THE CONCEPT OF METAPSYCHOLOGY IN PSYCHOANALYSIS: A CRITIQUE AND LOGICAL EXAMINATION OF CONTEMPORARY CRITICISMS

BY

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INTRODUCTION

There is a growing body of literature whose main thrust is an evaluation and criticism of psychoanalytic metapsychology. The roots of this line of thought can perhaps be traced back to Kubie's (1947) criticism of the psychic energy model. Since then numerous other authors have criticized various aspects of what they have labeled metapsychology. However, it was in a relatively recent paper that Klein (1970) made the first appeal for a wholesale rejection of metapsychology. More recently, Gill (1973), Holt (1974), and Schafer (1975), among others, have joined in this appeal.

While criticism of psychoanalysis is nothing new, these recent criticisms differ from previous ones in two respects. First and most importantly, there is a logical difference. Earlier criticisms of psychoanalysis ranged quite broadly from those which rejected it totally to those which were directed at some small part of it. In singling out metapsychology for criticism and rejection, these later criticisms implicitly make the assertion that a certain class of statements is logically separable from the rest of psychoanalytic theory. Further, it is asserted that this class of statements which contain certain assumptions previously considered basic to psychoanalytic theory can either simply

be eliminated or drastically changed, and that the resultant theory will still be psychoanalytic. This assertion takes several forms, the most common being that psychoanalytic theory is made up of two logically separable theories, the metapsychological and the clinical, and that the clinical is in some way more suited to the subject matter. Another form states that the language of psychoanalysis implies untenable metapsychological assumptions and therefore should be drastically changed.

The second difference is sociological. Earlier critics of psychoanalysis can be placed into two broad groupings. One group is made up of those who were outside of the psychoanalytic community. The second group is made up of those who started within the Freudian psychoanalytic community and, especially if their criticisms were substantive, left, sometimes to found new schools of thought with different premises or emphases. As a result, with few exceptions (e.g. Hook, 1958), discourses between the critics and those whose work they criticized was either non-existent or rife with parochial miscommunications.

The previously mentioned contemporary critics differ in that they remain firmly identified as psychoanalysts or psychoanalytic thinkers by both themselves and by most members of the psychoanalytic community. As a result, discourse around substantive disagreements may be beginning to take place. Thus far, however, this discourse has remained imprecise and arguments and counterarguments are

not often compelling. Little consensus has been reached and few if any new avenues for research or clinical exploration have been opened. It will be proposed that this is for two reasons. First, metapsychology is either undefined, idiosyncratically defined, or imprecisely defined. Second, criticisms of metapsychology often have questionable logical justifications. This thesis is therefore proposed as an examination of the concept of metapsychology and a review and critique of recent criticisms of the concept.

The conceptual underpinnings of this thesis are contemporary analysis of the logical structure of scientific theories and scientific explanations as presented most notably by Nagel (1961). Chapter I will present an overview of these analyses in order to lay the foundation for subsequent discussions. While some criticism of these analyses may be offered, the basic viewpoint will remain "orthodox" or "natural scientific."[1] An underlying assumption of this thesis is that the development of psychoanalysis as a science has hardly begun, and that an abandonment of the attempt to continue this development is premature. This thesis will attempt to support the contention that such an attempt to develop psychoanalysis as a natural

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[1]Natural science as used here does not refer to a system of explanation that is necessarily mechanistic or determinist nor to a system that necessarily models itself on any existing system of explanation. To be considered natural scientific a system of explanation need only 1) refer to data which is acquired by the use of what is commonly called "the scientific method;" 2) contain only explanations which are either logical, or if nonlogical, testable by the scientific method.

science is logically possible in refutation of those such as Gill (1976), Klein (1976), and Schafer (1976), who contend otherwise.

In Chapter I theories will be described along two dimensions: the components of a theory and the levels of a theory. The distinction between law-like statements and theoretical statements and the related distinction between theoretical terms and observational terms will be stressed, as will the distinction between theory, model, and metaphor.

Chapter II will review the uses of the term metapsychology. Freud's changing uses of the term will be examined. The uses of the term will be traced through the ego psychologists to contemporary psychoanalytic writers. It will be shown that the term has never been consistently applied to a logically distinct set of theoretical constructs. Rather, it will be demonstrated that the term has been inconsistently applied to logically different classes of statements.

With this as a background, Chapter III will review and evaluate some of the recent criticisms of metapsychology. The criticisms to be discussed share in common the fact that they have gained wide enough acceptance or at least mention to warrant critical examination. Additionally, most of the criticisms to be examined are, at least in the way they are most often presented, extra-theoretical. That is, these criticisms frequently take some vantage point external to the functioning of the theory and may be based on particular a priori notions about how a psychoanalytic theory should be

constructed or what it should eventually look like. One criticism that does not appear to be extra-theoretical will also be discussed. This criticism is that metapsychology is anthropomorphic. This thesis will attempt to demonstrate that the determination of whether or not a concept or formulation is anthropomorphic is more complex than is generally acknowledged.

Chapter IV will discuss some of the issues relevant to the position that psychoanalysis cannot or should not be a natural science. The position to be examined holds that explanations of human behavior differ in important ways from explanations in the natural sciences. In particular it is sometimes contended that whereas natural sciences invoke causes, mechanisms, functions, and the like as explanatory concepts; a science of human behavior should invoke meanings, reasons, intentions, etc. Chapter IV will examine this position in light of the traditional distinction between teleological and causal explanations. It will be proposed that there are no evident a priori grounds for deciding that psychoanalytic explanations should be either exclusively teleological or causal.

CHAPTER I

THE COMPONENTS AND LEVELS OF SCIENTIFIC THEORY

This chapter will describe some of the more important features of scientific theories. This will lay the groundwork for discussions of metapsychology in the subsequent chapters. The account of theories to be presented can be found in numerous sources in the literature of the philosophy of science and has been labeled the "standard" or "orthodox" view (Feigl, 1970). This appears to roughly correspond to what Rubinstein (1976) and other psychoanalytic writers have called "natural science" theories. Feigl (1970) gives a brief historical overview of the literature in the philosophy of science that deals with the logical structure of scientific theories. He states that though there are variations, developments, modifications, and terminological diversities, the analyses are essentially similar. Though there are several available accounts of the standard view of scientific theories, the one to be presented in this chapter will rely primarily on E. Nagel's (1961) exposition since his appears in many ways to be the most comprehensive. It should be noted that critiques of this "standard" notion of theories have recently been offered by

Feyerabend (1970) and others. However, a brief review of these critiques later in this chapter will show that they do not substantively affect the thesis to be presented.

Some Preliminary Definitions of Theories

A theory is a comprehensive system of explanation. The aim of a theory is the unification of knowledge; i.e. the comprehending of a maximum number of observations in terms of a minimum number of theoretical concepts and assumptions (Feigl, 1970). Hempel writes:

> Theories, it is generally agreed, are the keys to scientific understanding of empirical phenomenon: To claim that a given kind of phenomenon is scientifically understood is tantamount to saying that science can offer a satisfactory theoretical account of it.
>
> Theories are normally constructed only when prior research in a given field has yielded a body of knowledge that includes empirical generalizations or putative laws concerning the phenonmena under study. A theory then aims at providing a deeper understanding by construing those phenomena as manifestations of certain underlying processes governed by laws which account for the uniformities previously studied, and which as a rule, yield corrections and refinements of the putative laws by means of which these uniformities have been previously characterized. (1970, p. 142)

These preliminary definitions are incomplete. A full characterization will involve a description along two dimensions: the components of a theory, and the levels of a theory.

One caveat before proceeding. In order to understand the aim of the logico-deductive account of scientific theories, it is essential to distinguish it from the

historical, sociological, or psychological studies of scientific theories and their development. Much misunderstanding has resulted from failures to make this distinction. This distinction has been labeled by Reichenbach (in Feigl, 1970) as the distinction between "analysis in the context of discovery," and "analysis in the context of justification." It is one thing to retrace the historical origins, the psychological genesis and development, the social-political-economic conditions for the acceptance or rejection of scientific theories; and it is quite another thing to provide a logical reconstruction of the conceptual structure and the procedures for testing scientific theories. Though there is much "analysis in the context of discovery" that is pertinent to psychoanalysis (e.g. Ricoeur, 1970; Wollheim, 1974; Polanyi, 1958; Ellenberger, 1970), it falls beyond the scope of this thesis.

The Components of a Theory

Nagel (1961) outlines three components of a theory.

1. An abstract calculus that is the logical skeleton of the explanatory system and that "implicitly defines" the basic notions of the system.
2. A set of rules that in effect assigns an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment. These have variously been called rules of correspondence, coordinating definitions, operational definitions, semantical rules, epistemic correlations, or rules of interpretation.

3. A model or interpretation for the abstract calculus which supplies some flesh for the skeletal structure in terms of more or less familiar visualizable materials.

What follows will be a fuller description of each of these components. I will then suggest three additions to this schematitization.

The Abstract Calculus

The nonlogical terms of a theory can (at least in principle) be dissociated from the concepts and images that normally accompany them by ignoring the latter, so that attention is directed exclusively to the logical relation in which the terms stand to one another. When this is done, and when a theory is carefully codified so that it acquires the form of a deductive system (again in principle realizable), the fundamental assumptions of a theory formulate nothing but an abstract relational structure.

In this perspective, the fundamental assumptions of a theory constitute a set of abstract or uninterpreted postulates, whose constituent nonlogical terms have no meanings other than those accruing to them by virtue of their place in the postulates, so that the basic terms of the theory are implicitly defined by the postulates of the theory. Insofar as this is true, the postulates assert nothing and are statement forms rather than statement.

An example will help clarify what is meant by implicit definition (Nagel 1961, p. 91). In Euclidian geometry the postulates of the system are frequently stated with the

expressions 'point', 'line', 'plane', 'lies between', 'congruent with', and others as the basic terms. Although these are also words of everyday experience that characterize familiar spatial relations, these characterizations are irrelevant to the deductive elaboration of the postulates.

It is probably true that these familiar spatial configurations played a role in the development of the theory, and presently constitute the model (see below) for the theory. However, answers to questions like "What is a line?" can only be of the form, "A line is that which satisfies the conditions stated in the postulates."

Rules of Correspondence

Rules of correspondence indicate how a theory's implicitly defined terms are related to experimental laws. Without them a theory cannot be significantly affirmed and is scientifically useless. However, the ways in which theoretical notions are related to observational procedures are often quite complex and there appears to be no single schema to adequately represent them.

Correspondence rules do not provide explicit definitions of theoretical terms. They are neither unique nor precise and are changeable. When a term is said to be explicitly defined it can always be removed from any context in which it occurs, since it can be replaced by the defining expression without altering the sense of context. Correspondence rules do not do this. For example, the theoretical expression of "rise in temperature" corresponds to the volume expansion of mercury

in a mercury thermometer. However, it would be absurd to replace "rise in temperature" with "volume expansion of mercury" in such statements as, "The rise in temperature of a gas is proportional to the mean kinetic energy of the molecules which constitute the gas." It is also important to note that a theoretical expression does not explicitly define the observations which correspond to it.

These are perhaps two reasons why correspondence rules do not provide explicit definitions for theoretical concepts in terms of experimental procedures. First, when a theory is formulated in terms of a model, the language used in stating the model usually has connotations that the language of experimental procedure does not possess. Second, theoretical notions are frequently coordinated by rules of correspondence to more than one experimental procedure or concept which may have no obvious relationship to each other. To maintain that one of these procedures or concepts explicitly defines the theoretical notion would be to maintain that that procedure or concept stands in the same relationship to the other procedures or concepts as does the theoretical notion. For the same reason, it would be difficult to maintain that a theoretical concept is explicitly defined by more than one experimental concept or procedure. The fact that theoretical notions are only implicitly defined by the postulates of the theory, allows for a correspondence to a logically unlimited number of experimental concepts or procedures.

It is also very important to note that though there are rules of correspondence for some of the terms and notions

of a theory, there are terms and notions which are not linked to observations or experimental procedures. Yet these in combination with other terms may have correspondence rules attached to them. For example, there is no correspondence rule for the instantaneous velocity of a single gas molecule, yet there is such a rule for the theoretical notion of the average kinetic energy of gas molecules.

This characteristic appears to be factually true of all theories, though no convincing proof of its necessity has been offered. Quine's (1953) contention that science is empirically underdetermined is perhaps a relevant argument, but again not a convincing proof. In any case this characteristic appears to allow theories the flexibility to extend into areas very different than those for which they were originally devised (Nagel 1961, p. 102).

Models

A theory is usually not presented as an abstract set of postulates, augmented by an appropriate number of correspondence rules for the uninterpreted nonlogical terms implicitly defined by the postulates. It is generally presented by way of a model which sets forth the theory in terms of relatively familiar notions, so that instead of being statement forms, the postulates of the theory appear to be statements at least part of whose content can be visually imagined.

Models can be extraordinarily valuable in making a theory easier to grasp and in suggesting fresh lines of inquiry that might never present themselves when a theory is presented in

abstract form. However, it is important to note that models are in no way substitutes for rules of correspondence though they may be suggestive of them. Though a theory is presented in terms of a model, it does not follow automatically that the theory is linked to experimental concepts and observational procedures. Models vary greatly in their ability to suggest such links.

Maxwell's notion of analogy (Nagel 1961, p. 109) is very similar to this conception of model. Maxwell describes a "physical analogy" as "that partial similarity between the laws of one science and those of another which make each of them illustrate the other." An analogy may hold only partially or poorly yet nevertheless be useful as an "artificial method" for the solution of a certain class of problems.

Nagel suggests distinguishing between substantive and formal analogies. A substantive analogy is one in which a system of elements possessing certain already familiar properties, assumed to be related in known ways as a stated set of laws for the system, is taken as a model for the construction of a theory for some second system. For example, in the kinetic theory of gases certain assumptions are patterned after known laws of motion for macroscopic spheres, such as billard balls. In a formal analogy the system that serves as a model is some familiar structure of abstract relations. There are numerous examples in physics where the mathematical formalism of one theory served as a model for another. For example, in Newtonian mechanics the mass of a

body was thought to be independent of its velocity and equivalent to the ratio of the force acting on the body and its acceleration (m = f/a). However, in relativity theory, the mass of a body varies with its velocity and a relativistic mass (rm) must be computed. Yet the relationship between relativistic mass, force, and acceleration remain the same (rm = f/a). A similar formalism is involved in the application of psychophysical laws across different sensory modalities.

Models may be useful in other ways than in articulating new theories. Models may suggest new areas of inquiry or points at which rules may be introduced for establishing correlations between theoretical and experimental notions. Although a model does not by itself establish correspondence rules, it is often suggestive of them. In addition to their heuristic value, models may contribute to the achievement of more inclusive systems of explanation. A theory elucidated in terms of a familiar model may resemble in important ways the laws of theories which are assumed to hold for the model itself. As a result, the new theory can often be viewed as an extension of the older theory.[1] This relationship between theories is an important one and will be further discussed later in this chapter.

Though presenting a theory in terms of a familiar model is generally quite valuable, certain complications frequently

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[1]Some philosophers of science such as Suppe (1974), have made this a requirement. Feigl states it is a moot point.

arise. The most important of these is that an inessential feature of the model may be viewed as an essential feature of the theory. In the extreme case, the model may be confused with the theory itself. As a result investigators may be lead into unprofitable research, and discourse on pseudo-problems may distract from the significance of the theory. It is, however, impossible to tell in advance whether a model will stand in the way of the development of a theory or lead a theory into unexpected directions, since it is the slack of imprecision of the model which accounts for both alternatives. It is important to note that the same model may be both heuristically valuable and obstructive. Frequently a model starts out being invaluable to the development of a theory and ends up standing in the way of the theory's further progress.

Other Components

I would like to suggest three additions to Nagel's tripartite division. These are operational definitions, implicit metaphysics, and loose analogies. While I believe only the first is a logically necessary component of theories, the other two appear to function as aids in the application of theories.

Operational Definitions

Nagel groups together as equivalent the following terms: rules of correspondence, coordinating definitions, operational definitions, semantical rules, epistemic correlations, and rules of interpretation. These terms refer to the rules

which assign empirical content to the terms in the abstract calculus. I believe the term operational definition should be reserved for those rules which exactly specify those operations used to measure or observe that empirical content.[1] For example, if the theoretical term 'electron jump x' corresponds to the appearance of spectral line x, the operational definition would specify the procedure used to observe spectral line x. In many cases the operations may be so obvious as to not require explication. In the above example the observation of a given spectral line by a certain type of spectroscope, under certain conditions, with a certain margin of error, may be standard procedure. However, in other fields, notably psychology, standard procedures for the observation of empirical phenomena are quite lacking. Thus a theory may specify rules of correspondence and, lacking operational definition, remain untestable.

Implicit Metaphysics

This refers to the unexamined hypotheses and beliefs which may determine a scientist's choice of model or the general direction of research in a field.[2] An example is Freud's doctrine of psychic determinism. Another example is scientism such as the Helmholtz pledge (Schur, 1972;

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[1] Feigl and others have made this point.

[2] To the extent that a direction for research, a model, etc. may be logically implied by the implicit metaphysics, these implicit metaphysics may be said to play a logical role in the development of a theory.

Ellenberger, 1970). The belief that everything can be given a mechanistic explanation as well as the belief that there are certain phenomena which cannot be given a mechanistic explanation (Schafer, 1976) fall into this category. The search for structural similarities in disparate elements of reality may or may not fall into this category depending on how it is persued (von Bertalanffy, 1968; Piaget, 1965). Whether the position one takes on the mind-body issue may fall into this category or necessarily falls into this category, is unclear and will be further discussed in chapter three.

