for reference to occur. I agree with Kripke that one might refer even though all of the descriptions which one associates with a name might fail to pick out the intended object of reference (e.g. all of the descriptions which I associate with `Feynman´ might fail to pick out Feynman). However, I disagree with Kripke if he thinks that one might refer even though all of the descriptions which the entire language community associates with a name might fail to pick out the intended object of reference. Quite simply, we can´t all be wrong and still have reference occur. I will try to defend this claim throughout the remaining chapters. My main point here is to suggest that both Searle and Kripke offer examples which presume the plausibility of private reference

(and this, I suggest, weakens both their views). I now want to turn to the second major argument that Searle and Kripke are both committed to private reference, viz. the appeal to intentionality.

Intentionality

The very inclusion of intentionality as a (necessary) component of referring seems to be a commitment to privacy in the above sense, as it is obviously something that a speaker can do by "going into the privacy of his room." That is, the ability to stipulate that a name refers to an object by - at least in a baptismal event - uttering the name and intending to pick out a unique object seems to be something that can be done by a conceptually isolated speaker. (I will discuss, at the end of this chapter, whether intentionality is necessarily private. That is, I will consider the objection (to my claims above) that the inclusion of intentionality is not a commitment to privacy because there is a sense in which intentionality can be public.) My immediate concern is to show that both the cluster theory and the causal theory are committed to intentionality as a necessary component to referring. First Searle:

> The capacity of speech acts to represent objects and states of affairs in the world is an extension of the more biologically fundamental capacities of the mind (or brain) to relate the organism to the world by way of such mental states as belief and desire, and especially through action and perception. (1983, p.vii).

Searle seems to be saying here that reference (as a kind of

underlying mental (or brain) capacities. This is just another way of saying that for reference to occur (the `proper´) intention must be present, and this seems borne out in the seven rules of reference which he enunciates (see chapter 1). Furthermore, by couching reference in these terms it seems clear that reference can be and is a private matter. That is, because it is related to the individual organism´s mental (or brain) states, there is no necessary connection with it and other organisms (or, in the case of reference, other speakers). There is nothing in Searle´s account of reference to prevent speakers from, as Kripke says, giving "ourselves some properties which somehow qualitatively uniquely pick out an object and determine our reference in that manner." (1980, p.94). A further elaboration of Searle´s view of intentionality will help corroborate this claim. [I want to say that to the extent that a theory allows private reference, it is committed to private reference. I mean this in the sense that to the extent that a theory allows talk of (or quantification over) particular (kinds of) entities, it is committed to (the existence of) those entities.]

According to Searle, intentionality (intending, intentions) is one form of Intentionality (with a capital `I´). Other forms include believing, hoping, fearing, loving. The commonality here is that Intentionality is a property of (many) mental states and events by which these mental states and events are directed at (or about or of)

mental states and events are directed at (or about or of) objects and states of affairs in the world. Every Intentional state (including intending) consists of a representative content in a certain psychological mode. So, if a speaker intends to refer to Aristotle by using the name `Aristotle´ in the statement, "Aristotle taught Alexander," then the representative content of the Intentional state is the state of affairs of Aristotle having taught Alexander and the psychological mode is intending (rather than, say, wishing). Where the Intentional content (i.e. representational content) is a whole proposition and where there is a direction of fit (i.e. the Intention must `match´ the world), the Intentional content determines the `conditions of satisfaction´ (i.e. those conditions which, as determined by the Intentional content, must obtain if the state is to be satisfied). In the example above, Aristotle must have taught Alexander for the state to be satisfied.

With regard to reference, for Searle, there must be some Intentional content associated with the name for the speaker for reference to occur. Also, as mentioned in chapter three, Searle allows the Intentional content to be non-linguistic. For example, one can refer by way of "names, descriptions, indexicals, tags, labels, pictures, or whatever." (1983, p.259). The crucial point is that a certain mental (brain) state is directed toward the world (so that animals, too, have Intentional states). The important point for the present discussion on private

reference is that Searle associates Intentional states (including intentions) with the mental (brain) states of individual organisms. This, coupled with the claim that the Intentional content can be non-linguistic, seems clearly to commit Searle to private reference. For under his view, a speaker (or, for that matter, a non-speaker) can refer by having certain conditions of satisfaction satisfied by a certain Intentional content. The resulting Intentional state is identifiable with a particular brain state of the organism. Where this commits Searle to private reference is exhibited in the following example (a la Anscombe(1957)). Suppose that a particular mental (brain) state could be created in an aborigine (or even, say, in a cat) by stimulating a certain area of the brain (say, by implanting an electrode). The particular resulting mental (brain) state is the intention to go to the bank and withdraw money from the person´s (organism´s) account. Under Searle´s view, it seems that the aborigine (cat) does have the intention to withdraw money from the bank account because the appropriate mental (brain) state is realized. (Or, similarly, an intention to refer to Aristotle by `Aristotle´ could be induced.) Intentionality, and subsequently reference, under Searle´s view, then, allow private reference.

(In fairness to Searle, he insists that his view of Intentionality does not allow Intentional contents to determine their conditions of satisfaction in isolation:

> Rather, Intentional contents in general and experiences in particular are internally related in a holistic way to other Intentional contents (the Network) and to nonrepresentational capacities (the Background). They are internally related in the sense that they could not have the conditions of satisfaction that they do except in relation to the rest of the Network and the Background. (1983, p.66).

While at first glance this might seem to rescue Searle from a commitment to private intentionality (and private reference), on further examination it is evident that the commitment is still there. The commitment to privacy is still there because there is nothing in the introduction of a Network or Background that demands publicity. The Network and Background can still be restricted to a single organism, unrelated to any linguistic community or public `form of life´. For example, the Background only requires that there be a non-Intentional capacity to identify the object in the world and the Network only requires internal coherence, both of which are consistent with the aborigine (cat) having an entire Network of Intentional states induced by electrodes. (The aborigine (cat) could be a brain in a vat!) Nothing about the requirement of a Network or Background rules out the possibility of an aborigine (cat) intending to withdraw money from the bank account (or intending to propose in marriage or intending to refer to Aristotle by `Aristotle´).)

Now, not only is Searle committed to private reference, but to the extent that Kripke allows a cluster theory view of reference fixing descriptions (as in the Neptune case),



he, too, is committed to private reference. In spite of his insistence that reference depends not on just what we think ourselves, "but on other people in the community, the history of how the name reached one, and things like that" (1980, p.95), he does allow private reference at the baptismal point. There is nothing in Kripke's account of reference to prevent speakers from intending to pick out an object by (the use of) a name when a name is being introduced (where this intending is in the sense just discussed above). The aborigine (cat) in the above example could just as easily be a speaker in Kripke's view as in Searle's.

This is especially true of Devitt's conception of the causal theory of reference. In fact his discussion of grounding thoughts (which result from the act of perceiving an object and include mental representations of that object) sounds very much like Searle's portrayal of reference as an ability based on mental (or brain) states. Devitt explicitly allows for private reference:

> ... I see no reason to deny that beings which do not speak public languages could have grounding thoughts. If they do have them, then they must have appropriate mental representations. Call all these representations of the object in grounding beliefs "demonstrative representations." The act of perception leading to these representations defines a mode of presenting the object. It leads to an ability to designate made up of these grounding thoughts. Underlying the demonstrative representations in those thoughts is a d-chain grounded in the object in virtue of which they designate the object. (1981, p.133).

Both the cluster and causal theories of reference,

then, are committed to the possibility of private reference (in the sense of reference within a private language). What is the significance of this? Two points are significant. First, as I will argue now, to the extent that they are so committed, the theories are weakened. Second, the issue of private reference points to a deeper underlying commitment of the theories, a commitment to a particular view of the similarity relation and of individuation. (In the next chapter I will develop this second point to try to show that the cluster and causal theories share the same position on these two fundamental concepts.)

In criticizing the notion of private reference I want to focus on two points: (1) the plausibility of a private language (within which private reference is possible), and (2) the role of intention in private reference.[3]

Contra Private Intentionality

The issue of the plausibility and even possibility of a private language is an old one and I will not engage in a thorough analysis of it. I agree with Putnam (1981) that the notion of a private language is a self-refuting one and that Wittgenstein gave conclusive arguments against the possibility of a private language.

What is Wittgenstein's objection to the possibility of a private language? In a sentence, it is that the notion of rule-governance makes no sense within a private language and since all language must be rule-governed, there can be no private language. As we saw in the quote earlier,

Wittgenstein is talking about the possibility of making reference to one´s inner experiences, one´s private sensations. Now, Wittgenstein is not denying that we have sensations or feelings[4] or even that we can name them. Rather, he is claiming that naming - even naming feelings - presupposes a context, or `stage-setting´, which is possible only within a public language: "`To give a sensation a name´ means nothing unless I know already in what sort of game this name is to be used." (1968, pp.290-291).[5] So, naming presupposes a stage-setting which presupposes a public language. Why the presupposition of a public language? Because naming (and referring) stand in need of agreement and outward criteria, or rule obedience. (Wittgenstein, of course, speaks of this in terms of language-games.) The `game´ we play with `toothache´, for instance, entirely depends upon there being a behavior (or behaviors) which we call the expression of toothache. If someone claimed to have an unbearable toothache and yet exhibited none of the behavior which we have learned to associate with having a toothache, we would wonder whether the person was telling the truth or understood what having a toothache is. We would, after a while, wonder if the person knew what `toothache´ means (or if the person knew what `toothache´ refers to). Behind this insistence on public, behavioral criteria is the demand for rule obedience in order for understanding and communication to be possible:

> And hence also `obeying a rule´ is a practice. And to <u>think</u> one is obeying a rule is not to obey

> a rule. Hence it is not possible to obey a rule `privately´: otherwise thinking one was obeying a rule would be the same as obeying it. (1953, #202).

If one could in fact have a private language, one could not know whether or not one was following a rule, for the concept of rule involves publicity. Without rules of application, one cannot have a language, if for no other reason than simply because one would not know from one time to another whether one was giving the same name to whatever one was naming or whether one was referring to the same thing from one time to another.[6] (I will return to this point of `giving the same name´ in the next chapter. It is, I think, of paramount importance.) A private language, then, is no language at all, and to the extent to which the received theories of reference permit it (and in fact are committed to it), those theories are weakened.

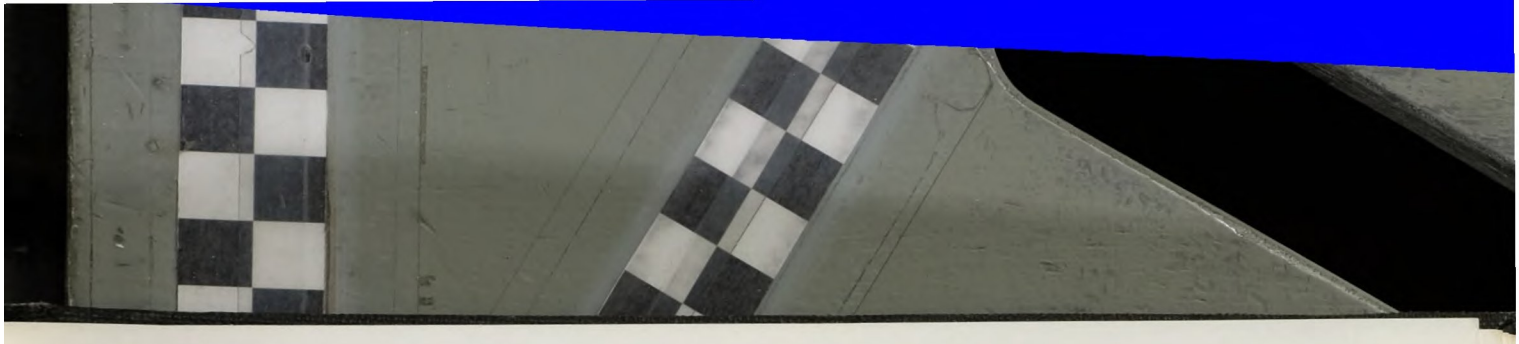
I said before that there were two points about private reference which demand attention. The first is that private reference in the sense of reference in a private language is impossible. The second point involves the role of intention in reference. It is to this point that I now turn.

Putnam (1981) gives an `abbreviated version´ of a Wittgensteinian argument that there is no necessary connection between names and objects and that intentionality does not enable reference to succeed or occur. Suppose, Putnam says, that in its crawling across the sand an ant traces a particular pattern of lines which we depict as a recognizable caricature of Winston Churchill (or that the

ant traces a particular pattern of lines which we depict as a recognizable shape resembling the name `Winston Churchill´). Would we say that the ant has referred to Winston Churchill? Putnam says: no. What may seem to be missing for reference to have occurred is the element of intention. However, this will not do:

> But to have the intention that anything...should represent Churchill, I must have been able to think about Churchill in the first place. If lines in the sand, noises, etc., cannot `in themselves´ represent anything, then how is it that thought forms can `in themselves´ represent anything? (1981, p.2).

Just as physical representations (e.g. lines in the sand) have no necessary connection to objects, so mental representations also have no necessary connection to objects. (Words are included here: a discourse might seem to be, say, a description of a tree, but if it was produced by monkeys randomly hitting keys on a typewriter for millions of years, then the words would not refer.) Now, the point that Putnam is making here is that intentionality does not yield the ability to refer, but actually presupposses the ability to refer. To intend to withdraw money from a bank account (or to refer to Aristotle by `Aristotle´) requires the ability to refer to those objects or actions or capacities which are the objects of the intentions. This is because, as Putnam puts it, to have the intention that something happen (e.g. money being withdrawn) or be represented (e.g. Aristotle being referred to by `Aristotle´), one must already have the ability to think



about (i.e. refer to) the event (or object) in the first place. (Furthermore, the ability to intend itself depends upon a public `form of life´, as I will argue at the end of this chapter. This bears on the question of whether there is any sense of intentionality which is not private and subsequently on the role of intention in reference.)

If Putnam is right (and I think that he is), this is a telling criticism of both the cluster and the causal theories, since, as we saw above, they are both committed to intention as an integral component of reference. (This is true also of Devitt´s account of a causal theory, even though he tries to avoid talk of intentions. His talk of mental representations avoids neither the commitment to intentions nor Putnam´s criticism.)

Another criticism of the role of intentionality in reference comes from Elgin (1983). She argues that the notion of intentionality is conceptually intermingled with that of interpretation and that this requires publicity. Says Elgin:

> ...intending to produce an effect by getting one´s audience to recognize one´s intention to produce that effect is not, or at least is not always, required for reference. Someone talking in his sleep or under the influence of anesthesia or, for that matter, someone who is unconsciously `thinking out loud´ may have no such intention. Whether to say that such a person refers depends on how his words are interpreted. If we can make no sense of his utterances we put them down to inchoate ramblings. If their interpretation is straightforward, we take his utterances to be sentences, and the terms in them to refer. It is the availability of a reasonable interpretation rather than the intention with which they were produced that is crucial in deciding whether his

words refer. (1983, p.17).

Now, Elgin's comments clearly are directed against Searle's theory (especially his rules 5 and 6) of reference as a speech act which is based on intentionality. She reiterates Kripke's speaker reference - semantic reference distinction by repeating that we can distinguish between what a speaker intends to refer to and what a speaker actually refers to. However, this same point can be directed against the causal theory as well, since, as we have seen, it, too, incorporates intentionality. Nor can the causal theory be defended by claiming that this criticism is appropriate to speaker reference (where intention does play the role that Kripke assigns it), but that it is not appropriate to semantic reference. This move does not help because, as Putnam's criticism points out, words have no more necessary connection to objects than do physical or mental representations. Yes, words (names) hook up to objects, but not by themselves. What counts as a name, and the fact that something functions as a name, is not inherent in the name (nor in the object). To extrapolate Elgin's point, interpretation not only determines (at least in part) what is conceptually legitimate about intention (and indeed what counts as intention), but also interpretation determines (at least in part) what is conceptually legitimate about naming (and indeed what counts as naming). It is true that naming and referring is public, but the causal theory's treatment of

semantic reference (and reference in general) does not capture the proper sense of publicity to account for successful reference. I will try to defend this last claim further in the next chapter. For now, I will consider the notion of intentionality in a public sense. Afterwards, I will conclude the discussion of private reference by saying that it seems to me that both the cluster and the causal theories are committed to private reference (at least in the way that they have been presented). Furthermore, they are both weakened by that commitment. Finally, the issue of private reference points to an even more fundamental underlying commitment of both the cluster and causal theories. When Wittgenstein objected to a private language and to private reference because one would never know whether one was using the same name to refer to an object (or experience) or was referring to the same thing from one time to another, he was pointing out the dependence of reference on both a principle of individuation and on a conception of sameness (or similarity). I want to argue in the next chapter that the cluster and causal theories share a commitment to a particular view of individuation and the similarity relation, a view which I want to criticize.

What I have been arguing in this chapter is that both the cluster and the causal theories of reference depend upon intentionality for successful reference to occur. In addition, I have been arguing that the sense of intentionality which both theories allow (are committed to)

is private. Given this allowance of (commitment to) private intentionality, I have been arguing that both theories allow (are committed to) private reference. To the extent that this is so, the theories are weakened. A question quickly comes to mind: Must intentionality be private? I will argue now that intentionality does not have to be private. However, as I have been arguing, both the cluster and causal theories allow (are committed to) private reference, so, given the theories as they are presented, the possibility of public reference does not rescue them. Furthermore, as the Putnam and Elgin criticisms indicate, even the possibility of public intentionality does not rescue the cluster and causal theories (or any theory relying on intentionality) from serious difficulties. The immediate consideration, though, is of the possibility of public intentionality.

I have been arguing that both Searle and Kripke (and Devitt) allow (and are committed to) a sense of intentionality which allows (is committed to) private reference, because the intentionality itself is private. However, is there a sense in which intentionality is (or could be) public? I want to argue: yes. In fact, I think that the conceptions of intentionality which Wittgenstein (1953, 1958, 1967) and Anscombe (1957) have laid out are examples.

In the Philosophical Investigations, section 337, Wittgenstein remarks that an intention "is embedded in its situation, in human customs and institutions." We could

never, for example, intend to steal second base if there were no game of baseball. What is crucial for Wittgenstein is that intentions, like other `mental´ events (or experiences or states) only arise in and are only intelligible within `forms of life´. These `forms of life´ are known to us only through shared behavior and practices. As humans we engage in certain behaviors and practices which enable us to make sense of and interact with the world. (As members of particular cultures or, say, religious groups or social groups we engage in certain behaviors and practices which enable us to make sense of and interact with the world in other ways.) Our ability to make sense of and interact with the world requires our participation in some `form of life´, known through publically observable behaviors and practices. As mentioned above, even `private´ experiences such as pain stand in need of outward criteria and can make sense only within the context of outward (behavioral) criteria. As Quine (1985) says, echoing Wittgenstein:

> Someone observes my joyful or anxious expression, or perhaps observes my gratifying or threatening situation itself, or hears me tell about it. She then applies the word `joy´ or `anxiety´. After another such lesson or two I find myself applying these words to some of my subsequent states in cases where no outward signs are to be observed beyond my report itself. Without the outward signs to begin with, mentalistic terms could not be learned at all. (1985, p.6).

Not only do we learn about intentions on the basis of communal behavioral criteria, and so epistemically intentions are basically public, but we can only make sense of what intentions are (ontologically) in a public sense.

In The Blue Book Wittgenstein insists that intentions are not particular states of mind or mental processes:

> Suppose I had made a move in chess and someone asked me "Did you intend to mate him?", I answer "I did", and he now asks me "How could you know you did, as all you knew was what happened within you when you made the move?", I might answer "Under these circumstances this was intending to mate him." (1958, p.147).

Several points are worth noting here. First, as noted above, intentions make sense only within public, communal behaviors and practices. One can intend to make a move in chess only if there is such a practice as playing chess. Second, in the context laid out in this quote, what intention is is dictated by the situation (e.g. under the circumstances, intending to mate his opponent in chess is nothing more than the behaviors and practices associated with and which characterize that situation). Does this mean that intentions are identical with behaviors? No, it means that intentions only make sense given shared behaviors and practices. While intentions are not behaviors, they are possible only because there are (publically observable) behaviors. (Behaviors and practices are the `natural expression´ of intentions, for Wittgenstein, as can be exemplified by looking at a cat when it stalks a bird. We say the cat has an intention only because of its behavior. If the cat did not stalk the bird, it wouldn´t make sense to say that it intended to stalk (or catch or eat) the bird. Likewise, while a particular intention might not be expressed behaviorally, the intention (and intention in

general) makes sense only because, as a rule, intentions are learned and expressed behaviorally.)

Anscombe (1957) reiterates and expands on Wittgenstein's remarks on intentionality. How do we tell someone's intentions, she asks:

> Well, if you want to say at least some true things about a man's intentions, you will have a strong chance of success if you mention what he actually did or is doing. For whatever else he may intend, or whatever may be his intentions in doing what he does, the greater number of things which you would say straight off a man did or was doing, will be things he intends. (1957, p.8).

While recognizing that not all intentions are carried out and that we often intend to do things without actually doing them (and, so, speak of intentions as being `inside us' or `private'), nonetheless these make sense and are possible only because of larger public contexts in which we do learn and express intentions behaviorally. To say, for example, that only a particular person can know her own intentions means nothing more than that there comes a point where a particular person can say, "This is my intention," and no one else can contribute anything to settle (or question) the matter. It does not mean, says Anscombe, that the person is evincing a knowledge known only to her.

With Wittgenstein, then (and contra Searle and Kripke), Anscombe maintains that having a particular `mental representation' or being in a particular `mental state' is not an adequate account of intentionality. The aborigine (cat) with the brain implant does not intend to withdraw money from the bank because the appropriate public context -

including the historical context - is missing. In a related example, Anscombe states:

> Let us suppose that the thought in [a man's] mind is `you silly little twit!' Now here too, it is not enough that these words should occur to him. He has to mean them. This shews once more, that you cannot take any performance (even an interior performance) as itself an act of intention; for if you describe a performance, the fact that it has taken place is not proof of intention; words for example may occur in somebody's mind without his meaning them. So intention is never a performance in the mind, though in some matters a performance in the mind which is seriously meant may make a difference to the correct account of the man's action - e.g. in embracing someone. But the matters in question are necessarily ones in which outward acts are `significant' in some way. (1957, p.49).

Not only is Anscombe arguing against a private notion of intentionality here, but she is also alluding to a point made above by Elgin, viz. the interrelation between intention and interpretation. Because intentionality makes sense only within the context of `forms of life', and because we participate in `forms of life' based on behaviors and practices, we determine what intentionality is, and, in the long run, what people's intentions are.

It is important to note that neither Wittgenstein nor Anscombe is denying that we have intentions or denying the meaningfulness of talking about intentions, so neither is behavioristic in a `strong' sense. However, they are insisting on a conception of intentionality which stands in need of outward behavioral criteria. With regard to theories of reference (such as Searle's and Kripke's) which require intentionality, they would rule out such theories

which allow (are committed to) private intentionality. As noted above, Putnam's and Elgin's arguments against intentionality in general militate against any theory of reference which requires intentionality - private or public - as a necessary component. With this in mind, we are now ready to turn to the underlying elements of the cluster and causal theories, and first to the underlying principle of individuation and similarity relation.

NOTES

[1]See especially Jones (1971).

[2]I need to make an important distinction here. In response to the following discussion of Searle's and Kripke's commitment to intentionality and private reference, one might object that a commitment to intentionality, even `private' intentionality, does not commit Searle or Kripke to a commitment to private language. The reason for this, so the objection goes, is that speakers intend to refer to public, not private, entities (i.e. entities which are `out there' independent of any speakers's intentionality). Being committed to intentionality does not entail being committed to private reference because the objects being referred to are public. I want to argue that this objection fails because it rests on a particular conception of individuation of objects. I will criticize this conception in chapter five below. For now, then, I want to critique intentionality (or, at least, private intentionality). After critiquing conceptions of individuation (and similarity) in the next chapter, I will return to the relation between intentionality, private reference and individuation. At that point, after having made my argument against the conception of individuation which Searle and Kripke hold, I will flesh out my argument that the objection mentioned here fails.

[3]Actually, my critique of the role of intention applies not simply to private reference, but to reference in general.

[4]See Wittgenstein (1967, #487) and (1953, #244).

[5]See also Wittgenstein (1953, #257, #290), (1958, p.69), (1967, #428) and (1969, #472).

[6]Rhees (1954) makes the same point: "I know a headache when I feel it, and I know I felt giddy yesterday afternoon, because I know what giddiness is. ...But the identity - the sameness - comes from the language... .But without language I could not have told whether this feels the same, either; if only because I could not have asked." (1954, pp.81,82).

CHAPTER FIVE

INDIVIDUATION AND SIMILARITY

As we have seen, reference, at least in naming, presupposes both a name and an object. (Of course, this does not mean that the object must exist; we have names for fictional objects, `future´ objects, abstract objects, etc.) We refer to X by (the use of) `X´. In considering the plausibility of private reference, we saw that reference is conceptually linked with interpretation and with public criteria for successful reference. Underlying these issues is that of individuation and similarity. At this point we need to get clearer not only on this connection between reference and individuation (and similarity), but also on just what individuation and similarity are.

Individuation and identity are two sides of the same coin. To ask what something is and to ask how something is individuated are not only interrelated, but are essentially to ask for the same information. Identity might be said to be a metaphysical issue and individuation an epistemic issue, but I do not think that characterizing them as such differentiates them in any significant way. Both are questions of partitioning the world. By saying, `This is what X is´, we are individuating X from Y; by saying, `This is (an) X and that is (a) Y´, we are saying what X is (e.g.

minimally something that is not-Y). The significance of this with respect to reference is that reference only makes sense within the context of an underlying conception of individuation. Our ability to refer to X by (the use of) `X´ rests on there being a prior understanding of what X is. There must be (in some sense) an X for us to refer to X. We could not know whether we have referred to Glub by (the use of) `Glub´ unless there were a thing which has been individuated as Glub. Indeed we could not refer to Glub if there were not an individuated Glub. (Again, this does not mean that Glub must exist; Glub could be fictional or `future´ or abstract, etc.) For reference to occur, individuation must already have occurred.

Similarity and sameness are also two sides of the same coin. To ask what makes this the same as that is the same as to ask what makes this similar to that. In both cases what is being asked for is criteria for identification. To say that X and Y have the same color is to say that they are similar with respect to color. With respect to ontology, sameness is the limiting case of similarity: to say that X is the same as Y is to say that X and Y are similar in all respects. But notice that there seems to be no difference between identity and sameness. X´s identity with Y just is X being the same as Y. We are led, then, to the realization that individuation and similarity are conceptually interconnected (not an unwelcome result, as far as I am concerned).

Now, what's the point of all this? As noted earlier, I want to argue that the cluster and causal theories of reference are committed to a particular view of individuation and the similarity relation. I want to show that they have a shared conception of these notions and it is at this fundamental level that they are more similar than different, that they are two versions of a received theory of reference. I then want to argue against their shared conception of individuation and similarity (and say more on why I take these notions to be so fundamental, not only to reference, but to many issues).

Individuation

In their discussions of reference, neither Searle nor Kripke speaks explicitly about individuation. Neither seems to indicate that there is any question about or problem with individuation. That is, while both might question whether this name picks out that individual, neither indicates any question or problem with a principle of individuation. Individuals are `out there' so to speak and reference is a matter of `the right' hook-up of names to individuals. Searle speaks of picking out "an independently located object" as though there is no question or problem with (conceptually) locating such an object. The question of what name picks out what individual is different than, and rests on, what individuals there are and how they are individuated. About the latter Searle says nothing. About the former he makes claims such as the following:

> In short, if none of the identifying descriptions believed to be true of some object by the users of the name of that object proved to be true of some independently located object, then that object could not be identical with the bearer of the name. (1969, p.169).

Besides statements such as this one, Searle's commitment to private reference indicates that he sees individuation as a straightforward matter, and the `fact' about individuation is that individuals are given in the world, independent of our concerns or interpretations. For Searle, there may be a question as to whether `Gödel' refers to Gödel or to Schmidt, but there is no question as to Gödel and Schmidt being independently located (or at least locatable - in the sense of being capable of being located) objects.

Kripke's acceptance of the view that individuation is a straightforward matter, essentially one of simply seeing or coming to see what individuals there are `out there', can be seen in the context of his discussion of possible worlds. One of the issues connected with the postulation of possible worlds is that of transworld identity. That is, how do we identify the same thing across possible worlds? What properties must a thing have in different possible worlds in order for that thing to be the same thing (e.g. what properties must a thing have in different possible worlds for that thing to be, say, Nixon)? In rejecting the idea that questions about transworld identification make sense only in terms of questions about component parts of an object, Kripke says the following:

> Theorists have often said that we identify objects across possible worlds as objects resembling the given one in the most important respects. On the contrary, Nixon, had he decided to act otherwise, might have avoided politics like the plague, though privately harboring radical opinions. Most important, even when we can replace questions about an object by questions about its parts, we need not do so. We can refer to the object and ask what might have happened to it. So, we do not begin with worlds (which are supposed somehow to be real, and whose qualities, but not whose objects, are perceptible to us), and then ask about criteria of transworld identification; on the contrary, we begin with the objects, which we have, and can identify, in the actual world. We can then ask whether certain things might have been true of the objects. (1980, p.53).[1]

Kripke's commitment is obvious here. By saying that we can "refer to the object and ask what might have happened to it," and by saying that we begin with the objects "which we have, and can identify," Kripke is clearly implying that there is no problem of individuation in this world. His later discussion of natural kinds reiterates this view. Our understanding of what gold `really' is (or what tigers `really' are) might change over time, but that there are natural kinds, say, elements (or species), is not in question. Gold is gold, and whatever it `really' is, it's that way independently of us. So, says Kripke:

> When I refer to heat, I refer not to an internal sensation that someone may have, but to an external phenomenon which we perceive through the sense of feeling; it produces a characteristic sensation which we call the sensation of heat. Heat is the motion of molecules. (1980, p.129).

So, both Searle and Kripke are clearly committed to a particular view of individuation, viz. one in which individuals are given in the world (i.e. independently

locatable and `really´ part of the world independently of any speakers of a language). I want to argue now that this `realist´ conception of individuation is mistaken and consequently both the cluster and causal theories of reference are weakened by their commitment to it.

Goodman (1965) has argued - correctly I think - that the partitioning of the world is (at least in part) determined by our cognitive interests and is language-dependent.[2] Specifically, his argument is about legitimate inductions based on lawlike generalizations, and hence about the partitioning of the world in the sense of reference classes. I want to extrapolate from Goodman and suggest that his argument applies to the partitioning of the world in the sense of individuals as well. That is, the point which he makes about reference classes (which I will review below) can also be made about individuals, viz. which ones there are is (at least in part) determined by our cognitive interests and is language-dependent.

Goodman asks us to consider a predicate `grue´, which he defines in the following way: the predicate `grue´ "applies to all things examined before [time] t just in case they are green but to other things just in case they are blue." (1965, p.74). For example, we would say that an emerald is grue iff it is examined before, say, January 1, 2000 and is green or it is not examined before that date and is blue. Up to time t we have evidence that emeralds are grue. Assuming that all emeralds so far examined have been

green, then it follows that they have all been grue. Before t, then, an English speaker, knowing these facts about emeralds, would make the inductive generalization that all emeralds are green, and the English speaker's prediction that the next emerald examined (after t) will be green will be based on this lawlike statement. However, a `gruesome' speaker, knowing these facts about emeralds, would make the inductive generalization that all emeralds are grue, and the gruesome speaker's prediction that the next emerald examined (after t) will be grue will be based on this lawlike statement. The immediate point is that the same evidence (green emeralds examined before t) seems to confirm equally two different predictions, and, in fact, contradictory predictions (because the English prediction is saying that the next emerald will be green, while the gruesome prediction is, in effect, saying that the next emerald will be blue). Says Goodman:

> Thus although we are well aware which of the two incompatible predictions is genuinely confirmed, they are equally well confirmed according to our present definition [of confirmation]. Moreover, it is clear that if we simply choose an appropriate predicate, then on the basis of these same observations we shall have equal confirmation, by our definition, for any prediction whatever about other emeralds - or indeed about anything else. (1965, p.74).[3]

So, says Goodman, the original problem of justifying induction (in a way, attempting to answer the question, `Why does a positive instance of a hypothesis give any grounds for predicting further instances?') has been displaced by a new problem of defining confirmation (in a way, attempting

to answer the question, `What is a positive instance of a hypothesis?´) and distinguishing confirmable and non-confirmable hypotheses (in a way, attempting to answer the question, `What hypotheses are confirmed by their positive instances?´). Past cases of green emeralds were used to confirm rival, mutually exclusive, hypotheses. Past regularities of green emeralds seem to confirm the prediction that the next emerald would be green, but the past regularities of grue emeralds doesn´t seem (to us) to confirm the prediction that the next emerald would be grue. However, the point of Goodman´s example is that saying that valid predictions rely on past regularities without saying <u>which</u> past regularities is completely unhelpful. "Regularities are where you find them, and you can find them anywhere." (1965, p.82).

Now, the significance of Goodman´s argument for the present concerns is that if he is right that regularities are where you find them and you can find them anywhere, then the conception of regularities (or reference classes) as being `out there´ in the world is mistaken. We do not `bump into´ regularities in the world, we make them up. We find what we´re looking for only after deciding what it is we´re looking for.[4] It is our cognitive interests that shape what regularities (reference classes) there are, and, as the grue example points out, what regularities there are depends upon the language we speak.

A major issue, of which Goodman´s grue example is an

instance, in the philosophy of science is whether or not (and to what extent) facts are independent of theory. Goodman's example is one example to argue that facts are not theory-independent. The debate about the theory-fact distinction has usually focussed on the question of whether or not observation is theory-laden. It seems to me that while this particular question is significant with respect to the theory-fact distinction, it is not crucial. That is, while I want to hold that observation is theory-laden (in the sense that we observe individuals in the world and individuation is, I want to argue below, theory-dependent), there is more to the discussion about the theory-fact distinction than simply questions of observation. Even if one could make the case that observation is theory-independent (so, for example, we all `see' the same thing or event), there still remains the question of what this observation means. We still must determine whether this particular observation is a relevant observation (that is, if indeed we are observing what we think we are observing). As Duhem (1962) argued, what is acceptable - and even what is possible - as scientific fact (e.g. experimental results) depends on theoretical interpretation. Without some theoretical justification for believing a given meter reading as a relevant datum (rather than, say, an instrument malfunction), we would not consider this datum a genuine `fact'. For example, without some theory of optics, we would not accept the claim that a new instrument, a

`fabriscope´, could allow the viewer to see through clothes (much like the x-ray glasses that used to be advertized in the back of comic books). Indeed, much of the dispute about Galileo´s use of the telescope to support his claims about heliocentricity centered on questions of the reliability of this new instrument. The point here is that even if observation were theory-independent (say, in the sense of sensations that all observers would experience), there is still the issue of whether or not these observations constitute `facts´. Questions about the theory-dependence of partitionings is another version of this issue.

Consider the following example to illustrate the interest- and language-dependence of world partitions (and of individuals): the (temporal) world is partitioned into, say, months and seasons. However, it seems obvious that this partition is based on our interests. For example, seasonal divisions are (or were) based on farming or hunting concerns. Likewise, geographic divisions are determined by our, say, political interests. Now, it might be objected that even if these examples do show partitioning to be interest- (and perhaps language-) dependent, they are artifactual and do not show that other, more `natural´, partitions are interest- (and language-) dependent. This objection, I think, is wrong. Consider the partition made in the objection itself: artifactual versus natural. The objection seems to rest on the assumption that the very division between what is artifactual and what is natural is,

so to speak, natural; it is `out there´. However, this is far from obvious. For example, through genetic engineering new organisms can be (have been) created, but which are now viable and reproductive. Or, there are many cases of hybredized plants, which were `created´ by humans (and, so, artifactual), but are `natural´, as they are reproductive. These cases are meant to show that the partition into artifactual versus natural is not a sharp one, so we cannot point to what is `out there´ in nature to argue against the interest- (and language-) dependence of partitionings (because what is `out there´ in nature, as opposed to what is artifactual, is not clear).

There are other cases to illustrate the interest- (and language-) dependence of partitionings. Our taxonomic divisions (kingdom, phyllum, etc.) are made on various bases. For example, divisions are made on the basis of whether or not organisms are caryotic or not; divisions are made on the basis of whether or not organisms are warm-blooded; divisions are made on the basis of dentition; divisions are made on the basis of pigmentation. Even to argue that these features (procaryotic versus eucaryotic, warm-blooded versus cold-blooded) are really `out there´ does not deny that our taxonomic categories are interest- (and language-) dependent. We choose to consider, say, dentition as a basis for partitioning rather than, say, eye color. We choose to classify an organism as a bird not because it flies (bats are not birds, but they fly;

ostriches are birds, but they don't fly). Instead, we choose to classify an organism as a bird on the basis of other criteria (say, being feathered). The point, of course, is that we choose to partition in certain ways and based on certain criteria, and those partitionings both reflect and are a function of our interests.

Another objection at this point is that the examples above have, at best, shown only that our partitionings are theory-dependent, not interest- (or language-) dependent. It remains to be shown that theories are interest- (or language-) dependent. This issue, it seems to me, has been at the heart of much (if not most) of the debates in the philosophy of science for quite some time (at least since Kuhn (1962)). There are, of course, numerous historical cases which illustrate that theories have been proposed and accepted on the basis of particular interests (interests both in a cognitive sense and in a moral, social, political sense). For example, Gould (1981) has argued forcefully that theories in intelligence testing have a long history of being interest-based. Harding (1981) has offered considerable evidence and argumentation that various antithetical approaches to social science research have shared normative sexist assumptions. Examples could be extended ad nauseum to show that actual, historical cases support the contention that theories are interest-dependent (e.g. Lysenko, and Einstein's remark that his physical theories followed from his epistemic and metaphysical

views).

Indeed, the view that theories are not interest- (or language-) dependent seems to me to rest on the assumption that there is a sharp distinction between the context of discovery and the context of justification. That is, one might argue that it doesn't matter whence our theories come, what matters is whether or not they are good theories (whatever `goodness' means with respect to theories). The host of objections to and arguments against this distinction are well-documented and don't need to be repeated here. I do, however, want to make the following point: it seems clear that theories are interest- (or language-) dependent in the sense that our cognitive (or what McMullin (1983) calls `epistemic') values determine our theories. That is, features such as simplicity, generality, testability, etc. are features which we insist on in theories because we value them (in a cognitive or epistemic sense). To the extent that we demand reliability from theories, we demand features such as testability, etc. (Cognitive, or epistemic, values are inherent in other aspects of science as well; for example, the demand for replicability of experiments.) The necessary connection between theories and cognitive values seems uncontroversial.[5] Given this point, it is not a great leap to get to the point that ethical (or social, political, etc.) values are inherent in theories. Just as our theories do not arise separate from our cognitive values, so the argument goes, our cognitive values do not arise separate

from our ethical/social/political values. Marxists have argued for this claim for years. Recently feminists have developed strong arguments that the normative assumptions and values that underlie scientific methodologies are predominantly based on male experience. (Whether or not cognitive values are a function of ethical/social/political values is not crucial to my main point here, which is that theories are interest- (or language-) dependent. However, if these arguments are sound, then they add support to my claim.)[6]

Now, even if the case can be made (as I have just tried to do) that our partitionings are theory- and interest- (or language-) dependent, this does not say anything with respect to individuation. That is, even if Goodman is right that reference classes are where we find them and we can find them anywhere, this says nothing about the individuals which make up the reference classes. They, it is generally held, are `out there´. Whether grass is green or grue, so the claim goes, grass is `out there´ and individual blades of grass are what they are. I now want to argue that this view is mistaken. I want to claim that individuals are where you find them and you can find them anywhere. Individuation, I want to say, is just as much theory- and interest- (and language-) dependent as partitioning at the level of reference classes (or predicates) is.

One argument for the treatment of individuation in a `non-realist´ way can be extrapolated from Goodman´s

gruesome language and from Quine (1953). In a context of discussing non-referring singular terms, Quine suggests that such terms can be translated into a description or even into a predicate. So, `Pegasus´ can be translated into `the thing that pegasizes´ or simply `pegasizes´. If Quine is right, and if there is no reason to restrict his point to <u>non-referring</u> singular terms (and I see no reason for such a restriction), then this result coupled with Goodman´s point of the language-dependence of reference classes (which are identified by predicates), seems to support the claim that individuation, too, is language-dependent.[7]

Another, perhaps more intuitive, argument for a `non-realist´ treatment of individuation comes from the consideration of the individuation of events. We clearly individuate one event from another.[8] For example, we speak of `this coin toss´ or `that die roll´. Many events have even been named, `the battle of Waterloo´, `the 1948 Presidential election´, and so on. Now, it also seems clear that the individuation of events is not merely a matter of `finding them out there in the world´. We decide what counts as an event and hence what events there are. This can be seen (somewhat, at least) on an intuitive level with the difficulty of saying what the parameters of a given event are. Exactly when was the 1948 Presidential election? When did it begin? On election day? On the day that the first candidate was nominated? In asking, `When did it begin?´ to what does the <u>it</u> refer? (The answer cannot be:

to the 1948 Presidential election, because that is circular and vacuous in this context.) Or, when was the battle of Waterloo? Did it start when the first shot was fired? When Napolean advanced his troops past a certain point? The point of these examples and questions is that the individuation of events is not a clearcut, straightforward notion even at an intuitive level.

At a more technical level, Hanna (1969, 1978) has made much the same point. He distinguishes generic experiments (or events) from specific experiments (or events). By a generic experiment he means "an empirical procedure which can be replicated by different experimenters at different spatio-temporal locations - each particular replication constituting a specific instance of the generic experiment." (1978, p.533). Scientific predictivity and explanation refer, says Hanna, not to specific experiments, but to generic ones. So, the scope of the prediction of Jones's specific experiment conducted at this place and time under these conditions is not limited to that specific experiment. It covers every specific replication of the generic experiment. The `problem', though, is that it is not a straightforward matter as to what counts as a replication of that generic experiment. Since predictions cover a potential infinity of observations and predicting the outcome of a generic experiment involves a commitment to corresponding projections for all replications of the experiment, it follows that scientific predictions can never

be established (or not) beyond question. Says Hanna:

> The relevance of any specific experiment, e.g., as performed by Jones in laboratory X, to prediction (A) is to a great extent a theoretical issue. The outcome is relevant only if Jones has created the appropriate stimulus situation: i.e., only if Jones has replicated the generic experiment. But...this latter issue is not a straightforward matter of observation; on some occasions it involves highly theoretical considerations. (1969, p.312).

Consider a simple coin tossing experiment. A coin is tossed and the outcome is tails. A prediction about the outcome of this event only makes sense if the experiment can be replicated. The question, however, is: what is the generic experiment of which this is a specific instance? We need the generic experiment to be individuated before we can say what this specific instance is an instance of, and the individuation of the generic experiment is what's at issue here. I am arguing (as is Hanna) that the individuation of the generic experiment is not `out there'. We decide what constitutes the generic experiment and that in turn determines what counts as an instance of it. To be able to replicate the experiment, we first need to know what it is we are attempting to replicate, and that depends upon our cognitive interests and concerns. We decide what the parameters of the coin toss are (e.g. whether the toss begins, say, when the coin loses contact with an other object - like my hand - or when it reaches the apex of its ascent or whatever). If indeed replication is (at least in part) "highly theoretical," then this strengthens the argument that the individuation of events is interest-

dependent and language-dependent. (This point of replication as `non-realist´ is also relevant to the issue of similarity, discussed below.)[9] It seems, then, that the individuation of events, at least, is language-dependent. Is is also the case that the individuation of other kinds of individuals is language-dependent? I want to argue: yes. First, following Quine´s talk of translating talk of individuals into talk of predicates, it seems that there is no reason to suggest that this doesn´t apply to all individuals (whether they are events or other kinds of individuals). While predicates such as `pegasizes´ or `is-the-battle-of-Waterloo´ sound a bit unusual (indeed, more gruesome than `grue´), there is no clear reason to forbid them. Furthermore, the distinction between objects and actions (i.e. between event-individuals and thing-individuals) is not so sharp. For example, storms seem intuitively to be events, not things, yet they are treated linguistically on a par with things (e.g. both storms and children can satisfy the sentence `x is savage´). Fists, too, seem to be actions (moving the hand into a particular formation), yet `fist´ is a noun, a grammatical feature which we associate with things. (What is taken to be an event and what is taken to be a object varies among natural languages. See, for example, Whorf (1956), on such differences between English and Hopi.)

This points to another argument for a `non-realist´ treatment of individuation. Formally, individuals are

whatever can satisfy the values of individual variables (e.g. whatever can replace the variable x in `x is red´). Informally, individuals are whatever can be referred to by a noun (e.g. whatever can be the subject of a sentence (in English)). As noted in the paragraph above, the distinction between events and objects then becomes blurred. Not only can storms be referred to by nouns, but particularly severe storms can be (and are) named (e.g. Hurricane Diana). Also, actions, such as running, become individuals when they are referred to as gerunds (e.g. `Running is good for one´s health´ or `Over-eating is unhealthy, but fun´.) The significance of this is that if event-individuals are individuated based (at least in part) on our theories/interests/language, and if there is no sharp distinction between event-individuals and thing-individuals, then there may be good reason to adopt the view that thing-individuals are also individuated based on our theories/interests/language.

With respect to individuals being whatever can replace individual variables or whatever is referred to by a noun, consider the following example. `July´, on these criteria, is an individual. For example, it is proper to say `July is my favorite month´. Clearly, however, July is not `out there´. What July is depends on our theories/interests/language. July is nothing more than a set of temporal coordinates. But, of course, we determine what the extension of that set is. Are `natural´ individuals any

different? I see no reason to assume so. `Natural´ individuals are simply sets of space-time coordinates. But, then, of course, we decide the extensions of those sets, too. We decide what set of space-time coordinates is this bird (or, for that matter, is all birds). We set the parameters for what counts as these sets of coordinates. (An objection here is that I am confusing epistemic claims with metaphysical claims. That is, even though epistemically we decide the parameters of these sets, nonetheless, metaphysically, these sets are independent of us. Of course, much of what I am arguing throughout this entire book is that this distinction between epistemic and metaphysical claims is bogus. The entire work is directed against this objection.)

I think, however, that another, stronger, case can be made that individuation per se is language-dependent. Perhaps the most intuitive notion of individuation (and of individuals) is that of ostension. The use of demonstratives (e.g. `I mean <u>this</u> mess! Clean it up now!´ or `Get a load of <u>that</u> guy!´) has been a paradigm case of individuation. Which mess? This one. Which guy? That one. This mess and that guy are `out there´ if anything is. The trouble here, though, is that, as both Wittgenstein and Quine have argued, even ostension is interest- and language-dependent. Wittgenstein offers several examples. Suppose, he says, that someone points to a pencil and says, `This is tove´ or `This is called "tove".´ Does ostension work here?

No, he says, because this could mean a number of things: this is a pencil, this is round, this is wood, this is one, this is hard, etc. Another example is `This is sosh.´ Wittgenstein states:

> For example, from "This is sosh" you would not understand the use of the word "sosh", though from "This color is sosh" you would. That is, if a person is to learn the meaning of a word from such a definition he must already know what sort of thing it stands for. The word "color" already fixes the use of "sosh". The ostensive definition is of use if you need to fill in only one blank. (1979a, p.45).

Quine (1960, 1969) also criticizes the notion of ostension as providing a clear means of individuation. Suppose, he says, that a native points to an object and says, `Gavagai.´ A non-native, on witnessing this, might believe that the native has pointed to a rabbit and `gavagai´ can be translated as `rabbit´. But things are not so obvious for Quine. It could be that `gavagai´ is more nearly translatable as `rabbithood´ or perhaps `undetached rabbit parts´ or even `rabbit stages´. Says Quine:

> The only difference between rabbits, undetached rabbit parts, and rabbit stages is in their individuation. If you take the total scattered portion of the spatiotemporal world that is made up of rabbits, and that which is made up of undetached rabbit parts, and that which is made up of rabbit stages, you come out with the same scattered portion of the world each of the three times. The only difference is in how you slice it. And how to slice it is what ostension or simple conditioning, however persistently repeated, cannot teach. (1969, p.32).

Where does this leave us? It leaves us with the conclusion that ostension, the paradigm of `realist´ individuation, offers no principle of individuation after

all. Ostension works only after the world has been sliced up, and as Goodman and others have pointed out, how the world is sliced up is language-dependent.

Returning to the issue of reference, it is precisely this view of individuation which has been found to be unwarranted, viz. the `realist´ view, that underlies both the cluster and the causal theories of reference. When Searle speaks of his principle of identification as "giving me an ostensive presentation" of "some independently located object" (1969, pp.168, 169), his commitment to a `realist´ view of ostension and individuation is clear. Kripke claims: "When you ask whether it is necessary or contingent that Nixon won the election, you are asking the intuitive question whether in some counterfactual situation, this man would in fact have lost the election." (1980, p.41). But just who this man is is not a straightforward matter. Indeed, who this man is depends upon how we partition the world, and Kripke, along with Searle, is mistaken in taking that to be given.[10] (Devitt, likewise, in his discussion of grounding as being dependent upon seeing an object as a member of a very general category talks as though these categories (or reference classes) are `out there.´ Of course, much of what I am arguing in this chapter is that they are not.)

At this point I want to return to an question left unanswered in the last chapter. In the discussion of private reference, I claimed that both Searle and Kripke

were committed to private intentionality and also to private reference. An objection which I mentioned, but did not rebut, was that a commitment to intentionality did not entail a commitment to private reference because reference was made to objects `out there´ in the world. Even if Searle and Kripke were committed to intentionality in a private sense (of being brain states or mental representations), so the objection goes, there is still publicity at the level of the objects to which speakers intend to refer. While I agree that objects are public, I want to claim that they are not public in any `realist´, theory-independent sense (which is the sense which Searle and Kripke suppose). As I have tried to argue in this chapter, the `realist´ sense of individuation to which Searle and Kripke hold (reflected, for example, in their statements about ostension) is mistaken.

Similarity

At the beginning of this chapter, I suggested that individuation and similarity are interrelated. I now want to turn to the notion of similarity and claim that many of the conclusions reached above concerning individuation are applicable to similarity as well, viz. similarity is not a straightforward matter, with objects being similar inherently; rather similarity is interest- and language-dependent.[11]

What does it mean to say that two objects are similar? Intuitively, it means that they share some property or

properties in common (or perhaps that they have some of the same properties). To say that an object is more similar to a second object than to a third means intuitively that the first and second share more properties (or a weighted more) in common than do the first and third. As it stands, of course, this will not do, because we need to know similar in what respect(s). Everything is similar to everything else with respect, say, to existing or being capable of being thought of. On the other hand, nothing is similar to anything else with respect, say, to being at a given spatio-temporal location. We say, then, that a hot, dry, still day is similar to a hot, rainy, windy day with respect to temperature, but not with respect to humidity. But where similarity simplicater is incoherent, does similarity relativized fare any better? Not, I want to say, if it is still assumed that similarity is independent of our language and cognitive interests. Consider the following `objects´:

e c E

The `object´ on the left is, of course, similar to the `object´ in the middle in a presumably denumerable number of respects (e.g. being printed on this page, being English letters, being a certain distance above this word). However, the `object´ on the left is also similar to the `object´ on the right in a presumably denumerable number of respects as well. And the `object´ on the left is dissimilar to the `object´ in the middle in a presumably denumerable number of respects (e.g. being less than one

mile to the left of it, being less than two miles to the left of it, etc.). Likewise for the `object´ on the right.

These considerations seem to argue against similarity simplicater, but what about similarity relativized? That, too, is in trouble, for, as these `objects´ show, the very notion of similarity per se rests on the notion of individuation. One of the ways in which the left and middle `objects´ are similar to each other and dissimilar to the right `object´ is that they are both curved and it is not. One of the ways in which the left and right `objects´ are similar to each other and dissimilar to the middle `object´ is that they are both instances of the English letter <e>. However, for these claims to be made, they rest on our acknowledgment of the existence of, say, English letters. That is, if our partitioning of the world does not include English letters, then it would make no sense to say that two objects are similar with respect to that or dissimilar with respect to that. What sense would it make to say that the left and middle `objects´ are similar to each other with respect to <u>glub</u> but dissimilar to the right `object´ with respect to <u>glub</u>? If glubness is not a recognized reference class, similarity (or dissimilarity) with respect to it is meaningless. And, of course, similarity - even relativized to certain respects - is incoherent if it is taken to be similarity independent of our theories, interests and languages.[12]

Issues of sampling and replication I take to be

subsumed within the issue of similarity and included in this conclusion. Goodman (1972, 1978) mentions sampling in the course of discussing similarity. Similarity, he says, does not account for our predictive and inductive practices (one of the points of the grue example). He relays the following incident:

> Consider again an ordinary swatch of textile in a tailor's or upholsterer's sample book. ...Mrs. Mary Tricias studied such a sample book, made her selection, and ordered from her favorite textile shop enough material for her overstuffed chair and sofa - insisting that it be exactly like the sample. When the bundle came she opened it eagerly and was dismayed when several hundred 2" x 3" pieces with zigzag edges exactly like the sample fluttered to the floor. When she called the shop, protesting loudly, the proprietor replied, injured and weary, "But Mrs. Tricias, you said the material must be exactly like the sample. When it arrived from the factory yesterday, I kept my assistants here half the night cutting it up to match the sample."
>
> The incident was nearly forgotten some months later, when Mrs. Tricias, having sewed the pieces together and covered her furniture, decided to have a party. She went to the local bakery, selected a chocolate cupcake from those on display and ordered enough for fifty guests, to be delivered two weeks later. Just as the guests were beginning to arrive, a truck drove up with a single huge cake. The lady running the bake-shop was utterly discouraged by the complaint. "But Mrs. Tricias, you have no idea how much trouble we went to. My husband runs the textile shop and he warned me that your order would have to be in one piece." (1978, pp.63-64).

Replication, too, as Hanna pointed out, is related to similarity. To say that a given experiment is a replication of another (or, to say that one specific instance of a generic experiment is a replication of that generic experiment) is to say that they are similar in relevant respects. But, as we saw, what counts as relevant is

determined by our cognitive interests and what generic events there are is a reflection of our partitioning of the world.

Where the issue of similarity most clearly touches base with reference is with respect to the notion of natural kinds. Natural kinds are sets (or classes or categories - groupings of some sort or other) of things that occur `in nature´, i.e. in the world, independent of speakers of a language. Examples abound: lions, tigers, oceans, cabbages, etc. The particular members of a natural kind are members of that kind because of some similarity (or similarities) that they share with other members of that kind. If one is committed to essentialism, then to be a member of a given kind is to have a particular property (or properties) that is necessary and sufficient to attain membership in that kind. For instance, if one takes `having atomic number 79´ as the defining property of something being gold, then all and only those things having that property of having atomic weight 79 are things that are gold. Or if being feathered is the necessary and sufficient condition for being a bird, then all and only those things that are feathered are birds. This property (or properties) which guarantees membership a in kind is, then, an essential property of that kind. (I will say more on essences and essentialism in the next chapter.)

The notion of a natural kind, however, does not necessarily depend upon the commitment to essential

properties or essences. Another conception of a kind, or of shared similarity, is what is called `family resemblances´. This notion, originating with Wittgenstein (1953), suggests that some kinds might not have essences. The members of a kind, say, might all be similar to one another, but not necessarily because they all share any common properties. For instance (and this is where the term `family resemblance´ comes from), the various members of a family might share certain features with one another, but not the same feature. A mother and son might have similarly shaped noses, but the son and father might have similarly colored eyes (although very different shaped noses); sister might `have mom´s hair´ and `have dad´s chin´, while only brother and sister might have no (obvious) features in common. They all have enough features in common with at least some other members of the family such that they have a `family resemblance´ with each other. Wittgenstein´s famous example to illustrate this notion is that of games. There is no essence, he claims, to what we consider games. He says: "we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities in detail." (1953, #66). This notion of family resemblance is not antithetical to that of natural kinds. One can still be committed to the notion of (and existence of) natural kinds, even if one denies that these kinds have essences (or, that the members of a kind have essential properties). (The notion of family resemblance

can be taken as either an epistemic notion or a metaphysical notion. That is, one might say that so far as we know a given kind has no essence, but maybe we just haven't found it yet, or one might say that in fact a given kind has no essence independently of our state of knowledge.)

Some problems of similarity have been noted above. Others, in connection with kinds, have been explicated by Quine (1969). Treating similarity as a dyadic relation (e.g. a is similar to b) won't work, says Quine, because it dismisses considerations of degrees. For instance, if colored things comprise a kind, then red things will constitute a set of things too narrow to count as a kind, whereas if red things comprise a kind, then colored things will constitute a set of things too broad to count as a kind. But there seems to be no reason to deny that kinds can overlap (e.g. both mammals and tigers are kinds). While viewing similarity as a triadic relation (e.g. a is more similar to b than to c) escapes the problem just noted, it results in a different problem, viz. almost any two things could count as members of some broad kind.

These problems of similarity are essentially problems encountered by trying to define similarity in terms of kinds. What of the converse, trying to define kinds in terms of similarity? This, too, won't work. In arguing why it won't, Quine introduces two notions: paradigm and foil. He states:

> If without serious loss of accuracy we can assume that there are one or more actual things (<u>paradigm</u>

> cases) that nicely exemplify the desired norm, and one or more actual things (foils) that deviate just barely too much to be counted into the desired kind at all, then our definition is easy: the kind with paradigm a and foil b is the set of all things to which a is more similar than a is to b. More generally, then, a set may be said to be a kind if and only if there are a and b, known or unknown, such that the set is the kind with paradigm a and foil b. (1969, pp.119-120).

This definition, however, won't work. For example, if we take a particular shade of red as the paradigm for a kind, we find that objects in that shade can come in all sorts of shapes, weights, sizes, smells, etc. So mere degree of overall similarity to any particular paradigm case (e.g. a certain apple) will afford little evidence of degree of redness, since it will depend also on shape, weight, size, smell, etc. For Quine, the notions of kind and similarity are substantially one notion.

There are other, related, problems with the notion of natural kinds. For example, most people countenencing natural kinds would probably agree that birds are a natural kind. Not only are birds a kind, but they are natural; the grouping of birds as birds reflects objective, external similarities existing among birds and separate from our languages. (Science, we believe, allows us to deal with what we intuitively might take as borderline cases, e.g. penguins or ostriches or bats.) Now, if we take birds as a natural kind, there seems no reason not to take, say, warblers as a natural kind, also. Indeed, one of the jobs and virtues of ornithology is that it has shown in what sense(s) warblers are a kind, distinguishable from other

kinds of birds. Warblers are not hawks and neither warblers nor hawks are ducks.

However, if we grant that warblers are a natural kind, it seems that problems arise, problems of a slippery slope sort. If warblers are a natural kind, what of Pine Warblers? Or Cape May Warblers? Or Wilson's Warblers? Are these, too, natural kinds? There is no obvious reason to deny that they are, and in fact ornithologists classify them all as separate species. Ornithologists distinguish species of warblers on the basis, at least at one level, of numerous and various sorts of considerations: plumage color, shape, size, geographic location, song, etc. But if Pine Warblers are a natural kind, different from the natural kind of Cape May Warblers, then, as I will now argue, I see no escape from the conclusion that this Pine Warbler is a natural kind and that Pine Warbler is another - separate - natural kind. For what is the basis for distinguishing Pine Warblers from Cape May Warblers? If the distinguishing criteria are phenomenal properties (or a property), then obviously every Pine Warbler is distinct from every other Pine Warbler at a specific enough level. If the distinguishing criteria are genetic, then the same conclusion is reached. If the distinguishing criteria are potential breeding and reproduction (as Mayr (1942) suggested), then, as Hull (1965) remarked, we carry a commitment to talk of potentiality and besides it rules out things like mules as a natural kind. Actual breeding and reproduction might fare

better than potential breeding and reproduction. Whether potential or actual, reproductive isolation might be a (or the) distinguishing criterion of a natural kind. But if the distinguishing criterion is reproductive isolation, then mules seem to stand as a counter-example (not only because they are sterile, and so not reproductively isolated, but also because they are the offspring of horses and donkeys, each of which is taken as a species (natural kind) separate from the other (yet not reproductively isolated).) If the distinguishing criteria are ancestry or historical lineage, then, as Holsinger (1984) claims, the determination of natural kinds becomes somewhat arbitrary:

> If we accept this definition of a reproductive community and this justification of the "morphological yardstick", however, we are left in a quandry. In any "sexual" species there will be a large set of nested and overlapping entities within a single interfertile unit corresponding to different family geneologies. Although we may be able to specify how far back we are to trace the ancestry for defining the reproductive community by using the morphological yardstick, there is no obvious reason for regarding that segment of geneology as any more important or fundamental than the segment of geneology shared by the members of a single population or group of populations or even the segment of geneology shared by members of what would usually be regarded as a genus. Indeed, depending on the processes that we were considering, any one of these levels might be the most important one. (1984, p.301).

There is no clear reason, then, to rule out the slide down the slippery slope to the conclusion that individual entities are kinds. However, this conclusion would destroy the very notion of a kind, at least to the extent of making it superfluous. There



seems no clear, non-arbitrary distinction between kinds and individuals.[13]

Whether or not this slippery slope argument works, it highlights the problem of what kinds are and Holsinger points to what I take to be an even more fundamental trouble with talk of natural kinds independent of our interests and language. Echoing and supplementing Holsinger's claim, Elgin (1983) suggests that like individuation and similarity (i.e. what we take to be individuals and what we take to be similarities), natural kinds (i.e. what we take to be kinds) are dependent upon our cognitive interests and our language:

> The dependence of our systems of kinds on our theories, and the dependence of these in turn on our interests, values, technology, and the like, make questionable at best the thesis that our predicates pick out real properties or natural kinds whose existence, extension, and metaphysical status are independent of any contribution of ours. And the claim that just these kinds or properties are required to answer scientific questions or provide scientific explanations supports that thesis only if backed by an account of why these questions or forms of explanation have priority - an account that does not in turn appeal to the practices or institutions of which they are a part, else all questions are begged. (1983, p.35).

Natural Kinds and Reference

Now, what is the significance of this view of natural kinds (and of similarity) with respect to issues of reference? Searle has said nothing about natural kinds in connection with reference. However, Kripke has said a great deal. Within the context of discussing identity statements

(and his view that for rigid designators, if identity statements are true, then they are necessarily true), he summarizes his view on natural kinds (and natural kind terms) in the following five claims:

> First, [Kripke's] argument implicitly concludes that certain general terms, those for natural kinds, have a greater kinship with proper names than is generally realized.
>
> Second, [Kripke's] view asserts, in the case of species terms as in that of proper names, that one should bear in mind the contrast between the a priori but perhaps contingent properties carried with a term, given by the way its reference was fixed, and the analytic (and hence necessary) properties a term may carry, given by its meaning.
>
> Third, in the case of natural kinds, certain properties, believed to be at least roughly characteristic of the kind and believed to apply to the original sample, are used to place new items, outside the original sample, in the kind.
>
> Fourth, scientific investigation generally discovers characteristics of gold which are far better than the original set.
>
> Fifth, and independently of the scientific investigations just mentioned, the `original sample' gets augmented by the discovery of new items. (1980, pp.134-139).

To get clearer on what Kripke is saying here and on what his commitments are with respect to natural kinds (and, so, with respect to similarity) each of these five claims will be inspected.

First, natural kind terms have a greater kinship with proper names than is generally realized. Kripke states that this holds "for certain for various species names" (1980, p.134), including count terms (e.g. `tiger') and mass terms (e.g. `gold'). It holds for terms designating natural



phenomena (e.g. `heat´) and corresponding adjectives (e.g. `hot´). This claim is made clearer by and supported by the arguments put forward for the other four claims, so at this point I will only say that for Kripke this first claim is true because he sees natural kind terms, just as proper names, as rigid designators. Identity statements involving natural kind terms are, for Kripke, necessarily true if true at all. The reasons for this will be seen as the remaining four claims are inspected.

Second, we must keep in mind the difference between the properties by which reference is fixed for a natural kind term and the properties which the term carries (which are given by its meaning). The reference fixing properties are a priori and (perhaps) contingent, whereas the `carried´ properties are analytic and (hence) necessary. Just as descriptions or ostension can establish a connection between an object and a proper name (e.g. the Neptune case), so, too, can descriptions or ostension establish a connection between a kind and a term (e.g. `gold´ designates the substance instantiated by the items over there, or at any rate, by almost all of them). To see why the reference fixing properties are a priori (and perhaps contingent), while the `carried´ properties are necessary, consider the following example. Suppose gold has been defined as `a yellow metal´ (`gold´ means - perhaps among other things - `a yellow metal´). Could we discover that in fact gold is not yellow? Perhaps because of some peculiar atmospheric

conditions or properties, we have always experienced an optical illusion in seeing gold as yellow; in fact, after rectifying those conditions, we come to see that gold is really blue. Would we then say that there is no gold (since gold is a yellow metal)? Kripke says, no; rather, we would say that gold is not really yellow. That stuff, gold, exists, but we were wrong to think it was yellow. Our designation of iron pyrite, which shares many of the phenomenal properties of gold, as fool's gold (not as gold), shows that we can change our descriptions which we associate with a natural kind term, yet say that we are nonetheless talking about the same stuff (e.g. gold, even if it turns out to be blue). So the properties which are associated with a natural kind when reference is fixed might well be contingent properties (such as being yellow), but once they have been used in the fixing of reference, they are a priori properties (because we have stipulated, in the fixing of reference, that this property is true of the object, or kind). On the other hand, since we could discover at a later time that those reference fixing properties do not, in fact, pick out the stuff we thought they did (e.g. gold turns out to be blue, not yellow), they are not necessarily true of the object, or kind. Rather, the properties 'carried' by the term (i.e. the properties that the stuff 'really' has) are the properties that are necessary.

Third, certain roughly characteristic properties, believed to apply to the original sample, are used to place

new items in the kind. These properties, Kripke tells us, need not hold a priori of the kind; we could at a later time establish that some of the properties of the original sample were peculiarities of that sample, and not generalizable to the kind as a whole. So, an object might not have all the properties of the original sample, but nonetheless belong to the kind (e.g. if `being capable of flight´ were the property which characterized the natural kind bird, then ostriches would not be birds, but they are). On the other hand, an object might possess the characteristics of the original sample by which the kind was established, yet not belong to the kind (e.g. if `being capable of flight´ were the property which characterized the natural kind bird, then bats would be birds, but they aren´t). It is an empirical matter, then, whether the properties originally associated with the kind are in fact defining properties of the kind. While the joint sufficiency of those properties might be true, they are not necessarily true. Universal applicability, however, is necessarily true, if true. Kripke claims: "`Cats are animals´ has turned out to be a necessary truth. Indeed, of many such statements, especially those subsuming one species under another, we know a priori that, if they are true at all, they are necessarily true." (1980, p.138).