It should be clear that these implicit metaphysics are not logically necessary components of a theory though they may play a role in a theory's development. Scientists can and do share the same theory without sharing the same metaphysic. However, the failure to distinguish these unexamined hypotheses and beliefs from theory proper may lead to unprofitable metaphysical discourse in the guise of scientific disagreements. On the positive side these metaphysics can help direct a theory into areas not logically suggested by the observed phenomena or the structure of the theory.

Loose Analogies

Under this category fall metaphors and allusions. These characterize notions which flesh out a theory in an evocative sense, yet do not have the status of a model. For example, "The ego's relation to the id might be compared with

that of a rider to his horse" (Freud, 1933, p. 77). This hardly seems to be part of an equestrian model, yet it is evocative and in some sense useful. Another example is Freud's frequent use of military analogies in describing intraphysic conflict. Deciding whether a frequently used characterization is meant as a model or as a loose analogy is not always as simple as in the above examples. It is not always clear to the author who uses the characterization. Frequently it is not possible to tell if a statement is a poor model or an interesting analogy.

Levels of Theory

Laws and Theories

As a comprehensive system of explanation, a theory has the form of a number of statements. This section will be concerned with that class of statements called laws. These are statements which explain or predict observations or other laws. There are two seemingly distinct types of laws: experimental laws and theories. These two types of laws constitute the level structure of theories with which proposed level structure analyses of psychoanalytic theory will later be compared. Before proceeding with an analysis of the level structure of theories, it is first necessary to characterize these two types of laws.

In general an experimental law formulates a relation between things (or traits of things) that are observable in at least a very loose sense of the term. Laws can be vali-

dated (even if only with some "degree of probability") by controlled observations of things mentioned in the law.

A theory employs terms which may ostensibly refer to nothing observable and the assumptions cannot be confirmed by experiments or observations of the things to which the terms ostensibly refer.

It is sometimes held that the distinction between laws and theories suffers from a difficulty in defining the word observable. Some such as Feyerabend (1970) contend that there is no such thing as theory-free observation and that it is impossible to state an observation free of implicit theoretical considerations. That is, when we report observations, we report not primitive sensory images but inferences based on our implicit theories.

Nagel (1971), on the other hand, contends that while there is no inherent difference between theoretical terms and observational terms, the terms may be differentiated on the basis of the way they function in the context of scientific inquiry. He states that observational terms are commonly used to identify in perceptual experience some object of process; to categorize that object or process so identified; to describe experimental instruments and procedures; to state the outcome of overt measurements; and to codify experimentally or observationally obtained data for the purpose of testing hypotheses.

Theoretical terms, Nagel continues, play very different roles in scientific investigation. He writes:

> ...theoretical terms sometimes codify highly idealized (or "limiting") notions, such as the notion of instantaneous velocity or a point-mass, introduced to simplify intellectual constructions or to make possible the application of powerful tools of calculation to the mathematically "imperfect" materials of the natural world. ...it can be said that theoretical expressions have two major functions in scientific investigations: to prescribe how the things identified in gross experience with the help of observation terms are to be analyzed (or otherwise manipulated), if the investigations are to be successful; and to serve as links in the inferential chains that connect the instantial experimental data with the generalized as well as the instantial conclusions of inquiry. (1971, p.29-30)

The question of theory-free observation is related to the meaningfulness and logical status of such distinctions as observable-nonobservable, law-theory, and synthetic-analytic. While the logical status of these distinctions remains much debated in the literature of the philosophy of science, there is little doubt that they can be quite useful heuristically and will be retained.

To emphaszie what was said previously, perhaps the most striking feature setting off laws and theories is that each descriptive (i.e. nonlogical) term in the former, but generally not each such term in the latter, is associated with at least one overt procedure for relating that term to some observationally identifiable trait. The procedure associated with each term thus gives the term a definite, even if only partial, meaning to each term in the law. Thus an experimental law, unlike a theoretical law or a definitional statement has a definite empirical content which can be determined by applying the observational procedure connected to each descriptive term in the law. This is perhaps a more precise way of restating that each descriptive

term in a law is in some sense observable and, most importantly, that characteristics of each term included in a law can be assertained independently of the law in which they occur.

It is, in fact, frequently the case that more than one overt experimental procedure is available for applying a term in an experimental law to concrete subject matter. This is generally the case when a term enters into more than one experimental law. Often one procedure is selected as the standard for defining the term.

In contrast, the meanings of many, if not all, descriptive terms occuring in theories are not specified by such an overt experimental procedure. Theories are frequently constructed so that most theoretical terms are associated with concepts and images derived from their generating or illustrating models, but the operative meanings of most theoretical terms are defined only implicitly by the theoretical postulates into which the terms enter, or are fixed only indirectly in the light of the eventual uses to which the theory may be put. As a corollary: while laws could in principle be proposed and asserted as inductive generalizations based on relations found to hold in the observed data, this can never be the case with theories.

Laws as well can receive not only direct but indirect support. This indirect support can frequently be supplied when the experimental law is incorporated into an exclusive system of laws or into an overall theoretical framework.

Laws are then explained by a given theory and are thus incorporated into its framework. Two characteristics continue to hold for the law: 1) It retains a meaning that can be formulated independently of the theory and; 2) it is based on observational evidence that should enable the law to survive the eventual demise of the theory.

Despite what appears to be the complete absorption of an experimental law into a theory, so that the special technical language of the theory may even be used in stating the law, the law must be intelligible (and must be capable of being established) without reference to the meanings associated with it because of its being explained by the theory. Were this not the case for the laws purportedly explained by the theory, there would be nothing for the theory to explain.

At the very least, on pain of fatal circularity, even if the terms of a law have meanings derived in part from some theory, the terms must have some determinate sense stateable independently of the particular theory which explains the law. In Chapter II an illustration of this circularity in the psychoanalytic theory of symptom formation will be offered.

Theoretical terms, on the other hand, cannot be understood apart from the particular theory that implicitly defines them. This follows from the circumstance that, although the theoretical terms are not assigned a unique set of determinate senses by the postulates of the theory, the

permissible senses are limited to those satisfying the structure of interrelationships into which the postulates place the term. Accordingly, when the postulates are changed, the meanings of the theoretical terms change, even if the same linguistic expressions are used.

This is a particular problem in psychoanalysis in which some of the postulates have undergone drastic change thus changing the meanings of theoretical terms contained within them. For instance, as the theory of anxiety has changed, the theoretical term anxiety has changed in meaning, even though the experiental term anxiety has not. This last is a peculiar problem: the same linguistic expression being both a high level theoretical term and at the same time an experiental one. In psychoanalysis, the terms pain and tension are particularly confusing examples of this. In addition linguistic expressions (words) are often borrowed from previously established sciences and placed in a new framework which importantly changes their meaning. This change in meaning is usually not acknowledged. Probably by using the same word one hopes to establish ties with the older science.

Another distinction between laws and theories is that an experimental law can without exception be formulated as a single statement (in fact, all laws or law-like statements can be placed in the form of generalized conditionals, in the simplest case represented by the schema, "For any x, if x is A then x is B'); while a theory is without exception a system of several related statements. A consequence of this

characteristic of theories combined with the fact that theoretical terms are not tied to definite experimental procedures is that theories have a greater generality and relatively more explanatory power than laws. A logical derivation of this consequence appears to be lacking.[1] However, the argument for this derivation generally takes the following form.

Each descriptive term in a law is tied to a definite observational procedure which gives the law a definite empirical content. The law can, in principle, be stated as an inductive generalization of that empirical content. Therefore, what is explained by that law must in some definite way be related to the procedures which give the law empirical content. Thus while experimental laws can be used to explain and predict the occurrence of individual events as well as to explain other experimental laws, that which is explained is in certain relatively definable respects similar, and constitutes a fairly definable class of things.

Theoretical terms, on the other hand, are not tied in any definite way to observational procedure, nor can they be explicitly defined by rules of correspondence. Theoretical terms are given implicit definition by their place in the theory to which they belong. Theories have a complex symbolic structure which provides 'slack' for extending into diverse areas. In fact, one of the most important functions of a theory is to exhibit systematic connections between

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[1] A logical derivation of this may be impossible in principle. This would depend on one's metaphysical viewpoint concerning the cognitive states of theories.

experimental laws about seemingly qualitatively different subject matters. A theory may function poorly in other respects (e.g. prediction) and still be significant for suggesting relationships between subject matters previously thought to be unrelated.

A Schematization of the Level Structure of Theories

The orthodox view of the component structure of scientific theories, and the distinction between experimental laws and theoretical laws, imply a level structure conceptualization of scientific theories and scientific explanation. This has been formulated explicitly by Feigl (1949, 1970). The account which follows refers to Figure 1 which attempts to combine Nagel's component structure with Feigl's level structure.

On level I is placed the explicandum, i.e. the individual fact or event to be explained. These are operationally defined. Logically, only singular sentences or conjunctions of them are placed on this level. Immediately above these are experimental laws (EL I). These are used to explain the events of level I. As previously stated, these laws have the form, 'For any x, if x is A then x is B' or 'if A then B'. The explanations of level I subsume the individual fact or event under a class specified in the experimental law. The experimental laws of EL I may be explained by other experimental laws (EL II) which subsume them under a larger class of laws. At some, as of now, seemingly unspecifiable distance

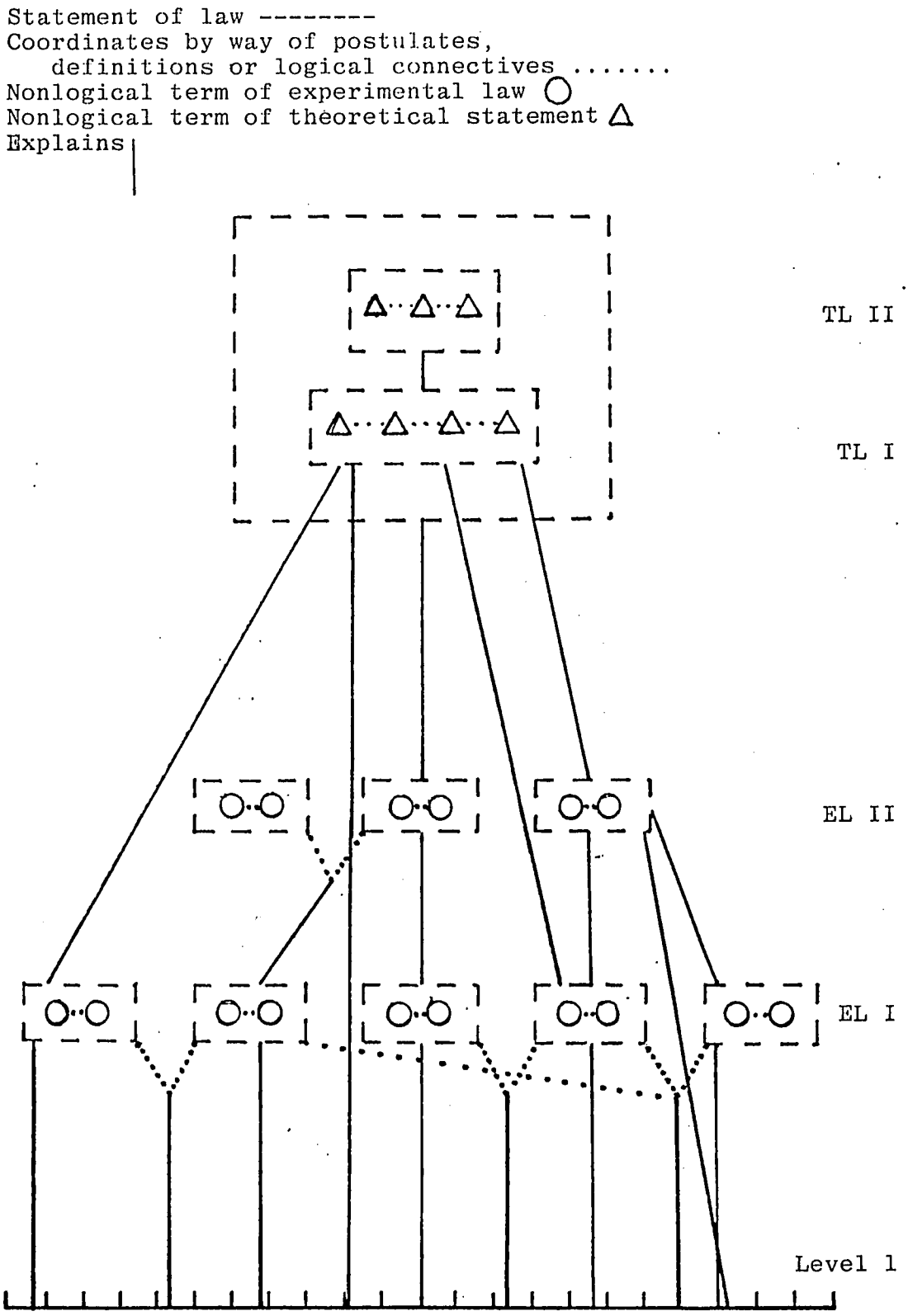

Fig. 1. Level Structure of Theories

from level I, the nonlogical terms of the laws become undefinable in terms of observables and only implicitly defined by the structure in which they occur, and further involve a complex coordination of statements. These are the theoretical laws represented by TL I. These in turn may be explained by theoretical laws of greater explanatory power (TL II) etc.

This level structure diagram illustrates the progress from empirical laws to theories of greater and greater explanatory power. In this conception it is the explanation-explicandum ratio which represents the explanatory power of theories.

This model of theories, however, has been called into question, particularly by Feyerabend. In addition to his criticisms of the theoretical-observational distinction previously mentioned, Feyerabend (cited in Feigl, 1970) has pointed out that there are hardly any examples which show strict deducibility of lower levels from higher levels even in theories with 100 per cent deterministic lawlike postulates. The most simple reason for this is that in straight deductive inference, there can be no terms in the conclusion that are not present in the premises and definitions. Many philosophers of science seem to believe that bridge laws and definitions would accomplish the task of making lower levels derivable from higher levels. However as Bromberger (1971) has pointed out, this belief implies the possibility of carrying out a program of axiomizing any scientific theory. Yet this program has not been carried out to any extent

beyond certain theories of mathematics and physics. Whether the obstacles standing in the way of this program are practical ones or whether they derive from some flaw in the view of theories here presented is open to question.

It may be that a level-structure axiomization is possible only in the case of a fully developed or static theory. In fact, in the development of theories, there appears to be a constant interplay between levels. Lower levels, which usually precede the construction of higher levels, are frequently revised in the light of the higher levels. This has been labeled by Feigl, "corrections from above." Similarly, higher level terms may receive increasing definition from experimental observation. Feigl has labeled this "upward seepage."

As importantly, the conceptual framework of different level theories may be so different as to exclude any deductive relationship; and bridge laws which may be constructed to relate them take on such complexity in themselves as to become "bridge theories." A "bridge theory" may be so successful as to reduce two conceptually different theories to a new framework, and thus in many important ways replace the theories it was designed to relate.

However, even given this lack of deducibility, which may in fact be related to the "slack" and the power of theories, this "orthodox" logical analysis of scientific theories can be useful in the following ways (Feigl, 1970): 1) It may make a theory clearer; this is at least of heuristic

value. 2) It provides at least some help in assessing the correctness of logico-mathematical derivations on the one hand, and the degree of evidential support (or disconfirmation) on the other. 3) Since no important theory is demonstrably monolithic, but rather consists of some number of independent postulates, a logical analysis of the sort presented may show which postulates rest on what empirical evidence.

CHAPTER II

A REVIEW OF SOME USES OF THE TERM METAPSYCHOLOGY

As stated in the introduction, many recent critics of psychoanalytic theory have focused on metapsychology with the explicit or implicit assumption that metapsychology is logically separable from the rest of the theory. This is an assumption made not only by critics of metapsychology but also by some of its foremost developers and advocates. This chapter will examine that assumption. It will attempt to demonstrate that not only has the term metapsychology not been applied to a logically distinct class of theoretical statements, but that the term has never received a consistent and generally accepted definition.

Freud

Freud's use of the term metapsychology has been reviewed by Leeuw (1969), Nagera (1970), and Gill (1976) and the following section owes much to their discussions. Freud's first uses of the term metapsychology are found in his letters to Fleiss. He never defines the term or states how it originated so that it is necessary to extract a definition from the context. His first recorded use of the term is in letter 41 to Fliess. This use is not cited by

Leeuw, or Nagera, or Gill, or to my knowledge any other source. Freud writes:

> I am continually occupied with psychology -- it is really metapsychology; Taines book L'Intelligence gives me special satisfaction. I hope something will come of it. It is the oldest ideas that are the most useful, as I am belatedly finding out. ... I shall read "Nose and Sex" immediately, of course, and send it back. I hope you mention some of our fundamental views on sexuality in it...(1897-1902, p. 157)

Freud's next use of the term appears in letter 84. This letter was written during the time that Freud was in the midst of writing The Interpretation of Dreams. He writes:

> ... It was no small feat on your part to see the dream book lying finished before you. It has come to a stop again, and meanwhile the problem has deepened and widened. It seems to me as if the wish-fulfillment theory gives only the psychological and not the biological, or rather metapsychological[1] explanation. (Incidentally I am going to ask you seriously whether I should use the term "metapsychology" for my psychology which leads beyond consciousness.) Biologically dream-life seems to me to proceed directly from the residue of the prehistoric stage of life (one to three years), which is the source of the unconscious and alone contains the aetiology of the psychoneuroses; the stage which is normally obscured by an amnesia similar to hysteria. ...(1897-1902, p. 246)

This quote appears to contain two somewhat contrasting uses of the term metapsychology. One use connects metapsychological with biological explanation and contrasts it with psychological explanation. The second use defines

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[1]The word is metapsychical in the Standard Edition (Freud, 1898, p. 274).

metapsychology as a psychology which goes beyond consciousness.