Fourth, scientific investigation generally discovers characteristics of gold (or any kind) which are far better than the original set. The standard of goodness here is

strict necessity, in the sense that identity statements which express necessary truths are `better´ than those which don´t. Kripke gives the example of gold being identified with the property `having atomic number 79´. If it is true that chemists have established that (pure) gold has atomic number 79 (i.e. an object is (pure) gold iff it has atomic number 79), then this identity is stronger (`better´) than one which is based on the phenomenal characteristics of the original sample (e.g. gold is a yellow metal). Kripke states:

> In general, science attempts, by investigating basic structural traits, to find the nature, and thus the essence (in the philosophical sense) of the kind. ...The type of property used in science seems to be associated with necessity, not with a prioricity, or analyticity. (1980, p.138).

Fifth, the `original sample´ gets augmented by the discovery of new items. Kripke doesn´t say further what he means by `augmented´ here, but I take it that he means that the extension of the term increases to include new or different objects than just the original sample (e.g. we find more chunks of gold or discover birds that were not discovered before - if for no other reason than that some of these new birds were just born!).

Just as with proper names, species names (or kind names) can be passed from link to link in a chain and their reference is determined by a causal (historical) chain, not by the use of any items. (A slight difference between kind names and proper names is that for some natural kind terms, sensed phenomena, the way that the reference was fixed seems

`overwhelmingly´ important to us, which, for Kripke, is not the case with proper names. The fact that we identify light in a certain way, he says, "seems to us to be crucial, even though it is not necessary" (1980, p.139).

Now, what is to be made of Kripke´s account of natural kinds? It is, I think, basically mistaken, and mistaken because it rests ultimately on `realist´ notions of individuation and similarity. As we saw above, both notions are coherent only as interest- and language-dependent notions. With respect to kinds, what I have argued above is that what `natural´ kinds there are depends upon what reference classes and similarities we recognize, and those are interest- and language-dependent. Perhaps the best way to see Kripke´s commitment to a `realist´ view of kinds is to look at his five claims about natural kinds.

His first claim, as we noted, was simply that natural kind terms have a greater kinship with proper names than is generally recognized. This claim, I said, would be borne out with the other four claims, and so I turn to them.

His second claim was that we must keep in mind the difference between the properties by which reference is fixed for a natural kind term and the properties which the term `carries´. As we saw, Kripke hypothesizes that the reference of a natural kind term (just like a proper name) can be/is fixed by ostension or association of certain descriptions. Both methods, I have argued above, are mistaken if taken in a `realist´ way. As we saw,



Wittgenstein and Quine showed ostension to be not a straightforward matter at all, but interest- and language-dependent. The association of descriptions, we saw, depends upon a conception of partitioning the world, and that was interest- and language-dependent.[14]

Besides commitments to talk of ostension and reference fixing, Kripke's talk of properties which a term `carries' is, if meant in a `realist' way, also mistaken. That is, a term `carries' properties only because terms are part of public language, and while - because of being public - these properties are indeed `carried' by the term independently of any given speaker, what properties a term `carries' depends upon how we partition the world. As I will argue in the next several chapters, Kripke's view that terms `carry' properties is committed to realism and essentialism, which in turn are dependent upon particular commitments with regard to individuation and similarity. Just as the latter are not independent of us, so the former also are not.

Third, certain roughly characteristic properties, believed to apply to the original sample, are used to place new items in the kind. There are several problems here. First, Kripke uses the notion of an original sample as though it were a straightforward matter. However, as we saw above, Goodman has shown that the notion of a sample depends upon the notion of similarity, and that is not straightforward. (My criticism here is with respect to

Kripke's apparent conception of a sample. Below I will criticize his apparent conception of an original sample.) A second problem with this claim of Kripke's is his view of the peculiarities of the original sample. Those properties roughly characteristic of a kind which are believed to apply to the original sample, Kripke tells us, need not hold a priori of the kind. He says: "later empirical investigation may establish that some of the properties did not belong to the original sample, or that they were peculiarities of the original sample, not to be generalized to the kind as a whole." (1980, p.137). The issue above was that what properties belong to the original sample (or to anything, for that matter) depends upon our interests and language; the issue here is the other side of the coin - what counts as peculiarities of the original sample (or of anything, for that matter) also depends upon our interests and language. As noted earlier, objects are both similar and dissimilar to other objects in a denumerable number of ways. So, too, there are a denumerable number of things which are peculiar and not-peculiar to an object (or, a denumerable number of ways in which an object is both peculiar and not-peculiar). Again, it depends upon our partitioning of the world. Our interests (or language or partitioning) might change in the future, so that what is taken as peculiar to the original sample (or to anything) changes, but that makes the notion of peculiarity no less dependent. A third problem with Kripke's claim is that I think he overstates his contention

that universal applicability, if true, is necessarily true. He remarks that `cats are animals´ is a necessary truth for us. While I am somewhat sympathetic to his view here, I think that it is far from obvious. Consider the following example. I believe that all coal is hard, black and rough until one day I read in the latest mineralogist journal that Professor X has uncovered coal with the exact characteristics of chocolate pudding. Coal is not always hard, black and rough states Professor X, but some types are in fact oozy, brown and velvety smooth. What would we make of such a claim? I suspect, like Kripke, that we would be more inclined to believe that (fantastically) Professor X had unearthed an underground vein of chocolate pudding than that some coal is oozy, brown and smooth. However, unlike Kripke, I do not think that such an inclination is obvious. The thought of unearthing underground pudding is not much less fantastic than the thought of some types of coal being oozy. (I am not suggesting that Kripke is wrong here, but that his confidence that he is right might be a bit questionable.)

Fourth, scientific investigation generally discovers characteristics of kinds which are far better than the original set. I will say more about this claim in the final chapter, and in fact argue against it, but for now I only want to remark that Kripke´s assertion that the type of property identity used in science seems to be associated with necessity not with a prioricity or analyticity is not

so clear. Kripke takes great care in distinguishing epistemic and metaphysical issues, between analyticity and necessity. However, I think that the distinction is not as clear as Kripke indicates. Using the coal example above, for Kripke, if it turns out that `coal is hard, black and rough´ is a true identity statement, then it is necessarily true. We do not recognize this oozy stuff as coal because it doesn´t have the properties of coal. It might not be chocolate pudding, but, whatever it is, it definitely isn´t coal. Now, I see little difference between saying something like that (or, `cats are animals´ is necessarily true) and saying that scientific identity statements are unfalsifiable (an epistemic notion). If `cats are animals´ expresses a necessary truth, then it seems to me that it also expresses an unfalsifiable statement. Where, then, is the difference between the metaphysical and the epistemic with respect to these sorts of identity statements? It seems that all necessary truths are unfalsifiable statements (something, I take it, that we <u>don´t</u> want as part of science). It may or may not be the case that all unfalsifiable statements are necessary truths, but whether they are or not, the distinction between metaphysical and epistemic issues is not so clear.

Fifth, the `original sample´ gets augmented by the discovery of new items. In a footnote to this claim, Kripke admits some hesitation to its formulation. He states:

> Obviously, there are artificialities in this whole account. For example, it may be hard to say

> which items constitute the original sample. Gold may have been discovered independently by various people at various times. I do not feel that any such complications will radically alter the picture. (1980, p.139, note 70).

Kripke's commitment to ostension and a `realist' conception of similarity seems to me obvious here. Gold could not be discovered, I want to say, by anyone anywhere independently of our particular partitioning of the world. We partition the world into, say, metals and non-metals, yellow and non-yellow, elements and non-elements, etc., and in that sense, gold is not discovered at all, but stipulated. An objection to this is that whatever we call it (or don't call it), this stuff was discovered (perhaps by various people at various times). But, of course, I want to ask: what stuff? We are right back to ostension and the issue of similarity, and, as I have repeatedly said, Kripke is mistaken to see these issues as interest- and language-independent.

Summary

At this point I would like to quickly summarize the main points I have tried to make so far in this (and the preceding) chapters. Both the cluster and the causal theories of reference, I have argued, share fundamental commitments to private reference, which in turn rests on particular commitments to and conceptions of individuation and similarity. Those conceptions, I have urged, are mistaken, and so those commitments to those conceptions weaken the theories. To the extent that both theories share

certain commitments and conceptions, I see them as theories which are more alike than different, and to the extent that they hold the commitments and conceptions that I say they do, they are both mistaken and faulty. I now want to show that they are further committed to essentialism, realism and a correspondence theory of truth, and to the extent that they hold certain conceptions about these commitments, they are again mistaken and faulty. In the final chapters I will discuss alternative commitments and conceptions and an alternative view of reference.

MICHIGAN STATE UNIV. LIBRARIES
31293007971009

MICHIGAN STATE UNIVERSITY LIBRARIES
3 1293 00797 1017

THESIS
pt. 2

LIBRARY
Michigan State
University

PLACE IN RETURN BOX to remove this checkout from your record.
TO AVOID FINES return on or before date due.

DATE DUE	DATE DUE	DATE DUE
9.27-93 1.4.6		

An Affirmative Action/Equal Opportunity Institution
c:\circ\datedue.pm3-p.

NOTES

[1]Or, another example: "When you ask whether it is necessary or contingent that Nixon won the election, you are asking the intuitive question whether in some counterfactual situation, this man would in fact have lost the election. (1980, p.41).

[2]Elgin (1980) makes this same point with regard to the notion of scientific progress.

[3]The reason we "are well aware which of the two incompatible predictions is genuinely confirmed," for Goodman, finally boils down to pragmatic considerations. English (at least in this case) works better than the gruesome language.

[4]Goodman repeats this theme throughout his writings:

> There are many different equally true descriptions of the world, and their truth is the only standard of their faithfulness. And when we say of them that they all involve conventionalizations, we are saying that no one of these different descriptions is exclusively true, since the others are also true. None of them tells us the way the world is, but each of them tells us a way the world is. (1972, p.31).
>
> Frames of reference, though, seem to belong less to what is described than to systems of description ... .If I ask about the world, you can offer to tell me how it is under one or more frames of reference; but if I insist that you tell me how it is apart from all frames, what can you say? We are confined to ways of describing whatever is described. Our universe, so to speak, consists of these ways rather than of a world or of worlds. (1978, pp.2-3).

[5]Even Nagel (1961) accepts the presence of `characterizing value judgments´ (as opposed to `appraising value judgments´) as part of science. These `characterizing value judgments´ are a version of what I am calling `cognitive values´.

[6]Another argument for the interest- (and language-) dependence of theories is that theories are, in large part, metaphorical (cf. Kuhn (1962, 1977, 1982), Hesse (1974, 1980, 1982) and Rothbart (1984)). Quite simply, metaphors reflect and display our interests and languages; these metaphors are an integral part of our theories.

[7]Such a treatment of individuals might seem strange because our common sense notions of individuals as clearly demarcated objects, each with a rather distinct spatio-

temporal continuity. However, Quine (1976) and Goodman (1972) have both argued that there is no good reason to forbid the conception of individuals which consist of widely scattered and very unlike parts. (Quine, for example, see nothing wrong with speaking of mass terms, such as sugar, as individuals.) Leonard and Goodman (1940) pursued this conception of individuals to the point of attempting to construct a calculus to accommodate it.

[8]There has been some debate whether events can be treated as individuals. Davidson (1970), I think, has argued conclusively that they can.

[9]Cf. Thomson (1965) for further argumentation for a `non-realist´ treatment of experimental replicability.

[10]In fact, Kripke´s presentation of his Gödel-Schmidt counter-example to what he calls thesis (3) of the cluster theory of reference begs the issue. Kripke says: "For example, it is argued, if we say, `Gödel proved the incompleteness of arithmatic,´ we are, of course, referring to Gödel, not to Schmidt." (1980, p.85, note 36). This begs the issue because by saying, "we are, of course, referring to Gödel" Kripke seems to think that this instance of `Gödel´ clearly refers to an independently located object. But it doesn´t. Who is he referring to here? It won´t help to say, "Well, to Gödel, of course" because that is just what is at issue. Kripke seems to take it that we could, if need be, rely on ostension to pick out Gödel.

[11]Indeed, much of the discussion above about partitioning could be interpreted as being more explicitly related to similarity than to individuation. For example, the green/grue question is a question of whether emeralds are similar with respect to color. Of course, one of the points of this chapter is that there is no clear distinction between issues of individuation and issues of similarity.

[12]For an interesting and detailed discussion of similarity and its significance for philosophy of science, see Barnes (1982).

[13]The issue of natural kinds has been one that has generated a lot of attention recently within the philosophy of biology. A large body of literature has flowered concerning the notion of species versus individuals (or species as sets versus species as individuals). See, for example, Hull (1978), Kitts and Kitts (1979), Dupre (1981), Holsinger (1984) and Kitcher (1984). Similar conceptual difficulties as those noted in the text above exist for asexual and non-biological entities.

[14]It is interesting to note that with regard to this point, a footnote of Kripke´s sounds quite reminiscent of

his thesis (5) of the cluster theory (viz. The statement, `If X exists, then X has most of the Ø´s´ is known a priori by the speaker). Kripke says in the text (with regard to the baptismal reference fixing): "Several features of this baptism are worthy of note. First, the identity in the `definition´ does not express a (completely) necessary truth: though each of these items is, indeed, essentially (necessarily) gold,[69] gold might have existed even if the items did not." (1980, p.135). His note 69 reads:

> Assuming, of course, that they are all gold; as I say below, some may be fool´s gold. We know in advance, a priori, that it is not the case that the items are typically fool´s gold; and all those items which are actually gold are, of course, essentially gold. (1980, p.135, note 69).

Now, it is not obvious to me that there is any difference between `If Aristotle exists, then Aristotle has most of the Ø´s´ and `If gold exists, then gold has most of the Ø´s´ While Kripke explicitly states that we must distinguish between the reference fixing properties of an object and the (I take it) essential properties of an object, this reminder does not prove to be so helpful. For example, suppose one of the reference fixing properties is an essential property, and we know it (supposing that there are essential properties and that we can in fact recognize them). It would seem, then, that Kripke is committed to the a prioricity thesis. A possible response to this claim is that even if one of the reference fixing properties is an essential property, that establishes a necessary truth, not a priori knowledge. One might say, it might be the case that an essential reference fixing property is (necessarily) true of an object, but it doesn´t follow that we know this a priori. However, it seems to me that if we fix the reference of a term a priori and if, when doing that, we know that one of the reference fixing properties is essential, then we know a priori that if that thing (stuff) exists, then it has - if not most of - one of the Ø´s. For example, suppose in 1947 (before Israel became a sovereign state, but knowing that it soon would be), someone said, "Anyone born in Israel is an Israeli citizen." It seems to me that the speaker knows a priori that if someone is born in Israel after time t (where time t is when Israel attains statehood), then that person is an Israeli citizen (i.e. if X exists, then X has one of the Ø´s). It might be argued that being a citizen, or being an Israeli citizen, is not a natural kind, but, of course, that is part of what is at issue (viz. what a (natural) kind is). Besides, the point here is that Kripke sounds quite a bit like the cluster theory of reference which he criticizes.

CHAPTER SIX

ESSENTIALISM

In the previous section we saw that the received theories of reference are committed to certain conceptions of individuation and similarity, conceptions which I argued were mistaken. An area of particular intercourse between issues of reference and issues of individuation and similarity is that of the notion of kinds, especially natural kinds. As we saw, in the discussion of natural kinds, the notion of essences (or essential properties) was integral. Among Kripke´s claims about natural kinds (natural kind terms) was his assertion that in general, science attempts, by investigating basic structural traits, to find the nature, and thus the essence of kinds. In this chapter I want to look more closely at essences and essentialism. I will argue that conceptions of individuation and similarity carry with them conceptions of essences and commitments to given views of individuation and similarity carry with them commitments to given views of essences. Furthermore, I will argue that both Searle and Kripke, by being committed to mistaken `realist´ conceptions of individuation and similarity, are committed to a mistaken `realist´ conception of essences (i.e. to essentialism as it is usually interpreted).

One of the difficulties in talking about essences or essentialism is that philosophers are not always in agreement about how they state the issue. Depending on whom one reads, one gets a different statement of what essences (essential properties) are or what essentialism is. Brody (1980), for example, speaks of essential properties as those properties necessary for an object's existence:

> What is the basic idea behind the claim that there is a distinction between the properties that an object has essentially and the properties that an object has accidentally? It seems to be the following: on the one hand, there are some properties that an object must have; if the object didn't have them, it wouldn't exist at all. These are the properties that an object has essentially. On the other hand, there are some properties that an object has but that it might not have. The possession of these properties is not necessary for the object's existence. These are the properties that an object has accidentally. (1980, p.84).

Plantinga (1974) offers several definitions of 'essence' including the following:

> E is an essence of [say] Socrates if and only if E is essential to Socrates and everything distinct from him has $\bar{E}$ (the complement of E) essentially. (1974, p.70).

> E is an <u>essence</u> if and only if there is a world W in which there exists an object <u>x</u> that (1) has E essentially, and (2) is such that there is no world W* in which there exists an object distinct from <u>x</u> that has E. (1974, p.72).

> S is an essence if and only if S is a complete and consistent set of world-indexed properties. (1974, p.77).

For Flew (1979) the "notion of a fixed and timeless possibility of existence" (1979, p.111) is how he defines 'essence'. Subsequently, he offers several conceptions of

essentialism. One conception of essentialism is: a position holding that something can have an essential property in virtue of a definition, or as described in a certain way. Another conception of essentialism is: a view that maintains that some objects - no matter how described - have essences; that is, they have essentially or necessarily, certain properties, without which they could not exist or be the things they are.

Hirsch (1971) characterizes essence in terms of spatio-temporal continuity of an object. He argues that among the properties which are true of an object, some play a special role in determining an object's identity. The terms which name those special properties (he calls them E terms) are, in a sense, necessary to the object's identity. All other terms which name properties true of an object, but which are not E terms, he calls A terms. He capsulizes his position (with the insistence that this is only preliminary) as follows:

> A term is an E term if it cannot possibly be true of an object temporarily; if it is true of an object at all, it is necessarily true of the object throughout the object's entire career. (1971, p.34).

The point, of course, of all these citations is that one of the difficulties in talking about essences and essentialism is that philosophers speak in a multitude of ways when discussing these issues. When I speak of essences or essential properties I will mean the following: to say that an object[1] has an essence (i.e. an essential property

or properties) is to say that the object has some property or properties that are necessary for that object to be that object (i.e. for that object to be what it is). Essentialism I take to be the view that objects have essences or essential properties. (I take the following definition of essence to be simply a different characterization of the definition I have just given: an object has an essence (i.e. an essential property or properties) if that property or properties is true of the object in all possible worlds; or, the object has a certain property or properties in all possible worlds.[2] Another characterization of this position I am trying to elucidate is given by Slote (1975): a property p is essential to an entity e if and only if e could not have failed to have p; essentialism is the view that various entities have certain of their properties essentially (1975, p.1).)

As noted above, one objection to essentialism is that not all objects classified as the same kind necessarily have a shared essence. (Keep in mind here that, for the moment, I am accepting the distinction between individuals and kinds, and, for the moment, I am speaking about essences with respect to kinds.) The notion of family resemblances is meant to illustrate this point. Some kinds might have an essence; an object might need to have a particular property in order to be a member of a certain set (e.g. for an object to be a bachelor it must be unmarried). However, this requirement of having an essential property might not hold

for all kinds (e.g. there might be no essence to games). The notion of family resemblance, of course, is not so much a direct criticism of the notion of essences as it is a criticism of the universality of the doctrine of essentialism. That is, even if the notion of family resemblance is true, it does not show (nor is it intended to show) that there are no essences or that essentialism is wrong, rather it shows that not all objects have essences (or that having an essence is not an essential property of objects!). While this is not so much a direct criticism of essentialism, it is a challenging `indirect´ criticism of essentialism. The challenging aspect of it is that, at least with respect to kinds, the doctrine of essentialism should not be assumed. It might not at all be the case that as science progresses it discovers the underlying traits, and hence the essences, of (some) things. There might simply be no non-arbitrary underlying trait to games.

There is a deeper challenge posed to the doctrine of essentialism by the notion of family resemblance. It is this: the notion of family resemblance calls into question the presumed sharp distinction between questions and issues of ontology and epistemology. Let me explain. The doctrine of essentialism presumes that if objects are considered to be members of the same kind (are called the same kind of thing; are in the extension of the same predicate) then they must share some property (or properties) in common. As science progresses the shared properties by which we group

objects into kinds (by which we call objects by a certain term) will be the underlying properties which we discover these objects (and kinds) to have. Ideally, science will result in the discovery of essential properties. But note that this view presumes that the ontic question of whether kinds have essences (and if so what they are) is separate from the epistemic question of how we (come to) know this. Likewise, the semantic question of what we call essential properties is presumed to be separate from the epistemic question of how we (come to) know this. The notion of family resemblance, however, calls this separation of questions into question. That is, if there is no compelling reason to believe that all games share a particular property (or properties) simply because they are called games, then there is no compelling reason to believe that kinds `really do´ have essences or that there `really are´ things which we call essential properties. After all, what sense does it make to claim that games `really do´ have an essence even if we can´t find it?[3]

While the notion of family resemblance is, I think, a strong challenge to the doctrine of essentialism with respect to kinds, it is not so clearly a strong challenge to the doctrine of essentialism with respect to the identity of objects. The doctrine of essentialism pertains both to kinds and to individuals. Not only is it claimed that for an object to belong to a given kind, the object must have an essential property (or properties) <u>of that kind</u>, but also it

is claimed that for an object to be what it is, rather than being some other object, it must have some essential property (or properties). While the notion of family resemblance challenges the doctrine of essentialism in the former case, it seems irrelevant to the latter case. It seems to make little sense to speak of an object having a family resemblance to itself. (The only sense it might make is trivially - it resembles itself since it is identical with itself. Indeed, if the doctrine of essentialism is true at all, then, as Cartwright (1968) has said, it is true in this sense, since the attribute of being self-identical is essential to everything.)

Now, it seems to me that the distinction between the essence of kinds and the essence of individuals is not so obvious (in one sense), nor does the doctrine of essentialism hold in either case. At this point I want to argue for this claim and then indicate how I see both Searle and Kripke as being committed to the doctrine of essentialism.

Individual Essence versus Kind Essence

Intuitively the distinction between the essences of kinds and the essences of individuals seems plausible. This is reinforced by the consideration that the notion of family resemblance makes sense with respect to kinds but not with respect to individuals. However, other than with regard to the applicability of the notion of family resemblance, what is the basis of the distinction? It seems to be this: it

makes sense to speak of an object being what it is independently of an object being an instance of a kind. True, Nixon is a member of a denumerable number of kinds (e.g. former presidents, Americans, males), but independently of all of those, Nixon is Nixon. In some possible world, he is not a member of the kind `former presidents´, but in all possible worlds Nixon is Nixon. The essential property (or properties) that Nixon has such that he is Nixon in virtue of having that property (or those properties) is different than the essential property (or properties) that Nixon has such that he is a former president in virtue of having that property (or those properties). For example, Nixon´s essence as Nixon might be his particular genetic make-up, whereas Nixon´s essence as a former president is his having been president at some time in the past. So the distinction between the essence of kinds and the essence of individuals is legitimate.

Now, I want to suggest that the intuitive legitimacy of this distinction is mistaken, or at least not so clear. We cannot speak of, say, Nixon as Nixon outside of speaking of Nixon as a member of some kind. What sense does it make to say that Nixon is Nixon regardless of whether he is a former president or an American or ...? Speaking of Nixon as the object with such-and-such a genetic make-up won´t help because that, too, places Nixon within a particular kind (e.g. biological organisms).[4] My point here is that we can never say who (or what) Nixon is without individuating

Nixon. But the process of individuating Nixon is also to place Nixon within kinds. To say that Nixon is this individual is to say that Nixon is not that individual (or anything else). This requires some principle of individuation. After all, we must use some criterion to individuate Nixon. But as I argued in the previous chapter, individuation is interest- and language-dependent. This, I take it, is much of the point and the force of Quine's opposition to essentialism:

> Mathematicians may conceivably be said to be necessarily rational and not necessarily two-legged; and cyclists necessarily two-legged and not necessarily rational. But what of an individual who counts among his eccentricities both mathematics and cycling? Is this concrete individual necessarily rational and contingently two-legged or vice versa? Just insofar as we are talking referentially of the object, with no special bias toward a background grouping of mathematicians as against cyclists or vice versa, there is no semblance of sense in rating some of his attributes as necessary and others as contingent. Some of his attributes count as important and others as unimportant, yes; some as enduring and some as fleeting; but none as necessary or contingent. (1960, p.199).

The gist of Quine's example, I believe, is that it makes no sense to speak of essential (or accidental) properties without the presumption of some `background grouping'. While Quine does not address the issue directly, I take his point as pertaining to both individuals and kinds. It makes no sense to say that a particular property is essential to an object unless it is clear what kind of object the object is. In addition, it makes no sense to say that a particular property is essential to an object unless it is clear what

object is being referred to and that the object is this object, not that object. This, however, presupposes the process of individuation, which in turn presupposes a principle of individuation. We cannot separate the issue of Nixon as an individual from the issue of Nixon as the member of kinds. Such a separation, though, is vital to the distinction between the essence of kinds and the essence of individuals.

At this point one might object that my claim is false that we cannot separate the issue of Nixon as an individual from the issue of Nixon as a member of kinds. The objection is that something must be some thing in order for it to be a member of some kind. To be a member of some kind is to be a member, i.e. to be an (identifiable) individual. Individuation, then, precedes membership (and partitioning). However, I think that this objection fails. It fails because, as I argued in the previous chapter, individuation (and partitioning) is theory- and interest-dependent. Individuals are not `out there´ waiting to be given membership into various kinds. Individuation requires a principle of individuation, which rests on some conception of similarity. As we saw, the paradigm case for supposed objective individuation is ostension. But, as we saw, ostension makes sense only when the objects referred to are partitioned into kinds (e.g. `sosh´ makes sense, and objects can be identifiable as being sosh, only when we know that, say, sosh is a color). While it is true that similarity

makes sense only when there are individuals which we can call similar or dissimilar (and, so, individuation precedes membership and partitioning), it is also true that individuation makes sense only when there is a principle of individuation (i.e. some similarity relation so that we can determine that an object is what it is and not something else). Partitioning, then, precedes individuation. I do not see this result as contradictory. Rather, it seems to me that it points to the interdependence between individuals and kinds, between a principle of individuation and a similarity relation. To ask which comes first conceptually is, to me, to ask which comes first, the chicken or the egg. With respect to the particular point at issue, I think that the objection fails which says that because individuals precede membership the distinction between essences of kinds and essences of individuals is not blurred.

Earlier I claimed that the notion of family resemblances was a strong criticism of essences of kinds but that it seemed irrelevant to essences of individuals. I then argued that the distinction between essences of kinds and essences of individuals is blurred. These two claims might seem to entail that the notion of family resemblances is relevant, after all, to the issue of the essences of individuals (since we can't sharply distinguish kinds and individuals). However, it is still not clear how family resemblances could be relevant to individuals. The reason that this is not clear, it seems to me, is that it rests on

an underlying conception of individuals as simple, indivisible entities. That is, family resemblance makes sense for comparing different entities, but it doesn't make sense when `comparing' a single entity to itself; the notion of comparison requires two or more entities being compared. This underlying conception, though, is questionable for two reasons. First, if Ghiselin (1974) and Hull (1978) are right, some individuals are not simple, indivisible entities (e.g. they argue that species can be considered individuals). I will return to their proposal at the end of this chapter. Second, as argued above, we cannot treat individuals as simple, indivisible entities outside of the context of treating individuals as members of kinds. Individuals are not just `out there' as simple, indivisible entities. I take talk of possible world individuals and cross-world identity to be an illustration of this. We speak of the same individual in different possible worlds (or in different counterfactual situations) and speak of identifying this individual in these various possible worlds. Criteria for cross-world identification might be the presence of some essential property (a la Kripke), but there is nothing incoherent with suggesting family resemblance as a criterion for cross-world identification. While Kripke is a philosopher in this world, he might be a plumber or an exotic dancer in other possible worlds, and there is nothing incoherent in claiming that we use family resemblance criteria for identifying him in these various

possible worlds. While this does not show the necessity of family resemblance criteria, it is not intended to. The notion of family resemblance was never intended to deny the existence or plausibility of essences; rather it was intended to deny the necessity of the existence or plausibility of essences.

Above I remarked that the separation of the issue of, say, Nixon as an individual from the issue of, say, Nixon as the member of kinds was vital to the distinction between essences of individuals and essences of kinds. In addition, it seems to me that it is vital to the essentialism to which both Searle and Kripke are committed. I want, then, to turn to them. I want to show in what sense they are both committed to essentialism and show how their commitment is based on this mistaken distinction and ultimately on their mistaken conceptions of individuation and similarity.

Reference and Essence

Searle does not directly address the issue of essentialism in his discussion of reference, however, his theory of reference does, I want to say, commit him to essentialism. As noted in chapter one, Searle states that it is "a necessary truth that Aristotle has the logical sum [inclusive disjunction] of the properties commonly attributed to him" (1969, p.173). It is a necessary condition for an object to be Aristotle that it satisfy at least some of the descriptions which speakers associate with the name `Aristotle´. This sort of claim strikes me as a

clear commitment to essentialism. There is some description (property) affiliated with Aristotle such that for an object to be Aristotle, it must satisfy this description (have this property). Granted, we might not know exactly what that description (property) is, nonetheless, within the disjunctive set of descriptions (properties) associated with `Aristotle´, it is necessary that for an object to be Aristotle it must satisfy that disjunctive set.[5]

Searle makes other essentialist claims. For example, with respect to the issue of whether or not proper names have senses, Searle interprets the question `Do proper names have senses?´ in two forms, a weaker and a stronger form. The weaker interpretation of the question `Do proper names have senses?´ is: `Are any propositions where the subject is a proper name and the predicate a descriptive expression analytic?´ To answer this question, for Searle, we need to be clear on the identity of the object referred to; we need to know that we are talking about the same object on different occasions. We need to know that a particular name names the same mountain, person, river, etc. on different occasions. So, he says, this gives an affirmative answer to the weaker version of the question of whether or not any propositions where the subject is a proper name and the predicate is a descriptive expression are analytic. Says Searle:

> Some general term is analytically tied to any proper name: Everest is a mountain, the Mississippi is a river, de Gaulle is a person. Anything which was not a mountain could not be

> Everest, etc., for to secure continuity of reference we need a criterion of identity, and the general term associated with the name provides the criterion. Even for those people who would want to assert that de Gaulle could turn into a tree or a horse and still be de Gaulle, there must be some identity criterion. De Gaulle could not turn into anything whatever, e.g., a prime number, and still remain de Gaulle, and to say this is to say that some term or range of terms is analytically tied to the name "de Gaulle". (1969, p.167).

Now, it seems to me that Searle is explicitly stating here that being a mountain is a necessary condition (an essential property) for being Everest, being a person is a necessary condition (an essential property) for being de Gaulle. etc. There are several points here which are worth noting. First, Searle does not seem (at least in this quotation) to acknowledge a difference in kind between essence of kinds and essence of individuals. What is essential for an object to be de Gaulle is for the object to be an instance of a certain kind, viz. a person. Of course, this particular quotation does not rule out that Searle accepts a distinction between essences of kinds and essences of individuals, but I have found nothing to indicate that he notes such a distinction. And, as seen in the above quote, when considering what is essential to de Gaulle, Searle first suggests that it is his personhood, not, say, his genetic make-up or historical origin.

A second point worth noting with regard to Searle's position is that he insists that a criterion of identity is necessary for continuity of reference. That is, we need to be assured that we are referring to the <u>same thing</u> when we

refer on different occasions. With this I agree, but where I think Searle goes wrong is that he seems to think that this is a fairly straightforward matter; a thing's sameness is given to us. Our ordinary language apparently provides us with no difficulty here because it reflects the thing's given sameness. Granting Searle his claim that Everest or de Gaulle couldn't turn into just anything, Searle makes no comment that Everest's mountainhood or de Gaulle's personhood is problematic. His lack of comment here, I want to say, is indicative of his acceptance of his `realist' conception of individuation and similarity. Searle is right, I think, in pointing out that we need to consider a thing's identity in terms of its sameness, but he is wrong in failing to question the `givenness' of this identity and sameness. That is, Searle fails to note the interest- and language-dependence of issues of individuation and similarity. Consequently, in discussing reference to an object, he stresses a conception of the object's essence, since that is ultimately how its identity will be assured (and, so, proper reference will be assured).[6]

As opposed to Searle, Kripke speaks frequently of essences and essentialism. He seems to carefully avoid making any explicit commitment to essentialism, and Salmon (1981) has argued that the causal theory of reference is not committed to any non-trivial version of essentialism, but, I want to argue, he is committed to essentialism all the same.

As we saw earlier, Kripke suggests that as science

progresses it discovers underlying traits, and hence essences, of natural kinds. And, as we saw earlier, Kripke ties natural kind terms to proper names by considering them both as rigid designators. It is with respect to this notion of rigid designation that I see Kripke's commitment to essentialism, particularly the dependence of this notion on a `realist' conception of individuation and similarity. To this end, we must look at Kripke's discussion of the necessary conditions for the identity of an object.[7]

Kripke offers several examples which, he says, suggest essentialist principles. One example is that of trying to imagine `this very woman' as having different parents than she did. He asks:

> How could a person originating from different parents, from a totally different sperm and egg, be this very woman? One can imagine, given the woman, that various things in her life could have changed: that she should have become a pauper; that her royal blood should have been unknown, and so on. ...But what is harder to imagine is her being born of different parents. It seems to me that anything coming from a different origin would not be this object. (1980, p.113).