Gill (1976) makes much of this dual definition. He argues that metapsychology is not psychology and that the use of the term "should be restricted to propositions about the material substrate, both neurological and biological, of psychic functioning" (p. 71). Gill's definition might be defensible if he were simply offering it as a new, clarifying definition; his attempt to justify it on the basis of Freud's statements, on the matter, however, seems questionable. In fact in Chapter VII of The Interpretation of Dreams Freud wrote:

> What is presented to us in these words is the idea of psychical locality. I shall entirely disregard the fact that the mental apparatus with which we are here concerned is also known to us in the form of an anatomical preparation, and I shall carefully avoid the temptation to determine psychical locality in any anatomical fashion. I shall remain upon psychological ground, and I propose simply to follow the suggestion that we should picture the instrument which carries out our mental functions as resembling a compound microscope or a photographic apparatus, or something of the kind. (1900, p. 536)

It is clear that Freud was using a physical model or analogy, though, not a biological one. It is equally clear that he was not proposing neurological or biological theory. It is one thing to say that Freud's metapsychological theorizing was influenced by his biological and neurological background and that some of the models of his theories were borrowed from those sciences, and quite another to say that at its core metapsychology is really

biological or neurological theory. Further it should be noted that Freud's use of the term biological in letter 84 quote above seems to imply a very broad definition of the term. When Freud writes, "Biologically dream life seems to me proceed directly from the prehistoric stage of life (one to three years), ..." it seems that biologically refers to the fact that person is a biological organism that passes through stages of development. The organism experiences these (psychosexual) stages and develops characteristic fantasies which later may show themselves in dreams. Later in the same letter Freud writes, "The repetition of experiences of the prehistoric period is a wish fulfillment in itself and for its own sake; a recent wish leads to a dream only if it can be associated with material from that period, if the recent wish is a derivative of a prehistoric wish or can get itself adopted by such a wish" (1897-1902, p. 246). Thus it would appear not that Freud is developing a neurobiological theory, but a psychological theory that takes into account that the person is also a biological organism.

It should also be noted that with few exceptions, the psychology of Freud's time was a psychology of consciousness (Ellenberger, 1970; Freud, 1915 Appendix A), and that Freud may have found it necessary to take pains to differentiate his discipline from the then contemporary psychology.

Freud's first published use of the term metapsychology occurs in The Psychopathology of Everyday Life (1901). Discussing superstition he writes:

> In point of fact I believe that a large part of the mythological view of the world, which extends a long way into the most modern religions, is nothing but psychology projected into the external world. The obscure recognition (the endopsychic perception as it were) of psychical factors and relations in the unconscious is mirrored -- it is difficult to express it in other terms, and here the analogy to paranoia must come to our aid -- in the contruction of a supernatural reality, which is destined to be changed back once more by science into the psychology of the unconscious. One could venture to explain in this way the myths of paradise and the fall of man, of God, of good and evil, of immortality, and so on, and to transform metaphysics into metapsychology. (p. 258-259)

Here metapsychology refers to a psychology that goes beyond consciousness or a psychology of the unconscious.

Freud's next recorded use of the term metapsychology the February 27, 1907 minutes of the Vienna Psychoanalytic. He states:

> However, if one wanted to create such a new elementary psychology (a metapsychology which ignores the difference between conscious and unconscious), one would need to make a sharp distinction betwen instincts and emotions. First, one would have to establish: instinct is a concept, a term for the dynamic or disturbing influence which organic (structural) needs exercise on psychic processes. The instinct bridges over from the organic. Emotion, however, belongs entirely to the psychic realm. Consciousness (the inner sense organ) is attuned to two end points of a broad arc of processes: that is, geared to perceptions and emotions. Everything else is not subsumed under the concept of consciousness. (Nunberg and Federn, 1962, p. 135)

Here Freud uses the term metapsychology to refer to a

psychology which goes beyond conscious phenomenon yet at the same time distinguishes between certain unconscious and conscious phenomena. Once again it seems apparent that Freud is attempting to develop a psychology that not only goes beyond consciousness, but also takes into account the biological substrate.

It is eight years before Freud's next recorded use of the term metapsychology. In "The Unconscious" he wrote:

> It will not be unreasonable to give a special name to this whole way of regarding our subject matter, for it is the consummation of psychoanalytic research. I propose that when we have succeeded in describing a psychical process in its dynamic, topographical, and economic aspects, we should speak of it as meta-psychological presentation. We must say at once that in the present state of our knowledge there are only a few points at which we shall succeed in this. (1915, p. 181)

In this use metapsychology has taken on a more complex meaning. It implies both to a way of organizing and systematizing observations and to a potential system of explanation. Further it is proposed that an explanation will be sufficient if it includes a statement of the dynamic, topographic, and economic conditions of the system under scrutiny. It might be argued that Freud is here proposing a mechanical system of explanation, that is, one that asserts that a change in the system[1] is a function of the magnitude (economic) and direction (dynamic) of the forces acting on it. However it is unclear whether Freud is proposing a

[1]The system is here described in topographical terms. Later this is replaced by structural theory. Gill (1963) refers to these both as systemic points of view.

mechanical theory or only stating a mechanical analogy. Certainly for any theory to be considered mechanical the parameters of force must be quantifiable on a ratio scale. This would lead one to view such psychoanalytic explanations as at best quasi-mechanical analogies.

As is frequently the case with Freud, a new use of a term did not preclude it being used in the old sense. Later in the same paper he writes:

> Hence consciousness stands in no simple relation either to the different systems or to repression. The truth is that it is not only the psychically repressed that remains alien to consciousness, but also some of the impulses which dominate our ego -- something, therefore, that forms the strongest functional antithesis to the repressed. The more we seek to win our way to a metapsychological view of mental life, the more we must learn to emancipate ourselves from the importance of a symtom being conscious. (1915, p.192-193)

Here again metapsychology refers to a psychology that goes beyond consciousness. Following this paper Freud continued to use the term in all the aforementioned meanings. In addition, he introduced others. In a footnote to the title of "A Metapsychological Supplement to the Theory of Dreams" he writes,

> This paper and the following one are derived from a collection which I originally intended to publish in book form under the title "Preliminaries to a Metapsychology". They follow on ... (the first three papers on metapsychology). The intention of the series is to clarify and carry deeper the theoretical assumptions on which a psychoanalytic system could be founded. (1917, p. 222)

This statement is interesting for several reasons. First because Freud makes reference to theoretical assump-

tions; but it is not clear to what these assumptions refer. Strachey seems to take the use of the term to indicate that it was Freud's intention in his metapsychological papers "to provide a stable theoretical foundation for psychoanalysis" (1957 p. 105). Strachey continues:

> The collection of twelve (metapsychological) papers (seven of which Freud destroyed) would thus have been a comprehensive one dealing with the underlying processes in most of the principal neuroses and psychosis ... as well as dreams, with the mental mechanisms of repression, sublimation, introjection and projection, and with the two mental systems of consciousness and the unconscious. (p. 106)

Rapaport and Gill (1959) interpret Freud's use of term assumption much more narrowly. They seem to take it to mean the postulates of the theory from which the rest of the theory could be derived or on the basis of which inferences are drawn. Though Freud may have at times used the term metapsychology to refer to assumptions of this sort, it seems that he more frequently used it in the broader sense to which Strachey refers.

In addition to illustrating Freud's pessimism this quote seems to confirm the previously mentioned point: Freud frequently uses the term metapsychology to refer to the whole system and not just the assumptions upon which that system is built.[1]

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[1]Leeuw (1969) says that depth psychology and metapsychology are not identical concepts. The former includes only the dynamic and topographical viewpoints while the latter adds the economic and structural viewpoints. It is not clear on what evidence Leeuw bases this distinction. However if Freud does make this

Before summarizing Freud's use of the term metapsychology, it will be of interest to look at his last published uses of the term. They are found in the same paper, "Analysis Terminable and Interminable," not very far apart, and yet carry somewhat different meanings. First in discussing 'taming' of the instinct, Freud writes:

> That is to say, the instinct is bought completely into the harmony of the ego, becomes accessible to all the influences of the other trends in the ego and no longer seeks to go its independent way to satisfaction. If we are asked by what methods and means this result is achieved, it is not easy to find an answer. We can only say: 'So muss denn doch die Hexe dran!' (We must call the Witch to our help after all!) -- The Witch Metapsychology. Without metapsychological speculation and theorizing -- I had almost said 'phantasizing' -- we shall not get another step forward. Unfortunately here as elsewhere, what our Witch reveals is neither very clear nor very detailed. We have only a single clue to start from -- though it is a clue of the highest value -- namely, the antithesis between the primary and secondary processes; and it is to that antithesis I shall at this point turn. (1937, p. 225)

Second, on the next page, in a footnote to a discussion

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distinction, it may be to distinguish his psychoanalytic metapsychology from the depth psychology of Janet and others. If this is the case then it is of primarily historical interest. However, it would be interesting to wonder if some subset of the metapsychological viewpoints might be sufficient to predict to a more limited class of events. This is certainly the case in other sciences. It is Lustman's (1957), almost certainly incorrect, contention that the economic viewpoint provides a sufficient explanation for certain infant behaviors. See Rosenblatt's and Thickstun's reply (1970).

In physics, Newtonian mechanics may be viewed as a limiting case of relativity theory. As v - 0 the results obtained using either set of postulates are insignificantly different. Thus, even an "incorrect" theory may be useful if the "correct" theory is more complex and yields insignificantly better results.

of the relative strength of the ego and the instincts, he writes:

> Here we have a justification of the claim to the aetiological importance of such non-specific factors as overwork, shock, etc. These factors have always been assured of general recognition, but have had to be pushed into the background previsely by psycho-analysis. It is impossible to define health except in metapsychological terms: i.e. by reference to the dynamic relations between the agencies of the mental apparatus which have been recognized - or (if that is preferred) inferred or conjectured by us. (1937, p.226)

In his third and final use Freud writes:

> We started from the question of how we can shorten the inconveniently long duration of analytic treatment, and still with this question of time in mind we went on to consider whether it is possible to achieve a permanent cure or even to prevent future illness by prophylactic treatment. In doing so, we found that the factors which were decisive for the success of our theraputic efforts were the influence of traumatic aetiology, the relative strength of the instincts which have to be controlled, and something we have called an alteration of the ego. Only the second of these factors has been discussed by us in any detail, and in this connection with it we have had the occasion to recognize the paramount importance of the quantitative factor and to stress the claim of the metapsychological line of approach to be taken into account in any attempt at explanation. (1937, p. 234)

Looking at these quotations we again see related but different senses in which the term metapsychology is used by Freud. In the first use, metapsychology refers both to a whole background of theoretical speculation which may be called forth for possible explanation when a new riddle is encountered and to a set of assumptions on the basis of which inferences are drawn. It is worth noting that Freud refers to metapsychology as speculation. In this case the

speculation relates to economic - structural hypotheses, but there is nothing to indicate that it need be confined to any particular subset of psychoanalytic hypotheses. Certainly his entire discussion is not "couched in 'economic' terms" as Gill (1976) claims.

In the second and third use, metapsychology refers to a system of explanation. In the second the stress is laid on the dynamic-structural aspect of explanation, and in the third the stress is on the economic-structural aspects, but reference is made to the importance of genetic (i.e. developmental) factors.

Summary of Freud's use of the term metapsychology

It appears that Freud did not rigorously define or consistently use the term metapsychology. This will be important to bear in mind later when we see that other authors write as if the term had an obvious and agreed upon meaning. For instance Gill contends that Freud was distinguishing between clinical and metapsychological theories because, "... it seems to me evident that, if he singled out certain types of propositions as specifically metapsychological, he was making such a distinction (Gill, 1976 p. 83)." Yet it is not at all evident what those certain types of propositions are.

However, we can see in Freud's writings different but related senses in which he uses the term. Since these uses are not precise and sometimes shade into one another,

enumerating the different meanings is a somewhat arbitrary procedure. I will enumerate six.

1. Metapsychology is something other than psychology and perhaps refers to something biological or is related to biology.

Comment: It appears that in this sense Freud is differentiating psychoanalysis from the psychologies of the day and placing it more in the context of the natural sciences. However it is clear that he is using biological in a very broad sense and certainly not completely equating it with neurology or physiology.

2. Metapsychology is more than psychology in that it concerns itself with more than mental content readily available to consciousness; it is a psychology that goes beyond consciousness. In an important sense, it ignores the difference between conscious and unconscious processes in that they are both important determinants of behavior; yet it also recognizes that whether a process is conscious or not may be important (i.e. consciousness is not epi-phenomenal). In this sense metapsychology coincides with psychoanalytic theory.

Comment: This is Freud's broadest use of the term which equates it with all of what is now called psychoanalytic theory. Freud continued to use the term in this way throughout his work.

3. Metapsychology refers to a system of organizing observations into three categories (viewpoints): dynamic, economic, and systemic.

4. Metapsychology refers to a potential system of explanation that may in some way be related to a mechanical system of explanation.

Comment: While metapsychology used in sense 3 or 4 usually refers to both, they are logically separable. The three categories of metapsychology - dynamic, economic, and systemic - may be used in only a classificatory or descriptive sense (3); or it may be held that a description of the system along the three coordinates will in fact yield explanations and predictions.

5. Metapsychology involves the study of the assumptions upon which the system of psychoanalytic theory is based.

Comment: Though it appears that Freud believed the study of theoretical assumptions (broadly defined) to be a necessary preliminary or an important aspect of metapsychology, he does not appear to have defined metapsychology exclusively as a study of those assumptions.

6. Metapsychology refers more specifically to the quantitative (economic) aspect of psychoanalystic theory.

Comment: Though there are indications that Freud considered the economic viewpoint to be the most unique to his theory, there is only slight evidence that he equated it with metapsychology (1937, p. 234). However, it will

later be shown that some critics of metapsychology have made this equation.

Since these uses are related, it is tempting at this point to try to summarize Freud's uses of the term metapsychology into a single coherent definition. Metapsychology refers to a psychological theory which is not yet developed. In the system of explanation of the theory the explicandum will be all mental content, both conscious and unconscious, and by extention all motivated behavior. (An assumption of this theory is that all motivated behavior can be explained by reference to mental content.) Within this theory, an explanation will always be sufficient if it exhaustively describes the state of the system (mental apparatus) along three coordinates, the dynamic, economic, and systemic. It remains to be seen whether this definition will be useful. It is offered in summary to point out that Freud conceptualized metapsychology neither as a logically separable part of psychoanalytic theory nor as tied to biology in any narrow sense of the term.

Hartmann

Hartmann uses the term metapsychology rather infrequently and for the most part his uses are clear and consistent. For example he writes (1959, p. 324)[1]:

[1]All of Hartmann's articles referred to in this section have been reprinted in Essays on Ego Psychology (1964) and the page numbers refer to that volume. No uses of the term metapsychology were found in Hartmann's other works (Hartmann, 1939; Hartmann, Kris and Lowenstein, 1964).

> And I have to present you, at least briefly, a discussion of what, in analysis, we call "metapsychology," a term that signifies not (as it might seem) that which is beyond psychology altogether, but simply those psychological investigations that are not limited to conscious phenomena, and that formulate the most general assumptions of analysis on the most abstract level of theory. Metapsychology is concerned also with the substructures of personality. ...

A few pages later he adds:

> The three aspects of psychoanalytic theory I have mentioned so far - the topographical (conscious-preconscious-unconscious), the dynamic, and the economic (energic) - represent Freud's first approach to what he call "metapsychology." It is postulated that a satisfactory explanation of human behavior includes its consideration in relation to all aspects of metapsychology. The "meta" in the term points to a theory going "beyond" the investigation of conscious phenomena. ... "metapsychology" is nothing but a term for the highest level of abstraction used in analytic psychology. (p. 328)

If one were to extract a summary definition of metapsychology from the above passages it would be very similar to the summary definition of Freud's uses offered previously. However there might be one important difference. Whereas Freud does not seem to have logically differentiated metapsychology from the rest of psychoanalytic theory, Hartmann may have. Specifically when he refers to "the most general assumptions of analysis on the most abstract level of theory," and "the highest level of abstraction," Hartmann appears to be dividing psycho-

analytic theory into metapsychology and something else.

Further, though this is not altogether clear, Hartmann may be making some very restrictive a priori assumptions. That is, he may be assuming that only those statements that can be characterized as topographic, dynamic, economic, or structural can also be characterized as metapsychological; and only statements so characterized are at the highest level of abstraction. For example Hartmann (1953) though he discusses many aspects of schizophrenia including" ... defense, object relations, language, reality testing, and so on (p. 205)," focuses on hypotheses concerning neutralization. He believed that these hypotheses helped account for and integrate the disparate data. He writes (p. 205): "The metapsychological hypotheses I used allowed me at least partly to bridge the gap between the instinctual and the ego aspects of schizophrenic psychosis, and to establish connections between them." If Hartmann is restricting these bridging and connecting statements to statements that can be characterized as topographical, dynamic, economic, or structural (the "four aspects of metapsychology") then this would seem to be a significant a priori assumption. It would mean that if useful bridging or connecting statements were developed that did not obviously fit into any of the four categories, either they would have to be ignored or designated non-psychoanalytic; or one or more of the four categories could be arbitrarily redefined;

or those statements could be designated as non-meta-psychological even though they were as useful in bridging and connecting as those statements designated metapsychological. Hartmann generally chooses the last of these alternatives. That is, when he formulates significant theoretical statements which might be characterized as object relational or adaptational, he does not, for example, label them as metapsychological. The logic behind this is not obvious. It may be that he believed that the most general level of the future psychoanalytic theory wuld be quasi-mechanical and that the rest of the theory would be deducible from that level. However, for any theoretician dealing with a very incomplete theory, such a belief is clearly a metaphysical belief. If Hartmann held this belief, it does not appear to have overly constricted the scope of his theorizing.

A more important question than whether or not topographic, dynamic, economic, and structural theoretical statements deserve special status is whether any class of theoretical statements deserves special status. That is, within any particular theory, is there any logical criteria for designating a (closed) set of statements as 'more abstract,' 'higher,' 'more general,' or the like? Rapaport was certainly the foremost advocate of making such distinctions in psychoanalytic theory. His work will now be examined to see if he developed a logical justification.

Rapaport

Rapaport probably deserves credit for popularizing the use of the term metapsychology and for clearly setting it apart from the rest of psychoanalytic theory. Certainly in establishing metapsychology as a separate domain within psychoanalysis, it was Rapaport's intent to focus upon, expand and build upon it. Others, some of his students being foremost among them, have used a similar logic to discard it.

In examining Rapaport's work one gets the impression that he believed that a partition of psychoanalysis into distinct levels was not only heuristically valuable but logically obvious. However, one is at a loss to find logical criteria for such a distinction clearly stated. Rapaport did define metapsychology several times; however, in light of his usual systematic thinking his definitions are suprisingly at variance with each other. This section will focus on the inconsistencies in his definitions and on his attempt to establish metapsychology as a logically separate class of theoretical statements.

It is interesting that in an early use of the term (1944), Rapaport equates metapsychology with psychoanalytic theory. He stated, "Freud strove to build up something called psychoanalytic psychology (metapsychology); he strove with the psychoanalytic technique of observation and therapy, to build up a system of theoretical

tenets of psychology" (p. 168). Though in this paper Rapaport refers to points of view, he does not specifically label them metapsychological.

Several years later (1950) Rapaport sets metapsychology apart from the rest of theory. He wrote, "...the psychoanalytic theory of thinking is part of metapsychology - which, dealing as it does with abstractions and being several steps removed from immediate therapeutic concern, remains the least familiar and to many the least palatable part of psychoanalytic theory" (p. 313). An analysis of this statement leads to some interesting conclusions. If one accepts that a) the psychoanalytic theory of thinking is part of metapsychology, and b) metapsychology deals with abstractions and is several steps removed from the data, then it would follow that c) the psychoanalytic theory of thinking is several steps removed from the data. Leaving aside for now the question of whether any theory can be said to be more removed from the data than any other theory, it is doubtful that a theory of thinking could exist in some way removed from the data of observation with no laws to explain. Would Rapaport contend that a psychoanalytic theory of thinking is more removed from the data than a psychoanalytic theory of symptom formation? If so, on what basis would he support this contention? These questions are left unanswered by Rapaport. Some people might think that there is at this point in time less

data and fewer laws on which to build a psychoanalytic theory of thinking, and therefore such a theory appears more abstract and removed and speculative than the theory of symptom formation. However, this is not logically necessary.

In subsequent work Rapaport develops and presents three rather different definitions of metapsychology. These are: One, metapsychology is a systematization of psychoanalytic theory along several, eventually five, viewpoints. Two, metapsychology is the study of the assumptions upon which psychoanalytic theory is based. Three, metapsychology is a distinct, higher or more abstract, level of psychoanalytic theory, at times equated with the level of general theory. Though these definitions sometimes overlap in Rapaport's work, they will here be discussed separately.

The first mentioned definition represents Rapaport's most frequent use of the term. I will attempt to show that this definition is an arbitrary one and at best a definition by convention. My criticism hinges around the undefined use of the term viewpoint. At no point does he state what this term might mean. Further he changes the number of metapsychological viewpoints. At times it is four (1950), at times three (1953), and eventually five (Rapaport and Gill, 1959).

What are these viewpoints? It is fairly clear that

Freud hoped that an analysis of a phenomenon along his three metapsychological coordinates would be necessary and sufficient for an explanation of that phenomenon. Though Freud did not elaborate on this, there appears to be a certain logic in his choice of the three particular categories (viewpoints). That is, they seem to represent an attempt at a quasi-mechanical system of explanation. Within such a system a necessary and sufficient explanation will contain statements about the constituents of the system being explained (topographic or structural), the direction (dynamic), and magnitude (economics) of the forces acting upon those constituents. Whether such types of explanations will prove to be useful when applied to human behavior is open to serious question. Certainly to allow only such explanation is to impose very limiting restrictions. However, what is relevant here is that the three types of statements in explanations of this sort form a logically consistent class. This does not appear to be the case with Rapaport's choice of metapsychological viewpoints. He and Gill write (1959):

> Metapsychology proper thus consists of propositions stating the minimum (both necessary and sufficient) numbers of independent assumptions upon which the phsychoanalytic theory rests. Metapsychology also includes the points of view which guide the meta-psychological analysis of psychoanalytic propositions, both observational and theoretical. Here we will group the assumptions according to the points of view to which they pertain. Since, for the moment, the only rest of the necessity, sufficiency, and fruit-fulness of such assumptions is the demonstration of

> their role in familiar psychoanalytic propositions, we will attempt to give such a demonstration on the propositions of the psychoanalytic theory of affects. (p. 798)

It would appear that Rapaport is concerned with necessity and sufficiency but of a different sort. He is concerned with a specific set of statements being necessary and sufficient for derivation rather than a general class of statements being necessary and sufficient for explanation. The viewpoints then become a way of classifying that set of statements into subcategories that serve as guides for analysis. However, if these viewpoints are merely guides then there is nothing logically obvious about which are central and how many there should be. Yet Rapaport clearly believed that the five viewpoints he and Gill (1959) outlined had special status. For instance in his monograph The Structure of Psychoanalytic Theory (1959), he presents ten points of view and then argues rather unconvincingly that they should be reduced to five. About the empirical, Gestalt, and organismic points of view, he can only say they," ... seem to be of a different character, and that lumping them together with the metapsychological points of view is another indication that the systematization here attempted is premature" (p. 66).

Schafer (1970 a) has similarly criticized Rapaport's designation of the genetic and adaptational viewpoints as metapsychological. He states that Freud considered his

three points of view to be sufficient for the explanation of psychological processes. Schafer writes:

> Explanation concerns immediate fields of force; history helps to determine the nature of these fields of force. The component factors of these fields are necessarily current factors and these are, in the final analysis, to be explained in terms of aims, (dynamics), emphases (economics), and organizations with varying degrees of independence, integration, stability and influence (structures). (p. 432)

Schafer labels explanations of this sort as field-theoretical formulations. They would seem to coincide with explanations I have labeled quasi-mechanical. He suggests that while genetic and adaptational explanations are indispensable, they belong to a different framework.

A second definition of metapsychology put forth by Rapaport is, "... metapsychology as the study of the assumptions upon which the system of psychoanalytic theory is based" (Rapaport and Gill, 1959, p. 796). As previously mentioned metapsychology was never defined in this way by Freud as Rapaport and Gill claim, but this is a minor point. However, it is difficult to see how Rapaport could justify a meaningful study of these assumptions at this point in the development of psychoanalytic theory. He acknowledges that, "...the only test of the necessity, sufficiency, and fruitfulness of such assumptions is the demonstration of their role in familiar psychoanalytic propositions, ..." (1959, p. 798). Yet this is not a test of necessity or sufficiency at all; it is a test of

familiarity. In fact, what Rapaport and Gill produced is a list of the guise of a deductive derivation. In their presentation, the relationship between the assumptions and the purported consequences is often unclear. It would be an interesting task to state certain assumptions and then to deduce the consequences of those assumptions, adding coordinating definitions when necessary. Rubinstein (1967) attempted something like this with certain hypotheses of the psychoanalytic theory of motivation. However, this is not what Rapaport and Gill attempted.

Further it is unclear how Rapaport distinguished inferences from observations and metapsychological assumptions. For instance he states that the existence of psychological forces is an assumption and continues, "This assumption does not distinguish between conscious and unconscious psychological forces because that distinction is not an assumption but an inference from empirical observations, and is thus a proposition of the special theory of psychoanslysis but not of metapsychology (Freud, 1914, pp. 16-17) (Rapaport and Gill, 1959, p.799). It is unclear on what basis, other than preference or convention, one would designate the existence of psychological forces an assumption and the existence of unconscious processes an inference. Neither are directly observable and both fit into an overall theoretical context. Both would appear to be inferences based on

observation and either could function as assumptions in a deductive system. To stress a point, a statement or set of statements functions as assumptions by choice and are not given a priori. In psychoanalytic theory one can label a chosen set of such assumptions metapsycholigical, but this choice appears to be a matter of convention and not logic.

A third way in which Rapaport uses metapsychology is to refer to a distinct, higher or more abstract, level of psychoanalytic theory. In this use, metapyschology is sometimes equated with general theory and sometimes not. This is the most problematic of Rapaport's uses of the term because it raises the question, in what way it is useful to speak of distinct levels of a theory? Before turning to this general question Rapaport's proposed distinctions will be discussed.

In 1957a Rapaport wrote:

> There exists however a fragmentary - yet consistent - general theory of psychoanalysis, which comprises the premises of the special (clinical) theory, the concepts built on it, and the generalizations derived from it. Since the general theory deals with what was in Freud's time beyond the scope of academic psychology, and since in relation to the clinical theory it is a metatheory, it was named metapsychology.(p. 670)

Rapaport goes on to say that the clinical theory remains experimentally untestable. He does not say why, but the implication is that the clinical situation is inaccessible to experimental verification. He concludes that meta-

psychology, which is here equated with general theory, "holds the promise that it can bring psychology to a level of theoretical generality where its unification with experimentally derived theories, or at least its experimental verification will be possible" (p. 673).

To state this schematically (See Figure 1), Rapaport is proposing that there is a clinical theory Tc which can explain putative clinical laws and observations Lc. However, Lc for a variety of reasons are not experimentaly verifiable which leaves open to dispute whether Tc explains anything. There is another theory in academic psychology Ta which explains certain laws and observations La. La is experimentally verifiable. There is another more general theory Tg which, with appropriate coordinating definitions, can explain both Tc and Ta as well as explain and predict to new, not as yet observed, laws Lg.

In a later use, Rapaport differentiates between metapsychology and general theory. He writes:

> This general theory is always built by a metapsychological analysis of the propositions of the special or clinical theory. Metapsychology proper consists of the minimal set of basic assumptions underlying all psychoanalytic propositions, and these fall into five groups: dynamic, structural, genetic, and adaptive assumptions. The five metapsychological points of view correspond to these five groups and serve as guides for metapsychological analysis. Metapsychological analysis applied to any set of propositions of psychoanalysis reveals which basic assumptions are and which are not implied in that set of propositons, and thus permits us to discover where any theory is incomplete or inconsistent or redundant.

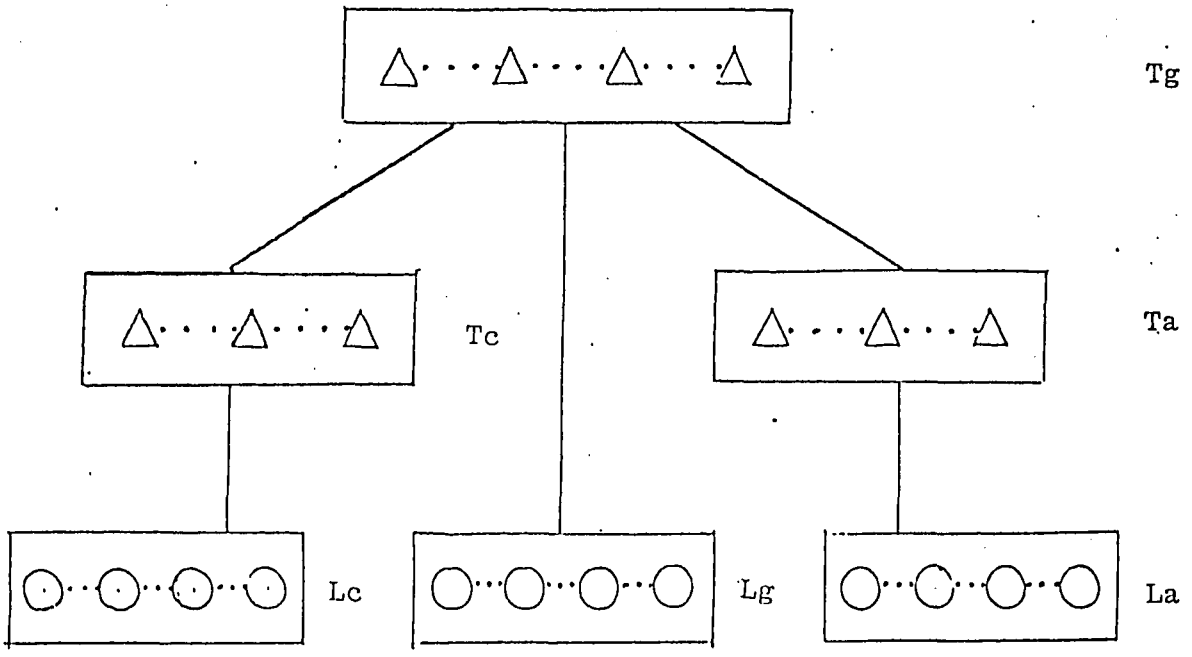

Figure 2

Rapaport's Conceptualization of General Theory

> Clearly I am not in a position today to give a metapsychological analysis of the superego concept and the psychoanalytic propositions pertaining to it. (1957b, p. 696)

In this quote, general theory and metapsychology are no longer equated. Metapsychology is used in the three ways previously discussed: 1) as a systematization of psychoanalytic theory along five viewpoints; 2) as a study of the assumptions on which psychoanalytic theory is based; and 3) as a higher, more abstract level of theory; additionally a method for the derivation of general theory is presented: "...general theory is always built by a metapsychological analysis of the propositions of special or clinical theory." This is curious for two reasons. First because it is an exclusive prescription and it is questionable whether theory building must necessarily follow any particular prescription. It is also curious because the metapsychological assumptions as they are here characterized become essentially empirical principles (Nagel, 1961). Empirical principles are assumptions which have become so well established that they may come to be referred to as 'laws of nature.' As such they come to be used as empirical principles in accordance with which inferences are drawn. For example, laws of conservation of energy and matter are so accepted that when they appear to be violated in high energy physics experiments new particles are postulated. Similarly, if the velocity of light

appears to be exceeded it is assumed to be illusory. In biology, laws of natural selection are frequently used as empirical principles. However, there is so little acceptance of the metapsychological assumptions that they would hardly seem to qualify as laws of nature. There is nothing to prevent them from being used as loose leading principles or inference tickets, but as such they occupy no special logical status and their choice is a matter of preference. The fact that Rapaport never carried through a "metapsychological analysis" of the superego or any other aspect of psychoanalytic theory would further seem to indicate that the attempt was premature.

We can now continue to pursue the question of whether Rapaport developed any logical justification for distinguishing metapsychology or general theory from the rest of psychoanalytic theory. Rapaport and Gill (1959, p. 796) propose distinguishing four types of statements and illustrate with examples. They write:

> For instance: empirical proposition: around the fourth year of life boys regard their fathers as rivals; specific psychoanalytic propositions: the solution of the oedipal situation is a decisive determinant of character formation and pathology; general psychoanalytic proposition: structure formation by means of identifications and anti-cathexes explains theoretically the consequences of the "decline of the oedipal complex"; metapsychological proposition: the propositions of the general psychoanalytic theory which explain the oedipal situation and the decline of the oedipus complex involve dynamic, economic, structural, genetic, and adaptive assumptions. (p. 796)

This is a model which Rapaport only mentions in passing and did not develop further. It might therefore be a mistake to make too much of it. However, it should be noted that no clear rational for the division is apparent. Using the conceptualization of Chapter I it would appear that what Rapaport labels an empirical proposition could be characterized as an experimental law of the form, 'if A then B'. The second level, that labeled specific psychoanalytic proposition, is less clear. It could with some modification be put in law-like form such as, 'If the oedipal situation were solved in manner A then character formation B would result'. However, it is not put in this form and we are left not knowing whether such terms as oedipal situation and character formation are capable of explicit definition or are theoretical terms. This is perhaps the most critical difficulty in doing a level analysis of psychoanalytic theory. Terms are frequently undefined and it is often not possible to know whether a statement is meant as a law, a definition, or something else. More will be said about this later.

This third level, general psychoanalytic proposition, appears to be meant to be a high level theoretical statement. However, it is taken out of context and in itself is not an explanation. It is an assertion of an explanation. It asserts that a host of statements would explain "the consequences of the decline of the oedipal complex".

Fully stated it would contain numerous law-like and theory-like statements such as, 'loss leads to identification', and 'identification implies structuralization.' Further since the 'consequences' being explained are undefined it is impossible to assess the generality of the proposed explanation.

The fourth level, metapsychology, as used here is not really a level of theory. It stands outside the theory as an assertion that certain types of statements will be necessary and sufficient for explanation. Criticisms of this assertion have been discussed previously. (See pp. above.)

Waelder

Before discussing further the general issue of levels of theory, it will be interesting to look at Waelder's (1962) proposed distinctions. Waelder distinguishes six levels of psychoanalysis. The first level he terms the "level of observation." These are "facts" which the psychoanalyst learns about people in the analytic situation. He then adds:

> These data then made the subject of interpretation regarding their interconnections and their relationships with other behavior or conscious content. This is the level of clinical interpretation.
>
> From groups of data and their interpretations, generalizations have been made, leading to statements regarding a particular type such as, e.g., a sex an age group, a psychological symtom, a mental or emotional disease, a character type, the impact of a particular family constellation, or of any particular

> experience, and the like. This is the level of clinical generalizations.
>
> The clinical interpretations permit the formulation of certain theoretical concepts which are either implicit in the interpretations or to which the interpretations may lead, such as repression, defense, return of the repressed, regression, etc. This is the level of clinical theory.
>
> Beyond the clinical concepts there is, without sharp boundaries, a more abstract kind of concept such as cathexis, psychic energy, Eros, death instinct. Here we reach the level of metapsychology.
>
> Finally, Freud, like other thinkers, had his own philosophy, his way of looking at the world, ... This may be called the level of Freud's philosophy. (p. 620)

The logic of these distinctions is for the most part fairly obvious. The level of observation would contain only singular statements or conjunctions of them. Levels two and three, clinical interpretations and clinical generalizations, would contain only law-like statements of the form, 'if A then B'. Level three would be distinguished from level two by being capable of explaining classes of events rather than specific events. Certain laws of level three should be potentially stateable independently of the theory that purports to explain them.