Another example is that of the origin of `this table'. Kripke asks:

> Now could this table have been made from a completely different block of wood, or even of water cleverly hardened into ice - water taken from the Thames River? We could conceivably discover that, contrary to what we now think, this table is indeed made from the river. But let us suppose that it is not. Then, though we can imagine making a table out of another block of wood or even from ice, identical in appearance with this one, and though we could have put it in this very position in the room, it seems to me that this is not to imagine this table as made of wood or ice, but rather it is to imagine another

> table, resembling this one in all external details, made of another block of wood, or even of ice. (1980, pp.113-114).

In footnotes to these examples, Kripke remarks that two principles are suggested. First, an `origin´ principle is suggested, viz. If a material object has its origin from a certain hunk of matter, it could not have had its origin in any other matter. Second, a `substance´ principle is suggested, viz. an object´s substance, not its origin, is what is essential to it. While Kripke does not explicitly endorse either principle, and while he claims that a full discussion of the problem of essential properties of particulars is impossible here, he offers several (obtuse) points for consideration. The first point is that ordinarily when we ask whether something might have happened to a given object, we ask whether the universe could have gone on as it actually did up to a certain time, but diverge in its history from that point forward "so that the vicissitudes of that object would have been different from that time forth." (1980, p.115, note 57). Perhaps, he says, this feature [the history of the object?] should be erected into a general principle about essence. The second point that Kripke makes is that he is not suggesting that only origin and substantial makeup are essential. For example, had the very block of wood from which this table was made been used instead to make a vase, then the table would never have existed. So, says Kripke, being a table seems to be an essential property of the table.[8] The third point is that

the question of whether an object essentially has a certain property can be vague [!]. The fourth point is that Kripke is not convinced by the counterexamples to the origin principle that appear in ordinary parlance. The exact analysis of those counterexamples, he says, is difficult and he cannot discuss them here.

Now, it seems to me that while Kripke is careful not to explicitly claim that he holds to essences, he nonetheless does. If we take an essential property of an object to be a property which it is necessary for an object to have to be that object, then clearly Kripke is committed to essentialism. His remark that "anything coming from a different origin would not be this object" and that this table must have come from the block of wood which it did, seem, despite his hesitance, to indicate that a particular historical origin is at least a necessary condition for this object to be this object. Likewise, Kripke´s belief that science discovers the essence of kinds (at least ideally) is also an obvious commitment to essentialism. The connection between these cases, and the underlying source of the commitment to essentialism, is in Kripke´s conception of rigid designation. As noted in chapter one, for Kripke, a rigid designator refers to the same object in all possible worlds in which that thing exists. What I want to argue, and what I have been indicating in both this chapter and the previous chapter, is that Kripke takes `the same object´ as given and unproblematic. In the previous chapter I argued

that this is not the case; individuation and similarity are not given and unproblematic, but are interest- and language-dependent. I now want to argue, and have been trying to indicate in this chapter, that it is at this point where Kripke's commitment to essentialism comes in. That is, I want to argue that Kripke's notion of rigid designation commits him to essentialism.

Now, it seems to me that this claim is rather obvious. Devitt takes a similar view. He spends several pages commenting on the doctrine of essentialism and suggesting that Kripke has made such a doctrine "both intelligible and plausible." (1981, p.209). Devitt remarks that Kripke suggests that "a person has such [non-trivial] essential properties as coming from a certain sperm and egg, being human, being of a certain sex, and being of a certain race." (1981, p.208). It is only after mentioning the doctrine of essentialism that Devitt talks about rigid designation. He does so, I believe, because the latter notion rests on the former one. He asks, in virtue of what is a name a rigid designator, and answers in the following way:

> Suppose we take `name' here to refer to designational names only. Such a name designates whatever is causally linked to it in the appropriate way. It can only be causally linked to an object in the actual world. So it designates an object in another possible world simply in virtue of that object being the same as the object to which it is causally linked in the actual world. It is this that makes the name rigid. (1981, p.212).

(It should be noted that for Devitt the important point here is that of the scope of the modal operator and the roles of

singular terms. He believes that the Kripkean account is suspect because it rests on the metaphor of `possible worlds´. He rejects this metaphor and instead suggests that the differing roles of names and descriptions can be accounted for in the following way: names do not give rise to ambiguities of scope because they are designational, whereas descriptions do give rise to ambiguities of scope because they are attributive (in the Donnellan sense of designational and attributive). My interest here is different than Devitt´s and his rejection of Kripke´s possible world talk is irrelevant to my point. Now, I am not trying to make Devitt sound as if he interprets Kripke as I do, but to indicate that even he sees Kripke´s notion of rigid designation as resting on the prior conception of the same object being referred to across possible worlds. From my perspective, Devitt´s reliance on the designational/attributive distinction does not get him (or Kripke) away from the essentialist commitment of the very notion of rigid designation.)

Kripke introduces the notion of rigid designation and then immediately discusses another, related, notion: transworld identity. He spends several pages arguing against the objection that rigid designation rests on criteria of transworld identity. He claims that it is because we can refer (rigidly) to, say, Nixon, and stipulate that we are speaking of what might have happened to him that transworld identifications are unproblematic (in those

cases). I think Kripke is right here - at least to the extent that rigid designation precedes transworld identification and that if actual world identification is unproblematic (for rigid designators) then so, too, is transworld identification. Where I think Kripke is wrong is in thinking that actual world identification is unproblematic, as I argued in the previous chapter. It is also clear, though, from this that rigid designation rests on a commitment to essentialism. Rigid designation rests on the possibility of being able to refer to the same object on different occasions and on a name referring to the same object on all occasions. As we saw, for Kripke, for something to be `this object´, and for us to speak of `the same object´, requires (at least for names) some essential property (or properties), such as genetic make-up or historical origin. For Kripke, Nixon is Nixon and `Nixon´ refers to Nixon regardless of the myriad of properties associated with him which could have been otherwise; but what makes Nixon Nixon and assures that `Nixon´ refers to Nixon is that there is (in principle) a clearly identifiable object which is Nixon and nothing else is that object.[9] There must be some property (or properties) common to Nixon in all possible worlds such that `Nixon´ refers to that object in all of those worlds. That property (or properties) is what is essential to Nixon. What else could guarantee that Nixon is referred to by `Nixon´ in all possible worlds?[10]

`Legitimate´ Essence

I have tried to show that Kripke´s notion of rigid designation rests on a prior conception of sameness (i.e. of individuation) and on the identification of the same object in virtue of some essential property (or properties).[11] Let me review the context in which I have made this claim. I have argued (a la Quine) that the doctrine of essentialism rests on a particular conception of individuation and similarity. Furthermore, both essences of kinds and essences of individuals rest on these prior conceptions. As I argued in the previous chapter `realist´ conceptions of individuation and similarity are mistaken, and, consequently, views of essentialism which are based on those `realist´ conceptions are also mistaken. I then argued that both Searle and Kripke are committed to essentialism, and indeed, given their `realist´ positions on individuation and similarity, they are committed to a particular version of essentialism. (To me, they are doubly wrong!) But this is not all. In the next chapter I want to argue that essentialism rests on the doctrines of realism and a correspondence theory of truth. I will suggest that both doctrines are mistaken. Since both the cluster and the causal theories of reference are based on these doctrines (which I will show), they, too, are mistaken.

Before turning to those issues, however, I want to make good on my promise earlier in this chapter to explain what I see as correct about Searle´s and Kripke´s views of

essences. While I hold that for the most part they are mistaken, I do think that they touch on particular cases which match my intuitions. The particular cases (and the general cases which these examples instantiate) are the following. For Searle, Aristotle can't be just anything. An object must have some property, say, personhood, in order for it to be Aristotle. That is, Aristotle can't be a mountain or a prime number even though there may be many properties which we associate with Aristotle (many descriptions which we associate with the name `Aristotle') which turn out to be false. For example, Aristotle might not have been the teacher of Alexander, but for `Aristotle' to refer to Aristotle there must be some property (description) which an object must have to be Aristotle. That property (description) might be personhood.

The Kripke example was that a particular person, to be that person, must have come from the parents which she did. As Kripke put it, this very woman could not have come from a totally different sperm and egg. Likewise, this table could not have come from a completely different origin than it did and still be this table.

Now, while I maintain that both Searle and Kripke are mistaken for the reasons which I outlined above in this chapter, I do share their intuitions somewhat. I will explain the sense in which I think they are correct by discussing the examples.

What I see as correct about Searle's example (and view)

is his insistence that there must be some property (description) associated with an object (a name) in order for it to be what it is and not something else. As I mentioned above, I think he is right that we need a criterion of identity to have continuity of reference. However, I insist that this is true only in a public sense (i.e. within a public language). That is, Searle is wrong to claim that a speaker must have some description associated with a name be true of a object for that name to refer to that object. Kripke´s counter-examples have shown that that view is troublesome. What I want to claim is that the language community (not necessarily any individual speaker) must have some description associated with a name in order for that name to be the name of a particular object. For example, if none of the descriptions associated with `Aristotle´ by the entire language community were true of Aristotle, then, I insist, reference has not occurred. Kripke rightly points out that language (and reference) is public. Indeed, I think that Kripke´s notion of semantic reference is meant in large part to capture what I am claiming here (although he dissociates it completely from talk of descriptions associated with a name). This ties in with Searle´s discussion of essences in the sense that there must be some property (description) which the language community - not an individual speaker - associates with an object (name). If the entire language community were willing to give up personhood as a necessary property for

something to have in order for it to be Aristotle, then, I want to argue, being a person is not essential to Aristotle. (For example, this could be the case for Homer. It might turn out that there was no person Homer. If the language community were to come to this conclusion, then being a person would not be essential to Homer.) A crucial point connected with what I am claiming here is that what properties turn out to be essential (if any) are determined by the language community. This is to deny the `realist´, metaphysical sense of essences and essentialism. If Aristotle has an essence, it is not because Aristotle has an essence; rather it is because the language community agrees that some property is necessary for something to be Aristotle. Searle is right, then, but only to the extent that essences are public, linguistic (and ultimately epistemic) entities.

This insistence on publicity also underlies my assessment of Kripke´s examples (and view). My intuitions are, with Kripke´s, that this very woman could not have come from a different sperm and egg or this table from a different block of wood. However, I maintain that this is the case only within the context of publically-determined individuals. That is, given that language is public and given that individuation is linguistically determined (i.e. given that the language community determines who this very woman is and what this table is), then, yes, this very woman could not have come from a different sperm and egg. Again,

however, what is being abandoned is any commitment to a `realist´, metaphysical conception of essences. A possible (and, I think, plausible) objection to what I have said here is that how we, as the language community, determine who this very woman is is by appeal to her origin, i.e. to what particular sperm and egg she came from. Consequently, so the objection goes, it makes no difference in the analysis to insist on a publically, linguistically-determined individuation (and essence). The objection fails, though, because it rests on the assumption that what counts as the origin of this very woman is given `out there´ in the world (i.e. a `realist´ view of origins). Such an assumption is untenable for the reasons which I claimed in the previous chapter concerning individuation and similarity. That is, histories are no more `out there´ than are individuals or kinds. As I argued, what counts as a particular event-individual is not `out there´, but is determined by our theories/interests/language. For example, we decide what counts as this coin toss (or as the 1948 Presidential election, etc.). If this is true, then we cannot appeal to the history (origin) of an individual (e.g. this very woman) or a kind as though the history is not determined by our theories/interests/language. Past event-individuals and thing-individuals are just as theory- and interest- and language-dependent as are present or future event-individuals and thing-individuals (e.g. the 1948 Presidential election). Hence, the objection fails to the

extent that it relies on a `realist´ conception of history (origins). Again, given that we, as the language community, have determined what the origin of this very woman is, then we can appeal to her origin to individuate her.

I want to make one final point in connection with what I have just said. Earlier I commented that the distinction between individuals and kinds (and essences of each) might not be so sharp. I mentioned that recent debate in the philosophy of biology has included the consideration that species can be treated as individuals. This debate is related to the present discussion of the origin (and essence) of individuals because in large part the argument for treating species as individuals rests on looking at the history of given species. That is, an argument for considering species as individuals runs along the following lines. Just as for `normal´ individuals, for which spatiotemporal continuity tends to be the criterion (or, at least, one criterion) for individuation, so, too, for species, spatiotemporal continuity should be the (a) criterion of individuation. Phenotypic features, even phenotypic similarity, is irrelevant for individuation. For example, identical twins might be phenotypically identical, but they are, nonetheless separate individuals. On the other hand, an individual can change (drastically) phenotypically yet remain the same individual (e.g. a caterpillar/butterfly). What does/should matter for individuation is historical origin and spatiotemporal

continuity. As Hull puts it: "Organisms are not included in the same species <u>because</u> they are similar to the type specimen or to each other but <u>because</u> they are part of the same chunk of the genealogical nexus." (1978, p.353). Just as we treat mass terms (such as `gold´ or `water´) as an individual, so we can/should treat species as individuals. In both cases, just as with `normal´ individuals, particular hunks of gold or particular organisms are part of the whole (the individual) because of sameness of origin, not sameness of properties. This sounds as though it supports a Kripkean-like analysis of individuals and of essences (because having a particular historical origin can serve as the essential property). However, it does so only under the constraints noted above concerning the public language-dependence of individuation both synchronically and diachronically. While I think that a fuller treatment of the issue of species as individuals can shed some light on the issue of essentialism, the point I have just made concerning it will suffice for the present purposes.

NOTES

[1]I use `object´ here to refer to both individuals and kinds. For example, as I will note below, species can be thought of as individuals. As will be seen, the distinction between individuals and kinds has been important. It is a distinction which I reject (at least as it is usually presented).

[2]I am not making an `existence´ restriction here. That is, I am not saying that for an object to have essential properties, it must have them in order for that object to exist, but simply in order for it to be what it is. I take the issue of essences and essentialism as issues of identity, not of existence.

[3]I will return to this issue, and strengthen my claims here, shortly. First I need to explicitly address the distinction between individuals and kinds.

[4]Extensionally, there are often cases where kinds are indistinguishable from individuals that make up the kind (e.g. `even prime numbers´ and `2´).

[5]Actually, there is a sense in which I do not think that Searle is committed to essentialism here. I will make this explicit at the end of this chapter when I discuss what I see as a legitimate way of talking about essentialism.

[6]This `realist´ commitment is borne out in such claims as the following: "In order that a name should ever come to be used to refer to an object in the first place there must be some independent representation of the object. This may be by way of perception, memory, definite description, etc., but there must be enough Intentional content to identify which object the name is attached to." (1983, p.259).

[7]I should add at this point that, just as I see much of what Searle claims as correct to an extent, so, too, I see much of what Kripke claims as correct to an extent. At the end of this chapter I will argue for what is basically right about both of their views on essences. First, however, just as I criticized Searle, I want to now criticize Kripke.

[8]This sounds like Searle´s claim that being a person is analytically true of this person (e.g. de Gaulle).

[9]Of course, for Kripke, "a clearly identifiable object" is meant metaphysically. Nixon is Nixon whether we know it or not, and `Nixon´ is a rigid designator because it refers to that object in all possible worlds.

[10]An important objection that might be raised here is

that I have misrepresented Kripke's position when I accuse him of being committed to essentialism through the notion of rigid designation. I claim that there must be some property (or properties) common to, say, Nixon in all possible worlds such that `Nixon' refers to that object in all of those worlds. That property (or properties), I suggest, is what is essential to Nixon. What else, I asked, could guarantee that Nixon is referred to by `Nixon' in all possible worlds? One answer to this question - other than the requirement of essential properties - is that Kripke simply stipulates that `Nixon' refers to the same object in all possible worlds. Indeed, this seems to be Kripke's explicit position. He says:

> Don't ask: how can I identify this table in another possible world, except by its properties? I have the table in my hands, I can point to it, and when I ask whether it might have been in another room, I am talking, by definition, about it. I don't have to identify it after seeing it through a telescope. If I am talking about it, I am talking about it, in the same way as when I say that our hands might have been painted green, I have stipulated that I am talking about greenness. Some properties of an object may be essential to it, in that it could not have failed to have them. But these properties are not used to identify the object in another possible world, for such an identification is not needed. Nor need the essential properties of an object be the properties used to identify it in the actual world, if indeed it is identified in the actual world by means of properties (I have up to now left the question open). ...So, we do not begin with worlds (which are supposed somehow to be real, and whose qualities, but not whose objects, are perceptible to us), and then ask about criteria of transworld identification; on the contrary, we begin with objects, which we have, and can identify, in the actual world. We can then ask whether certain things might have been true of the objects. (1980. pp.52-53).

Now, I want to say (repeat) several things about this view. As I have said above, the issue that I see as the important one is not that of transworld identity or transworld identification or transworld individuation, but of identity/identification/individuation in this world. Kripke's quote seems to me to indicate that we start with an identified object ("I am talking, by definition, about it") and we can then simply stipulate that we are talking about that object. Much of what I am arguing for in this chapter and the previous one is that Kripke is mistaken in suggesting that he can do just that. (This will come out clearer in the final two chapters.) When Kripke says that he is, by definition, talking about it, I want to ask: about

what? Kripke seems to presume that we can clearly separate out issues of individuation from issues of identification. I want to suggest that we can't. We cannot, contra Kripke, simply stipulate that we are talking about, say, Nixon and presume that we are referring to Nixon in all possible worlds merely by using the name `Nixon'. Indeed, I am somewhat perplexed that Kripke would want to make such a move, since it seems (to me, anyway) that this divorces the name from any public, historical, linguistic context - the very point for which he criticizes Searle's theory of names. While it may be true that a move to postulating the stipulation of (the identity of) an object across possible worlds relieves Kripke from a commitment to essentialism, the cost, I would want to argue, is too great. The issue is whether or not one can stipulate in this world the identity (or individuation) of an object. To the extent that this can be done, it is possible only given a public language and only to the extent that the language community accepts such a stipulation. As Kripke himself says, I cannot refer to Schmidt by (the use of) `Gödel'. Nor, I want to add, can I do so by stipulating that I am referring to Schmidt by (the use of) `Gödel' (unless the language community agrees, and, as a result, `Gödel' becomes another name for Schmidt). Additionally, I cannot separate out the issues of the individuation of an object (say, Nixon) from the issues of the identification of that object. That is, I think Kripke is simply wrong to claim that he is talking about <u>it</u> (say, Nixon) and this makes no commitment to talking about the means of identifying <u>it</u>. When Kripke says he's talking about <u>it</u>, and I ask, about <u>what</u>, Kripke cannot respond by saying that my question misses the point because it is asking for the identification of the object that has already been individuated (by stipulation). Kripke's position seems to be committed to the following: in a given possible world Nixon is not only not a president, but Nixon is not even a human; rather Nixon is a particular dime. When I ask: how do you know that <u>this</u> dime is Nixon and not <u>that</u> dime, Kripke's answer, it seems to me, can only be: because I said so!

If indeed this is Kripke's move to avoid a commitment to essentialism, then I suggest that he has avoided the commitment, but only at the cost of incoherence.

[11]Of course, what I have said about rigid designation and essentialism with respect to names applies <u>mutatis mutandis</u> with respect to natural kinds.

CHAPTER SEVEN

CORRESPONDENCE AND REALISM

I have been arguing that questions and issues about reference can be addressed only within the context of prior questions and issues about individuation and similarity. Theories of reference, I have claimed, presuppose and are committed to theories of individuation and similarity. In particular, I have maintained that the cluster and causal theories of reference presuppose and are committed to a `realist´ theory of individuation and similarity. This is borne out in their commitment to private reference and essentialism. I have also been arguing that the issues of individuation and similarity are ultimately epistemic issues. Underlying (and occasionally surfacing from) what I have been saying is the view that metaphysical and semantic issues and positions rest on epistemic concerns. That is, the basic metaphysical issues of individuation and similarity must finally be addressed within the context of asking how we pick out individuals, how we partition the world. What things there are in the world and what properties they have are, in the final analysis, dependent upon our language and cognitive interests. While this seems to be a metaphysical claim, it is only meaningful, I hold, as an epistemic one. (It makes no sense to say that there

are an even number of stars in the universe if we can never know this.) This openly verificationist position has been (for the most part) in the wings throughout the previous chapters. It will come on stage in its fullest glory in the next.

Besides suggesting that metaphysical issues ultimately rest on epistemic priorities, I am maintaining that semantic issues, too, must be framed within an epistemic context. In particular, issues of reference presuppose and are committed to epistemic theories. Likewise, views on truth depend upon views on knowledge. In this section I want to argue that the cluster and causal theories of reference are committed to a correspondence theory of truth, which itself entails realism (in the sense of a particular conception of individuation and similarity). That conception, I have argued, is mistaken. I now want to suggest that theories of realism and correspondence which rest on it are also mistaken. Furthermore, theories of reference which rest on those theories of realism and correspondence are mistaken as well. I will first lay out what I take a correspondence theory of truth to be (and will discuss Tarski's semantic theory of truth in this context). I will argue that such a theory rests on a commitment to realism. After explicating what I take realism to be, I will criticize it. Having put forth a critique of realism and correspondence, I will argue that both Searle and Kripke are committed to correspondence (and hence realism), suggesting that they are consequently

weakened. Finally, I will conclude that the received theories of reference are, because of their commitments to the same faulty epistemic positions, mistaken and inadequate theories of reference.

Truth

What is a correspondence theory of truth? Quite simply, it is a theory that takes the truth of a proposition[1] to consist not in its relations with other propositions, but in its relations to the world, to facts. A proposition is true if and only if it corresponds to the facts of the world. `Snow is white´ is true because snow is white. Of course, this definition is vacuous without some explication of what `correspondence´ means. Perhaps the most famous contemporary explication is Wittgenstein´s (1922) picture theory of meaning, in which correspondence consists in an ordered isomorphism between words in (true) atomic propositions and (simple) facts (or states of affairs) in the world.[2]

An older, and perhaps even more famous, explication (or, at least, characterization) of correspondence comes from Aristotle (Metaphysics Gamma, 7, 27): "To say of what is that it is not, or of what is not that it is, is false, while to say of what is that it is, or of what is not that it is not, is true." While Aristotle apparently takes the nature of this correspondence as a given, Tarski (1956) tries to give an account of truth which captures this sense of `true´. Tarski´s conception, and theory, of truth,

however, have come to be known as a semantic theory of truth, rather than a correspondence theory of truth. I want to lay out what Tarski's theory is and consider the claim that it is actually a version of a correspondence theory of truth. I want to argue that appealing to Tarski's theory to shore up and explicate the correspondence theory of truth fails on two grounds. First, even if it is a version of the correspondence theory (and is not philosophically neutral), then I think the theory begs the question. Second, I think it can be forcefully argued that Tarski's theory of truth is philosophically neutral and so does not do the job of shoring up or explicating a correspondence theory.

Tarski states that in discussing the concept of truth in formalized languages his task was to construct "with reference to a given language" (1956, p.152) a materially adequate and formally correct definition of the term `true sentence'. He remarks that he is concerned "exclusively with grasping the intentions which are contained in the so-called classical conception of truth (`true - corresponding to reality')" (1956, p.153).[3] Indeed, when discussing the concept of truth in colloquial language, he claims that a semantic definition of `true sentence' can be expressed as follows:

> A true sentence is one which says that the state of affairs is so and so, and the state of affairs indeed is so and so.

The task of a semantic definition is to make this intuitive meaning more precise and clear. He suggests several means

of doing this. First, quotation-mark names, denoting the names of sentences, could be used (e.g. `It is snowing´ is a true sentence if and only if it is snowing). Second, structural-descriptive names, describing the words which compose the expression denoted by the name along with the signs of which each word is composed and the order of the words, could be used (e.g. An expression consisting of three words of which the first is composed of the two letters ay and tee (in that order), the second word of the letters ay and ess (in that order), and the third word of the letters ess, en, o, double-u, ay, en and gee (in that order), is a true sentence if and only if it is snowing). However, because of semantic paradoxes (such as the liar paradox, in which a sentence is true if and only if it is false), Tarski concludes that:

> the very possibility of a consistent use of the expression `true sentence´ which is in harmony with the laws of logic and the spirit of everyday language seems to be very questionable, and consequently the same doubt attaches to the possibility of constructing a correct definition of this expression. (1956, p.165).

Leaving natural languages aside, then, Tarski proceeds to define the notion of truth for formalized languages (i.e. artificial languages in which the sense of every expression is unambiguously determined by its form). In particular, he restricts himself to the calculus of classes.

Before laying out a conception of truth, Tarski distinguishes between object language and metalanguage. An object language is a language in which we speak; a

metalanguage is a language used to speak about the object language. With regard to truth, the object language is the language for which truth is defined and the metalanguage is the language in which truth is defined. For example, to say, "Noseworthy looks bad today" is to speak a sentence in an object language. However, to say, "`today´ contains five letters" is to speak metalinguistically about that object language. (As this example shows, natural languages, such as English, serve as both object language and metalanguage. This is a reason why semantic paradoxes such as the liar paradox are possible, and hence, why a semantic definition of truth cannot, for Tarski, be given for natural languages.)

Following Haack (1978), we can say that there are two parts to Tarski´s theory: (1) adequacy conditions (i.e. conditions which any theory of truth must/ought to fulfill) and (2) a definition of truth (at least for the calculus of classes), which Tarski claims is adequate.[4]

There are two adequacy conditions to be met. First, material adequacy (i.e. setting the limits on the possible content) and, second, formal correctness (i.e. establishing the possible form of any acceptable definition of truth). The spirit of the material adequacy condition was noted above in remarking on Tarski´s discussion of truth in natural languages. Briefly, the material adequacy condition says that any acceptable definition of truth should have as a consequence all instances of Convention T, that being:

(T) S is true iff p,[5]

where `p´ is referred to by `S´ (either as a quotation-mark name or as a structural-descriptive name). Convention T is actually a schema, as `p´ can be replaced by any sentence of the object language (i.e. the language for which truth is being defined) and `S´ is to be replaced by a name of the sentence which replaces `p´. An example is "`Snow is white´ is true iff snow is white." Of course, for Tarski, Convention T is <u>not</u> a definition of truth, but simply a material adequacy condition, which states that all instances of it must be entailed by any definition of truth which is to count as being materially adequate.[6] Tarski places this stipulation on definitions of truth, again, because he wants to capture the classical (i.e. correspondence) conception of truth in his semantic theory.

The second adequacy condition, formal correctness, follows from Tarski´s commitment to avoid the semantic paradoxes noted above. Assuming that the laws of logic hold, these semantic paradoxes arise because natural languages are semantically closed (i.e. the languages contain not only expressions, but also the means of referring to those expressions, along with such semantic predicates as `true´). While the object language/ metalanguage distinction resolves this problem (by yielding semantically open languages), a corollary is that the metalanguage, M, must contain the object language, O, or translations of all sentences of O, plus the means to refer

to the expressions of O (since all equivalences of the schema Convention T must be implied by the definition of truth, which will occur in M). M must have both O (or translations of O) and the means to refer to the expressions in O because instances of T have an expression denoting a sentence of O (viz. S) and a (translation of a) sentence of O (viz. p). Additionally, for Tarski, the structure of the object language and the metalanguage must be `formally specifiable´, because in order to define truth in the object language it must be possible (and is essential) to pick out the wffs of the object language (since they are what truth in the object language, what `true-in-O´, applies to).

Before turning to Tarski´s definition of truth (in O), a final word needs to be said about Convention T. It might be (and sometimes is) thought that Convention T provides an obvious way of giving a definition of truth. Two ways are proposed, both motivated perhaps because Tarski admits that each instance of Convention T could be thought of as a partial definition of truth. First, it might be thought that a conjunction of all the instances of T (one for each sentence in O) would constitute a complete definition of truth (in O). However, Tarski claims that this is impossible because the number of sentences of O might be infinite, so the conjunction could never be formed. Second, it might be thought that Convention T could be stated as a universal quantification and could thus constitute a definition. (So, the definition D might be: (p)(S is true-

in-O iff p), where S=`p´.) This, however, is unacceptable because quantifying into quotation marks is meaningless. Snow is white is no more a part of `Snow is white´ than rat is a part of `Socrates´. Rejecting these proposals, Tarski offers his own definition of truth (in O).

Tarski provides his definition of truth by the following steps: specifying the syntactic structure of the object language, O; specifying the syntactic structure of the metalanguage, M (including expressions of O, or their translations, syntactic vocabulary, and the usual logical apparatus); defining the notion of satisfaction; and defining the notion of truth (in terms of satisfaction).[7]

The following characterization can serve as an example of a Tarski-like presentation:

(1) Syntax of object language O

Individual constants: a, b, c, ...
Variables: x, y, z, ...
Predicate letters: F, G, ...
Logical connectives: ~, &
Quantifier: (...)
Grammatical punctuation: (,)

(2) Syntax of metalanguage M

Atomic sentences of O are strings of expressions which consist of an n-place predicate followed by n individual constants or variables.

All atomic sentences are wffs.
If A is a wff, then ~A is a wff.
If A and B are wffs, then (A&B) is a wff.
If A is a wff, then (x)A is a wff.[8]
Nothing else is a wff.

(3) Definition of satisfaction

Open sentences (i.e. sentences containing free variables) are not true or false, but are satisfied. For example, `x is a number´ is

satisfied by 2, `x is greater than y´ is satisfied by <3,2>, `x is between y and z´ is satisfied by <2,1,3>, where `<...>´ indicates an ordered n-tuple of the n objects appearing between the brackets. Satisfaction is a relation between open sentences and ordered n-tuples of objects. Since open sentences may have any number of free variables, Tarski defines satisfaction as a relation between open sentences and infinite sequences. (So, $F(x_{1}...n)$ can be satisfied by $\langle o_1...o_n, o_{n+1}...\rangle$ just in case it is satisfied by the first n members of the sequence; subsequent members can be ignored.)[9]

The negation of an open sentence S is satisfied by those sequences which do not satisfy S.

The conjunction of two open sentences S_1 and S_2 is satisfied by those sequences which satisfy both S_1 and S_2.

The existential quantification of an open sentence S is satisfied by a sequence of objects just in case there is some other sequence of objects (differing from the first sequence in at most the ith place, where the ith is the variable bound by the quantifier) which satisfies the open sentence S resulting from dropping the quantifier. (For example, the sequence <4,1,3,...> satisfies the sentence `( x)(x is between y and z)´ because <2,1,3> satisfies `x is between y and z´ and <2,1,3> differs from <4,1,3,...> in at most the first place, i.e. the variable bound by the quantifier.)

(4) Definition of truth

Given a definition of satisfaction, Tarski quickly offers a definition of truth. A sentence S is true just in case it is satisfied by all sequences and false just in case it is satisfied by none.[10] For example, `x is a number´ is satisfied by <2,...> regardless of the second or subsequent members of the sequence; `x is greater than y´ is satisfied by <3,2,...> regardless of the third or subsequent members of the sequence; (x)(x is a number) is satisfied by <...,...> regardless of the first or subsequent members of the sequence, since there is a sequence (e.g. <2,...>) which differs from any arbitrary sequence in at most the first place and which satisfies `x is a number´. Any closed sequence, then, will be satisfied by all sequences or by none. Hence, truth is defined in terms of the satisfaction of all sequences.[11]

While explicitly restricted to formalized languages, Tarski´s semantic definition and conception of truth have been profoundly influential in analytic philosophy. For example, the theory has been taken to be a vindication and legitimation of a correspondence theory of truth. Popper (1963), for instance, says: "Tarski´s greatest achievement, and the real significance of his theory for the philosophy of the empirical sciences lies, I believe, in the fact that he re-established a correspondence theory of absolute or objective truth which showed that we are free to use the intuitive idea of truth as correspondence with the facts." (1963, p.223). Now, assuming that this view is correct, there are, I want to claim, serious problems for a correspondence theory of truth as exemplified by the semantic theory.

As stated above, for a correspondence theory of truth, a proposition is true if and only if it corresponds to the facts of the world. Such a claim seems almost a truism, and, as mentioned, the onus is to lay out exactly what `correspondence´ means. This involves not only stating the nature of the correspondence, but also indicating the objects which correspond. As noted above, the nature of the correspondence is a structural isomorphism between words and facts. Of course, this is not very helpful if one wonders about correspondence in the first place. It is, at best, a restatement of the claim that a proposition is true if and only if it corresponds to the facts. It still remains to

say what propositions are, what facts are and what correspondence is. While the question of what propositions are has received a great deal of attention, it is not crucial to the present discussion, so it will be left aside. The question of what facts are has also received much attention, and it will only be addressed here in the sense of saying that two related issues must be considered when addressing this question. One issue to be considered is what are facts (i.e. what does `facts´ mean?); another issue to be considered is what are the facts (i.e. what facts obtain?). In the next chapter I will address these issues and suggest that the two issues are intimately interrelated, such that the first cannot be answered without answering the second. For now, I will simply say that I take the answer to both to be epistemic. In the next chapter I will explain what I mean.

As we saw, truth, for Tarski, is defined in terms of satisfaction. A sentence is true (in a language) just in case it is satisfied by all sequences (in the language). Satisfaction is a relation between open sentences and ordered n-tuples of objects. Now, in one sense, it is almost trivially true that this conception of truth is a correspondence theory. Correspondence (intuitively captured by Convention T) is revealed simply as satisfaction. `Snow is white´ corresponds to the facts (of snow being white) quite simply because there is a relation between `Snow is white´ and <snow, white(ness),...>, viz. the latter

satisfies the former.