Levels four and five are levels of theory. Statements on these levels contain terms which are not capable of explicit definitions, but are implicitly defined by their place in the theory. This theory should be capable of explaining the laws and observations of the first three levels. It is not clear on what basis Waelder makes the

distinction between levels four and five. Though he says it is a distinction without sharp boundaries, he offers the suggestion that the theoretical terms of level five are somehow more abstract. I would suggest that the designation of certain theoretical term as being more abstract is a questionable procedure. A term would seem either to be abstract or not. Those terms which are characterized as more abstract would seem to be those terms that are less well defined, either inconsistently or vaguely, and less well integrated into the overall theory. It may be that Waelder and others who use the characterization 'more abstract' to refer to theories or theoretical terms mean something different, but this is not clear.

Finally, the level of Freud's philosophy corresponds to what was referred to in the first chapter as implicit metaphysics. As such it is logically not a level of the theory. That is, though it may importantly influence the development of a theory, it plays no logical role in its explanations.

Levels of Theory in Psychoanalysis

We can now turn to the question of whether it is useful to distinguish between levels of psychoanalytic theory. It would seem that certain distinctions are both logical and useful. At the most basic level, it is frequently impossible to determine which terms in psycho-

analysis refer to observable events and which refer to theoretical entities. This problem has been discussed by Kline (1972), Martin (1964), and Farrell (1961, 1964) among others and will not be extensively discussed here. A brief example will suffice to illustrate this point.

In his initial definition of neurotic symptoms Fenichel (1945) states, "In all neurotic symptoms something happens which the patient experiences as strange and unintelligible," and, "All symptoms give the impression of a something that disturbs the continuity of the personality and that is outside the realm of the conscious will" (p. 18). This is a definition that would allow the labeling of certain observable events as symptoms. However, Fenichel then goes on to speak of "neurotic phenomenon of a different kind." He states that in "neurotic characters" the personality is often, "so involved in the illness that one cannot say at what point the 'personality' ends and the 'symptom' begins." The distinguishing features of neurotic symptoms become progressively blurred such that Fenichel concludes, "it can be stated that the common denominator or all neurotic phenomena is an insufficiency of the normal control apparatus" (p. 19).

Still later Fenichel discusses ways in which people remain unaware of their symptoms (pp. 457-459) and symptoms which are not ego-alien (p. 464). Certainly Fenichel offers many other "definitions" of symptoms such as

"discharge phenomena that occur without the consent of the ego" (p. 21); and symptoms as expressions of conflict between drive and defense (p. 193). However, thus defined symptoms cease to be observables.. One could argue that symptoms are now theoretical terms only implicitly defined by the theory. However, if this were the case, what would the psychoanalytic theory of neuroses explain? An elegant theory would be left hanging in air, explaining nothing observable.

It should be obvious that if there are no well-defined observables, there can be no lawful statements; and it is questionable whether a theory without observables and laws is a theory at all.

This thesis has sidestepped the question of psychoanalysis as a scientific theory, and for the most part will continue to do so. This question has been the subject of much debate with predictably little agreement. The reader is referred to compilations by Hook (1958) and Wollheim (1974), and to an interesting debate between Martin and Farrell previously referred to. Generally, the debate revolves around the issue of whether a law can be stated acceptably in a refutable form. Certainly there is nothing logically to prevent psychoanalytic propositions from being restated in law-like form in which the observables are operationally defined. The problem is doing so in a way that remains acceptable to the proponents of the theory. Martin (1964) contends that to restate a theory

is to alter it. This is a moot point. The important question is not whether the psychoanalytic theory is testable, but whether a psychoanalytic theory can be developed which is testable.

In a different context Holt (1974) has stated, "Every science must at some point include the precise formulation of the obvious. ..." (p.15). Holt appears to be calling for an explication of the observations and laws that may be hidden in what we refer to as psychoanalytic theory. Similarly Schafer's (1976) attempt to develop an action language might be seen as a way to systematically state lawful relationships.

It should be stressed that the observations and laws referred to need not be obvious or simple to observe. Certain observations as well as explanations may require the presence of a highly trained observer. For example, it might be that only if type of person A has had training and experience X and knows certain specifiable information will he be able to make consistent observations of the phenomena under study. This type of statement may take the vague form, "Only one who has suffered can understand suffering," or a somewhat more precise form like, "Only persons with certain analytic training can understand the data of psychoanalytic sessions." There is nothing in the logical structure of these types of observational or explanatory statements that make them different from accepted observational statements in other sciences. Person A is

like the instrument of observation (as well as being the usually trained observer). He is like the telescope which is constructed out of certain materials (A), put together in a certain manner (X). This telescope may be usefully employed in the observation of astronomical phenomena even if its materials, construction, and adjustment must be empirically determined (i.e. there is no theory of telescope construction). Unless it can be demonstrated that rules concerning types of person A or training and experience X are necessarily unstatable, or that a theory that would include an understanding of how A performs his task is necessarily undiscoverable, or if the observations A performs are not replicable, then the use of a certain type of trained observer seems scientifically acceptable.

The next level to be considered is that of theoretical statements. It was stated in Chapter I that all existing scientific theories that have been studied contain slack. That is, there are terms which do not receive explicit definition. It is to be expected that this will be true of a well developed psychoanalytic theory. However, at this point it is not possible to determine which those terms are. It is often not clear which terms are meant to be implicitly defined by the theory, and which are simply inadequately or inconsistently defined. Despite Freud's frequently quoted apprehension about rigorous definitions, the need for rigorous and consistent definition of terms in their theoretical context is

apparent. Schafer's (1968b) book on internalization would seem like a beginning in this direction. What this work lacks is clearly stated operational definitions of the observations explained.

This approach to examining psychoanalytic theory by viewing it as a three level structure is logical and seems potentially useful, but what of Rapaport's and Waelder's other proposed distinctions? As stated previously, Rapaport's four level conceptualization is cursorily stated, however it appears to suggest either two levels of laws and two levels of theory or one level of law and three levels of theory. Rapaport's earlier conceptualization of psychoanalysis are being divisible into metapsychology-general theory and clinical-special theory explicitly posits one level of observation, two level of law, two levels of theory, and one level of implicit metaphysics. The level of implicit metaphysics has been previously discussed and will be further mentioned.

The question can now be restated. Is there any logically consistent way to distinguish between a) levels of law and b) levels of theory? Though a completed answer would be quite complex (see Nagel, 1961, esp. chapt. 3 & 11), the relevant aspects can be summarized. Certain laws may be categorized with respect to their level of generality. Let L_1 be a law of the form 'If A then B', and L_2 a law of the form 'If C then D'. L_1 can

be said to be more general than L_2, if and only if, 'If C then A' is logically true but its converse 'If A then C' is not. If both 'If C then A' and its converse are true, then L_1 and L_2 can be said to be equally general. If neither is true, then the laws cannot be compared by this criteria with respect to their generality. For example, if L_1 is 'If an analyst makes an interpretation then the transference will be altered, and L_2 is 'If an analyst makes a premature interpretation to an obsessive patient then the patient will intellectualize'. L_1 is more general than L_2 since if an analyst makes a premature interpretation to an obsessive patient then an analyst is making an interpretation is logically true, while its converse is not. This criteria holds for all logical transformations of law-like statements such as 'All A is B'. Thus all mammals are warmblooded[1] is more general than all schizophrenics are thought disordered, since all schizophrenics are mammals is logically true while its converse is not.

If law-like statements cannot be compared by this criteria (neither All A is C nor its converse are true), there is another broader sense in which they may be compared. Simply stated (see Nagel, p. 41 for a full logical statement): Let a_1, a_2, ...a_n be a set of adjectives (or

[1]It should be noted that there must be some independent criteria other than warmbloodedness (B) for ascertaining that A is a mammal. Otherwise the statement is not subject to verification and is true by definition.

predicates) that can meaningfully, whether true or not, be applied to A. Similarly let c_1, c_2, ... c_n be a set of adjectives that can meaningfully be applied to C. If all the adjectives of the set a_1, a_2 ... a_n can non-absurdly be applied to C, but some adjectives of the set c_1, c_2 ... c_n cannot non-absurdly be applied to A, then L_1 (All A is B) can be said to be more general than L_2 (All C is D). For example if L_1 states 'All metals are electrically conductive', and L_2 states 'All humans are warmblooded', then we can conclude L_1 is more general than L_2 for the following reason. Any adjective that can be applied to metals can non-absurdly be applied to humans. Thus we can, though we may not usually, characterize humans as being or not being heavy, hard, malleable, etc. However, it would be absurd to characterize metals, either truly or falsely, as intelligent, warmblooded, thought disordered or the like.

This criteria can also be applied to entire theories where a_1, a_2 ... a_n and b_1, b_2 ... b_n are the descriptive expressions of theories A and B respectively. Thus we can, in this sense, say that physics is more general than biology since the descriptive expressions occurring in physics are descriptive of a more inclusive class of things than the descriptive expressions occurring in biology. To put this another way, physics abstracts from many things that are not included in biology. It

should be stressed that to say that one law or theory is more general than another law or theory is not to claim that the more general law or theory can explain the less general one. Though a satisfactory explanation must contain at least one premise that is more general than its explicandum, there are several other requirements that must be met. Some of these will be discussed in the next chapter. However, they are not pertinent to the present discussion.

For several reasons that will become apparent, the application of these criteria of generality to present-day psychoanalytic theory is at best a difficult task. Certainly it has not been systematically attempted. Obviously, it is difficult to characterize the law-like statements of a theory if those statements have not been clearly formulated. Even if laws and law-like theoretical statements are formulated, the terms contained in these statements are seldom carefully and consistently defined. For example, it is impossible to determine whether 'All ego-alien behaviors are accompanied by anxiety' is more general than 'All symptoms are discharge phenomenon' since it is not clear whether all symptoms are ego alien.

This difficulty is not obviated by applying the second, broader criteria of generality. This is once again because terms are defined, whether it be explicitly for observational terms or implicitly for theoretical

terms, inadequately. Particularly with reference to theoretical terms, there is frequently no consensus concerning what adjectives can non-absurdly be applied to particular theoretical entities. For example, there is no consensus whether the adjectives fearful, controlling, viscious, etc. can non-absurdly be applied to the superego; or whether the adjectives aggressive, libidinal, neutral can be non-absurdly applied to psychic energy. It should of course be noted that the determination of whether an adjective can non-absurdly be applied to a theoretical entity depends not only on the definition of the entity but on the definition of the adjective as well.

One more point concerning the logic of distinguishing between levels of theory on the basis of generality of statements remains to be made. Let us say that psychoanalytic theory had been developed and systematized to the extent its statements could be grouped and hierarchally arranged with respect to their generality. Let A, B, C, D, E. F and G represent theoretical statements so arranged from least to most general with each letter representing a class of equally general theoretical statements. (The same logical argument would hold for laws.) One could choose to label classes E, F, G as metapsychological, but someone else could choose to limit that designation to F, G. The choice would be dictated by preference not logic. One could decide to label only the most general

level G as metapsychological. However, as theories develop, statements at higher levels of generality may be formulated. Thus the designation highest is subject to change. Further, since theoretical terms are in principle never fully characterized, the level to which a particular theoretical statement is assigned may change as the theory changes. Therefore, in any but a completely developed and static theory, to designate a particular class of theoretical statements metapsychological on the basis of its level of generality is at best somewhat arbitrary. Given the present state of psychoanalytic theory, it seems premature.

Critics of Metapsychology

Given Rapaport's characterization of metapsychology as "most abstract," "least familiar," "least palatable," "removed from the clinical situation," etc., it is not suprising that many within the psychoanalytic community have chosen to disregard it and others, viewing it as a major cause of psychoanalysis' woes, have chosen to discard it. In this vein Mayman (1976) writes, "Rapaport's neglect of the data from the psychoanalytic process and his tacit downgrading of clinical theory have put some of the most important parts of this work in an essentially untenable position". (p. 207).

It may also be that great expectations have become great disenchantments. Rapaport seems to have implicitly

held out the promise of a revolutionary theoretical achievement. Metapsychology would be a theory that would explain not only the data of psychoanalysis but the data of academic psychology as well. His great personal enthusiasm (Gill and Klein, 1961; Mayman, 1976) may have helped sustain his students' faith. Perhaps too, the complexity of his formulations led to dissatisfaction; or the realization that though elegant they were frequently post hoc. Yet as Gill and Klein (1961) and Wachtel (1969) point out, research deriving from Rapaport's metapsychological formulation has fruitfully been pursued. Whatever the reasons, calls for the rejection of and announcements of the death of metapsychology have become commonplace. The next chapter will discuss some of the specific reasons offered for this rejection. The remainder of this chapter will briefly attempt to illustrate that metapsychology has been no more consistently defined by its critics than by its developers.

Klein

Klein's 1970 paper "Two Theories or One?" represents a milestone of sorts in contemporary psychoanalytic thinking. To quote from Gill's (1973) introduction," ... Here is a paper which was one of the first to reject psychoanalytic metapsychology wholesale but at the same time accord the clinical theory a dignity equal to that which most analysts now confer on metapsychological

theory" (p. 99). Klein accepts Rapaport's premise that psychoanalysis contains two kinds of theory. Though he retains the labels "clinical theory" and "metapsychological theory" or "general theory", he rejects Rapaport's contention that metapsychology is at a higher order of generality from or that it explains clinical theory. In fact, Klein is careful to stress that he views clinical concepts as being as abstract and as theoretical as metapsychological concepts. Further he writes, "...being speculations about mans behavior and experience, the clinical concepts constitute a general theory of behavior, no less than metapsychology" (p. 103). However, Klein seems to contradict himself when he soon afterward states that clinical concepts are closer to the activities of clinical observation, more responsive to pressures imposed by data, and hence potentially more capable of systematic modification. It is difficult to see how concepts can be equally theoretical and abstract yet differ with respect to their distance from the data, their responsiveness to change, and the like.

In any case, since Klein rejects Rapaport's definition of metapsychology, he must offer his own. In some instances he clearly identifies metapsychology with the "energy-discharge model" or the "drive-discharge theory" and in his 1976 book in the index under metapsychology it says, "see drive-discharge theory." Yet he also states

that the important difference between the two theories of psychoanalysis is not the energy-discharge model. Klein overcame this seeming contradiction by proposing that what distinguishes metapsychology from clinical theory is that they utilize two incompatible modes of explanation. He proposes that clinical explanation attempts to answer "why" questions and unlock meanings. It takes the subject's vantage point and is concerned with intentionality and purpose. Metapsychological explanation, on the other hand, attempts to answer "how" questions. It does not take the subject's vantage point and views the person as a physical process. It is concerned with the causes and the mechanisms of behavior. It is not an essentially psychoanalytic perspective. Further, Klein contends that those who attempt to place the economic model with a cybernetical or informational model are "of a mind not to relinquish the metapsychological effort, but to replace the metapsychological model" (1970, p. 107).

That these two types of explanation Klein outlines are incompatible is at least debatable. This will be discussed in the fourth chapter. In the context of the present discussion, the important point to bear in mind is that Klein has arbitrarily, though perhaps usefully, redefined metapsychology; and by strange coincidence, metapsychology as he defines it, is what is wrong with psychoanalysis as he perceives it.

Holt

Holt (1974, 1975) has recently joined the ranks of those calling for the abandonment of some large vaguely defined portion of psychoanalytic theory i.e. metapsychology. This seems to represent a change in Holt's line of approach. His earlier papers (1962, 1965, 1967) are well reasoned critiques of particular psychoanalytic theoretical concepts. He recognized that metapsychology "...is at best ... only a sketch and a program, not a developed and thoroughly elaborated model which can thoroughly explain a complete behavioral process" (1967, p. 32). A consequence of the looseness of the model, Holt seemed to believe, is that a criticism applied to one particular concept does not apply by logical extension to the entire model. However, for a reason that is not readily apparent, Holt began to characterize metapsychology monolithically, though not consistently. In some cases (1974), he adopts Klein's characterization. At another point (1975), he defines it with reference to its distance from the data, and at still another point (1974) by its untestability.

Schafer

Schafer's use of the term undergoes an even more striking transformation. From 1964-1970, Schafer attempted critiques and clarifications of what he freely, though vaguely, labeled metapsychological concepts and formulations. Like many psychoanalytic theorists he did not

define the term explicitly but used it frequently. For the most part, he used the term to refer to the five Rapaportian viewpoints, to the assumptions entailed in those viewpoints, and to the 'general theory' of psychoanalysis. While Schafer by no means felt that metapsychology was well developed, consistent or without serious flaws, it is clear that he viewed it as useful and open to change. He wrote:

> Nowadays, metapsychological discussion tends to be partial to the economic, structural, genetic, and adaptive points of view. (which) gives the impression - which, unfortunately, is usually grossly exaggerated - that metapsychology is so far removed from specifically psychoanalytic clinical observation and practice as to be unhelpful, if not irrelevant. ... It is partly in order to diminish the apparent distance between metapsychology and clinical practice ... that I shall emphasize the dynamic ... though I shall make use of the other metapsychological points of view too (1968, p. 12).