While it might be claimed that Tarski's theory has fortressed the correspondence theory of truth by giving a precise definition of truth and a clear explication of the nature of correspondence, it has been claimed that Tarski's theory does not bolster the correspondence theory, rather it presupposes the correspondence theory. The argument for this claim also seems somewhat obvious. Tarski's explicit acknowledgement that he wants to capture the intuitions of the classical (i.e. correspondence) theory seems to indicate a clear commitment to the latter. Beyond this, though, there are further reasons for asserting that Tarski presupposes a correspondence theory. The major argument goes as follows: The very notion of satisfaction requires a specifiable set of objects to which statements are related. That is, without identifiable (and individuated) objects, the notion of satisfaction would make no sense because there would be nothing to which statements could be related. The sequence $\langle x,y \rangle$ could not satisfy the statement $(\exists x)Rxy$ if there were no objects x and y. (This is not an ontological claim about what kind of objects x and y are, but a conceptual claim about the plausibility of the notion of satisfaction.) So, for satisfaction to get off the ground, there must be a prior set of specifiable objects. But what does this mean to say that there must be a prior set of specifiable objects? Doesn't this mean that for satisfaction to be possible, it must be true that there is a

prior set of specifiable objects? If so, we already have the concept of truth built in to the notion of satisfaction. Appealing to satisfaction, then, to define the notion of truth is question-begging.

It might be objected that it is not truth that is presupposed by satisfaction, but simply a set of objects. That is, what is required for satisfaction to make sense and be applicable is only objects, not conceptions of truth. I think this is false. The following example (I hope) illustrates why. Suppose we say that $(\exists x)Rxy$ is satisfied by <glub, sosh> (and R stands for the relation `is flibber than´). In what sense can we say that glub is flibber than sosh (or light is faster than sound or two is greater than one)? The answer, it seems to me, is because glub <u>is</u> flibber than sosh. But, then, the notion of satisfaction is superfluous, as we´ve already said that glub is flibber than sosh in order to say that <glub, sosh> satisfies `x is flibber than y´.

Another objection to what I have suggested is that, because truth and satisfaction are defined in the metalanguage rather than in the object language, there is no question-begging in the semantic conception of truth. Indeed, it is for this reason that Tarski claims that the semantic conception is neutral with respect to metaphysical (and epistemic) positions. That is, truth in the object language is philosophically neutral because any commitment to or connection with other philosophical considerations

takes place in the metalanguage, not in the object language. So, even if it were the case that the notion of satisfaction is philosophically `loaded´, this would have no bearing on truth in the object language; satisfaction is `loaded´ in the metalanguage, not in the object language. With respect to the object language, then, the semantic theory is just that - a semantic theory, with no commitments to a correspondence (or any other) theory.

This is a powerful objection, and I am sympathetic to it somewhat. However, in the end I think it is either mistaken or it removes the semantic theory from redeeming a correspondence theory. Speaking metaphorically, I think such an objection (at best) wins the battle, but loses the war. (Or, perhaps, more topically: wins the debate, but loses the election.) Assuming that the objection is sound, it is just such an argument, I believe, that has led many writers to complain that the semantic theory is all form and no substance. Precisely because the semantic theory is (said to be) neutral to metaphysical and epistemic issues and positions, it is said to be of little interest and value. After all, why do we want a theory of truth? Isn´t it so we know what truth is and know what is true? That is, why do we care about a theory of truth if not to give us a criterion for determining whether or not a claim is true? (So, a theory of truth will tell us whether or not `Galileo invented the microscope´ is true.)[12] If this is why we are interested in a theory of truth, then how does the semantic

theory fare? Not well, I want to say, unless it presupposes a correspondence theory (and even then not so well). Clearly, if we want to know whether or not `Galileo invented the microscope´ is true, it is of no value to be told that `Galileo invented the microscope´ is true just in case Galileo invented the microscope (or that `Galileo invented the microscope´ is true just in case gold has atomic number 79). Nor is it of any value to be told that `Galileo invented the microscope´ is true just in case `Galileo invented the microscope´ is satisfied by all sequences in the language. We want to know whether or not Galileo invented the microscope, and the semantic theory does nothing to answer this. (This does not mean that the semantic theory is of no value at all, but that it does not do the job that we intuitively want a theory of truth to do.)

It might be objected that the semantic theory <u>does</u> give a definition for `truth´ and so <u>does</u> serve as a adequate theory of truth. It might not tell us what is true (i.e. what `facts´ are true), but it does tell us what truth is (i.e. what `true sentence´ means/refers to). But note that the definition of truth in the semantic theory depends upon the notion of satisfaction, which in turn depends upon a specifiable set of identifiable individuals (i.e. on the fact of there being these individuals). But this is simply to hold that the truth of the world is that this set of individuals <u>is</u> the set of individuals upon which

satisfaction depends (i.e. that it is true that this set of individuals are in the world in order for satisfaction to obtain).

Related to this last point, Putnam (1981) has argued that the semantic theory of truth is philosophically neutral (i.e. it is consistent with any theory) because it misses the interesting philosophical problems. As he says, the problem is not that we don't understand `Snow is white'. Rather, the problem is that we don't understand what it is to understand `Snow is white'. What is needed for a theory of truth to do its job is some standard of rational acceptability, of what is considered a rational way to pursue an inquiry, what standards of objectivity there are, when it is rational to terminate an inquiry, what grounds will be regarded as providing good reason for accepting one verdict over another, etc. In a nutshell, truth is the idealization of rational acceptability. Says Putnam:

> ...truth is an <u>idealization</u> of rational acceptability. We speak as if there were such things as epistemically ideal conditions, and we call a statement `true' if it would be justified under such conditions. `Epistemically ideal conditions', of course, are like `frictionless planes': we cannot really attain epistemically ideal conditions, or even be absolutely certain that we have come sufficiently close to them. But frictionless planes cannot really be attained either, and yet talk of frictionless planes has `cash value' because we can approximate them to a very high degree of approximation. (1981, p.55).

(I mention Putnam's claims here primarily to indicate the sort of view about truth which I think is correct and which I will elaborate upon and defend in the next chapter.)

The upshot of all this is the following. Tarski's semantic theory of truth in which truth depends upon satisfaction is either committed to a correspondence theory (if satisfaction takes place as a hook-up between the object language and the world) or is philosophically uninteresting (if satisfaction takes place as a hook-up between the object language and the metalanguage). In the former case, it is committed to correspondence because satisfaction is simply another way of trying to lay out the structural isomorphism between words and objects and presupposes a specifiable set of identifiable individuals (furthermore, this former case, I think, is mistaken as Putnam suggests); in the latter case, it is philosophically uninteresting because it fails to do what we want a theory of truth to do (i.e. give us a means of distinguishing true and false sentences, or helping us determine what is true). We are still left with trying to account for truth for Searle and Kripke. It is to this which I now turn.

Reference and Realism

As with essentialism, Searle says little explicitly about truth and even less about a theory of truth. Accordingly, we must cull from brief comments and passages the sort of theory of truth to which he is committed. Within the context of speech act analysis, Searle discusses the notion of truth. He claims that we characteristically call something true "only if a comment, remark, assertion, statement, or hypothesis, or the like, has already been made

or is at least in some way under consideration; in short, only if a proposition is already in the offing." (1969, p.153). For example, if your house is on fire, I do not announce to you that it is true that your house is on fire. We affix the `it is true that´ prefix to a proposition only in a context in which that proposition is already being entertained. Because of this, and because a proposition is characteristically entertained or put under consideration by the performance of some illocutionary act, then calling something true places us in a certain relation to the initial illocutionary act (e.g. in a relation of agreement, disagreement, endorsement, etc.). However, this does not give us a theory of truth or help us determine what truth is (or what is true).

To get a sense of what view of truth Searle is committed to, we must turn to a distinction made within his theory of reference, viz. the primary-secondary aspects under which reference is made. Recall that for Searle, the primary aspect under which reference is made is that aspect which, if not satisfied, would yield a statement that cannot be true. The secondary aspect is any aspect which the speaker expresses such that the speaker utters it in an attempt to secure reference to the object which satisfies the primary aspect, but which is not intended as part of the truth conditions of the statement the speaker is attempting to make. For example, the speaker, looking at someone in the room, says, `Smith´s murderer is insane.´ Now, the

speaker and the hearer might agree that the speaker has referred to, and made a true statement about, that particular person being looked at even though that person (and perhaps everyone) fails to satisfy the expression `Smith's murderer'.[13] The speaker could, on demand, fall back on another aspect, say, one expressed by `the person I am looking at'. If it turns out that there is no one being looked at, only a hologram perhaps, then the speaker could fall back on another aspect, say, one expressed by `the person arrested by the police and accused by the District Attorney as Smith's murderer'. If it turns out that there is no such person, then the speaker could fall back on another aspect. Eventually, however, an aspect must be reached such that if no one satisfied it, then the statement could not be true (nor reference be made). The primary aspect under which reference is made is the `last' aspect, the aspect which either works or results in a statement which is false. The other aspects are secondary.

What is important about this in the present context is that this treatment of aspects under which reference is made indicates a commitment to a particular theory of truth, indeed a Tarski-like theory of truth. As Searle says:

> there must always be some...aspect under which the speaker could have referred to the object and which is satisfied by the object. Furthermore, this aspect is such that if nothing satisfies it the statement cannot be true. (1979, p.144).

This insistence on there being a match in terms of satisfaction between the object in the world and the primary

aspect under which reference is made is certainly Tarskiesque, and, I want to argue, not accidentally. Just as the semantic theory of truth can be construed as being philosophically neutral (and consequently philosophically uninteresting), so Searle's implicit conception of truth can be likewise construed. That is, simply demanding satisfaction between a primary aspect under which reference is made and a state of affairs in the world could be neutral concerning epistemological and metaphysical commitments, but only if this satisfaction is a relation between an object language and a metalanguage. Since Searle (1969, p.72ff) seems to reject such a distinction between object languages and metalanguages, this move seems unlikely. The alternative interpretation of Searle's view here is that satisfaction between a primary aspect under which reference is made and a state of affairs in the world is a relation between, not languages, but language and the world. This is certainly consistent with his view of reference as a speech act (and speech acts as a type of human action based finally on brain states). If satisfaction is a relation between words and states of affairs, then it seems that this makes the same commitments that we saw Tarski's theory making, viz. there is a specifiable set of identifiable individuals (in this case states of affairs) `out there' in the world. As we saw with Tarski, this is to presume the truth of claims as to what individuals there are. Two features stand out here. First, as with Tarski, Searle fails to give a

theory of truth that does what we want a theory of truth to do. Second, as with Tarski, there seems to be a further commitment here to realism. That is, the presumption of a specifiable set of identifiable individuals in order to account for reference seems, for Searle, to be an implicit commitment to a realist ontology. At this point I want to explain more fully what I mean by realism and then return to my claim that Searle seems to be committed to realism. I will then turn to Kripke (and Devitt) and argue likewise.

Throughout this book I have suggested that particular views and conceptions are `realist´. I have intentionally put the word `realist´ within quotes because I did not provide any sort of explanation or definition of what I took realism to be. More specifically, I have argued that particular conceptions of individuation and similarity are `realist´. Most especially I have put the word `realist´ in quotes in that context because, in fact, I think that realism is simply a reflection of those particular conceptions. That is, what I take realism to be is the view that there is a (specifiable) set of (identifiable) individuals which exist independently of our theoretical, cognitive and linguistic interests. Furthermore, I also take realism to be the view that similarities among individuals and groups exist independently of our theoretical, cognitive and linguistic interests. As a corollary to this, I take realism to hold that metaphysical, logical and semantic issues are independent of epistemic

issues, such that the question of what individuals exist is independent of how we can know (or decide) what individuals exist.

One of the problems with dealing with the issue of realism is that it means so many different things to so many different authors. For example, many authors distinguish common-sense realism and scientific realism (e.g. Devitt (1984)). Others distinguish realism about theories from realism about entities (e.g. Hacking (1983) and Cartwright (1983)). There is not even clear agreement as to what scientific realism means. For example, Leplin (1984) notes at least the following ten conceptions of scientific realism:

1. The best current scientific theories are at least approximately true.

2. The central terms of the best current theories are genuinely referential.

3. The approximate truth of a scientific theory is sufficient explanation of its predictive success.

4. The (approximate) truth of a scientific theory is the only possible explanation of its predictive success.

5. A scientific theory may be approximately true even if referentially unsuccessful.

6. The history of at least the mature sciences shows progressive approximation to a true account of the physical world.

7. The theoretical claims of scientific theories are to be read literally, and so read are definitely true or false.

8. Scientific theories make genuine, existential claims.

9. The predictive success of a theory is evidence for the referential success of its central terms.

10. Science aims at a literally true account of the physical world, and its success is to be reckoned by its progress toward achieving this aim.

Although these conceptions of (scientific) realism are slightly different from one another, they all make a connection between truth and reference. They all, I think, are captured by my notion of realism as being a commitment to a particular conception of individuation and similarity, viz. that individuation and similarity are independent of our interests. In chapter five I criticized that conception of individuation and similarity and argued that it is mistaken, so I will not repeat the arguments here. Suffice it to say, since I identify realism with that conception, I find realism to be wrong.

What I want to suggest at this point is that, to the extent that the semantic theory of truth is interesting, it is committed to realism. Likewise, I see Searle as being committed to realism, as well. Now, it seems to me to be obvious that the semantic theory of truth is committed to realism. The definition of truth in terms of satisfaction entails, as noted above, that a specifiable set of identifiable individuals exists. Furthermore, this set exists prior to and independent of our interests. That is, to the extent that the semantic theory of truth is philosophically interesting and does the job that we want a theory of truth to do, it is not philosophically neutral, but is committed, as we saw, to a correspondence theory of

truth in which satisfaction plays the role of correspondence between language and the world. In addition, the set of individuals which satisfy open sentences is independent of our interests; the set is `out there´.[14]

As we saw, Searle shares this view of satisfaction. There are other indications of his commitment to realism. For example, in his discussion of the nature of Intentional states, he claims that there is a `mind-to-world´ direction of fit (i.e. our beliefs must conform to the way the world is. While I agree with Searle, I agree only to the extent that what he is claiming here entails a publically-determined direction of fit. That is, he is right, but only if private Intentionality (and the resulting direction of fit) is ruled out. However, as we saw in chapter four, Searle´s theory does not rule out such private Intentionality. (I will say more about this in the next chapter.)

Like Searle, Kripke has said relatively little in terms of a theory of truth and even less in connecting his theory of reference to a a theory of truth. In his two books (1980, 1982) and in his essay on speaker reference versus semantic reference (1977) he makes no mention or proposal of a theory of truth. He does, however, provide an `outline of a theory of truth´ (1975). His claims here are quite similar to Tarski´s semantic theory, and, like Tarski, Kripke seems motivated by seeking a resolution of semantic paradoxes. Kripke suggests that the notion of truth (i.e.

the assignment of truth-values to sentences) can be explained by the concept of `groundedness´. For Kripke, one can legitimately assert that a sentence is true just in case one can legitimately assert the sentence (and one can legitimately assert that a sentence is not true just in case one can legitimately deny the sentence). So, if one can legitimately assert that snow is white, then one can legitimately assert that `Snow is white´ is true. This can be extended to different `levels´, e.g. one can then legitimately assert that `"Snow is white" is true´ is true. Groundedness is the idea that a sentence is grounded just in case the sentence will eventually get a truth-value in this process. (Semantically paradoxical sentences, Kripke says, will not get grounded.) In Kripke´s words: "Given a sentence A of [language] $\mathcal{L}$, let us define A to be <u>grounded</u> if it has a truth value in the smallest fixed point $\mathcal{L}_\sigma$; otherwise, <u>ungrounded</u>." (1975, p.706). (The smallest fixed point is the point at which the set of true sentences is the same as the set of true sentences at the previous language level.) Kripke proceeds to give a formal account of this intuitive presentation of a theory of truth. For the present purposes, what is important is that, like Tarski´s theory, Kripke´s theory misses the philosophically interesting problem and his theory really fails to do the job that we want a theory of truth to do. As we saw, the theory only gets going if a speaker can legitimately assert a sentence. But that is exactly what is at issue! What we

want to know, and what we want a theory of truth to do is to let us know, if a speaker can legitimately assert a particular sentence. We want to know if it is true that snow is white, and it is unhelpful to be told that if we can legitimately assert that snow is white, then `Snow is white´ is true.[15] Another important point to note with respect to Kripke´s outline of a theory of truth is that it makes no explicit connection to his theory of reference. Devitt (1984), however, has attempted to bolster the causal theory of reference by fleshing out an accompanying theory of truth and of realism. To this attempt we will now turn.

Devitt separates ontic issues and questions from epistemic issues and questions from semantic issues and questions. One of his stated maxims is not only to separate these different kinds of issues and questions, but also to settle the ontic issues and questions first. For Devitt:

> Realism is an overarching ontological doctrine about what there is and what it´s like. It is committed to most of the physical posits of common sense and science, and to the view that these entities are independent of the mental. It has an epistemic aspect: the entities do not depend for their existence or nature on our opinion; they exist objectively. (1984, p.227).

Devitt distinguishes between `common-sense realism´ and `scientific realism´. Common-sense realism is defined as: tokens of most current observable common-sense, and scientific, physical types objectively exist independent of the mental. Scientific realism is defined as: tokens of most current unobservable scientific physical types objectively exist independently of the mental. Realism

(along with (the possibility of) human learning and teaching) entails Correspondence Truth. This latter concept (i.e. Correspondence Truth (x)) is defined as follows: sentences of type (x) are true or false in virtue of (1) their objective structure; (2) the objective referential relations between their parts and reality; (3) the objective nature of that reality. A crucial point to notice is that, for Devitt, truth rests on a prior conception of (objective) reference. Another crucial point to notice is that given this definition of truth, there is no necessary connection between Correspondence Truth and epistemic concerns, e.g. evidence. Says Devitt: "In sum, to hold to Correspondence Truth is to believe in a notion of truth that is not explained in terms of evidence. Questions of evidence are epistemic ones quite distinct from the constituative question of the nature of truth." (1984, p.32). A third crucial point to notice, related to the epistemic issue just mentioned, is that the purpose of a theory of truth is to explain, not reference, but communication (particularly learning and teaching).

In a nutshell, then, Devitt suggests that realism holds, that we `bump into the world´ in a causal way and refer to independently existing objects through a causal nexus. This causal hook-up allows us to posit a theory of truth which he calls `Correspondence Truth´. This view is clearly at odds with the position which I have put forth in this and previous chapters. Indeed, I have spent the past

several chapters arguing against just this sort of view. As I showed in chapter five, theories of reference depend upon prior conceptions of individuation and similarity. The very realist conceptions which Devitt assumes, I have argued, are mistaken. Furthermore, theories of reference reveal a commitment to the ontic issue of essentialism (or anti-essentialism) based on the underlying conceptions of individuation and similarity. Finally, those conceptions of individuation and similarity entail a commitment to particular theories of truth, with both the cluster and causal theories of reference being committed to a correspondence theory of truth (because of a commitment to a particular conception of individuation and similarity). In a sense, then, I have been agreeing with Devitt that truth is entailed by reference and reference in turn rests on prior commitments concerning what there is. However, I have been arguing that realism is wrong, whereas Devitt assumes that it is right. In addition, by showing that realism is mistaken, that individuation and partitions are interest-dependent, I have suggested that ultimately ontic and semantic issues and questions rest on epistemic issues and questions.

Returning to Devitt's account of the causal theory of reference, we can see his commitment to correspondence and realism. Recall that Devitt summarizes the theory as follows:

> The central idea of a causal theory of names is that our present uses of a name, say `Aristotle',

> designate the famous Greek philosopher Aristotle, not in virtue of the various things we (rightly) believe true of him, but in virtue of a causal network stretching back from our uses to the first uses of the name to designate Aristotle. It is in this way that our present uses of a name "borrow their reference" from earlier uses. It is this social mechanism that enables us all to designate the same thing by a name. (1981, p.25).

In the simplest case, the naming baptism, we simply are perceptually (causally) connected to an object and have `grounding thoughts´ (including a mental representation) about the object. So, for example, I look at my newly-acquired kitten (say, newly-born) and, having the appropriate mental representation, I say, "I call you Nana." Thus Nana is named. I perceive the cat and correctly believe it to be a cat (this latter epistemic aspect is necessary, for Devitt; see above, chapter two, p.52 and corresponding footnote). By being causally connected to the cat in this way and by saying, "I call you Nana," I have named (and referred to) Nana.

Now, notice the various commitments involved here. First, there is a clear commitment to realism. I can only be causally connected to the cat if in fact there is a cat there to which to be causally connected. Quite simply, the cat has to be there. Devitt is explicit about this when he insists that I must see the object which I am naming (referring to) as a member of a very general category. Not only is Devitt committed to saying that the cat is there, but he is committed to the view that I see the object as, say, a cat. For my concerns, what this means is that there

must be a prior partitioning established so that I am referring to and causally connected with a cat instead of an object of some other very different kind of category (single-headed entity? cat time-slice? non-green entity?). So, not only am I committed to a particular view of individuation (with respect to whether and what this individual is), but also I am committed to a particular view of partitionings (with respect to what kind of thing this object is). There is a further, epistemic, commitment in the sense that, according to Devitt, I must believe that there is an individual there and an individual which is a member of a certain category. Finally, there is the commitment that my mental representation corresponds to the state of affairs in the world (i.e. that it is true that I am causally connected to this particular object which I believe is a cat). As can be seen, the simple act of naming is, philosophically, not so simple, after all.

There is one final point I want to make with respect to the causal theory of reference and the issues of correspondence and realism. The point is a major one and a full treatment of it is beyond the present purposes, but I want to raise the point, anyway. The point is this: to the extent that the causal theory of reference relies on the concept of causality, it divorces itself from correspondence and realism. Let me explain briefly. Obviously, the causal theory of reference rests on there being causality. We must be caused, for example, to have the appropriate mental

representations of the objects to which we refer. However, what sense can we make of causality? It is not enough to say that we are causally connected to an object by perceiving it. For one thing, this would rule out reference to abstract and `future´ entities, but, as we saw in chapter two, the causal theory wants and needs to account for our ability to apparently refer to those sorts of entities. Besides this, it won´t do to say that perception explains causal connections because we need causality to explain how perception would do the job. That is, how does perception connect us with the object? We still need a causal explanation for perception. So, Devitt´s identification of perceiving an object with our being causally connected to the object won´t work. Indeed, when we look at what sense can be made of causality, we see that the causal theory of reference which holds objectivity as a desideratum is weakened. I want to suggest (though not fully defend the view) that the notion of causality depends upon a prior conception of normalcy. That is, to say that `x causes y´ is to say, in effect, that x was a variation from the norm for a given situation. For example, to say that the cause of A´s death was A being shot makes sense because it is not normal for A (or things like A) to be shot. If A were shot during a bank robbery, it would not be appropriate to claim that the cause of A´s death was A´s being in the bank (or even A´s being in the bank at that time) because A´s being in the bank (or things like A being in the bank) is not

abnormal. Had A not been in the bank, A might not have died, but simply A's being in the bank is not the cause of death. On the other hand, A's being shot is abnormal for A (and things like A). That could, then, be the cause of death (and in many contexts would count as the cause of death). Nor does the emphasis on normalcy drop by moving to a different level of explanation. For example, it won't do to object that it wasn't being shot that was the cause of death, but the loss of a certain amount of blood that was the cause of death. Again, this would make sense only because it is not normal for A to lose that amount of blood. If A was found dead having lost a teaspoon of blood, we would not assign loss of blood as the cause of death. Losing a teaspoon of blood is not abnormal. So, causality (at least for the purposes of this brief and simple characterization), and the identification of causes, depends upon there being variation from `normal' conditions and situations.

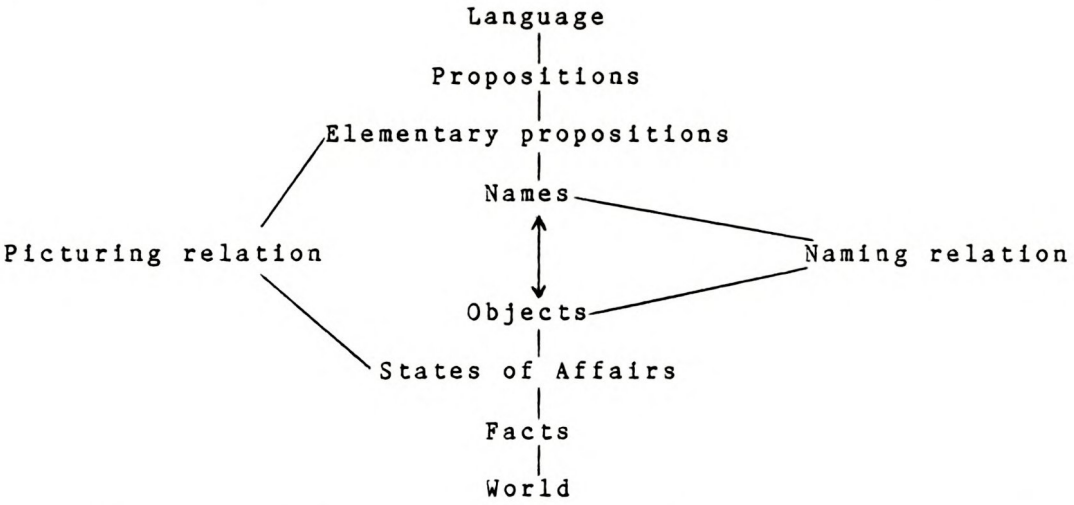
However, normalcy - what counts as being normal - depends upon prior individuation and partitioning. That is, what counts as normal situations for A depends upon what sort of thing A is (i.e. what partitioning is done). If A is a bank robber or a bank security guard, then what is normal for A is different than if A is someone who has never been in a bank before. Take an example from `nature'. Suppose we ask, "What is the cause of quasars travelling at the amazing speeds that they do?" Well, what is normal

travelling speeds for quasars is different from what is normal travelling speeds for non-quasars. We might appeal to particular laws of physics and theories about the Big Bang and suggest that the group of objects about which we are referring is those objects created in the Big Bang. So, we can ask for the cause of those amazing quasar velocities because what is normal in this situation is not what is normal for quasars, but what is normal for things created in the Big Bang. But, again, what is crucial is how we break up the world, and, as I argued in chapter five, how we break up the world is interest-dependent. We have certain interests (say, cognitive, theoretical interests) in breaking the world into quasars versus non-quasars rather than things created in the Big Bang versus not-(things created in the Big Bang). (Of course, at one time it might be in our interest to partition the world one way and at another time in our interest to partition the world in a different way.) The point here is that causality depends upon the prior notion of normalcy, which, in turn, depends upon the particular individuation and partitioning of the world. The latter is interest-dependent. To the extent, then, that the causal theory of reference rests on causality, it needs to either give a realist, objective account of causality or to give up causal talk as a way of accounting for reference (and truth).[16]

NOTES

[1]I am not interested here in whether or not propositions (as opposed to, say, sentences) are the bearers of truth. I will use the term `proposition´, but in doing so, I am not taking a stance on this issue.

[2]Graphically, the relation, for Wittgenstein in the Tractatus, between language and the world is as follows:



[3]However, in the shorter, informal presentation of his semantic theory (1944), he insists that the semantic conception of truth is "completely neutral" with respect to epistemic and metaphysical attitudes. As I argue below, I believe he is mistaken about this; his theory is committed to realism.

[4]The following synopsis of Tarski draws heavily from Haack´s (1978) discussion of him.

[5]While Tarski´s (1944) definition of Convention T is: "(T) X is true if, and only if, p," his original (1956) definition was as follows: "Convention T. A formally correct definition of the symbol `Tr´, formulated in the metalanguage, will be called an adequate definition of truth if it has the following consequences: (a)all sentences which are obtained from the expression `x ∈ Tr if and only if p´ by substituting for the symbol `x´ a structural-descriptive name of any sentence of the language in question and for the symbol `p´ the expression which forms the translation of this sentence into the metalanguage; (b)the sentence, `for any x, if x ∈ Tr then x ∈ S´ (in other words `Tr ⊆ S´)." (1956, pp.187-188). [`S´ here refers to an arbitrary class sequence.]

[6]As Haack put it, in a manner of speaking, Convention T



fixes the extension of `true´.

[7]The reason that Tarski defines satisfaction first is because closed, compound sentences are formed from open sentences rather than from closed, atomic sentences. For example, `(∃x)(Fx v Gx)´ is formed from `Fx´ and `Gx´ by the operations of disjunction and existential quantification. `Fx´ and `Gx´ are not true or false; they are satisfied by objects. The definition of satisfaction is recursive (i.e. definitions are given first for the simplest open sentences and then the conditions are stated in which compound open sentences are satisfied).

[8]Actually, if A is a wff, then (∃∅)A is a wff, where ∅ stands for any variable.

[9]Tarski´s stated definition of `satisfies´ is: "The sequence f satisfies the sentential function x if and only if f is an infinite sequence of classes and x is a sentential function and these satisfy one of the following four conditions: (a)there exist natural numbers k and l such that $x='k,l$ and $f_k \subseteq f_l$; (b)there is a sentential function y such that x=y and f does not satisfy the function y; (c)there are sentential functions y and z such that x=y+z and f either satisfies y or satisfies z; (d)there is a natural number k and a sentential function y such that $x=\cap ky$ and every infinite sequence of classes which differs from f in at most the k-th place satisfies the function y." (1956, p.193).

[10]Tarski´s (1956) stated definition is: "x is a true sentence - in symbols x ∈ Tr - if and only if x ∈ S and every infinite sequence of classes satisfies x." (1956, p.195).

[11]Another, perhaps simpler, way to present Tarski´s procedure is as follows: Let I be a function from expressions (in O) to objects (in the world, or in a model), such that to each individual constant I assigns an object; to each variable I assigns an object; to each n-place predicate I assigns a set of ordered n-tuples of objects. Then, `Fa´ is true under I iff I(`a´) ∈ I(`F´); `Fab´ is true under I iff <I(`a´), I(`b´)> ∈ I(`F´); ``Fa´ is true under I iff I(`a´) ∉ I(`F´); `A & B´ is true under I iff A is true under I and B is true under I; `Fx´ is true under I iff I(`x´) ∈ I(`F´); `(∃x)Fx´ is true under I iff there is an interpretation I´ which differs at most in what it assigns to `x´ which makes `Fx´ true under I.

[12]To suggest at this point that what the semantic theory of truth (or any theory of truth, for that matter) aims at giving is not a criterion (or criteria) for telling us what is true, but rather a definition of truth will not, I think, work. I still want to ask why we would want a

definition of truth. It is not simply to give us a definition, but because we want to be able to distinguish between true claims and false claims. The purpose for this (or, at least, a purpose) is to give us a criterion for accepting inferences based on true (but not false) claims and for acknowledging, say, warrantability of actions and practices based on particular (true) claims. Arguing that the semantic theory gives us a definition of truth, not a criterion of demarcating true claims from false ones, does not, I think, meet the objection I am raising in the text.

[13]Or, consider the case of humoring someone and speaking about that person as the present king of France. So a speaker says, `I see the present king of France is hungry.´ The speaker and hearer (assuming both know that the speaker is humoring the person being looked at) might agree that the speaker has referred to, and made a true statement about, that particular person being looked at.

[14]One might want to argue that the semantic theory of truth does not entail that the set of individuals exists independent of our interests. One might say that we can agree that we determine what individuals there are and in what ways they are (dis)similar and furthermore hold the view that - given those individuals - truth is definable in terms of their satisfying open sentences. So, satisfaction is just as legitimate for gruesome speakers as for English speakers. I am somewhat sympathetic to this claim, but I am hesitant to endorse it for two reasons. First (and less importantly), I don´t think that this is what Tarski had in mind in proposing his theory. Second (and more important), this begins to rob the notion of correspondence of any significance and uniqueness. The semantic theory fails to serve as a vehicle for determining what is true if it allows correspondence between language and any individuals (and partitionings). Besides, we would still be left with no clearer meaning of what correspondence is than our original intuitions. Yet the semantic theory was meant, in part, to clarify those intuitions, to give us a more precise conception of what correspondence is. Nonetheless, if one argues that the semantic theory of truth and its corollary conception of correspondence is consistent with an interest-determined principle of individuation and similarity relation, then I find it unobjectionable, because it seems to me to then be indistinguishable from a coherence, verificationist point of view.

[15]Kripke may be admitting this when he confesses that, for ordinary statements, "there is no intrinsic guarantee of their safety (groundedness) and that their `level´ depends on empirical facts" (1975, p.707).

[16]Putnam (1981) also suggests that the causal theory of reference fails to the extent that it relies on causality,

or at least on appropriate causal chains. He states:

> ...if I say `the word "horse" refers to objects which have a property which is connected with my production of the utterance "There is a horse in front of me" on certain occasions by a causal chain of the appropriate type', then I have the problem that, if I am able to specify what is the appropriate type of causal chain, I must already be able to refer to the kinds of things and properties that make up that kind of causal chain. But how did I get to be able to do this?
>
> The conclusion is not that there are no terms which have the logic ascribed by the similitude [i.e. cluster] theory, any more than the conclusion is that there are no terms which refer to things which are connected to us by particular kinds of causal chains. The conclusion is simply that neither similitude nor causal connection can be the only, or the fundamental, mechanism of reference. (1981, p.66).

Precisely!

A CONSTRUCTIVIST ALTERNATIVE

"I have never doubted the truth of signs, Adso; they are the only things man has with which to orient himself in the world."
(Umberto Eco, The Name of the Rose)

INTRODUCTION

In this final section I want to suggest an alternative view of reference, a view based not on realism and a correspondence theory of truth, but on verificationism and a coherence theory of truth. Like Kripke, I claim to offer only a `better picture´ of a theory of reference. Like Kripke, I also claim not to offer a set of necessary and sufficient conditions for (a theory of) reference. In chapter eight I will present (admittedly minimally) a case for coherence and verificationism.[1] In chapter nine I will present a glimpse of what a view of reference would be based on coherence and verificationist commitments. The case I present in these final two chapters is admittedly minimal partly because I have already been indicating my reasons for such views in the discussion up till now of my misgivings with the received theories of reference and their underlying commitments. Besides this reason for giving here a minimal case for coherence and verificationism (and a subsequent view of reference), I also see the project of fully (and adequately) giving my case as beyond the scope of this present book. What I have been hoping to do here is primarily to show the received theories of reference as both being committed to privacy, essentialism, realism and correspondence. As such I see them as two sides of the same



theory. I have tried to identify their shared underlying commitments and, at the same time, criticize those commitments. Having done that, I now want to consider an alternative view of reference, based on alternative commitments. The form that this consideration will take here is simply that of initial feelers which suggest a plausible alternative. These final two chapters, then, are little more than a statement of a scope of work to be done, not a final theory of reference based on coherence and verificationism.

As mentioned in the preface, this work is intended to do at least two (related) things. One is to show that theories of reference are not simply semantic matters, but carry with them commitments to other issues and positions. To fully understand and appreciate theories of reference, we need to make as explicit as possible these underlying commitments. The second intention I have in mind in this work is to show that commitments to, say, theories of truth, bear directly on other matters (for example, on theories of reference). These two intentions are, perhaps, merely two angles on the single point to which I am committed, viz. our philosophical positions are understandable only as coherent wholes, with no sharp separation between issues of ontology, epistemology, semantics, etc.

Now, before suggesting an alternative view of reference, I want to review my account of the received theories of reference. I will do this by indicating what I

see as the strengths and weaknesses of the received theories. First, what I see as the major strength of Searle's cluster theory is that he insists that reference must be understood as a speech act, that it is intimately tied up with our extra-semantic, pragmatic concerns. I agree completely and, in fact, see one of the major weaknesses of Searle's position as his abandoning of this insistence when he allows private reference. The formal presentation of Searle's theory has, I think, been soundly criticized by Kripke (though, as I argued in the first chapter, I think that Kripke attacked something of a simplified version of Searle). What I see as the strength of Kripke's causal theory is his insistence that reference is public, that we do not refer simply because we intend to. Of course, as I argued in chapter four, I think that while Kripke talks as though publicity is necessary for successful reference to occur, his own theory (especially in its fullest - Devittian - form) allows private reference. The (somewhat) formal presentation of Kripke's theory faces difficulties with ordinary cases of reference to, say, abstract and `future' entities (cases which are of no problem to Searle). Furthermore, without some satisfactory account of causality, I don't see the causal theory as offering a very plausible alternative to Searle or the descriptivist accounts of refcrence. As I suggested in chapter seven, appealing to causality as a basis for (or even as an element of) a theory of reference will not cohere

well with the sort of `objective, realist´ view which Kripke (and Devitt) want to hold. Opting for talk of an historical theory of reference (a la Donnellan) rather than a causal theory, fares no better unless one can make the assumption of an `objective, realist´ account of history. However, as I argued in chapters five and six, such an assumption is suspect (at least). In sum, then, I think that at best the received theories each offer a necessary condition of successful reference. A Searlean cluster theory, I want to say, is fine, but only to the extent to which it requires public reference. So, when Searle claims that at least one description associated with a name (and used in the primary aspect) must be true of an object for the speaker to refer to the object by the use of the name, then I agree, but only to the extent that it holds, not for the speaker, but for the language community. I can refer to Feynman by (the use of) `Feynman´ even if I have no true descriptions associated with `Feynman´ to enable me to pick out Feynman. However, this is only because names are public and I am a member of a language community. If the descriptions associated with a name failed for the entire language community, then I would argue that reference fails. (I am not here suggesting that Searle is right to claim that there is a cluster of descriptions which each speaker associates with a given name. As I will argue in chapter nine, this view of the speaker having a `ready made´ cluster of descriptions associated with a name distinguishes his account of

reference from Wittgenstein's account, and weakens his (Searle's) position.)

So, while Searle's account may, at best, provide a necessary condition for successful reference, the same is true for Kripke's causal account. Objects are historical entities and reference is an historical process; both are public. For reasons already mentioned, I think that in the end, the causal theory fails to give a plausible theory of reference, however, the intuitions which Kripke pointed to in explicating his causal view are, I think, correct in large part. Kripke is right, I think, to recognize that reference must be understood diachronically. Where he goes wrong is in thinking that this diachronic feature of reference is also a realist feature. In the following two chapters, I will draw on what I see as the positive aspects of the received theories. As they have been presented by their adherents, they are fundamentally mistaken. However, they are not completely mistaken. To the extent that they are not mistaken, they can (and will) be incorporated into the alternative view which I will now offer.

NOTE

[1] I want to stress at this point that the sense of coherence I will employ in the following chapters is coherence as consistency and completeness. However, by this I do not mean simple logical consistency and completeness. (Likewise, when I speak of verification(ism), I do not mean logical verification(ism).) Such a purely logical conception of coherence and verification(ism), divorced from human practices, behaviors and institutions, is, I will argue, a mistaken way of seeing coherence and verification(ism).



CHAPTER EIGHT

COHERENCE AND VERIFICATIONISM

In this chapter and the next, I want to suggest a view of reference that is an alternative to the received theories. The alternative takes the form primarily of an alternative with respect to underlying commitments. On the surface, this view of reference looks much like the theories proposed by Searle and Kripke. It is the nature and meaning of reference, not its theoretical structure, which is most important in the present context. Indeed, the theoretical structure of reference can only be understood, I want to claim, within the context and confines of its nature and meaning. Ultimately, reference is (and can only be) understood within the behavior and practices (i.e. the Wittgensteinian `forms of life´) that encompass it and give it not only its meaning but also its possibility. As I have argued up to now, theories of reference carry with them, and are based on, underlying (epistemic) commitments. I have argued that the received theories of reference share particular epistemic commitments and rest in large part on particular conceptions of individuation and similarity. Those conceptions I have identified as being correspondence and realism (and, subsequently, essentialism and privacy), and I have indicated that I find those conceptions as being

fraught with difficulties. In this chapter I want to consider alternative conceptions, viz. coherence and verificationism. I will present what I consider to be plausible views of these two positions and their conceptions of individuation and similarity. I will then consider objections to those views. In the next chapter I will propose (albeit tentatively) a view of reference consistent with these views and suggest how I see such a view of reference as being superior to the received theories of reference.

First, then, I want to present what I take a coherence theory of truth to be. In simplest terms, a coherence theory of truth is the view that the truth of a statement is a function of its coherence (i.e. consistency) with the set of statements accepted as true. Several features stand out from this simple statement. First, the coherence theory of truth emphasizes the issue of justification. That is, what justifies the assertion that a claim is true is that the claim coheres with the set of accepted statements. Second, the coherence theory of truth is (or, at least, appears to be) strictly epistemic. Note that there is no mention in the `definition´ of coherence of reality or the world. (As will be addressed below, this feature has been criticized by opponents of the coherence view.)

The coherence theory of truth historically has been closely associated with philosophical idealism. However, coherence also historically has been associated with logical



positivism. This might indicate that the theory is truly epistemic and makes no ontic commitments to, say, idealism or realism. (Both White (1970) and Haack (1978), for example, discuss coherence as separable from ontic commitments. I disagree, and will return to this issue below.) Contemporary versions of a coherence theory include those of Blanshard (1939), Quine (1972) and Rescher (1973).

While coherence is usually spoken of in terms of consistency, coherence theorists stress that consistency alone is not the mark of truth or a good view of truth. For example, a theory which contained a single atomic proposition would, on just the basis of consistency, constitute a consistent set of (accepted) statements. Yet, intuitively, this is not a plausible view of truth. What is also required (minimally) is completeness or comprehensiveness. What is desired is not simply a set of accepted statements, but the most complete set of consistent (accepted) statements. So, for example, in Rescher's (1973) presentation of a coherence theory of truth he refers to a maximal consistent subset (MCS) of beliefs (of a set of beliefs), which he defines as follows: S' is a maximal consistent subset of S if it is a non-empty subset of S which is consistent; and to which no member of S not already a member of S' can be added without generating an inconsistency. (As will be noted below, this characterization of coherence, and ones like it, have been criticized as, in Russell's words, failing to distinguish

truth from consistent fairy tales. I will discuss this criticism below.) Minimally, then, a coherence theory of truth demands not only consistency but also completeness.

Whether or not there is a necessary connection between coherence and ontic commitments, there has definitely been a connection between coherence and other epistemic concerns such as verifiability. For example, often (if not always), a test of someone's statement as being true or false depends upon the statement's consistency (or lack of it) with accepted statements. For instance, if a speaker insists that she saw a ghost (or, for that matter, a rose-breasted grosbeak), a minimal test for the acceptance of that claim as true depends upon its consistency with accepted statements. We would probably be sceptical about the truth of seeing a ghost given the accepted statements we hold today. While we would probably tend to accept a claim about the sighting of a rose-breasted grosbeak, we wouldn't if the claim were made in particular contexts (for example, in my basement or in New York City during February). Again, consistency with accepted statements seems crucial for the acceptance of a given statement as true. This is especially true for, say, mathematical claims. To say that Goldbach's Conjecture is true seems to rest fundamentally on its consistency with accepted mathematical statements. (Indeed, as Russell's paradox of set membership indicates, internal inconsistency is sufficient for rejecting not only a given mathematical statement, but an entire system.) Another

intuitive sort of statement for which consistency is primary is that of historical claims. For example, the claim (see Gould (1983)) that Teilhard de Chardin was responsible for the Piltdown hoax is determinable only on the criterion of consistency. We accept the claim as true only if there is sufficient confirming or corroborating evidence and reject it as false if there is disconfirming evidence (i.e. if the claim is inconsistent with accepted statements). In none of these cases is there any mention of correspondence to facts. Besides this, however, there is an apparent connection between coherence and verification (or, at least, testability). (An objection to the presentation just given is that coherence is not necessarily connected with verification because a claim can cohere with (accepted) statements whether or not we could verify this coherence. I will consider this objection below.)

Before considering the various criticisms of coherence which I have mentioned above, I want to address an issue associated with coherence which I do not consider to be particularly significant. The issue is that of degrees of truth. If coherence is simply a matter of internal consistency along with completeness, then, it is said, to the extent that a given statement is consistent with more or fewer other accepted statements that given statement is more or less true. So, a claim (S) that is consistent with more accepted statements than is another statement (S´) is more true than that other statement. (The calculation of `amount

of consistency' could be the following: the degree of truth of statement S is a function of the number of accepted statements with which S is consistent minus the number of accepted statements with which S is inconsistent. Call this value `the relative truth' of the statement ($T_r(S)$). So, S is more true than S' if and only if $T_r(S) > T_r(S')$.) (Of course, in one sense this definition is inadequate - and the objection is incoherent - because given any statement S, its `relative truth' with respect to other statements depends upon the other statements. If a statement S is inconsistent with other statements in a set, then it is inconsistent with statements in any larger set (which includes ~S).) I don't think that the issue of degrees of truth is significant for the present concerns. In part, I say this because this way of looking at coherence seems to presume that consistency is a matter of degree and that there is already a specifiable set of accepted statements with which to compare a given statement. As will be seen, in the conception of coherence which I propose below, the notion of consistency as a matter of degree doesn't make sense, nor does the notion that there is an already-established specifiable set of accepted statements. (Indeed, as I will argue in the next chapter, this is analogous to a difference between Searle's theory of reference and Wittgenstein's view of reference; for Searle, it makes sense to speak of an already-established disjunctive set of descriptions associated with a name, while, for Wittgenstein, this makes no sense.) I will

mention this issue of degrees of truth again later, but for now I will turn to criticisms of coherence and try to respond to those criticisms.

Criticisms of Coherence

I will consider four objections to coherence. First, it is claimed that advocates of the coherence theory of truth confuse a definition of `truth´ with a criterion for truth. Second, it is claimed that coherence as consistency and completeness fails to make contact with the world; it fails to distinguish truth from consistent fantasy. Third, it is claimed that coherence fails to offer any connection with verification. Fourth, it is claimed that `truth´ and `true´ apply not to particular statements, but only to systems or sets of (consistent and complete) statements; this is minimally unintuitive. I will flesh out and consider each of these objections.

Objection One: The coherence theory of truth confuses a definition of `truth´ with a criterion for (the acceptance of) truth. It may well be that we demand coherence among our statements as a criterion for testing statements, but this does not serve as a definition of `truth´. That is, it may well be that when determining whether or not it is true that Teilhard de Chardin was responsible for the Piltdown hoax we demand coherence between this claim and other accepted statements, but this only entails that we want coherence, not that truth is identifiable with coherence. Furthermore, coherence is a desideratum because the world is



coherent. Coherence does not entail truth, it reflects reality (and truth).

Objection Two: The coherence theory of truth fails to make any hook-up with the world. Taken literally, the coherence theory of truth fails to distinguish between truth and coherent fantasy. Two complete and internally consistent sets of statements could be inconsistent with each other, yet, under the coherent theory of truth, they would both be true. For an external point of view, then, the coherence theory of truth would countenance inconsistency (and hence be incoherent).

Objection Three: The coherence theory of truth, while apparently relying on verification for determining the truth of statements, fails to make any necessary connection with verification. The theory simply demands consistency (and completeness), but this says nothing about whether we can verify any coherence among statements.

Objection Four: The coherence theory of truth allows truth to apply only to systems or sets of statements, not to particular statements. It is not the case, then, that `Teilhard de Chardin is responsible for the Piltdown hoax´ is a true (or false) statement. What is true (or false) is the set of accepted statements of which one member is the statement `Teilhard de Chardin is responsible for the Piltdown hoax.´ Minimally, this is unintuitive. Surely, truth applies to particular statements.

I now want to respond to these objections. (While I

will specifically address a particular objection at any given time, my response to each overlaps with my response to the others. In effect, I see the objections as interrelated; consequently, my responses will be interrelated and will be presented as responses to a single, disjunctive objection.)

The first objection is that the coherence theory of truth confuses a definition of truth with a criterion for (the acceptance of) truth. (My response to this objection will usher in the verificationist elements of my view.) It has been argued that coherence serves as a criterion for (the acceptance of) truth or truth claims, but that this does not constitute a definition of `truth´. The conflation here, it is claimed, is reflective of a conflation of two related but separate questions: (1) what is coherence (i.e. what does `coherence´ mean)?, and (2) what coheres (i.e. what (kinds of) things exhibit coherence)? An answer to the first question is a definition of `truth´, while an answer to the second question is a criterion of truth. One might accept the view that coherence is important (essential, necessary, etc.) for serving as a criterion for (the acceptance of) truth. Indeed, we <u>do</u> demand coherence among our claims. However, this hardly shows that what `truth´ <u>means</u> is coherence. The definitional question can be answered by, say, `correspondence to the facts´, while the criteriological question can be answered by an appeal to coherence among our statements. In fact, under this view,

since `truth´ means `correspondence to the facts´, we would insist on the criterion of coherence, since we would want to assert (assume?) that facts cohere with one another. (Here I am not only arguing that the sharp distinction between coherence as criterion and as definition cannot be drawn, but also that coherence can (and does) serve as a definition, as well as a criterion, of truth.)

Now, what could it mean to say that there is a distinction between saying that something <u>is</u> true and saying that we are justified in <u>classifying</u> something as true? What can it mean to say that it really is true that Teilhard de Chardin was responsible for the Piltdown hoax without at the same time saying that we are justified in classifying the statement `Teilhard de Chardin was responsible for the Piltdown hoax´ as true? Could we ever meaningfully assert that a statement was true (really, actually true!) without being committed to claims concerning the justifiability of such an assertion? At least in this case, the difference between definition and criteria does not seem to me to make a difference. What, after all, would a (proper) criterion (or definition) of truth do or seek other than, say, necessary and sufficient conditions for a statement´s being warranted and justifiably classified as true? A (proper) criterion, we would suppose, establishes the conditions for justifiably accepting a statement as true. However, if it is not sufficient for establishing the statement as true, why is is sufficient for establishing the statement as

justifiably acceptable (as true)?

What I have done so far is to suggest that the difference between coherence as a definition of `truth´ and as a criterion for (the acceptance of) truth is not obvious. Minimally, the establishment of a difference needs greater motivation. What provides this greater motivation comes, I think, from considerations which prompt the second objection, viz. there must be some hook-up between coherent systems and the world. The criterion for warranted assertability might well be coherence with accepted statements (belief, theory, etc.), but the hook-up with the world comes with correspondence with the facts (or, say, with Tarskian satisfaction). This brings us to the second (and, perhaps, most important) objection: the coherence theory fails to make any hook-up with the world. As mentioned above, Russell claimed that the coherence theory of truth fails to distinguish between truth and consistent fantasy. Along these lines, Russell (1912) suggests the following difficulty with coherence:[1]

> ...there is no reason to suppose that only <u>one</u> coherent body of beliefs is possible. It may be that, with sufficient imagination, a novelist might invent a past for the world that would perfectly fit on to what we know, and yet be quite different from the real past. In more scientific matters, it is certain that there are often two of more hypotheses which account for all the known facts on some subject, and although, in such cases, men of science endeavour to find facts which will rule out all the hypotheses except one, there is no reason why they should always succeed. (1912, p.122).

In part, because the coherence theory fails to guarantee any

hook-up with the world, any number of coherent systems could be true. The coherence theory offers no criterion for distinguishing between coherent systems. Furthermore, although Russell does not bring in this point explicitly, it will not work to say that the coherence theory does distinguish between coherent systems by insisting on completeness as well as consistency. The reason that this defense won't work is because there could be numerous (perhaps infinite) consistent and complete systems. Coherence, then, even conceived of as consistency coupled with completeness, fails to offer a criterion for distinguishing between different coherent systems. Quite simply, even if coherence is a necessary condition for truth, it is not a sufficient condition.

Now, it seems to me that this objection (i.e. coherence fails to establish a connection with the world) is fundamental as a challenge to coherence. I want to offer two responses to this challenge. First, I want to argue that the coherence theory of truth does connect with `the world'. Second, the objection itself presupposes realism by presupposing that there is a world `out there' with which coherence must connect. I want to question this presupposition on two grounds; first, that there is a (i.e. one) world `out there', and, second, that there is a world out there (i.e. independent of our systems).

First, then, I want to argue that coherence is not simple logical consistency (and completeness). Rather,

coherence rests on our behaviors, practices and institutions, what Wittgenstein has called our `forms of life´.

Coherence, I want to claim, is a semantic, linguistic concept. As such, it is a public concept. That is, as I argued in chapter four, language is necessarily public; consequently, notions of coherence, being linguistic, are also necessarily public. The arguments which point to the publicity of language and coherence also point to the extra-linguistic nature and basis of language and coherence. A brief summary of Wittgenstein´s arguments concerning language, publicity and forms of life will help here.

Wittgenstein (1953) says:

> "So you are saying that human agreement decides what is true and what is false?" - It is what human beings <u>say</u> that is true and false; and they agree in the <u>language</u> they use. This is not agreement in opinion but in form of life. (1953, #241).

The conceptual context for this claim is the following. Wittgenstein saw the meaning of a word (at least for "a <u>large</u> class of cases") as its use in a language. Words and sentences are `tools´ which are used for specific purposes within a language. The employment of words and sentences is understandable only within `forms of life´. The notion of meaning as use within (different) contexts led Wittgenstein to his famous conception of `language games´, a notion which is meant to capture the fact that language is both rule-governed (like games) and a human artifact (like games). This is not to say that words can mean anything we want them

to. As children, when learning a language, we learn to use it correctly by the rules which govern the language. We learn these rules to an extent explicitly (e.g. we study grammar in elementary school), but for the most part we learn these rules implicitly (e.g. by the way people speak to us). These rules are learned by how we and others use expressions, and obeying a rule (which is necessary for language) is a practice. The simple fact that we learn the meaning (and form) of expressions by how they are used prevents us from making up whatever rules we wish, because we would not be able to communicate with others by using private rules (besides, conceptually, as noted in chapter four, private rules make no sense). We follow (perceived) regularity of behavior.

This (perceived) regularity of behavior points to two important features, the fundamental nature of forms of life (and language games) and the importance of verification. For Wittgenstein, what has to be accepted, what is given, are forms of life (i.e. our behaviors, practices, institutions, etc.). There are social contexts within which our concepts and language games operate. (E.g.: "`I know all that.´ And that will come out in the way I act and in the way I speak about the things in question." (1969, #395).)[2] Consider the following analysis of doubting as illustrating the fundamental nature and importance of forms of life. According to Wittgenstein, the language game of doubting cannot be universal in scope. That is, we can´t

doubt everything. If we tried doubting everything, we would not get as far as doubting anything. As he puts it, the game of doubting presupposes certainty and this comes from the very nature of language:

> That is to say, the questions that we raise and our doubts depend on the fact that some propositions are exempt from doubt, are as it were like hinges on which those turn.
>
> That is to say, it belongs to the logic of our scientific investigations that certain things are in deed not doubted.
>
> But it isn't that the situation is like this: we just can't investigate everything, and for that reason we are forced to rest content with assumption. If I want the door to turn, the hinges must stay put.
>
> My life consists in my being content to accept many things. (1969, ##341-344).

For language to be possible rules must be obeyed. This obedience is a practice; as such, our behavior is consistent. Of course, Wittgenstein is not simply appealing to common sense here as support for or proof of the notion of forms of life. As he says:

> Giving grounds, however, justifying the evidence, comes to an end; - but the end is not certain propositions striking us immediately as true; i.e., it is not a kind of seeing on our part; it is our acting, which lies at the bottom of our language-game. (1969, #204).

This leads, then, to a conception of truth. Wittgenstein remarks that what a proposition is is in one sense determined by the rules of sentence formation and in another sense by the use of the sign in the language game. The words `true' and `false' function just as other constituents of the game and are not separate from it.[3] Wittgenstein uses an example of a chess game to illustrate

this. If one says `The king in chess is the piece that one can check´, one has said no more than that in chess we only check the king. To speak of the truth of the statement outside of its context of chess is nonsense. (Also, `One can steal second base but not first base´ is true, but not outside the context of baseball.) When Moore tried to determine the truth or falsehood of `I know here is my hand´, for example, as though the truth value were part of the context preceding language, he was mistaken. He was trying to treat truth as something outside of language (and perhaps language as something outside of our practices, or forms of life).

This leads back to the quotation above initiating this discussion of Wittgenstein. Does our language determine what truth is (and what is true)? Are we led to a subjectivist, relativist view of truth? As noted above, his response is that what is true, for him, is what is rooted in our language, which is rooted in our forms of life. Communal agreement and use (based on communal practices) is fundamental to the notion of truth. Now, it is a simple move from use as basic to utility as basic for the conception of truth, and indeed Wittgenstein remarks that he is trying to say something that "sounds like pragmatism." (1969, #422). For Peirce (1955), talk of truth is meaningful only within the actual and practical bearings which frame human activity. (For Peirce, however, the practical is the sort of inquiry which characterizes

scientific activity.) James (1977) saw the practical as the importance of beliefs to the values and behavior of individuals. (For James, to think of some sort of independent truth is beside the point. If beliefs are verified, if they satisfy what is expected or intended, they are true. If something works, it is right; if it satisfies, it is true.) Dewey (1950) stressed more the use of beliefs than their utility, within the context of our actions and inquiries. (The traditional correspondence view of the relation of fact and belief is not enough, for Dewey, since we must know what purpose, what goal is being aimed at in our inquiry or activity. We cannot separate the inquiry from its context.) The similarity between these views and Wittgenstein's are, I think, obvious.

Now, this demand for use within context seems to lead toward the emphasis of a coherence view, and this does indeed seem to be Wittgenstein's leaning. He states:

> When we first begin to <u>believe</u> anything, what we believe is not a single proposition, it is a whole system of propositions. (Light dawns gradually over the whole.)
>
> It is not single axioms that strike me as obvious, it is a system in which consequences and premises give one another <u>mutual</u> support. (1969, ##141-142).[4]

Quite simply, for Wittgenstein, we do not have experiences, or make statements, or hold beliefs in isolation. They all occur within our language games and are based on our forms of life. The significance of this for the present concerns is that it is not the case that a coherence theory of truth fails to make any hook-up with `the world' (or, as I prefer

to say, it is not the case that a coherence theory of truth fails to incorporate a non-linguistic component). The hook-up (or component), however, is not some sort of correspondence with facts; rather, it is an acknowledgement that truth (as coherence) emerges from communal language, which in turn emerges from communal behaviors, practices and institutions. Coherence as consistency and completeness is not, then, simply logical consistency and completeness, but communal, practical, consistency and completeness. (Note that this also illustrates the verificationist element of coherence. Because truth (as coherence) is ultimately based on language and practice, truth claims must, in effect, be verifiable through that language and practice. Unverifiable truth claims make no sense in a context in which truth is based on language and practice.)

In considering the objection that coherence fails to make any connection with the world (and allows an infinite number of possible coherent systems), I remarked that I had two responses to this objection. The first (just discussed) was that I think one can argue for a non-linguistic component of coherence (viz. forms of life). The second response, I said, was that the objection rested on realist assumptions. As I indicated in the previous chapter, I find realist assumptions questionable. I now want to flesh out my second response to the objection.

Russell thought there was a problem with coherence because it allowed an infinite number of possible coherent

(but different) systems. Glibly, my response is: so what? Following Goodman (1972, 1978), I think it is a mistake to speak of the way the world is. Instead, we should speak of the ways the world is. Since our most fundamental ontic concerns, viz. individuation and similarity, are, as I argued in chapter five, theory-, language-, and interest-dependent, and since our communal practices ultimately determine notions of truth, then to suggest that there is a way the world is, is, I think, mistaken. English speakers might well decide that the world is `really´ partitioned into green things and not-green things, while gruesome speakers might well decide that the world is `really´ partitioned into grue things and not-grue things. But is grass `really´ green or grue? Well, it´s neither and it´s both. It´s neither if that means that the statement that grass is green is true independent of a language community; it´s both if that means that different language communities view it as one color or the other and that view is coherent with their other beliefs (both synchronically and diachronically). The recognition that there are different ways of individuating objects and different ways of partitioning the world removes the motivation for insisting that there is a way the world is.[5] Furthermore, it seems to me that nothing is lost in making this recognition. That there might be various legitimate interpretations of a given event does not mean that each interpretation is illegitimate. The plausibility of alternative

interpretations does not show the disvalue, simple-mindedness or falsity of any particular interpretation; rather it shows the enormous complexity of the event in question.[6]

Besides the fact that Russell's objection to coherence presumes that there is a way the world is, it also presumes that there is a world out there. The above discussion, along with my remarks in chapter seven, speak against this realist assumption, so I will not repeat them here. Suffice it to say, I don't see that Russell has shown his presumption to be warranted.

The raising of verificationism above leads into the third objection that was mentioned regarding coherence. The objection is that there is no necessary connection between coherence and verification(ism). Statements can cohere (or fail to cohere) independent of whether or not we can verify them as coherent or as warrantable. Yet the discussion of coherence seems to rely on verification(ism) as a criterion for acceptability (since correspondence with facts is not available to the view of coherence presented here). I want to suggest that this objection fails and it fails because it presumes that this sharp distinction can be made between the logical or semantic issue of whether or not statements cohere and the epistemic issue of whether or not (and how) we can verify that statements cohere. As I have indicated throughout this book, one of the points I am arguing for is that the former issues cannot be isolated from the latter;

epistemic issues and concerns underlie ontic and semantic issues and concerns. With regard to the issue at hand, I want to suggest that it makes no sense to claim that statements can and do cohere apart from saying that we can verify this. Consider again that semantic issues are linguistic and that language is a public process based on communal activities and practices. As such, semantic and linguistic issues arise from and are determined by those underlying forms of life.

The move from verifiability to coherence is, it seems to me, quite straightforward. As Waismann (1965) argues:

> Suppose there is a metal ball in front of me, and I have the task of finding out whether the ball is charged with electricity. To do that I connect the balls with an electroscope and watch whether the gold leaves diverge. The statement `The gold leaves of the instrument diverge´ (s) describes the verification of the statement `The ball is charged´ (p). Now what exactly am I doing when I describe the verification of the statement p? I establish a connection between two statements by declaring that the one (s) is to follow from the other (p). In other words, I lay down a rule of inference which allows me to pass from the statement `The ball is charged with electricity´ to another that describes an observable situation. By doing this I connect the statement with another one, I make it a part of a system of operations, I incorporate it into language, in short, I determine the way it is to be used. In this sense giving the verification of a statement is an important part of giving its use, or, to put it differently, explaining its verification is a contribution to its grammar. (1965, p.123).

The problem is not to make the move from verification to coherence - this is what Waismann is arguing for here - but to make the move from coherence to verification(ism). That is, while there might be a necessary connection from

verification to coherence, is there a necessary connection from coherence to verification(ism)? It seems to me that there is and it is based on the same motivations as for the converse connection. To be committed to coherence, as we saw above, is to be committed to a conception of truth that emerges ultimately from communal language and forms of life. To claim that a statement is true or that two statements cohere is, in the final analysis, meaningless apart from some communally-based understanding (i.e. apart from some public, social, behavioral - verifiable - criterion of understanding). To make the claim that the semantic issue of coherence can be dealt with apart from the epistemic issue of verification is to presume exactly what has been argued is a misconception of language, that it is not rooted in shared human practices. Coherence and verification(ism), then, are inherently connected.[7]

Finally, I want to consider the fourth objection to coherence noted early in this chapter. The objection is that under a coherence theory of truth it is systems of statements, not particular statements, that are true. This, it is said, is unintuitive. Surely, individual statements have truth values. Now, it seems to me that this objection is simply mistaken (and I must admit that I am somewhat bewildered as to why such an objection has ever been raised). A coherence theory of truth does not deny that particular statements have truth values, but the truth values that they have are determined their relation with

other statements. To say that truth is determined by or a function of correspondence with facts is not to deny that particular statements have truth values. Likewise, to say that truth is determined by or a function of coherence is not to deny that particular statements have truth values. It is simply to give an account of how the particular statements have the truth values that they have.

Where, then, does this all lead? Even if we can make sense of a coherence theory of truth, so what? What work can this do for us? As I will argue in the next chapter, one thing that this can do for us is to provide a view of reference that is an alternative to the received theories of reference. Given a coherence theory of truth and a related, non-realist, conception of individuation and similarity we can begin to construct an alternative view of reference that does not suffer the problems of the received theories of reference. To this task, then, we will now turn.

NOTES

[1]Russell sees a second difficulty, viz. coherence rests on the notion of correspondence. He suggests that coherence presupposes the truths of the laws of logic. His argument is as follows:

> Two propositions are coherent when both may be true, and are incoherent when at least one must be false. Now in order to know whether two propositions can both be true, we must know such truths as the law of contradiction. For example, the two propositions, `this tree is a beech´ and `this tree is not a beech´, are not coherent, because of the law of contradiction. But if the law of contradiction itself were subjected to the test of coherence, we should find that, if we choose to suppose it false, nothing will any longer be incoherent with anything else. Thus the laws of logic supply the skeleton or framework within which the test of coherence applies, and they themselves cannot be established by this test. (1912, p.123).

I want to make three points with regard to this argument. First, Russell seems to be identifying coherence with non-contradictoriness. One of the points of this entire chapter is to suggest that this identification is somewhat mistaken, so I will say no more about it at this point. Second, given Russell´s own theory of types and his endorsement of Tarski´s object-language/metalanguage distinction, I am somewhat surprised by his comments here. What is wrong with saying that coherence pertains to the object-language? His move to considering whether or not the law of contradiction is coherent can be construed as a move to the metalanguage. If it is, it´s not obviously fatal to the object-language notion of coherence. Third, Russell seems to be speaking as though the laws of logic are `out there´ prior to the rest of our beliefs, independent of any other system. Yet, as Quine remarks:

> Mathematical and logical laws themselves are not immune to revision if it is found that essential simplifications of our whole conceptual scheme will ensue....Logical laws are the most central and crucial statements of our conceptual scheme, and for this reason the most protected from revision by the force of conservatism; but because again of their crucial position, they are the laws an apt revision of which might offer the most sweeping simplification of our whole system of knowledge. (1972, p.3).

Russell´s conception of the status and nature of the laws of logic is, I think, mistaken. Hence, his conclusion that any abrogation of the laws of logic so as to yield coherence incoherent is also mistaken.



[2]This theme runs throughout Wittgenstein's later works. For example, cf. (1953, ##43, 133, 150, 595) and (1969, ##31, 229, 472, 476, 509, 554).

[3]Cf. J.H. Gill (1966).

[4]Cf. (1969, ##225, 410).

[5]If there are, say, two internally consistent systems which are inconsistent with one another, then we can either accept that they are inconsistent with each other (yet internally consistent) or we can try to remove the inconsistency (and show one system to be false) by proposing that perhaps for the sake of completeness each system adopt a new statement S, which would result in one system remaining internally consistent, but more complete, while the other system becoming inconsistent. For example, the debate between evolution and creationism might be seen as this sort of situation. So, it might be said, carbon dating has shown that the world was not created, say, in 4004BC, or was not created in seven days. The difficulty with this is that creationist do not see this sort of evidence as inconsistent with their view. Following Duhem and Quine, one can argue that it is never a single hypothesis that is tested or falsified. Instead a whole system is tested and can gel and transform if need be. In actual cases of conflicts between internally consistent systems, it is rarely (if ever) the case that, external to those systems, a statement can be presented which will demonstrate that one system is false and the other true.