Further, Schafer (1970a) at one point seemed to feel that psychic energy was not a necessary assumption of metapsychology, and that metapsychology could survive its drastic redefinition or elimination. However, in the very same year (1970b) he writes: "...the entire metapsychology is committed to and organized around one basic assumption; this assumption is that it is necessary to postulate psychic energy for explanatory purposes."

That Schafer contradicts himself is of little consequence. He cannot be criticized for changing his views. However, he asserts as a fact something which is far from

obvious: that metapsychology is a logical structure built around psychic energy, and that criticisms of psychic energy are damaging to the entire structure. This appears to be once again a case of defining metapsychology to be that aspect of psychoanalysis that one wishes to criticize. A similar logic is apparent when Schafer later (1975) defines metapsychology as a language and then proceeds to criticize that language.

Arlow

Finally, Arlow's (1975) definition of metapsychology will be considered. Arlow was chosen because he is usually not considered a critic of metapyschology. In fact Holt (1975) specifically characterizes his work as metapsychological. Yet Arlow writes, "I tried to show how biological and metapsychological speculation has interfered with disciplined and consistent inference from the data obtained in the psychoanalytic situation" (p.522). This seeming paradox is accounted for by Arlow's somewhat idiosyncratic definition of metapsychology as, "a priori assumptions beyond hypotheses derivable within the clinical setting." He further states:

> By encompassing topographic, economic dynamic, structural, adaptive, and genetic concepts under the same heading, it is easy for psychoanalysts to fall into the assumption that these conceptualizations are of the same order of abstraction and of observation. Clearly this is not the case. Economic concepts, for example, based on the principles of thermodynamics borrowed from physics, constitute a priori assumptions applied to psychoanalysis. ... Structural concepts, on the other hand, may be

> viewed as organizing hypotheses which marshal the data of clinical observation without making a priori assumptions beyond data which can be derived from the psychoanalytic situation. (pp. 515-516)

Arlow's point that it is an assumption to place the six mentioned concepts on an equal level of abstraction is well taken. However, it is equally presumptuous to assert that economic concepts are a priori assumptions and that structural concepts involve no a priori assumptions. Certainly there are those that would argue that economic concepts pertain directly to and may be derivable from the data of the psychoanalytic situation (Horowitz 1977, Wallerstein, 1977) and others that would argue that the structural concepts involve untenable a priori assumptions (Holt 1974, Schafer 1976). This will be further discussed in the following chapter.

Chapter Summary

This chapter reviewed some uses of the term metapsychology. It was demonstrated that the term has never been consistently applied to a logically distinct class of theoretical statements or constructs. The uses of the term metapsychology discussed include:

1. Metapsychology refers to psychoanalytic theory.

2. Metapsychology refers to the highest or most abstract level of psychoanalytic theory.

3. Metapsychology refers to a system of organizing observational and theoretical statements into a number (three

to five) of categories or viewpoints.

4. Metapsychology refers to a quasi-mechanical system of explanation.

5. Metapsychology refers to empirical principles in accordance with which inferences are drawn.

6. Metapsychology refers to a system of explanation that includes only causal or mechanistic explanations.

7. Metapsychology refers to concepts involving a priori assumptions borrowed from other sciences.

8. Metapsychology refers only to those propositions concerning the neurological or biological substrates of psychic functioning.

9. Metapsychology refers only to the economic or drive-discharge aspect of psychoanalytic theory.

Though it is possible that certain of these definitions might be applied to a logically distinct class of statements, this has never been done with any kind of consistency. Further, certain of these definitions imply significant a priori assumptions that could restrict the scope of psychoanalytic theory. Additionally in light of the heretofore inconsistent use of the term, it is unlikely that any proposed single definition would gain general acceptance. Therefore, it is suggested that the term metapsychology not be used in future psychoanalytic discourse and that reference to past uses of the term include a clear statement of the intended definition.

CHAPTER III

RECENT CRITICISMS OF METAPSYCHOLOGY

This chapter will discuss some of the recent criticisms of psychoanalytic metapsychology. The criticisms to be examined share in common the fact that they have gained wide enough acceptence or at least sufficiently frequent mention to warrant critical examination. Additionally, most of the criticisms to be discussed are, at least in the way they are most often presented, extra-theoretical. That is, these criticisms are frequently directed at something other than at what a particular aspect of psychoanalytic theory purports to do - explain certain phenomena. Rather these criticisms take some vantage point external to the functioning of the theory. (In Chein's terms these might be termed metatheoretical criticisms.) In some cases the extra-theoretical vantage point is obvious. For instance, historical analyses in themselves can say nothing about the current functioning of a theory. In other cases a criticism might appear to be based on a particular view of the cognitive status of theories. In still other cases a criticism may be based on an a priori notion of what a psychoanalytic theory should look like. This chapter will examine examples of each of these types of criticisms.

One criticism that does not appear to be extra-theoret-

ical will also be discussed. This criticism is that metapsychology is anthropomorphic. As usually presented this criticism is vaguely stated and difficult to assess. This thesis will attempt to demonstrate that the determination of whether or not a concept or formulation is anthropomorphic is more complex than is generally acknowledged.

Obviously Extra-theoretical Criticisms

These are criticisms that say nothing about the scientific status of the theory or aspect of theory under scrutiny. However, criticisms of this sort frequently appear in discussions of metapsychology. Often they appear in a form such that their irrelevence is not immediately apparent. In this section four such criticisms will be discussed: 1. the historical analysis, 2. the argument by analogy, 3. the psychological analysis, 4. the ad hominon attack.

1. The historical analysis

This can take at least two forms. In the first a theoretical term or concept is placed within the historical framework or scientific Weltanshauung surrounding it genesis. Here the critic examines the historical or scientific concept under consideration and, finding that certain conditions in the context have changed, concludes that the term or concept is outmoded or at least questionable. The illogic of this type of argument is obvious, yet it appears with surprising frequency. Holt's (1965) "A review of Freud's biological assumptions and their influence on his theories" contains analyses of

this sort. For example Holt states that, in line with the tradition of the Helmholtz school, Freud assumed that scientific explanations must refer to forces and energies. Further, in agreement with most of the scientific thinking of the time, Freud viewed the organism as a closed system with a fixed amount of energy. Holt concludes that with the advent of information theory and open-system conceptions, the main arguements of the economic viewpoint are no longer valid.

A second form of historical analysis involves drawing comparisons between an historically outmoded term or concept and the concept under criticism. For example, Holt (1967) draws convincing parallels between the concept of psychic energy and discarded mechanistic and vitalistic concepts.

While historical analyses say nothing about the scientific status of a theory, and are therefore uninteresting in the sense mentioned previously, they can in other ways be quite interesting and valuable. They may aid in understanding the origin of a theory and may help in determining if and when concretistic adherence to the generating analogies of a theory may have been detrimental to the development of a theory; or they may aid in showing in what ways model and theory have become confused. However, to historically analyze a theoretical concept or theory in the ways mentioned says nothing about that concept's or theory's contemporary status. For example, in physics comparisons have been drawn between the discarded concept of ether and the contemporary concept of black body radiation. Yet no

one has suggested we discard the concept of black body radiation for reason of that comparison.

It should also be noted that appeals to historical precedent have no place in the defense of a scientific concept. In the psychoanalytic literature such appeals may take the form of turning to Freud for confirmation of a concept or formulation. In this regard, Hartmann, Kris, and Lowenstein write:

> Freud's authority within psychoanalysis led to the traditional and current abundance of Freud quotations in psychoanalytic writings. But rarely is the attempt made to select the quotation which fits into that phase of theoretical assumptions which the author has chosen to adopt. Quoting Freud is, as a rule, meaningful only if it is part of a laborious but unavoidable attempt to gain insight into the position of the quoted passage within the development of Freud's thought. To put it differently: quoting has sense only if the hierarchy of propositions is kept in mind. Without such caution, quotations of Freud help to embellish a statement and to lend it apparent authority, but they scarcely advance the progress of investigations. (1957, p.128-129)

2. Argument by analogy

This argument states that certain psychoanalytic (metapsychological) terms are the same as terms in other sciences. However since the concepts to which they refer are different, their use is illegitimate. The term referred to is almost always psychic energy or related concepts. This form of criticism has been most clearly stated by Holt (1965), and Rubinstein (1967); and borrowed in toto by Schafer (1976), Peterfreund (1971), and many others. This general acceptance is remarkable in light of the once again obvious illogic of the argument. A term in a theory means what a theory designates it to mean. The energy concept in psychoanalysis may

be a poor one. But if it is poor, it is poor because it is not useful (or perhaps because it is redundant) not because it is very unlike the energy concept of physics.[1] This will be further discussed later in this chapter in connection with Rubinstein's position that psychoanalytic theoretical terms must be protoneurophysiological.

It is the position of this thesis that such extra-theoretical discourse about the concept of psychic energy distracts attention from more important questions. That is, does the term psychic energy enter into statements that have explanatory power? Though a discussion of the concept of psychic energy is beyond the scope of this thesis, it should be pointed out that this is a much debated question. Many analysts such as Lustman (1969), Wexler (Panel, 1970), Horowitz (1977), state that they find the concept clinically useful, i.e. explanatory in the clinical situation. Others such as Rosenblatt and Thickstun (1970, 1977) and Swanson (1977) argue that this is illusory. They argue that the explanations are circular and simply rename the phenomena supposedly being explained. Two things are striking about this debate. First, it is global. Illustrating one instance of psychic energy being used in a tautological manner is held to be sufficient to brand the concept non-explanatory. Second, data from outside the clinical setting are simply

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[1]Similarly the defense of a concept by analogy has no scientific relevence. In this regard, Rapaport's (1959) likening of the energy of physics to the energy of psychoanalysis is both incorrect and unnecessary.

not considered. As Wachtel (1969) has pointed out, economic concepts have played a role in the development and testing of non-trivial psychoanalytic hypotheses by several contemporary investigators. Not mentioned by Wachtel but also deriving in part from economic formulations is the work of Silverman (1972).

Holt in 1967 wrote that the then current attempts to measure psychic energy, "have in common a noteworthy indirectness. Libido or hypercathexis can only be rated or estimated from behavioral signs that are coordinated to psychic energy by fiat, and the nomological net in which the theoretical statements using such energy are woven is far too loose-meshed to permit a crucial test of basic assumptions like a conservation law" (pp. 27-28).

Leaving aside that one tests hypotheses and not assumptions, Holt's criticisms still may be valid but they do not logically imply his conclusion that it is unlikely that the concept of psychic energy played an essential role in the generation of the studies he cites. Certainly it played at least an important heuristic role. Whether it played a logically necessary role can only be determined by formulating clearly the postulates underlying the assumption of psychic energy, and the correspondence rules which link it both to other theoretical concepts and to observational procedures. That these assumptions and correspondence rules are nowhere clearly stated provides the basis for a much more powerful criticism of the economic viewpoint than any possible comparison of psychoanalytic economic concepts with concepts in

other sciences can provide.

It should also be noted that once the above mentioned criticisms of psychic energy are accepted, frequently one and sometimes several illogical steps then follow. The most common goes something like this: Accepting that psychic energy is a fallacious concept, it then follows that all of metapsychology or even all of psychodynamics is counterfeit. To quote Schafer (1970,p. 436), "...metapsychology is committed to and organized around one basic assumption: this assumption is that it is necessary to postulate psychic energy for explanatory purposes." It then follows that, "It is time to stop using the mixed physico-chemical language of Freudian metapsychology" (1975 p. 41). It is only a brief further leap of illogic to simply label a term metapsychological and summarily dismiss those concepts which one finds bothersome. This appears to be what Holzman (1976) does with the concepts "presymbiotic and preindividuation." Yet it is unclear on what basis he designates these particular concepts metapsychological.

In certain of these criticisms there appears to be the implicit assumption that psychoanalytic theory is unified, cohesive and interdependent, and that a failing of any one theoretical postulate should cause the collapse of the entire theory or that part of the theory designated metapsychology by the critic.[1] Meehl (1970) and others have

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[1] In a different context Kennedy (1958) has noted that some critics seem to contradict themselves by arguing both that metapsychology is untestable yet at the same time is refuted by certain data.

pointed out that even in advanced sciences such as physics proper subsets of postulates are constantly being changed without significantly altering the rest of the theory. Certainly psychoanalytic theory is not more tightly integrated.

3. The psychological analysis

In this type of criticism, a developer or adherent of a particular theory is analyzed psychologically, usually with reference to his or her motives for developing or adhering to that theory. For example, Schafer, in discussing spatial metaphors, states:

> In fact, however, by using the spatial metaphor we introduce primary process modes of thought into systematic thinking, and so, as we do in the spooky theory of introjects, we contaminate the explanation with what is to be explained. In this light we can see that 'catharsis' expresses an anal-explosive fantasy! The anger that is pent up, simmers, explodes, or spills over expresses a volcanic anal fantasy; it is psychological content to be explained, not psychological explanation. 'Catharsis', thus is peculiarly well suited for expressing in an aseptic fashion archaic ideas about anger as a spatially localizable, destructive substance or quantity; it cannot be a useful theoretical term. (1972, p. 426)

One could as easily say that the concept of relativity of motion expresses a particularly borderline fantasy concerning the stability of the self. The difference is, of course, that few doubt the explanatory value of the concept of relativity while many doubt the explanatory value of the concept of catharsis.

Again, as in the case of the historical analysis, though not relevent to the scientific status of the theory under scrutiny, the psychological analysis may be interesting in other respects. It may shed light on the creative

process or, as Kuhn (1970) has done in reference to the theory of relativity, it may help demonstrate the non-historical development of a theory.

4. The ad homonym attack

This is rarely seen in blatant form in scientific discourse since it is clearly unscientific. To say something of the form: "He is a communist, Jew,metapsychologist, psychoanalyst, unanalyzable, etc., therefore his theory is questionable" is clearly an undefensable argument. However it occasionally appears in the guise of a historical or psychological analysis. For example Schafer writes:

> It is curious to say the least, that practitioners of a discipline that is so specifically concerned with human subjectivity and action should have continued to devote themselves to the impersonal rhetoric of natural science ... Analysands and metapsychologists sound strangely alike.(1976, p. 103)

Criticisms that Rely on a Realist Perspective

This section will focus on relatively recent criticisms put forth by Rubinstein and Schafer. In particular it will attempt to examine to what extent these authors' positions rely on a certain view of the cognitive status of theories. The criticisms to be examined are Rubinstein's assertion that metapsychology does not explain (and that it is dualistic), and Schafer's assertion that metapsychology necessarily reifies. Though other authors have made similar criticisms, and some of their work will be referred to, the logic of Rubinstein's and Shafer's positions is most readily analyzable.

Before discussing aspects of Rubinstein's and Schafer's

work, it is necessary to give a brief overview of different ways Nagel (1961) has categorized views of the cognitive status of theories. Central to the issue of the cognitive status of theories are two specific questions. (A) Is it appropriate to characterize a theory as being true or false? (B) If a theory is accepted does this imply an acceptance of the physical reality of its theoretical terms (nonlogical terms, concepts, etc.)? Restating this last question, are there some terms and concepts in theory-like statements that cannot in principle be directly verifiable? An example will help clarify the types of questions referred to.

Let us take the concepts in physics of instantaneous electron jumps and 3+Nth dimensional space. If one considers electrons as physical units (and for the sake of this example they will assumed to be) then in the statement 'electrons move from ring to ring instantaneously', all terms are couched in observational language. The question, however centers around whether there can be a physical meaning to the concept of electrons moving from ring to ring instantaneously. In this example it is assumed that whatever one's view of this statement, there is no question that during a certain time in the history of science, the concept of instantaneous electron jumps was (and perhaps is) the most useful concept available in helping to explain certain phenomena in atomic physics. Granting that this is true, the questions then become how does one view such a statement, and is there any way of verifying such a statement? Since

statements about electron jumps seem to elicit pictoral referents, it may be useful to consider concepts that elicit no such referents, e.g. statements that postulate more spatial dimensions than are accesible to sensory experience or to any known measuring instruments.

Let us assume that 3+Nth dimensional space is a useful theoretical concept in astrophysics. We know that it can be described mathematically. We can then ask questions A and B about statements that contain the concept 3+Nth dimensional space: (A) Is it appropriate to characterize such statements as true or false? and (B) If such a statement is accepted does it imply anything about the physical reality of the terms that enter into that statement? Again the answer to these questions depends on ones view of the cognitive status of theories.

Before briefly summarizing the different positions concerning the cognitive status of theories, a clarification is in order. This discussion is concerned only with the question of the cognitive status of a theory within a given period of time. That is, one could say that in their view all theoretical terms, even terms like instantaneous electron jumps or 3+Nth dimensional space, will be translated into directly verifiable terms, i.e. receive explicit definition. This may or may not be a position that one agrees with, but to talk of the future course of theoretical concepts is to take a metaphysical position, and this thesis cannot be concerned with debating metaphysical positions. The

different views of the cognitive status of theories will now be outlined.

The Cognitive Status of Theories

The realist view

According to this view it is a meaningful question to ask if a theory is true or false, even if the question can only be answered with less than certainty. A corollary of this view is that if a theory is well supported by empirical evidence, the entities ostensibly postulated by the theory (e.g. atoms, psychic structures, etc.) are regarded as having a physical reality. As will be shown, this view can be as broad or as narrow as one's definition of physical reality.

The instrumentalist view

This position maintains that theories are primarily logical instruments for organizing our experience and ordering experimental laws. Theories function as rules or principles in accordance with which empirical observations are analyzed or inferences drawn, rather than as premises from which factual conclusions are deduced; and they cannot therefore be usefully characterized as either true or false. As a consequence, the nonlogical terms of theories cannot meaningfully be regarded as being physically real.