[6]Paulos (1985) presents a humorous example showing that there is no single,`right' interpretation of events:

> Since a Roman scholar was visiting Timur's court, the emperor asked the Mulla to ready himself for a battle of wits with the scholar.
>
> The Mulla piled books with ficticious, but impressive-sounding titles on his donkey, books such as The Theory of Universal Bifurcants, Erosion and Civilization, A Critique of Tolerant Purity, Social Origins of Mental De-activiation.
>
> The day of the contest the Mulla appeared in court with his donkey and books. His native wit and intelligence overwhelmed the Roman scholar, who finally decided to test the Mulla's knowledge of theoretical matters. The Roman scholar held up one finger.
>
> The Mulla answered with two fingers.
>
> The Roman held up three fingers.
>
> The Mulla responded with four.
>
> The scholar showed his whole palm to which Mulla responded with a closed fist.
>
> The scholar then opened his briefcase and

out an egg. The Mulla responded by digging an onion out of his pocket.

The Roman said, "What's your evidence?"

The Mulla answered, "The Theory of Universal Bifurcants, A Critique of Tolerant Purity," etc.

When the Roman sputtered that he'd never heard of those titles, the Mulla responded "Of course, you haven't. Look and you will see hundreds of books you've never read."

The Roman looked and was so impressed that he conceded defeat. Since no one had understood any of this, later, after refreshments had been served, the Emperor leaned over and asked the Roman scholar the meaning of it all.

"He is a brilliant man, this Mulla," the Roman explained. "When I held up one finger, meaning that there was only one God, he held up two to say that He created heaven and earth. I held up three fingers, meaning the conception-life-death cycle of man, to which the Mulla responded by showing four fingers, indicating that the body is composed of four elements - earth, air, water, and fire."

"Well then, what about the egg and the onion," the Emperor pressed.

"The egg was the symbol of the earth (the yolk) surrounded by the heavens. The Mulla produced an onion indicating the layers of heavens about the earth. I asked him to support this claim to assign the same numbers of layers of heavens as there are layers of onion skin and he supported his claim by all those learned books of which I alas am ignorant. Your Mulla is a very learned man indeed." The dejected Roman then departed.

The Emperor next asked the Mulla about the debate. He replied, "It was easy, Your Majesty. When he lifted a finger of defiance to me, I held up two, meaning I'd poke both his eyes out. When he held up three fingers indicating, I'm sure, that he'd deliver three kicks, I returned his threat by threatening four kicks. His whole palm, of course, meant a slap in my face, to which I responded with my clenched fist. Seeing I was furious, he began to be friendly and offered me an egg so I offered him my onion."

[7]The connection I am making here between coherence and verification(ism) is not the connection made by the `logical positivists', viz. the meaning of a term is its method of verification. (A number of authors have suggested the sort of non-positivist verification(ism) that I am considering in the text. Cf. Waismann (1965), Dummett (1978) and Luntley (1982).)

CHAPTER NINE

COHERENCE AND REFERENCE

In this final chapter I want to expand on the conception of coherence explored in the previous chapter by considering a view of reference based on such a conception (and which is committed to such a conception). This view of reference is, following Kripke, suggested as a `better picture´ and is only in its conceptual nascency. This view of reference, which I see as a genuine alternative to the received theories, is not, again following Kripke, a theory of reference in the sense of offering a set of necessary and sufficient conditions for successful reference to occur. I do not, however, see this as a weakness of the view, because I think that reference is too complex a process to be capturable by the elaboration of necessary and sufficient conditions. Furthermore, I don´t even see the establishment of a synchronically-determined, permanent set of necessary and sufficient conditions as a desideratum (as I think my stand against essentialism, correspondence and realism suggest). In presenting this view of reference, I want to first argue that the view of reference which the later Wittgenstein proposes is very much in line with the sort of view I am proposing. I want to show how and why Wittgenstein´s conception of reference is not the same as

Searle's (contra Kripke) and not the same as Kripke's (contra Cappio (1981)).[1] I will then remark on what I take Wittgenstein's view of reference to be. Finally, I will suggest what I see as the strengths and weaknesses of his view of reference, indicate why I think it is a more plausible view than the received theories of reference, and, in conclusion, argue for the (initial) plausibility and superiority of my alternative view of reference. First, then, I want to turn to Wittgenstein and show that he does not endorse a cluster theory of reference.

Kripke (1980) claims that a good example of the cluster theory of reference is found in Wittgenstein (1953). Kripke quotes part of paragraph 79 as introducing the idea of family resemblances (which is incorrect; family resemblances are introduced in paragraph 67). In addition, says Kripke, this paragraph is a forceful statement of the view that "the reference of a name is determined not by a single description but by some cluster or family" (1980,p.31) and is a good example of the view that "what we really associate" with a name is a family or cluster of descriptions. Kripke quotes the following part of paragraph 79:

> Consider this example. If one says `Moses did not exist', this may mean various things. It may mean: the Israelites did not have a single leader when they withdrew from Egypt-or: their leader was not called Moses-or: there cannot have been anyone who accomplished all that the Bible relates of Moses-...But when I make a statement about Moses,-am I always ready to substitute some one of those descriptions for `Moses'? I shall perhaps say: by `Moses' I understand the man who did what the

> Bible relates of Moses, or at any rate a good deal of it. But how much? Have I decided how much must be proved false for me to give up my proposition as false? Has the name `Moses´ got a fixed and unequivocal use for me in all possible cases?[2]

Now, there is an important jump made here which is a clear misunderstanding of Wittgenstein´s view. Kripke doesn´t say much more directly related to this misunderstanding, but I think it is fair to believe that he is guilty on this point. Swartz (1977), however, explicitly commits the error, viz. taking Wittgenstein to be saying that we only associate names with a family of descriptions; taking Wittgenstein to be saying that words have meaning only in virtue of family resemblances. Says Swartz:

> According to one contemporary version [of a descriptionist theory of reference] there is <u>not</u> a <u>conjunction</u> of properties associated with each term, but there <u>is</u> a <u>cluster</u> of properties associated with it. For example, it is held that we cannot define `game´ by a conjunction of properties such as having a winner and a loser, being entertaining, involving the gaining and losing of points, because some perfectly acceptable games lack some of these features. According to the cluster theory, something is a game because it has enough features from a cluster of properties like these. A cluster theorist would claim that there need not be any property in the cluster that is sufficient for the application of the term, but he nevertheless holds that the cluster taken as a whole determines the extension of the term. Wittgenstein´s position that there are only family resemblances among the individuals in the extension of many ordinary terms can be construed as a version of the cluster theory. (1977, p.15).

True, Wittgenstein´s position could be construed as a version of the cluster theory, but only on a misunderstanding and false representation of his position.

Wittgenstein's point in introducing the notion of family resemblances is to show that it is a mistake to insist that because we use the same word to refer to different objects that there must be some essential property - some common and peculiar characteristic(s) - of those objects in virtue of which we use the same word for them all.[3] This is not to say that words have only family resemblances; some words may well have a single (conjunction of) description(s) giving the meaning of the word, e.g. `bachelor' (`adult human male who has never been married') or `mule' (`offspring of a female horse and a male donkey'). Instead, it is to say that many words do not have a single (conjunction of) description(s) giving the meaning of the word.

Furthermore, if indeed the cluster theory "holds that the cluster taken as a whole determines the extension" of a term, it is obvious that this is not Wittgenstein's view. The major reason for this is that Wittgenstein denies that it makes any sense to even speak of "the cluster taken as a whole." This is because words (at least some words) are not "closed by a frontier" (1953, #68); they have, in Waismann's (1965) terminology, an `open texture'.[4]

This notion of `open texture' is important and needs to be fleshed out before expounding on Wittgenstein's view of reference. In paragraph 68 of (1953) Wittgenstein says:

> "All right: the concept of number is defined for you as the logical sum of these individual interrelated concepts: cardinal numbers, rational numbers, real numbers, etc.; and in the same way the concept of a game as the logical sum of a corresponding set of sub-concepts."[5] -It need not be so. For I <u>can</u> give the

> concept `number´ rigid limits in this way, that is, use the word "number" for a rigidly limited concept, but I can also use it so that the extension of the concept is not closed by a frontier. (1953, #68).

What Wittgenstein is getting at is, on the one hand, the projectibility of words (not Goodman´s sense of projectibility); that is, the propriety and legitimacy of using a word or term in a novel manner or context (e.g. `feed´ in `feed the parking meter´); and, on the other hand, the lack of distinct boundaries or a clear extension of a word or term (e.g. `game´).

Waismann, who coined the term `open texture´, states that open texture may mean that we could get acquainted with some totally new experience or that some new discovery could be made which affects our interpretation(s) of certain facts and events. He offers the following example:

> The notion of gold seems to be defined with absolute precision, say by the spectrum of gold with its characteristic lines. Now what would you say if a substance was discovered that looked like gold, satisfied all the chemical tests for gold, whilst it emitted a new sort of radiation? `But such things do not happen.´ Quite so, but they might happen, and that is enough to show that we can never exclude altogether the possibility of some unforeseen situation arising in which we shall have to modify our definition. Try as we may, no concept is limited in such a way that there is no reason for any doubt. We introduce a concept and limit it in some directions; for instance, we define gold in contrast to some other metals such as alloys. This suffices for our present needs, and we do not probe any farther. We tend to overlook the fact that there are always other directions in which the concept has not been defined. And if we did, we could easily imagine conditions which would necessitate new limitations. In short, it is not possible to define a concept like gold with absolute precision, i.e. in such a way that every nook and cranny is blocked against entry of doubt. That is

> what is meant by the open texture of a concept. (1965, p.126).

Now, the point here is this: when it comes to giving an account of (most) words (and names), we are not able to state a set of necessary and sufficient conditions for the applicability of those words. Specifically, for names, we are not able - contrary to both the cluster theory and the causal theory - to establish such conditions prior to and independent of the use of names in language games (and, finally, in our forms of life). We cannot say `ahead of time´ as it were what conditions must be met for a name to refer to an object and for what, in general, naming is. A mistake of the cluster theory is the attempt to establish these conditions through the notion of a disjunctive set of descriptions associated with a name; a mistake of the causal theory is the attempt to establish these conditions through the notion of a causal chain connecting an utterance of a name with an original baptismal act in which an object is referred to by the (use of the) name. This aspect of open texture is, for both Wittgenstein and Waismann, an empirical fact about our use of language (and names); we do in fact often use names without any fixed meaning.[6]

This leads to a question which was touched upon earlier in laying out Searle´s cluster theory, and Wittgenstein´s answer to it which not only differentiates his view from Searle´s, but also shows its greater plausibility over Searle´s. The question was: does at least one member of the disjunctive set of descriptions associated with a name need

to be true for reference to successfully occur? Searle's answer seemed to be `yes'. Wittgenstein's `answer' is that the question is a wrong-headed one; it displays a misunderstanding of the relationship between names and descriptions. For Wittgenstein, names can be used as abbreviations for descriptions, although in such cases the name would not function as a name, but as a description. The mistake which Searle (in his theory) and Kripke (in his criticisms) make is to think that names are synonymous with (one or more) descriptions. For Wittgenstein, names can be explained by descriptions, but they are in no way synonymous with them (cf. (1953, ##43-45)). For example, suppose Jones says something about N and Smith wants to know who N is, to whom reference is being made). Jones remarks that N is the man at the bus stop over there. Now this description does not define `N' in any sense - the description is not a synonym and no necessary and sufficient conditions of reference are laid down - rather it helps to identify N for Smith by providing instructions (by giving an explanation in a sense) of finding out who N is. If the explanation fails - if Smith cannot pick out N - another description may be given (perhaps, `N has on a grey hat'). It is these sorts of considerations that Wittgenstein has in mind when he speaks of various descriptions that we could use to give the meaning of `Moses' and that we use a name without any fixed meaning. Although descriptions explain (and perhaps, in some cases, make possible) the use of names, the meanings

(or uses) of the descriptions are not identical with the meaning (or uses) of the names.[7]

The above remarks sound quite similar to what Kripke has to say about descriptions as reference fixers and indeed it is similar. However, Kripke's account of descriptions as reference fixers seems to miss Wittgenstein's main point, viz. the open texture of names. Kripke says:

> Now another view might be that even though the description in some sense doesn't give the meaning of the name, it is what determines its reference and although the phrase `Walter Scott' isn't synonymous with `the man such that such and such and such and such', or even maybe with the family (if something can be synonymous with a family), the family or the single description is what is used to determine to whom some-one is referring when he says `Walter Scott'. ...some of the attractiveness of the [descriptionist] theory is lost if it isn't supposed to give the meaning of the name; for some of the solutions of problems that I've just mentioned will not be right, or at least won't clearly be right, if the description doesn't give the meaning of the name. ...If, on the other hand, `Moses' is not synonymous with any description, then even if its reference is in some sense determined by a description, statements containing the name cannot in general be analyzed by replacing the name by a description, though they may be materially equivalent to statements containing a description. (1980, pp.32-33).

Attractiveness notwithstanding, one of Wittgenstein's concerns in talking about open texture is precisely that it's mistaken to think that a name can in general be analyzed by replacing it with a description (cf. (1953, #63). An identifying description helps to pick out the referent of a name for a given speaker or listener in a given context; in a different situation with a different speaker or listener the identifying description might not

succeed in picking out the referent, whereas another description might. Nor is it Wittgenstein's view that offering descriptions as identifying descriptions rather than as synonyms of names is intended to replace talk of meaning of names. That is, to replace talk of descriptions as synonyms of names with talk of descriptions as reference fixers (i.e. identifying descriptions) is not to abandon talk of meanings for Wittgenstein. Again, Kripke seems to be assuming that for names to have meanings, they must have fixed meanings, which, as noted above in the discussion of open texture, Wittgenstein denies.

Besides the error of supposing that names are synonymous with their identifying descriptions, philosophers have committed the error of supposing that they are able to uniquely pick out objects prior to or separate from some use of identifying descriptions. This, for Wittgenstein, is a mistaken picture of how reference (and language) works. As he says:

> For naming and describing do not stand on the same level: naming is a preparation for description. Naming is so far not a move in the language-game- any more than putting a piece in its place on the board is a move in chess. We may say: <u>nothing</u> has so far been done, when a thing has been named. It has not even <u>got</u> a name except in the language-game. (1953, #49).

A candidate which is commonly offered in support of our ability to uniquely pick out objects prior to or separate from some use of identifying descriptions is ostension. However, as shown in chapter five, ostension only works within agreed-upon contexts (i.e. in language-games).

Rather than the (objective, realist) identity of an object giving us the use (referent) of a name, the converse is the case; it is the use of a name which gives the identity of an object. This points to the important social nature of names and naming, and it also points to the epistemic primacy of naming. This epistemic primacy becomes apparent with the elaboration of the issue of the place of evidence in reference. Consider this situation: Jones is an incoming student in the first term of graduate study in philosophy. The faculty which Jones has so far encountered at Philosophy University have all been bearded and elderly. Jones decides to enroll in a course taught by Noseworthy. That is, Jones enrolls in a course, the description of which in the college catalog states is taught by Noseworthy. At the first class meeting a clean-shaven, young-looking person enters and begins lecturing. During the course of the lecture Jones overhears several students remark that they think the lecturer is a graduate assistant (being so young-looking). Later that day Jones sees the same lecturer sitting in an office which Jones knows is not the office of any other faculty member. The following day Jones passes the same office and sees the same lecturer there. As Jones continues down the hall, Jones meets Smith, who asks, "Have you seen Noseworthy around today?" Jones replies, "He's in his office right now, down the hall." Now, in this case Jones successfully refers; indeed Noseworthy is the clean-shaven, young-looking lecturer. Here is what I find important about

this example: the causal theorist will claim that Jones referred to Noseworthy in virtue of the causal chain connecting Noseworthy and Jones's contact with the name `Noseworthy' which Jones read on the course description in the college catalog. But what seems involved in this case is the relevance of the evidence Jones had which led Jones to infer that this lecturer was in fact Noseworthy. It seems much more likely in this case than in many others which the causal theorists cite that evidence plays a genuine role in Jones being able to hook up a name with an object. On the relation between naming and evidence, Steinman (1982) has argued that the causal chain connecting an object and an utterance of its name is not necessary and suggested a spectrum of cases that involve the relationship between naming and evidence. Steinman offers the following example to support his position. Suppose Bob is at a party and sees the wife of his friend Dave. During their conversation Dave's wife informs Bob that Dave names all of his pets `Sal'. She also tells Bob that, Dave being a one-pet man, there is never any confusion as to what he is referring to when he utters the name `Sal'. At any time Dave has at most one pet and that pet's name is `Sal'. Several days later Bob comes over to Dave's house and Dave shows Bob his new pet goldfish. Upon seeing the fish for the first time, and not having heard Dave mention the pet's name, Bob remarks, "Sal sure is a fine specimen." In this case, Bob's utterance of the name is not causally connected

to the object, even though he successfully referred to the object. The evidence that Bob had about the name of the object (e.g. Dave is a one-pet man; Dave names all his pets `Sal´; Dave´s wife is known to be a reliable, truthful person, etc.) is what made reference possible.

Back to the Noseworthy example (in which evidence played a part for reference to have occurred), the point is not to deny that a causal chain existed between Noseworthy and Jones´s utterance of `Noseworthy´. The presence of a causal chain connecting utterances of a name with the referent of the name does not seem to be a point of contention between the causal theorists and Wittgenstein; both would admit that a causal chain exists between an object and the name-utterance(s), at least in many cases. Where the conflict of views occurs (at least in part) is in the referring status granted to descriptions. (Of course, this is only symptomatic of deeper differences. For example, as Putnam (1981) argued, causal chains cannot be established or determined without reference having already taken place.) Quite simply, for Wittgenstein, there is no essence to naming. He directly speaks to this in paragraphs 37 and 38 of (1953):

> What is the relation between a name and the thing named?...This relation may...consist, among many other things, in the fact that hearing the name calls before our mind the picture of what is named; and it also consists, among other things, in the name´s being written on the thing named or being pronounced when the thing is pointed at. ...we call very different things `names´; the word `name´ is used to characterize many different kinds of use of a word, related to one another in

> many different ways. ...Naming [under Russell's account] appears as a queer connection of a word with an object.-And you really get such a queer connexion when the philosopher tries to bring out the relation between name and thing by staring at an object in front of him and repeating a name or even the word `this' innumerable times. ...And here we may indeed fancy naming to be some remarkable act of mind, as it were a baptism of an object. (1953, ##37-38).

Tied in with this belief that there is no essence to naming is the main point of difference between Wittgenstein's view and the causalist view. The difference is found in paragraph 87 of (1953):

> Suppose I give this explanation: "I take `Moses' to mean the man, if there was such a man, who led the Israelites out of Egypt, whatever he was called then and whatever he may or may not have done besides."-But similar doubts [about meaning and reference] to those about `Moses' are possible about the words of this explanation (what are you calling `Egypt', whom the `Israelites' etc.?). Nor would these questions come to an end when we got down to words like `red', `dark', `sweet'.-"But then how does an explanation help me to understand if after all it is not the final one? In that case the explanation is never completed; so I still don't understand what he means, and never shall!"
> -As though an explanation as it were hung in the air unless supported by another one. Whereas an explanation may indeed rest on another one that has been given, but none stands in need of another-unless we require it to prevent a misunderstanding. One might say: an explanation serves to remove or to avert a misunderstanding-one, that is, that would occur but for the explanation; not every one that I can imagine. ...The sign-post is in order-if, under normal circumstances, it fulfils its purpose. (1953, #87).

An immediate point of these remarks in relation to the problems of naming and reference is this: in some cases (at least) the presence of a causal chain is unnecessary as an explanation of how reference occurs. Causal chains may well

serve as descriptions of how reference occurs (i.e. Searle's `external observer') and they may even serve (in some cases) as explanations of how reference occurs, but if what we want is an understanding of how we refer by naming, then causal chains are not needed. Wittgenstein's reasoning is that we require explanations in order to solve problems of failure to understand. Explanations serve to remove or avert misunderstandings, but these explanations are needed only in such a capacity; if there were no misunderstanding that needed removing or averting, the explanation would not be needed. When it comes to naming, the problems that actually occur, such as knowing the correct name of someone or when to use one name rather than another when referring, are problems such that any misunderstandings that might arise (at least, generally) can be handled without having to bring in talk of underlying causal chains. This is not to deny that causal chains may be invoked in some explanations, it is only to claim that in general such explanations are not necessary to provide an understanding of how naming and referring occur and are successful.

Now, I think that the view of reference that Wittgenstein has suggested is a more plausible view than the views proposed by the received theories of reference, not only because it gives a more plausible account of ordinary cases of referring, but also because it both captures and reflects the coherence, verificationist perspective outlined in the previous chapter (and does not suffer the flaws of

commitments to privacy, essentialism, correspondence and realism). I will now try to support this claim by considering the sorts of cases which Searle and Kripke discuss as well as suggest other cases which indicate the greater plausibility of the Wittgensteinian view.

Examples

As noted in chapter one, Kripke thinks that Searle's theory of reference is "wrong from the fundamentals" to the extent that the theory holds that "we give ourselves some properties which somehow qualitatively uniquely pick out an object and determine our reference in that manner" (1980, pp.93-94). Kripke and others have offered counter-examples which are intended to show that a speaker can refer to an object even though the speaker has no descriptions associated with a name which are true of the object to which reference is made. (Kripke's Columbus, Einstein and Gödel examples, for instance, are meant to show this.) Searle responded that the Kripkesque counter-examples fail to do their job because it's not the case that a speaker has no descriptions associated with a name which are true of the object to which reference is made and yet reference succeeds. For Searle, the particular descriptions which we have associated with a name and which are not true of the object to which reference is made are usually descriptions which reflect the secondary aspect under which reference is made.[8] As such, reference can succeed for Searle because the speaker can always fall back on another description to

secure reference. Whichever description succeeds in securing reference reflects the primary aspect under which reference is made and can do so even if the description is not true of the object to which reference is made. For Searle, then, Kripke's counter-examples fail to show his theory to be mistaken. However, as I claimed in chapter one, given Searle's theory it does seem (at least for the primary aspect under which reference is made) that at least one member of the disjunctive set of descriptions associated with a name must be true of an object for the name to refer to that object. Whether or not Kripke's particular counter-examples work, one might construct a counter-example that does work. I then offered two such counter-examples (at least two plausible candidates as counter-examples). The first was the case of my neighbor complaining to her friend, "My weird neighbor kept me up last night yelling something about `that damn turtle' or `Gödel' or something like that. I had to pound on the wall to get him to quiet down." Has my neighbor, I asked, referred to Gödel? Given Searle's theory as presented in his (1969), his rule 5 (and perhaps rule 6) is violated, so reference, it would seem, is not made. [Rule 5: S intends that the utterance of R will pick out or identify X to H. Rule 6: S intends that the utterance of R will identify X to H by means of H's recognition of S's intention to identify X and he (S) intends this recognition to be achieved by means of H's knowledge of the rules governing R and his (H's) awareness

of (context) C.] In this case, my neighbor does not seem to intend to pick out any object by her utterance of `Gödel´, so reference is not made.[9] (It should be noted, however, that in (1983) Searle backs away from this view and suggests that in some cases - perhaps this case - reference can be made "where one is almost totally dependent on other people´s prior usage to secure reference" (1983, p.261). To the extent that Searle moves to this sort of view, he is, I think, abandoning his (1969) theory and adopting the sort of view which I have identified with Wittgenstein.)

Now, again, has my neighbor referred to Gödel? For Searle, no, because she didn´t intend to pick out Gödel. This answer seems (to me, anyway) intuitively plausible. However, it runs into the sort of problems that Kripke and others point out, viz. that names are historical, public entities and whether or not one intends to pick out Gödel by (the use of) `Gödel´, that name (usually) picks out Gödel. For Wittgenstein, as opposed to Searle, my neighbor has not referred because she is not in the appropriate context to refer to Gödel in this case. That is, her lack of intention to refer to Gödel might play a part in the lack of reference, but it certainly is not sufficient. Rather, she was not engaged in the language game of referring. Here, the use/mention distinction (cf. note 9 above) would be relevant because it indicates a difference of language games in which the speaker is engaged.

A second counter-example to Searle´s position which I

proposed for consideration was the following: Suppose my obsession with Gödel has gotten to the point that I often blurt out my innermost thoughts concerning him. On one such occasion, while standing at a bus stop, I mutter, "Ha! Gödel couldn´t hold a candle to Cantor. Now, there was a man!" Upon hearing this, a bemused and intrigued stranger next to me asks, "Who is Gödel?" Has the stranger, I asked, referred to Gödel? Searle, I suggested, would say no, because it´s not clear that the stranger intended to pick out a particular object (Gödel). Indeed, the very question, `Who is Gödel?´ shows that the speaker has no clear referent associated with the name, and in fact the question can be paraphrased with no loss of meaning with a different question, `Whom are you talking about?´ or `Whom are you referring to when you say "Gödel"?´ (Again, in his (1983) Searle seems to back away somewhat from his earlier theory.) For Wittgenstein, it seems to me, reference could easily occur in this case. Within the language game of questioning, one could refer. Indeed, given the public nature of names, the stranger could refer to Gödel, but only because the publically- and historically-determined nature of names allows a speaker to use names which have an already publically-determined sense (referent). This, of course, is much of the motivation for Kripke´s critique of Searle, and to this extent he is, I think, right. For other various reasons (which I have indicated in the previous several chapters), however, Kripke´s alternative account is

mistaken. Not only can Wittgenstein's view account for reference being made in this case by the stranger to Gödel, but his view can also account for reference being made to abstract and future objects, something troublesome for the causal theory. Because Wittgenstein's view does not require the intentionality of the speaker for reference to succeed, it is not constrained in the ways that Searle's theory is. In addition, because Wittgenstein's view does not require appropriate causal chains for reference to succeed, it is not constrained in the ways that Kripke's theory is. For example, Wittgenstein's view can account in similar ways for successful reference in the following three cases: `Who is Gödel?', `Who is God?' and `What is infinity?', each spoken by a speaker who has never heard the name (or concept) in question. Reference can occur in each of these cases (and the myriad of cases of which they are instances) because the `objects' referred to and the names (or, in the case of infinity, the term) used are shared, public, communal conventions. A speaker who has no (true) descriptions associated with Gödel (other than, perhaps, `the name I just heard') can nonetheless refer to Gödel by (the use of) `Gödel', not in virtue of some appropriate causal connection, but because the speaker is a member of a linguistic community and understands (at least tacitly) the language game (and the practices and conventions upon which it rests) of naming and of referring. Not only is the ability to name and refer something that is social, but so,

too, is the object to which reference is made (as I argued in chapter five). Likewise, reference to God is made in the same way and for the same reasons. The name/term `God´ functions in certain ways in certain communities and given certain practices. As such, speakers can refer to God because who and what God is is determined socially. The name/term has a `life´ independent of any given speaker, but not independent of all speakers. `God´, like `Gödel´ is a public, social, historical entity (as is, in one sense, God) and reference can be made only because of this public, social, historical nature. (This holds also for infinity and other abstract or `future´ entities, i.e. the sorts of `objects´ which the causal theory of reference has difficulty.)[10] The Wittgensteinian view of reference, then, I have argued is not troubled by the sorts of counter-examples which plague the cluster and causal theories of reference. However, I think that the strengths of this alternative view can be seen more sharply by considering two current issues which implicitly involve questions of reference. These examples also show the close conceptual connection between issues of reference and issues of individuation and similarity. As I will argue, these `real-life´ examples will demonstrate the strengths of this alternative view of reference (and its underlying epistemic commitments). The first example is that of the current discussion of the hypothesis that our sun is part of a binary solar system. The second example is that of the

controversy concerning the discovery of the cause of AIDS (acquired immune deficiency syndrome).[11]

In 1977, Alfred Fischer and Michael Arthur (both of Princeton University) suggested that a periodic interval of about 32 million years elapsed between mass extinctions of life on the earth. Six years later, David Raup and John Sepkoski (both of the University of Chicago), after examining paleontological records dating back 250 million years, argued that preliminary evidence, backed by statistical tests, suggested a regular, periodic extinction of life on the earth every 26 million years. Included in the series of extinctions is the extinction of the dinosaurs, approximately 65 million years ago (which marks the end of the Cretaceous period and the beginning of the Tertiary period). Numerous proposed explanations for the extinction of dinosaurs have been advanced, including oncoming, major climatic changes and early mammalian predation on dinosaur eggs. However, more than 75 percent of species on the earth were extinguished, so predation seems unlikely to be an adequate explanation. Climatic change also seems questionable, given recent views that dinosaurs (at least some species of dinosaurs) were not reptilian, but were in fact warm-blooded. In any case, a recent account for the mass extinction at the end of the Cretaceous period is that a comet struck the earth, creating a prolonged period of darkness over the earth's surface, killing off basic photosynthetic elements of the food chain,

and finally resulting in the extinction of most life forms on the planet. An important piece of evidence offered in support of this account is the widespread and high levels of iridium found in the clay separating the Cretaceous and Tertiary geologic periods. Iridium is rare on the earth's surface, but more abundant on extra-terrestrial bodies. Also, the distribution of this iridium is such that the difference in levels across the globe suggest a highest concentration near Iceland, the levels gradually decreasing away from this localization.

In early 1984 scientists met at a conference to discuss the work of Raup and Sepkoski and the proposed cataclysmic, extra-terrestrial account of periodic mass extinctions. Among the scientists at the conference were Marc Davis, Richard Muller (of the University of California, Berkeley) and Piet Hut (of the Institute for Advanced Studies) who suggested that our sun is part of a binary system. (Others, e.g. Daniel Whitmire and Albert Jackson IV, have also suggested that our sun is part of a binary system.) The companion star to our sun, it is claimed, is the cause of these noted periodic mass extinctions of terrestrial life forms. Davis, Muller and Hut have dubbed the companion star `Nemesis' and have suggested that its orbit brings it close enough to our solar system to encounter (or at least disturb) the Oort Cloud, a halo of comets and matter on the fringes of our solar system. The presence of Nemesis creates cometary havoc and results in comet showers within

our solar system, with enough comets striking the earth so as to cause prolonged darkness and intense volcanic activity on the earth's surface. The orbit of Nemesis, it is claimed, brings it within the crucial proximity every 26 million years.

Whether or not this hypothesis concerning the existence of a companion star is true is not important for the concerns here, except in a sense which I will explicate. What is important is the reference to Nemesis and the use of `Nemesis' as a name. There are several points which I think emerge and are highlighted by this example. First, the introduction of the name `Nemesis' was done within the larger context of proposing a scientific hypothesis. Naming Nemesis (and naming in general) was not done privately, external to any other epistemic concerns, but as part of a larger practice and activity. Prior to the particular scientific context, established by the various public publications and positions espoused, the naming of Nemesis would have made no sense. Even with questions and criticisms about the hypothesis and the existence of Nemesis, the introduction of the name `Nemesis' is plausible (including the choice of the name) and conceptually coherent. The conceivability of someone proposing to introduce the name `Nemesis' for (or, at the very least, successfully referring to) a companion star prior to the introduction of a reasonable hypothesis which could serve as a background context for this name is doubtful.

This leads to a second point which emerges from this example, viz. it is the acceptance by the community that secures reference, not any intention by a particular speaker nor any specific, appropriate causal chain. The name `Nemesis´ has, in a sense, stuck, and even critics of the hypothesis use the name. For example, in scientific newsmagazines Nemesis is often a topic worthy of report (cf. Science News 4/21/84, 9/1/84, 11/3/84, 3/16/85). Scientists, both in favor of and in opposition to the hypothesis, use the name `Nemesis´ in discussing the (de)merits of the hypothesis. The simple introduction of a name does not suffice for naming to take place. Rather the communal acceptance and adoption of a name that is introduced is necessary for naming to occur (and reference to succeed). For a speaker to intend to pick out an object by using a name (or, better, by dubbing it) does not guarantee that naming has occurred. Naming, as a social, linguistic practice, requires social, linguistic adoption. (This will be seen more readily in the discussion of the AIDS case below.)

A third point is that naming (and reference) is intimately tied up with epistemic and verificationist issues, ultimately reflecting a coherence position. Whether `Nemesis´ actually names anything, whether it continues to function as a name, is not determined by any a priori set of conditions that are met, but by the scientific community. For example, consider this scenario (which has been

proposed): Astronomers are presently attempting to infer where and how Nemesis might be found so as to test the claims of the hypothesis that such an object exists. Even if scientists can reasonably predict where they ought to be able to `find´ Nemesis, the failure to find it does not mean that Nemesis does not (did not) exist. It could have been destroyed since it last approached the Oort Cloud (approximately eleven million years ago). Or, it could have finally escaped its orbit, leaving our sun a unary sun. (Or, Nemesis might not exist and there is another explanation for the periodic mass extinctions, e.g. the wandering of our solar system throughout the Milky Way relative to other stars and systems. As the solar system enters different areas of the galaxy, in a periodic oscillatory pattern, cometary activity could result, causing mass extinctions of life on earth.) Now, suppose it were the case that Nemesis is not found and no other evidence of its existence is found. The name would, I suggest, drift out of the scientific vocabulary (except perhaps anecdotally, like Vulcan, the supposed `invisible´ planet thought to explain the perturbations of Mercury´s orbit). Without some verifiable evidence adding credence to the existence of Nemesis, `Nemesis´ will fade from serious scientific discussion and will not be seen as the name of an object at all (i.e. will not be accepted as a name). Quite simply, if there is nothing to support and substantiate the use of the name, the name will not be used; in effect, it

will not be a name. This is true even if, as proposed, Nemesis caused the periodic mass extinctions, but has now left its old orbit and is now undetectable.[12] The causal theory of reference, of course, must hold that if the appropriate causal connection `is there´, then reference is made. In this case, we might actually be referring to Nemesis by (the use of) `Nemesis´ even if, because of lack of corroborating evidence, we reject the hypothesis that Nemesis exists. On the causal theory, we might really be referring even if we aren´t aware of it and decide that we aren´t really referring. This strikes me as an obvious problem with the causal theory. (What sense would it make, I want to ask, to claim that we really did refer to Vulcan, even though we never found any evidence to suggest that we referred to Vulcan by (the use of) `Vulcan´?)

Now, this example of Nemesis nicely illustrates the strength of the alternative view of reference which I associate with Wittgenstein and for which I am arguing. The linguistic, semantic issue is not whether or not Nemesis exists, but whether or not the name `Nemesis´ is accepted and adopted by the scientific community (and later by larger, different communities). For the causal theory of reference, however, the ontic issue is significant, because causal chains can only be established to objects in the world. Names, for the causal theory, name things; if those `things´ don´t exist, then those `names´ don´t name (and aren´t really names). In this case, I want to suggest that

whether or not Nemesis exists, `Nemesis´ can still be used as a name (e.g. if the scientific community decides that Nemesis did cause the mass extinctions, but it has left its orbit, so we won´t find it). Besides, in practice right now, `Nemesis´ is functioning as a name. Not only is it not the case that ontic questions must be settled for semantic (referential) questions to be settled, it is the settling of the latter that can determine the former. The diachronic acceptance of `Nemesis´ as a name of an object ushers in the diachronic acceptance of the individuation of the object, Nemesis.

This example also exhibits the significance of coherence for reference. As noted, only with publically verifiable corroborating evidence granting support for the individuation of Nemesis will the name `Nemesis´ continue to be used and accepted. The claim that `Nemesis caused the mass extinctions on the earth´ must cohere with our accepted theories or we will not accept Nemesis as an object or `Nemesis´ as a name. If it is decided that the claim that `Nemesis caused the mass extinctions on the earth´ does not cohere with other (accepted) statements, then it will also be decided that reference was not made to Nemesis (despite any descriptions associated by a given speaker or intentions by a given speaker and despite any causal chains that might possibly be established). At present, `Nemesis´ is a theoretical term for the scientific community, and whether it remains so or becomes an observational term or is

discarded as non-referring depends finally on the coherence of the claims within the scientific community and their acceptance by the scientific community.[13] The determination of reference is made by a public, verifiable process, not by a given speaker, and, at least in this case, independent of any causal connection. The dubbing by Davis, Muller and Hut of `Nemesis´ to the (theoretical) object Nemesis did not establish any baptism or any causal chain to an object, nor did it establish a name to be used by the community. `Nemesis´ became a name, not by a dubbing (accompanied by intentions or mental representations), but by the gradual adoption by the scientific community of a linguistic label to an `object´ whose existence is in question. (This clearly relates to natural kind terms and issues of individuation and partitionings. For example, see Kuhn (1962) on the gradual acceptance of reference to oxygen. The diachronic acceptance of oxygen as a natural kind correlates with the diachronic adoption of `oxygen´ as a natural kind term. The acceptance of both was determined by the scientific community and was shaped by their coherence with the theoretical frameworks of that community. Again, this sort of claim will be illustrated perhaps more clearly in the AIDS case to be discussed below.)

This Nemesis example illustrates the socially constructed character of reference (and of individuation). The following AIDS example also illustrates this character, however, where the Nemesis case deals with a presently-

considered theoretical individual, the AIDS case deals with issues of partitionings and the establishment of causal agents (both within a social context).[14]

AIDS (acquired immune deficiency syndrome) supposedly first appeared in 1981. Efforts to uncover a (the) cause led to two independent announcements of the discovery of a (the) cause.[15] In the spring of 1983, French scientists at the Institut Pasteur announced that they had discovered the cause of AIDS. The cause, they claimed, was a virus, which they dubbed LAV (lymphadenopathy-associated virus). Several months before this announcement, scientists reported that SAIDS (simian acquired immune deficiency syndrome) appeared to be caused by a virus. SAIDS, a disease held to be similar to AIDS in symptoms and pathology, having a viral-infectious agent, led researchers to look for a similar cause for AIDS. The viral organism responsible for AIDS, according to the French researchers, was so-named because of its initial discovery in a patient with the swollen lymph glands and flu-like symptoms often associated with AIDS.

Almost a year later, in April 1984, American scientists announced that they had discovered the cause of AIDS. The virus which these scientists claimed was responsible was dubbed HTLV-3 (human lymphotrophic retrovirus-3). At the public announcement of the American researchers's discovery, it was claimed that HTLV-3 was probably the same organism as that identified as LAV, but genetic analysis and immunological comparisons would be necessary to determine

this. It was suggested that both HTLV-3 and LAV belong to a class of retroviruses which use RNA rather than DNA as their basic genetic material. The American scientists dubbed the culprit organism as HTLV-3 because they linked it to another retrovirus called HTLV-1, which is associated with a rare form of human leukemia (found primarily in Japan, the Caribbean and parts of Africa). According to public reports, the French researchers were not sure whether or not the virus they construed as the causal agent was a member of the HTLV family. When the American announcement was made, however, the French researchers claimed: "We are very happy that another team has found the same thing" (Science News 125(1984): 285). "From its effect, and the descriptions of it, it can only be the same one [virus]." (Ibid.) LAV, they said, was only a "provisional name."

Several interesting `events´ occurred over the course of the year following the American announcement. The first is that the scientific community continued to use both `LAV´ and `HTLV-3´ to designate the virus which they investigated and studied. For example, in a noted science newsmagazine (Science News), fourteen articles appeared on AIDS between July 7, 1984 and April 27, 1985. In five of those articles, both `LAV´ and `HTLV-3´ were cited as two names for the same virus (probably). In four articles HTLV-3 was cited as the viral cause and LAV was not mentioned. In no articles was LAV cited as the cause and HTLV-3 not mentioned. (In five articles neither was cited as the cause, but in those

articles, the cause of AIDS was not discussed.)[16] No official announcement has yet been issued that LAV and HTLV-3 either are or are not the same virus.

Related to the debate over whether or not LAV and HTLV-3 are the same virus, at least one researcher, Jay Levy (of the University of California at San Francisco) even opted for a third name, ARV (AIDS-associated retrovirus). Levy and collegues isolated a virus from an AIDS victim and cloned the entire genome while trying to develop a screening test (and, potentially, a vaccine). Reportedly, pending a comparison of LAV and HTLV-3, Levy called his isolates ARV.

Another interesting `event´ associated with the naming and referring issues here is that, following the identification of HTLV-3 (and of LAV), researchers have gone back to look for a (the) historical cause for AIDS. In March 1985, it was reported that AIDS "or something very like it" existed in Africa in 1972. (As mentioned above, originally AIDS was said to have emerged in 1981.) Scientists thawed frozen blood samples from Uganda taken in 1972 and 1973. They looked for the presence of antibodies to HTLV-3, found it in two thirds of the samples, suggesting that "the virus detected may have been a predecessor of HTLV-3 or HTLV-3 itself but existing in a population acclimated to its presence." (Science News 127 (1985): 173). What is significant about this is that, given the apparent social identification of HTLV-3 as the cause of AIDS, scientists then looked for earlier instances of AIDS, and,

in effect, located (or determined) a different historical nexus for the disease. (On April 20, 1985 it was reported in Science News that Max Essex (of Harvard University) has suggested that African green monkeys may harbor AIDS virus and could be the source of AIDS in humans. If this is so, the historical nexus could again be altered.)

Now, with regard to the issues of naming and referring, what is the significance of the AIDS example? The significance, I think, is that, as with the Nemesis example, the Wittgensteinian view of naming and of reference gives a better account of this case than does the Searlean or Kripkean view. Several features are worthy of attention here. First, as with the Nemesis case, the identification of a natural kind (the cause of AIDS), was (is) a social, diachronic process. Although two groups of researchers dubbed some virus type as the cause of the disease, the name that actually gets accepted and used is one that depends not on the intentions of any particular speaker (a la Searle), but on the agreement of the social community. Nor is the name established (a la Kripke) by an appropriate causal chain, initiated by a speaker's perceptually causal hook-up to an object (conjoined with the appropriate mental representation) because much of the debate itself concerns just what the causal hook-up connects. Determining what object is there to be causally hooked up with is what's at issue. This issue will be settled over time by the scientific community and in so doing, the scientific

community will establish the proper reference, along with the appropriate partitioning (into, minimally, the cause of AIDS versus not the cause of AIDS). The variability over the past couple of years about the appropriate name for the virus indicates that the naming has not been settled and that reference has not been determined because it hasn't been determined what is being referred to. This seems to suggest the Wittgensteinian view of reference as being more appropriate here than the cluster or causal theories. As the Wittgensteinian view contends, reference seems to be a matter of communal agreement, not simply in the `linear' manner associated with the causal theory, but in the language-community consensus sense. That is, the communal agreement is not just that once reference has been established (a la Kripke), then future reference is parasitic upon that initial naming, but that communal agreement in the sense of the community as a whole (or, given Putnam's notion of linguistic division of labor, the relevant portion of the community) over time comes to adopt a name for reference, as well as an object (and kind) for reference.

Conclusion

I want to conclude by restating two points mentioned earlier in this chapter. First, I have presented this Wittgensteinian view of reference not as a fully worked-out theory, but as a tentative, better, view of reference. It captures, I think, the strengths of both the cluster and the

causal theories, without making the (questionable) underlying commitments of each. Furthermore, as I have tried to show, it can account for the cases which were troublesome for each of the received theories as well as the two `real-life´ scientific cases just presented. The second, final, point I want to make is that, at least in one sense, this Wittgensteinian view does not deny that there might be cases which both the cluster and causal theories can account for very well. It could well be that in some contexts reference can be determined by descriptions associated with a name or by a causal chain. However, to the extent that these accounts work, I want to hold, they work because they can be subsumed within the larger Wittgensteinian view and work only when their underlying commitments are abandoned. To the extent that a cluster or causal theory of reference can be separated from privacy and realism (along with corollary notions of essentialism and correspondence), such a theory might be appropriate and adequate in given situations. As general theories of reference, however, they are inferior to the Wittgensteinian (constructivist) view.

NOTES

[1]Much of the following discussion of Wittgenstein is based on my (1978).

[2]The entire paragraph 79 reads as follows:
Consider this example. If one says `Moses did not exist´, this may mean various things. It may mean: the Israelites did not have a single leader when they withdrew from Egypt-or: their leader was not called Moses-or: there cannot have been anyone who accomplished all that the Bible relates of Moses-or: etc. etc.-We may say, following Russell: the name `Moses´ can be defined by means of various descriptions. For example, as `the man who led the Israelites through the wilderness´, `the man who lived at that time and place and was then called "Moses"´, `the man who as a child was taken out of the Nile by Pharaoh´s daughter´ and so on. And according as we assume one definition or another the proposition `Moses did not exist´ acquires a different sense, and so does every other proposition about Moses.-And if we are told `N did not exist´, we do ask: `What do you mean? Do you want to say.....or.....etc.?´

But when I make a statement about Moses,-am I always ready to substitute some one of these descriptions for `Moses´? I shall perhaps say: By `Moses´ I understand the man who did what the Bible relates of Moses, or at any rate a good deal of it. But how much? Have I decided how much must be proved false for me to give up my proposition as false? Has the name `Moses´ got a fixed and unequivocal use for me in all possible cases?-Is it not the case that I have, so to speak, a whole series of props in readiness, and am ready to lean on one if another should be taken from under me and vice versa?-Consider another case. When I say `N is dead´, then something like the following may hold for the meaning of the name `N´: I believe that a human being has lived, whom I (1) have seen in such-and-such places, who (2) looked like this (pictures), (3) has done such-and-such things, and (4) bore the name `N´ in social life.-Asked what I understand by `N´, I should enumerate all or some of these points, and different ones on different occasions. So my definition of `N´ would perhaps be `the man of whom all this is true´.-But if some point now proves false?-Shall I be prepared to declare the proposition `N is dead´ false-even if it is only something which strikes me as incidental that has turned out false? But where are the bounds of the incidental?-If I had given a definition of the

> name in such a case, I should now be ready to alter it.
>
> And this can be expressed like this: I use the name `N´ without a fixed meaning. (But that detracts as little from it usefulness, as it detracts from that of a table that it stands on four legs instead of three and so sometimes wobbles.)
>
> Should it be said that I am using a word whose meaning I don´t know, and so am talking nonsense?-Say what you choose, so long as it does not prevent you from seeing the facts. (And when you see them there is a good deal that you will not say.)
>
> (The fluctuation of scientific definitions: what to-day counts as an observed concomitant of a phenomenon will to-morrow be used to define it.)

[3]Wittgenstein´s immediate concern here is to show that it is mistaken to suppose that there is a general form of propositions, something common to all that we call language. That is, it is mistaken to object to the notion of language games because no essence of a language game is (can be) given. That issue is beyond the present concerns.

[4]I will say more about the notion of `open texture´, but I want at this point to remark that this is precisely one of the issues of greatest divergence between Wittgenstein´s and Searle´s views. Where Searle admits the sense and usefulness of speaking of the logical sum or inclusive disjunctive set of descriptions associated with a name, Wittgenstein does not. Rather Wittgenstein speaks of the lack of any fixed meaning of a name. (Cf. note 2 above.) It is worth noting that Linsky (1977) also recognizes this difference between Wittgenstein and Searle, as do Ishiguro (1974), Carney (1979) and Cappio (1981). Says Linsky:

> [Wittgenstein and Searle] say that the main difficulty with [Russell´s] theory is that it ignores a certain looseness in our use of proper names. Proper names, they say, are not, in general, associated by their users with unique descriptions as Russell´s theory requires. Here Searle and Wittgenstein part company. Searle thinks that in place of the unique description required by Russell there are a cluster of descriptions which the users of a name associate with it. He thinks that it is a necessary truth that some "sufficiently large" (but indefinite) number of the open sentences of which these descriptions are built are uniquely satisfied by the referent of the name. Wittgenstein´s correction to Russell´s theory is rather different. He thinks proper names are

characteristically used without any fixed meaning; there is neither a unique description nor a cluster of them which fixes the sense of our names. We use these names without a fixed sense. (1977, p.93).

[5]This sentence is presented by `the antagonist´, i.e. someone objecting to Wittgenstein´s claims. (Note the similarity between this sentence and Searle´s disjunctive set of descriptions associated with a name.)

[6]This insistence that we often use names without fixed meaning is tied in with Wittgenstein´s notions of language games and rule-following, and his rejection of the distinction between the contexts of discovery and contexts of justification. This is not to say that no names have fixed meanings. We can (and do) fix the meaning of some names (or, at least, some words, such as `bachelor´ and `mule´). But it is mistaken to think that we always can (or do). What is also important in connection with this fixing of meaning when it does occur is that it is done as part of an activity (within a language game), for certain purposes. (In this regard, see Linsky (1977, pp.104-105).) In other words, a word is a name only when it is used as one (as opposed to being used as, say, a demonstrative), and to be used as a name means that we employ it in calling for, asking about, saying something about, etc., the object being referred to. Cf. Canfield (1977), where he considers Kripke´s case in which a speaker mistakenly says that Jones is raking leaves (when actually Smith is):

> "Is it correct to say that either he´d attribute the predicate [`is raking leaves´] to Jones or to Smith depending on how we take the question. How we take the question is something we have to learn to do. We can take the question in a way that will give only the results that Donnellan intends. But then the right thing to say is that it is the series of examples he gives that teaches us how to take the question. In Wittgensteinian terms Donnellan teaches us a certain language game, one of establishing the speech act referent by answering a certain question in a certain way." (1977, pp.108-109). (Also, see Ishiguro (1969), p.25.)

[7]Wittgenstein´s remarks here sound very much like Searle´s distinction between the primary aspect and secondary aspect under which reference is made. However, Wittgenstein´s view is not the same as Searle´s because, as argued in chapter four, Searle´s view is consistent with private language, whereas Wittgenstein´s view is not. As Ishiguro (1969) says:

> Throughout his later works, Wittgenstein discusses the temptation to look in the wrong

> direction for a mark which would tell us why a word refers to a certain object. We look for mental processes that go on as we utter the words rather than the rule governing the use of the words that we can come to grasp. (1969, p.23).

As noted previously, Searle is guilty of looking for mental processes (brain states) which would tell us why a word refers to a certain object. His primary aspect-secondary aspect distinction captures in form Wittgenstein's point, but within the context of Searle's theory, only the form (and not the substance) is captured.

[8]There are cases, for Searle, in which the descriptions which reflect the primary aspect under which reference is made can nonetheless not be true of the object to which reference is made (e.g. referring to a usurper as `the king').

[9]One might argue that in this case the name `Gödel' is not used, but is only mentioned. Consequently, reference isn't made for the causal theory, either, even though there is a causal connection between my neighbor's utterance of the name and the object. However, it is not clear to me that such a move could legitimately be made by a causal theorist. Why, for example, should reference not be made (to Gödel) in the following case: "`Gödel' is the name of the man who proved the incompleteness of arithmetic."? If the use/mention distinction is relevant for the causal theory in giving an account for reference, then it is not at all clear to me how the causal theory can retain its uniqueness as a theory of reference. The only account I can foresee would be such that the theory would have to take into account the pragmatic (Wittgensteinian) aspects of reference. A full fleshing out of my claims here is beyond the present point. (The point of raising this issue at all is that given a `pure' causal theory of reference, it seems that my neighbor has referred to Gödel, since one could demonstrate an appropriate causal connection.)

[10]Clearly, one feature of this Wittgensteinian view of reference is that reference to fictional entities is not an important issue, nor are negative existential statements a problem. These sorts of cases can be treated in the same way as `normal' cases because the ontological status of the `objects' in question is not crucial. Cf. Elgin (1983) for a sustained treatment of reference to various sorts of `objects' and various kinds of modes of reference (such as reference to fictional entities and metaphorical reference).

[11]Cf. Fleck (1979) for an account of the socially-determined `fact' of the cause and nature of syphilis.

[12]This last sentence, of course, is the sort of claim that I find somewhat unpalatable (i.e. Nemesis really caused

those extinctions, but we'll never find it because it has left its orbit). Without some sort of verifiable evidence to support this claim, it serves no function in our set of truths and fails to cohere with other (accepted) statements. It ranks, with me, with statements about the Absolute or ghosts, which are there, but we'll never detect them.

[13]I am certainly not intending to suggest by this claim that I hold to a theoretical/observational distinction (at least a sharp one). The distinction, I think, is untenable. I am simply suggesting that the status of the claim that `Nemesis caused the mass extinctions on the earth' changes based on the criterion of coherence.

[14]Another example of the social nature of naming, at least within a scientific context, is illustrated by the following item from a science newsmagazine:

> Physicists are currently pondering the `Proposal for the Systematic Naming of Hadrons', yet another attempt to agree on names for the growing population of subatomic particles. It's a tedious chore, and it has physicists recalling the lament attributed to Enrico Fermi in the early 1950s - back when particles were just starting to proliferate. "If I could remember all those names," said Fermi, "I would have been a botanist." (Science 85 6(1985): 9).

[15]As will be noted below, one point at issue is whether or not AIDS has one cause or more than one, hence my use of `a (the)'.

[16]Of course, the fact that Science News is an American publication could explain why HTLV-3, and not LAV, was more often cited as the cause. I am unacquainted with the French literature and, consequently, am hesitant to claim too much here about the sociology of naming in the scientific community. However, my main points about the social and diachronic character of naming and referring does not rest on the French reports.

CONCLUSION

A journey of a thousand miles begins with a single step.
(Lao Tzu, Tao Te Ching)

CONCLUSION

The overriding thesis of this long (and, for the reader, torturous) book has been the proposal of constructivism as an alternative to what is today called realism. In a statement, I have been arguing for a view which asserts that reality is socially constructed. I have argued, through the vehicle of the issue of reference, that epistemic issues underlie ontic and semantic issues, that theories of reference rest on (and are committed to) conceptions of individuation and similarity, and that these conceptions of individuation and similarity are dependent upon our theories, languages and cognitive interests. This thesis is by no means new; parts of it (at least) are found in Kuhn, Goodman, Wittgenstein, Quine, Marx, the American pragmatists and many others. What I think is new is the sustained analysis of a constructivist position through an analysis of the specific contemporary theories of reference and their epistemic commitments. (Even parts of this are preceded by the work of Goodman, Elgin and Putnam.) Specifically, I have argued that the received theories of reference - the cluster theory associated with Searle and the causal theory associated with Kripke and Devitt - are more alike than different and that they share the same fundamental epistemic commitments. They share particular

conceptions of individuation and similarity, and subsequent conceptions of (and commitments to) privacy, essentialism, correspondence and realism. Each of these conceptions and commitments, I have argued, is mistaken. Consequently, the theories of reference reflected by these conceptions and commitments are mistaken as well. I then suggested the outline and beginnings of an alternative view of reference, a view based on constructivist conceptions and commitments. Throughout this study, then, I have suggested not only that theories of reference must be looked at in light of their underlying commitments, but also (the other side of the coin) that particular epistemic commitments and positions are borne out in, for example, theories or views of reference. To fully understand a theory or view of reference, then, we must look at its commitments and underlying conceptions; to fully understand a particular epistemic position, we must look at its manifestations, e.g. in the view(s) of reference that follow from it. Constructivism as an overarching and guiding position (along with its corollaries, coherence and verificationism) is, I think, the most plausible epistemic view, though one which needs to be explored and fleshed out in much greater detail. This book, I hope, is one small contribution to that effort.

BIBLIOGRAPHY



BIBLIOGRAPHY

Armstrong, G.E.M. Intention. Oxford: Oxford University Press, 1957.

Barnes, Barry. T.S. Kuhn and Social Science. New York: Columbia University Press, 1982.

Blanshard, Brand. The Nature of Thought. London: Allen and Unwin, 1939.

Boer, Steven E. "On Searle's Analysis of Reference." Analysis 32 (1972): 154-159.

Boersema, David B. "Wittgenstein on Names." (Unpublished manuscript.)

Brody, Baruch A. Identity and Essence. Princeton: Princeton University Press, 1980.

Canfield, John V. "Donnellan's Theory of Names." Dialogue 16 (1977): 104-127.

Cappio, James. "Wittgenstein on Proper Names." Philosophical Studies 39 (1981): 87-105.

Carney, James D. "Wittgenstein's Theory of Names." Australasian Journal of Philosophy 57 (1979): 59-68.

Cartwright, Nancy. How the Laws of Physics Lie. Oxford: Claredon Press, 1983.

Cartwright, Richard. "Some Remarks on Essentialism." Journal of Philosophy 65 (1968): 615-627.

Davidson, Donald. "The Individuation of Events." In Essays in Honor of Carl G. Hempel. Edited by Nicholas Rescher. Dordrecht: D. Reidel, 1970. Pages 216-234.

Devitt, Michael. Designation. New York: Columbia University Press, 1981.

_____. Realism and Truth. Princeton: Princeton University Press, 1984.

Dewey, John. Reconstruction in Philosophy. New York: Mentor, 1950.

Donnellan, Keith. "Reference and Definite Descriptions." Philosophical Review 75 (1966): 281-304.

_____. "Proper Names and Identifying Descriptions." In Semantics of Natural Language. Edited by Donald Davidson and Gilbert Harman.

Dordrecht: D. Reidel, 1972. Pages 356-379.

Donnellan, Keith. "Speaking of Nothing." Philosophical Review 83 (1974): 3-31.

Duhem, Pierre. The Aim and Structure of Physical Theory. New York: Atheneum, 1962.

Dummett, Michael. Truth and Other Enigmas. Cambridge: Harvard University Press, 1978.

Dupre, J. "Natural Kinds and Biological Taxa." Philosophical Review 90 (1981): 66-90.

Elgin, Catherine Z. "Lawlikeness and the End of Science." Philosophy of Science 47 (1980): 56-68.

_____. With Reference to Reference. Indianapolis: Hackett Publishing Co., 1983.

Evans, Gareth. "The Causal Theory of Names." Aristotelian Society Supplementary Volume 47 (1973): 187-208.

Fleck, Ludwig. Genesis and Development of a Scientific Fact. Chicago: University of Chicago Press, 1979.

Flew, Anthony. A Dictionary of Philosophy. New York: St. Martin's Press, 1979.

Ghiselin, M. "A Radical Solution to the Species Problem." Systematic Zoology 23 (1974): 536-544.

Gill, J.H. "Wittgenstein's Concept of Truth." International Philosophical Quarterly 6 (1966): 71-80.

Goodman, Nelson. Fact, Fiction, and Forecast (2nd ed.). Indianapolis: Bobbs-Merrill, 1965.

_____. Problems and Projects. Indianapolis: Bobbs-Merrill, 1972.

_____. Ways of Worldmaking. Indianapolis: Hackett Publishing Co., 1978.

Gould, Stephan J. The Mismeasure of Man. New York: W.W. Norton & Co., 1981.

_____. Hen's Teeth and Horse's Toes. New York: W.W. Norton & Co., 1983.

Haack, Susan. Philosophy of Logics. Cambridge: Cambridge University Press, 1978.

Hacking, Ian. Representing and Intervening. Cambridge: Cambridge University Press, 1983.

Hanna, Joseph F. "Explanation, Prediction, Description, and Information Theory." Synthese 20 (1969): 308-344.

_____. "On Transmitted Information as a Measure of Explanatory Power." Philosophy of Science 45 (1978): 531-562.

Harding, Sandra. "The Norms of Social Inquiry and Masculine Experience." In PSA 1980, Volume 2. Edited by P.D. Asquith and R.N. Giere. East Lansing: Philosophy of Science Association, 1981. Pages 305-324.

Hesse, Mary. The Structure of Scientific Inference. London: Macmillan, 1974.

_____. Revolutions and Reconstructions in the Philosophy of Science. Brighton: Harvester Press, 1980.

_____. "Comment on Kuhn's `Commensurability, Comparability, Communicability." In PSA 1982, Volume 2. Edited by P.D. Asquith and T. Nickles. East Lansing: Philosophy of Science Association, 1983. Pages 704-711.

Hirsch, Eli. "Essence and Identity." In Identity and Individuation. Edited by Milton K. Munitz. New York: New York University Press, 1971. Pages 31-49.

Holsinger, Kent E. "The Nature of Biological Species." Philosophy of Science 51 (1984): 293-307.

Hull, David. "The Effect of Essentialism on Taxonomy." British Journal for the Philosophy of Science 15 (1965): 314-326.

_____. "A Matter of Individuality." Philosophy of Science 45 (1978): 335-360.

Ingber, Warren. "The Descriptional View of Referring: Its Problems and Prospects." Journal of Philosophy 76 (1979): 725-738.

Ishiguro, Hide. "Use and Reference of Names." In Studies in the Philosophy of Wittgenstein. Edited by Peter Winch. London: Routledge & Kegan Paul, 1969. Pages 20-50.

_____. "Review of Michael Dummett's Frege: Philosophy of Language." Philosophy 49 (1974): 438-442.

James, William. The Writings of William James. Edited by John J. McDermott. Chicago: University of Chicago Press, 1977.

Jones, O.R. (ed.) The Private Language Argument. New York: Macmillan, 1971.

Kitcher, Philip. "Species." Philosophy of Science 51 (1984): 308-333.

Kitts, D.B. and Kitts, D.J. "Biological Species as Natural Kinds." Philosophy of Science 46 (1979): 613-622.

Kripke, Saul. "Outline of a Theory of Truth." Journal of Philosophy 72 (1975): 690-716.

_____. "Speaker's Reference and Semantic Reference." Midwest Studies in Philosophy 2 (1977): 255-276.

_____. Naming and Necessity. Cambridge: Harvard University Press, 1980.

_____. Wittgenstein on Rules and Private Language. Cambridge: Harvard University Press, 1982.

Kuhn, Thomas S. The Structure of Scientific Revolutions. Chicago: University of Chicago Press, 1962.

_____. "Second Thoughts on Paradigms." In The Structure of Scientific Theories. Edited by Frederick Suppe. Urbana: University of Illinois Press, 1977. Pages 459-482.

_____. "Commensurability, Comparability, Communicability." In PSA 1982, Volume 2. Edited by P.D. Asquith and T. Nickles. East Lansing: Philosophy of Science Association, 1983. Pages 669-688.

Leonard, Henry and Goodman, Nelson. "The Calculus of Individuals." Journal of Symbolic Logic 5 (1940): 44-55.

Leplin, Jarrett (ed.) Scientific Realism. Berkeley: University of California Press, 1984.

Linsky, Leonard. Names and Descriptions. Chicago: University of Chicago Press, 1977.

Luntley, Michael. "Verification, Perception, and Theoretical Entities." Philosophical Quarterly 32 (1982): 245-261.

Mayr, Ernst. Systematics and the Origin of Species. New York: Columbia University Press, 1942.

McKinsey, Michael. "Kripke's Objections to Description Theories of Names." Canadian Journal of Philosophy 8 (1978): 485-497.

McMullin, Ernan. "Values in Science." In PSA 1982, Volume 2. Edited by P.D. Asquith and T. Nickles. East Lansing: Philosophy of Science Association, 1983. Pages 3-28.

Nagel, Ernest. The Structure of Science. New York: Hartcourt, Brace & World, 1961.

Paulos, John Allen. I Think, Therefore I Laugh. New York: Columbia University Press, 1985.

Peirce, Charles S. Philosophical Writings of Peirce. Edited by Justus Buchler. New York: Dover, 1955.

Plantinga, Alvin. The Nature of Necessity. Oxford: Oxford University Press, 1974.

Popper, Karl R. Conjectures and Refutations: The Growth of Scientific Knowledge. New York: Harper & Row, 1963.

Putnam, Hilary. Reason, Truth and History. Cambridge: Cambridge University Press, 1981.

Quine, W.V.O. From A Logical Point of View. Cambridge: Harvard University Press, 1953.

_____. Word and Object. Cambridge: M.I.T. Press, 1960.

_____. Ontological Relativity and Other Essays. New York: Columbia University Press, 1969.

_____. Methods of Logic (3rd ed.) New York: Holt, Rinehart and Winston, 1972.

_____. "Whither Physical Objects?" In Essays in Memory of Imre Lakatos. Edited by R.S. Cohen et al. Dordrecht: D. Reidel, 1976. Pages 497-504.

_____. "States of Mind." Journal of Philosophy 82 (1985): 5-8.

Rescher, Nicholas. The Coherence Theory of Truth. Oxford: Oxford University Press, 1973.

Rhees, Rush. "Can There Be A Private Language?" Proceedings of the Aristotelian Society Supplement 28

(1954): 77-94.

Rothbart, Daniel. "The Semantics of Metaphor and the Structure of Science." Philosophy of Science 51 (1984): 595-615.

Russell, Bertrand. "On Denoting." Mind 14 (1905): 479-493.

_____. The Problems of Philosophy. New York: Henry Holt & Co., 1912.

Salmon, Nathan U. Reference and Essence. Princeton: Princeton University Press, 1981.

Schwarz, Stephan P. (ed.) Naming, Necessity, and Natural Kinds. Ithaca: Cornell University Press, 1977.

Searle, John R. "Proper Names." Mind 67 (1958): 166-173.

_____. Speech Acts. Cambridge: Cambridge University Press, 1969.

_____. Expression and Meaning. Cambridge: Cambridge University Press, 1979.

_____. Intentionality. Cambridge: Cambridge University Press, 1983.

_____. Minds, Brains and Science. Cambridge: Harvard University Press, 1984.

Slote, Michael A. Metaphysics and Essence. New York: New York University Press, 1975.

Steinman, Robert. "Naming and Evidence." Philosophical Studies 41 (1982): 179-192.

Strawson, P.F. Individuals. London: Methuen & Co., 1959.

Tarski, Alfred. "The Semantic Conception of Truth." Philosophy and Phenomenological Research 4 (1944): 341-375.

_____. "The Concept of Truth in Formalized Languages." In Logic, Semantics, Metamathematics. Oxford: Clarendon Press, 1956. Pages 152-278.

Thomson, Sir George. "Some Thoughts on the Scientific Method." In Boston Studies in the Philosophy of Science, Volume 2: In Honor of Philipp Frank. Edited by Robert S. Cohen and Marx W. Wartofsky. New York: Humanities Press, 1965. Pages 81-92.

Waismann, Freidrich. "Verifiability." In Logic and

Language. Edited by A.G.N. Flew. Garden City: Doubleday Anchor Books, 1965. Pages 122-151.

White, Alan R. Truth. Garden City: Doubleday Anchor Books, 1970.

Whorf, Benjamin Lee. Language, Thought & Reality. Cambridge: M.I.T. Press, 1956.

Wittgenstein, Ludwig. Tractatus Logico-Philosophicus. London: Routledge & Kegan Paul, 1922.

_____. Philosophical Investigations. New York: Macmillan, 1953.

_____. The Blue and Brown Books. Oxford: Blackwell, 1958.

_____. Zettel. Oxford: Blackwell, 1967.

_____. "Notes for Lectures on `Private Experiences´ and `Sense Data´." Philosophical Review 77 (1968): 275-320.

_____. On Certainty. Oxford: Blackwell, 1969.

_____. Wittgenstein´s Lectures, Cambridge, 1932-1935. Edited by Alice Ambrose. Totawa: Rowman and Littlefield, 1979.

MICHIGAN STATE UNIV. LIBRARIES
31293007971017