The descriptive view

This position maintains that, "a theory is a compendious but elliptic formulation of relations of dependence between

observable events and properties" (Nagel, 1961, p. 118). According to this view, though the individual statements of the theory cannot be characterized as true or false, the entire theory can be so characterized inasmuch as it is translatable into observational statements. Theoretical terms like atoms are viewed as shorthand for a complex set of observations and not as physically real but unseeable. However, Nagel points out, that no one has demonstrated that the entities of a theory are translatable into the language of observation either in fact or in principle. And unless this translation can be demonstrated, this position, for all practical purposes, coincides with the previous one. For our purposes the two positions will be considered as one and referred to as the descriptive-instrumentalist position.[1]

To reiterate the distinctions outlined, proponents of the realist position maintain that it makes sense to speak of a theory as true or false and that if a theory is considered true, the entities postulated by the theory (non-logical terms) are held to be physically real. Conversely if those entities are not real, the theory cannot be considered true. Proponents of the descriptive-instrumentalist position, on the other hand, maintain that theories are to be regarded as instruments for inquiry and not usefully considered as true or false. Further, it would be inconsistent with this view to ask if nonlogical theoretical terms such as instan-

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[1]The descriptive position is logically like the realist position but practically like the instrumentalist.

taneous electron jumps actually occur since those terms function only as conceptual links in rules of representation and inference.

Although it might appear that the realist and descriptive-instrumentalist viewpoints are irreconcilable, Nagel (1961) demonstrates that this is far from true and that unless one takes a rather extreme realist position, "the opposition between these views is a conflict over preferred modes of speech" (p.152). What allows this reconciliation is the usually rather broad definition of physical reality. Most scientists who hold a realist perspective define real in such a way as to be compatible with a descriptive-instrumentalist perspective. Some senses in which the term physical reality is used will make this clear.

a) In one view every nonlogical term of a (theoretical or experimental) law is assumed to designate something physically real if that law is well supported and accepted by the scientific community.

b) A somewhat more restrictive sense of the term states that in order for a term to be considered designating something physically real, the term must enter into more than one experimental law with the proviso that the laws are logically independent from each other and none of them is logically equivalent to two or more laws. It should be noted that terms not fitting these criteria are not excluded from laws, they are simply not considered to refer to something real and may be viewed as stopgap concepts.

There are other more restrictive senses in which the term real is used, however these would require a more formal exposition. The important point is that even those who hold to the most restrictive sense of real mentioned by Nagel still allow for the place in a theory of terms not designating anything real.

There is however, at least one sense in which the term real can be interpreted that would lead to a realist position incompatible with a descriptive-instrumentalist position (and with most of modern science). According to this view, for any thing or event to be regarded as physically real, that thing or event must be capable of being publically perceived under certain specifiable conditions. If those conditions were not obtainable, there must be good reason to believe that the thing would be as expected if those conditions were realized. This extreme realist position is logically equivalent to an extreme descriptive position which would maintain that all entities postulated by a theory must be capable of translation into the language of observation. Either position (they are really different ways of saying the same thing) would lead to a view of theories quite different from that presented in Chapter I. Since all terms would be capable of explicit definition the result would be a compilation of law-like statements. The next sections will attempt to demonstrate that certain of Rubinstein's and Schafer's criticisms depend on such a position.

It should be noted that Rubinstein or Schafer might not

agree that certain aspects of his work are based on particular philosophy of science positions and might contend that the logic of his arguments is misstated. Nevertheless, even if this is the case it may be instructive to see how arguments similar to the ones to be outlined depend on a particular philosophic stance.

Metapsychology Does Not Explain

It may seem odd to characterize this criticism as extra-theoretical in that it is dependent on a particular restrictive view of the cognitive status of theories. In the first chapter a theory was defined as a comprehensive theory of explanation. Thus it would seem that to accuse a theory of not explaining would be a particularly cogent and potentially devastating criticism. In fact in the second chapter it was pointed out that it is difficult to assess much of psychoanalysis as a theory since it is difficult to clearly specify what psychoanalysis is attempting to explain. However in characterizing aspects of metapsychology as non-explanatory or "merely descriptive" Rubinstein (1967) does not, for the most part, concern himself with whether or not the theory is consistent with certain specifiable data. Rather he so characterized it on the basis of certain restrictive conditions for explanations.

Rubinstein concerns himself solely with deductive explanations.[1] He states that a deductive explanation of the

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[1]There are those who would appear to contend that psychoanalysis is not concerned with deductive explanations

form 'A explains B' can be said to be satisfactory if the following conditions are met: 1.) B is deducible from A. 2.) A is more general or abstract than B. 3.) A is reasonably well known to be true.

Before proceeding to the main point, a few vagaries of Rubinstein's position should be made clear. He states that, "the explaining statement must be a higher level of generality from the former; otherwise the latter would not be deducible from the former" (1967, p.22). This is not strictly true. 'All humans are mortal' is deducible from 'All humans have a finite life span', but the first statement is not more general than the second nor does it explain it. Presumably Rubinstein has in mind a notion of generality similar to the one discussed in the previous chapter. (He too refers to Nagel.) Presumably also Rubinstein takes for granted, but does not state, that the explicandum (B) should not logically imply the explanation. (A). More significant than these relatively minor oversights, is Rubinstein's only very cursory mention that theoretical explanations always consist of several statements. More will be said about this later.

The main point of discussion pertains to Rubinstein's third criterion for explanation, that "A be reasonably well

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(Szasz,1961; Home,1966; Rycroft,1966; Schafer,1976) and it is therefore not important if psychoanalysis does not explain deductively. Though not agreeing with this view, further discussion of this is beyond the scope of this thesis. See also Sherwood (1969) for an interesting view of psychoanalyic explanation and Eagle (1973) and Rubinstein (1973) for discussions of Sherwood's view.

known to be true." He contends that if A is false or probably false then the statement is not an explanation but a "pseudo-explanation"[1] or a "mere description." From this alone -the application of the criterion of truth or falsehood to a theoretical explanation- one might infer that Rubinstein held a realist perspective. A further examination of his position will reveal that it is a restrictive one.

Rubinstein interestingly demonstrates that it is possible to derive what he defines as higher level clinical hypotheses from certain "metapsychological hypotheses." In providing this demonstration he allows only symbolic (logical term) representation for all theoretical terms thus stripping them of their "everyday meaning." He states:

> The result of these maneuvers is an abstract system of largely uninterpreted formulas. It turned out that from this system one could deduce the higher-level clinical hypotheses and the clinical correlational statements that are deducible from the correspondingly verbally formulated metapsychological hypotheses. This result is of course what we would expect. The fact that the formulas behave as expected is, however, less trivial than it might seem. It shows that the metapsychological hypotheses can function as premises for deduction <u>independently</u> of whatever meaning we may give their terms. As a deductive system, the set of formulas may be said to represent the core meanings of the metapsychological hypotheses from which it was derived. (1967, p.61)

Rubinstein then offers three possible interpretations of the "core"meaning of the metapsychological terms, one

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[1]It is interesting that in a later paper Rubinstein (1976) states, "that <u>logically</u> an explanation is an explanation, whether true or false, newly discovered or obsolete" (p. 233).

"nonexistential" and two "existential." He asserts that the nonexistential interpretation is the same as a pure psychological theory and from it no empirical consequences can be deduced; that one version of an existential interpretation is mentalistic and thus necessarily dualistic; that the second existential interpretation is protoneurophysiological, and that since psychoanalytic theoretical terms such as psychic energy are not consistent with current neurophysiology, the statements into which they enter are not explanatory but merely descriptive. Each of these assertions will now be examined.

Let us first examine the assertion that a nonexistential interpretation is the same as a pure psychological theory. The most obvious difficulty with this assertion, as well as the other two mentioned, is the implicit view that either no theoretical terms refer to 'real things' (i.e. they are nonexistentially interpreted), or that they all refer to 'real things' (they are existentially interpreted). Since a theory must at some point touch base with the observational world of reality via correspondence rules, theoretical terms may be thought of as being more or less existentially interpretable, as they vary from close to explicit to only implicit definition. A theory in which all the terms are left nonexistentially defined would be a theory with no correspondence rules to observations and no model. Such a theory could yield no explanations nor could it even evoke suggestions of explanations. It in fact would not be a

theory but the abstract calculus of a theory. Thus it is difficult to understand the contention that if metapsychological hypotheses are interpreted nonexistentially they "...can be regarded as purely psychological ... Moreover, thus interpreted, only clinical (and other psychological) evidence can be relevent for their confirmation. Accordingly, the explanations they yield are strictly clinical also. According to this view, the only 'reality' that counts is reality describable in terms of the psychological thing-event language" (1967, p.62). Perhaps Rubinstein views psychological reality and clinical data as non-existential, but if this is so, it is at best a confusing application of the term. Gill (1976) similarly questions Rubinstein's use of the term nonexistential.

Rubinstein contends that a pure psychological theory cannot account for the relationship between behavior and experience and "other organismic events" (presumably meaning physiological events).[1] This is true since as Rubinstein defines it a pure psychological theory can account for nothing. This seems to be a misrepresentation of Rapaport's notion of a pure psychology. In fact in the Rapaport paper (1951) to which Rubinstein refers Rapaport discusses a "purely psychological model" and not a pure psychological

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[1]Holt's (1967) contention that, "...pure psychologies have been worthless to the clinician, who must remain true to his commitment to the physicochemical structure we call the body" (p.17) appears to rest on similar logic.

theory. It is not clear whether Rapaport was making the same distinction between models and theory referred to in Chapter I, however, it is clear that he believed that a psychological model might, at least some day, be related to the data of neurology and physiology.

Returning to Rubinstein's argument, he contends that to account for the relationships between behavior and body, theoretical terms must be interpreted existentially. At the risk of being repetitious, it should again be noted that this is a necessary argument only if one begins with a realist perspective. He continues that there are two possible existential interpretations of theoretical terms, the mentalistic and the proteneurophysiological. Further he states that to interpret theoretical terms mentalistically is to commit oneself to a dualist interpretation of the mind-body problem. Again this is true only if one views theoretical terms as referring to substantive, real entities.

Though these issues can only be briefly considered here, some discussion of the mind-body problem is in order. Rubinstein (1965) seems to accept uncritically Ryle's (1949) argument that mentalism necessarily implies dualism. Briefly this argument proposes that if theoretical terms do not refer to neurological entities, behavior, or at least behavioral dispositions, they must refer to some other substance. Fodor (1968) and Wilson (1973) convincingly counter that even if one holds to a realist (in their terms materialist) perspective, one can be a mentalist without being a dualist.

Their counterargument might be summarized as follows: Let us say a theory contains mental terms that are defined without reference to behavior. The mental terms are logically distinct from behavior descriptions or dispositions and cannot be explicitly defined by them. However we can still maintain that human beings are physically real. Thus it is maintained that mental terms and physical terms may apply to the same real things, i.e. human beings.

Of course one could use mentalistic terminology and be a two substance Cartesian dualist; or be a descriptivist-instrumentalist who thinks the whole mind-body problem is irrelevent. The only philosophical position that would prima facia not allow mentalistic terminology is an extreme realist one in which terms must apply to verifiable entities. The use of mentalistic terminology in psychoanalytic theorizing will be further discussed in the section on anthropomorphism.

According to Rubinstein the third way that one can interpret psychoanalytic theoretical terms is neurophysiologically. If this is done he says, then one can decide if psychoanalytic theoretical statements are explanatory or merely descriptive. In order for the statements to be considered explanatory the terms that enter into them must be defined in such a way as to be in accord with current neurophysiology, or at very least they must not be implausibly neurophysiological. In Rubinstein's terms they must be protoneurophysiological. Thus since psychic energy as tra-

ditionally defined (having quality, being directional, etc.) is not protoneurophysiological, statements into which it enters must be considered only merely descriptive.

This is a strange position for a scientific discipline to be in. It connotes a kind of reductionistic thinking that holds if one's findings are at variance with a more "basic discipline" then there must be something wrong with those findings. This type of thinking is based on certain metaphysical assumptions about what ultimate psychoanalytic theoretical explanations will look like, i.e. they will be physico-chemical. Whether or not one agrees with that metaphysical position there is a more practical issue involved. The notion is that current neurophysiological propositions are more valid or more likely to be ultimately true than current psycholanalytic propositions. This is certainly not a logically necessary position, yet is one that Rubinstein, Holt, and Amacher seem to propose. In a paper in preparation, Ellman and Moskowitz (1978) examine some of the evidence that both Rubinstein and Holt (1965) cite as damaging to psychoanalytic economic propositions. In particular they examine the contention that findings from intracranial self-stimulation (ICSS) studies conflict with a drive reduction hypothesis of motivation. They start by interpreting the psychoanalytic hypothesis as simply stating that mammalian organisms strive for reduction of internal stim-

ulation.[1] The question then arises; even if these hypotheses are stated in this simple form, do results from self-stimulation research contradict a drive-reduction hypothesis. In the first ten years of self-stimulation research the answer would unequivocally have been that findings from ICSS studies do not lend support to a drive-reduction hypothesis. However, they conclude that some more recent research not only does not contradict a drive-reduction hypothesis but may in fact lend support to some forms of such a hypothesis. Certainly findings which Rubinstein terms "experimentally confirmed" are no longer well accepted or at least are actively disputed.

Rubinstein seems not to allow for the fact that current neuropsychology and current neurophysiology are developing sciences with changing theories. The requirement that psychoanalytic theoretical terms be protoneurophysiological makes no more logical sense than the requirement that neurophysiological terms be protopsychoanalytic. This is not to say that psychoanalytic theory should not be or is not influenced by current neuropsychology and neurophysiology, but the influence may as well go in the opposite direction (Wilson, 1973; Adrian, 1946).

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[1] It should be noted that Rapaport (1960) has discussed the difference between Freudian and Hullian concepts of motivation, as well as psychoanalytic hypotheses that would account for stimulation seeking behavior.

Metapsychology Reifies

Reification "consists in mistaking as things entities which are not things (Carnap, 1956)," or in treating an abstract noun such as 'reason' as though it referred to some thing-like entity (Reichenbach, 1951). For Hull (1943), "to reify a function is to give it a name and presently to consider that the name represents a thing, and finally to believe that the thing so named somehow explains the performance of the function..." (All quotes in Eacker, 1975, pp. 33-34).

Reification has long been considered a potential problem in psychology. This is especially true if one takes a realist perspective since one is loath to use nominatives unless they are well tied to observational procedures. Even so, given most definitions of the term 'real', many non-palpable theoretical entities may be so designated at least provisionally; and those nominatives not designated as real may be accepted as intervening variables.

Reification becomes a problem when adjectives are carelessly transformed into nouns, and those nouns are taken to designate (explanatory) entities. A similar problem can occur when a noun that is used to classify events or entities is used to explain that which it classifies. This has been referred to as the tendency to confuse naming with explaining. It should be stressed that reification is generally viewed as an error that can be avoided by more careful formulation and not as an inherent flaw of a particular theory.

Schafer (1972) appears to be referring to the problem of reification when he writes:

> ...The habit of thought or type of language I am calling into question is one that relies on concretistic, substantial references to abstract or at least no substantial ideas. The fact is that habitually we speak of thoughts, feelings, motives, and entire personalities as though they had the properties of things such as extention, location, and momentum" (p. 421).

Schafer, however, goes much further than simply reminding us to avoid reification. He appears to be saying that we should not describe nominatives which are not palpable with adjectives usually reserved for thing-like entities. It is proposed that this view implies a restrictive realist perspective and probably derives from a view of language in which words cannot mean what one chooses them to mean, but in an uncontrollable way continue to connote and evoke old meanings.

This line of thought can be seen earlier in Schafer's work. In "The Mechanisms of Defense" Schafer (1968a) cites the Oxford English Dictionary definition of mechanism as, " 'a structure, or mutual adaptation of parts, in a machine or anything comparable to a machine'; also as 'a system of mutually adapted parts working together.' " He then writes:

> Accordingly, the word cannot fail to suggest machinery, substance, fixed arrangement of parts, automatic processing and exact prediction of output. Thus, when applied to mental processes, mechanism has unfortunate implications: although numerous psychoanalytic propositions have mechanistic aspects, psychoanalytic thought itself is, I believe, fundamentally antipathetic to machine models of human functioning. (p. 52)

Here we can see one type of argument that pervades Schafer's later work: that the ordinary use of a word has

implications for its scientific use. In this case Schafer goes on to state that the semantic consequences need not be decisive and, "The best way to establish the meaning of a word or a term if to study its uses and their effects..." (1968a, p. 52). However, this attitude is not sustained in later works.

In "Internalization: process or fantasy" (1972) Schafer clearly declares his realist perspective. He writes that to conceptualize the mind spacially,

> ...is to be guilty of reification, that is, to be mistaking abstractions for things. For mind itself is not anywhere; logically, it is like liberty, truth, justice, and beauty in having no extention or habitation, requiring none and tolerating none. It is pure abstraction. The boundaries of mind are those of a concept, not of a place. Only certain referents of these abstractions have place and substance. (p. 416).

Schafer follows this reasoning and then dispenses with all terms that have special connotations, and in later work, with all terms that have thing-like connotations. That this position rests on an extreme realist perspective is apparent if one looks at theoretical terms in other sciences. Fourth dimensional space, and gravitational fields, for example, are terms which have spatial connotations. It is further perfectly acceptable to speak of inside or outside of gravitational fields or fourth dimensional space. Yet these terms may be considered as abstract as liberty, beauty, and the like, and are certainly not names for palpable entities. What gives these terms theoretical dignity is that they fit into a theoretical matrix, enter in explanations, and are linked, even if very loosely, by correspondence rules to

observational procedures. There is nothing inherent in psychoanalytic internalization terms that would prevent them from functioning similarly. In fact, it would appear that this is what Schafer (1968b) attempted to do in Aspects of Internalization. The terms were placed in a theoretical matrix and law-like theoretical statements were generated. Examples of such statements are: "Objects appear to become introjects (to be introjected) in crisis," and "Typically, introjection decreases the influence of the external object" (1968, p. 73). What is missing from Schafer's attempt are correspondence rules that would give these terms independent characterization. Again, however, there is nothing logically to prevent this. Blatt's (1976) Rorschach studies demonstrate one possible line of research in this area.

As stated previously Schafer's restrictive realist perspective seems to stem, at least in part, from his belief in the great importance of words. He is concerned both with the ordinary language connotations of some psychoanalytic theoretical terms (e.g. internalization words) and the physical science connotations of others. Concerning the physical science connotations he writes:

> ...It is high time we stopped using this mixed physio-chemical and evolutionary biological language altogether. I am referring to the eclectic language of, on the one hand, force, energy, cathexis, mechanism, and sublimation of neutralization, and on the other hand, function, structure, drive, object, and adaptation, (1976, p. 3)

His concern with the ordinary language connotations of theoretical terms is seen in his discussions of defense,

internalization, and resistance, among other concepts. The following quote is illustrative:

> There is a set of terms that makes up a negative and an incomplete and thereby potentially unanalytic perspective within psychoanalysis. Among these terms are resistance, defense, negative therapeutic reaction, negativism...Left in their negative and incomplete forms, these words are pejoratively tinged.(1973, p. 280)

Schafer gives no cogent reasons why terms cannot be borrowed from other sciences and ordinary language and redefined and "mixed" in whatever manner is useful. Perhaps he believes that the language must in some way fit the subject matter. For instance, he states that a language of human behavior must differ from a language of animal behavior. He writes:

> I have devised my alternative specifically for human beings. We show properties of mental functioning which cannot be attributed to any other creature. We are unique in existing culturally and intellectually through a language that embraces self, other, time, metaphor, hypotheticals, morality and so forth. Therefore we must build our discontinuity with respect to all other species into any psychological theory we attempt. (1976, p. 8)

Schafer's proposed discontinuity of humans with all other species is at best an unexamined assumption, and certainly not in accord with much contemporary ethology or psychology.

It is important to stress that some of Schafer's criticisms are undoubtedly well founded. It is certainly possible to find many instances of naming in the guise of explaining, value judgements posed as scientific statements, and the like. But accepting these criticisms does not

necessitate a shift to a restrictive realist perspective and a consequent impoverishment of theoretical language. With reference to language, it should be noted that not only does Schafer's proposed action language seem to stem from a restrictive realist perspective, but that its adoption and strict application would impose that perspective on any future theoretical discourse. An examination of the rules of action language (1976, pp. 9-15 and 363-373) show quite clearly that they do not allow for the postulation of theoretical entities. As such any 'theory' that might be developed following these language rules would not be a theory in the usual sense but a compilation of law-like statements. Perhaps Schafer should not be criticized for this for it is clearly his intent that psychoanalysis not be a "natural science," that it should refer to reasons not causes, that it make no distinction between explanation and description, and so on. The contention that psychoanalysis need be a different sort of science will be examined in the next chapter. However, two more points about action language are relevant to the present discussion. First, one may accept some version of action language without accepting its premises or carrying it through to its logical conclusion. For example, accepting that observational and theoretical languages in psychoanalysis are often confused, one might choose to use some version of action language as the observational language, bearing in mind that greater specificity of description does not rule out attempting to formulate explanations. Alternately one might accept Schafer's (1972, 1973, 1976) position that action language is

a better language for clinical interpretations in that it highlights disclaimed action. This could be empirically tested.

The second point is that Schafer's action language may not be as free of theoretical terms as he might like. In particular, person and action are nouns that do not seem to receive explicit definition. It may be contended that person is clearly an observational term, and Schafer would contend the same is true of action. For instance, one class of unconscious actions includes actions that occur out of awareness and are observationally verifiable. The same claim would probably be made about conditional action and higher order actions, that they may be observationally verified by observing dispositions to action and constituent actions. However, it is questionable if by being thus verified they are in fact explicitly defined. This appears to have been Ryle's (1949) viewpoint and one that Schafer has accepted. This point of view has come under increasing criticism in the philosophy of science (Fodor, 1968). It remains for those who hold such a view to demonstrate that those observations and explanations formulated in mentalistic-entity language can be translated or reformulated into an action-dispositional language. Such a reformulation has been attempted only for a relatively small number of statements. It would be an interesting task to attempt such a reformulation for some rather large and complex theory-like construction such as the theory of neuroses or dream theory.

Before leaving this section, it is worth noting that many others have criticized metapsychology for its reifications, though no one else would seem to propose banning all theoretical terms for fear of their being reified. Holt (1975) views the concept of the ego, and by extention structural theory, extremely difficult to use without reification. In particular he refers to statements like, "His lack of self control is explained by his weak ego." Though this seems like an obvious example of naming in the guise of explaining, weak ego being another name for lack of self control, it is the position of this thesis that this determination cannot be made by the examination of a single statement in isolation. The determination of whether a term is a reification depends on its position in its overall theoretical context, and in particular whether it receives independent characterization. The notion of independent characterization is a crucial one and will be fully discussed in the section on anthropomorphism.

In an interesting paper Wilson (1973) has persuasively argued that a fear of reification may lead to a structural hypothesis of severely limited scope and reduced potential explanatory power. In particular he refers to the claim that psychic structures be defined by their functions (Hartmann, Kris, and Lowenstein, 1946; Hartmann, 1950; Arlow and Brenner, 1964; Beres, 1965) or as classifications for organizing clinical data (Beres, 1965; Arlow, 1975). Such a claim if carried to the extreme limits theoretical terms to abstractive accounts of data , i.e. what might be

termed a restrictive descriptivist position. Wilson points out that Hartmann and his collaborators, in fact, end up quite loosely defining the psychic structures. The particular functions attributed to each structure shift. The structures are also defined in relation to each other and to external reality, and to other theoretical concepts and the like. However, Wilson contends that attempts to specify the meanings of these psychic structures solely in terms of empirical content is misguided and reveals that many psychoanalytic theoreticians have failed to take note of "the most important aspects of the contemporary philosophy of science" (1973, p. 324).

Wilson's contention may be overstated. Certainly many psychoanalytic theorists refer to Nagel; and Nagel makes much of the notion of implicit definition of theoretical terms. Rather it seems that psychoanalytic theorists continue a long tradition of debating the meaning of theoretical terms, while largely ignoring the fact that they have not agreed upon a body of observations to be explained.

Metapsychology is Anthropomorphic

The charge of anthropomorphism has been leveled against metapsychology by many recent critics, among them Holt (1965, 1975), Peterfreund (1971), and Schafer (1976). Schafer writes:

> In this chapter I focus on the anthropomorphism that both pervades and artificially sustains Freudian metapsychology. I identify the manifestations of this anthropomorphism and argue that it is an inescapable consequence or correlate of Freud's mechanistic and organismic mode of theorizing.

A few points about this statement are in order. First, Schafer is stating not just the contention that anthropomorphism can be found in metapsychology, but that it is in some way inherent in a particular mode of theorizing. This would be an extremely difficult, and very important, contention to prove for any vaguely stated theory (i.e. psychoanalysis), and Schafer in no satisfactory way attempts to demonstrate its proof.

Secondly, Schafer in his assertion leaves key words undefined - anthropomorphism, mechanistic,[1] organismic - and leaves them so. It is interesting that he is not alone in not defining anthropomorphic. Even Nagel who spends much time defining key terms and speaks of, "...the pathetic fallacy of lapsing into anthropomorphism, (1961, p. 402)" gives us no good definition of this term. Perhaps he, as Grossman and Simon (1969), are content with a rather vague definition of anthropomorphism as "ascribing any human attribute to something other than man." (p. 84) As we shall see later a more rigorous definition is needed.

Since Schafer appears to rely heavily on Grossman and Simon for confirmation of his assertion that metapsychology is anthropomorphic and many others refer to them, it is important to examine their paper. Grossman and Simon in reviewing the literature on anthropomorphism in psychoanalysis nicely show that opinion is sharply divided about

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[1]Nagel (1961) devotes an entire chapter to defining mechanism and mechanistic explanation.

whether anthropomorphic formulations need to be expunged. Few would disagree that anthropomorphism exists. To cite a few quotes from their article: Freud said, "it is not at all necessary to outgrow it [anthropomorphism]. Our understanding reaches as far as our anthropomorphism." Laplace agrees, "...one sees that the model here is no longer one borrowed from the physical sciences, but is thoroughly marked by anthropomorphism. ...To this extent, the scientific theory of the psychic apparatus tends to approach the manner in which the subject sees himself in fantasy and, perhaps, has even constructed himself." On the other hand, more in the spirit of modern science, Hartmann writes, "An occasional lack of caution in the formulation of its propositions, or Freud's liking for occasional striking metaphors, has lead to the accusation against analysis of an anthropomorphism of its concepts. But in all those cases a more careful formulation can be substituted which will dispel this impression."

Seemingly contrary to the trend of contemporary science which views anthropomorphism as an impediment, Masserman writes, "the allegation of 'anthropomorphism' is itself a tautology since all data is derived from the personal experience and interpretations of the individual and all conceptual terms have reference to essentially human forms and meanings. In effect, then, all 'systems of thought' in all languages, whether applied to the conduct of electrons, cats, or theorists is basically 'anthropomorphic' and the term itself becomes particularly redundant when applied to any theory of behavior whatsoever." (This is one way of

making a term meaningless.) Thus not all psychoanalytic authors agree with Nagel that anthropomorphism is "a pathetic fallacy"; that is, there is question about whether the criticism, if true, is devastating. Again, it will be shown later that this is due to the term being relatively undefined.

Grossman and Simon turn to the work of Hartmann and his collaborators to see if those authors who see anthropomorphism as a flaw have successfully expunged it. They ask, can the anthropomorphism be removed from psychoanalytic theory either by more careful formulation of psycholanalytic propositions or by a change in terminology? Grossman and Simon rather convincingly demonstrate that at least up until now, it has not. They particularly focus on the use of physical models. They state that the physical model is not particularly empathetically evocative so that it lends an impersonal quality to the formulation. "[The] abstractness and utility of the formulations can be illusory, however, unless these formulations can be systematically interrelated and shown to have referents other than the observation metaphorically represented"(Grossman and Simon, p. 100)." At best, renditions of physical theories in ordinary language represent a limited sense of the full force of the physical conception which is to be found in mathematically formulated relationships.

Discussing the use of the term tension in an example from Hartmann, Kris, and Lowenstein, Grossman and Simon state:

> the substitution of tension for the clearly anthropomorphic terms of self-love, self-approval, and the ego being love objects of the superego does not seem

> entirely successful. 'Tension' in this setting appears to be evocative and not strictly denotative. It is a subtle form of evocation, however, since its use depends on the fact that it refers both to the experience of tension and to tension as a physical concept...Hence, 'tension' serves merely as a restatement of the proposition that there is a conflict between two psychic agencies.(p. 102)

Concluding their discussion of anthropomorphism, Grossman and Simon assert that there are two levels of psychoanalytic theory: "clinical psychoanalytic theory" and "higher order explanatory propositions." They find anthropomorphic language compatible with the former but not with the latter though it pervades both. They relate this to the distinction between meaning and motive on one hand and causality on the other. As was shown in the previous chapter, such a distinction is questionable. While not questioning the utility of the stated dichotomy, Grossman and Simon do hope for the future development of a superordinate schema that would encompass both kinds of discourse.

We are, still, left with several crucial questions. Is the charge of anthropomorphism a devastating one? Is it in fact true that anthropomorphic sounding explanations have clinical utility, or is that utility illusory? What is the relationship of anthropomorphic or mentalistic concepts to other theoretical concepts? Does it make any sense to speak of anthropomorphic or mentalistic conceptualizations as 'stand-ins' for future physical conceptualizations?

Thomas Nagel (1974) offers an excellent consideration of some of these questions. He begins with the premise that Freud believed that the psychic apparatus he was describing

was in reality a physical system though too little was known about neurophysiology at the time to permit the development of a psychology in physical terms. Whether Freud believed this fully, or believed it at all is besides the point. The important question concerning this that Nagel raises is:

> ...in what sense is it possible to think about a physical system in mentalistic terms, taken from the vocabulary of experience, perception, desire, etc., without having any idea of the physical significance of those descriptions. This question bears not only on psychoanalytic theory, but also on current disputes about the status of mentalistic terms in linguistics, and in other areas where it is maintained that a mentalistically or anthropomorphically described process can have a physical realization. What is the meaning of such claims? (p. 11)

Freud's stand on this question seems clear:

> The hypothesis we have adopted of a physical apparatus extended in space, expediently put together, developed by the exigencies of life, which gives rise to the phenomena of consciousness only at one particular point and only under certain conditions - this hypothesis has put us in the position to establish psychology on foundations similar to those of any other science, such, for instance, as physics.... Reality will always remain 'unknowable'. The yield brought us to light by scientific work from our primary sense perceptions will consist in an insight into connections and dependent relations which are present in the external world, which can somehow be reliably reproduced or reflected in the internal world of our thought and a knowledge of which enables us to 'understand' something in the external world, to foresee it, and possibly to alter it. Our procedure in psycho-analysis is quite similar. We have discovered technical methods for filling up the gaps in the phenomena of our consciousness, and we make use of these just a physicist makes use of experiment. In this manner we infer a number of processes which are themselves 'unknowable' and interpolate them in those that are conscious to us. And if, for instance, we say: 'At this point an unconscious memory intervened,' what this means is: 'At this point something occurred of which we are totally unable to form a conception, but which, if it had entered our consciousness, could only have been described in such and such a way.'(1940, p. 196 - 197)

It would appear, then, that Freud's contention is that psychoanalytic theoretical terms and mental imagery stand in the same relationship as the theoretical terms of physics and visual imagery. Mental imagery in the first case and visual imagery, in the second, provide a model which fleshes out the theory. T. Nagel raises several questions about this viewpoint. Two will be considered here: First, is the analogy of the mentalistic imagery of psychoanalysis with the visual imagery of physics accurate? Second, does this view supply a rationale for the employment of mentalistic concepts, taken from the psychology of consciousness, in theorizing about processes about whose physical nature we are presently unable to form a conception?

With reference to the first question, T. Nagel says (this is largely paraphrased) it is certainly true that we find visual imagery helpful in thinking about structures that are invisible either because they do not reflect light or they are too small. Thus we can visualize the DNA molecule as a double helix. Does this mean that if our vision was acute enough that is how it would look to us? Perhaps in some cases such as this, but for other theoretical notions such as photons, fourth dimensional space, visual characterizations of relativistic situations, etc., this realistic perspective seems to make little sense. It appears rather that we believe that there is some structural similarity between the 'invisible' thing that we are talking about and some other visible thing. An important consequence

of this is that the 'thing' being imagined can be independently characterized. For instance, a double helix can be described in purely geometrical terms, without reference to its visual appearance, and it is the former and not the latter that the DNA molecule and the visual model have importantly in common.

It is generally contended that such independent characterizations for psychoanalytic theoretical terms are lacking. For example, there appears to be no independent way of characterizing, "those unconscious mental processes which if they had entered consciousness, could only be described in such and such a way." Criticisms of this type have been offered by E. Nagel (1959), Hook (1959), Rubenstein (1967), Grossman and Simon (1969), T Nagel (1974), and many others. Two potential rebuttals will be offered, the first mine and the second T. Nagel's.

The first concerns the relatively undefined concept "independent characterization." This concept is unclear. Most broadly it seems to mean that in order to be so characterized a theoretical term must enter into explanatory statements about at least two "qualitatively different" phenomena. But now we are left with the undefined notion of qualitatively different phenomena.

To take an overly simple example, if the term psychic energy enters only into explanatory statements concerning fatigue (which for the sake of this example is measurable), then the term has no independent characterization. We could say that fatigue is in some way related to the capacity to

do work, and therefore psychic energy is somewhat like physical energy which can loosely be defined as the capacity to do work. But at best, all we would have then is a vague analogy. Using the conceptualization of Chapter I, we could say that unless psychic energy could be related to something different from fatigue, then it could be said to be explicitly defined by its relationship to fatigue, and the term could be replaced by that relationship in any statement without loss of meaning.

What, however, if psychic energy were used in explanatory statements concerning disordered thinking (e.g. Silverman, 1972) or learning (e.g. Schwartz and Schiller, 1969)? Are these phenomena qualitatively different from fatigue? It would appear so, but Rubinstein (1967), for example, would contend otherwise. He would contend that fatigue, disordered thinking and learning are all psychologically defined phenomena and for a psychoanalytic theoretical term to have value it must be independently characterized by phenomena in other sciences (or be potentially so characterizable). In the case of psychoanalysis he chooses neurophysiology. However, there is no obvious reason for this choice nor does he offer any cogent arguments for the contention that the independent characterization of a theoretical term must come from another science. Certainly a theoretical term in physics, like heat, can be independently characterized solely by reference to qualitatively different physical phenomena.

It may in fact be that some theoretical terms in psycho-

analysis have no independent characterization and are therefore redundant or mere reifications. However, the determination of this in all but trivial cases would require a greater explication of the logical structure of the theory than has yet been attempted. It should also be noted that in the realist perspective this discussion is probably moot, since all theoretical terms are viewed as potentially capable of explicit definition. That is, they are seen as merely stopgaps in the pursuit of lawful statements, and the task of science is seen as demonstrating the lawful relatedness of all phenomena and their consequent qualitative similarity.

The second rebuttal to the contention that psychoanalytic theoretical terms have no independent characterization is offered by T. Nagel. He states that even if we cannot demonstrate independent characterization of, let us say, unconscious mental processes, we can speak of the possibility of this characterization. Thus, although at present we are unable to form a conception of an unconscious process, we suppose that it shares certain features in common with corresponding conscious mental processes and that those features are partly responsible for a process appearing in consciousness in the form it does. It may be that we do not now possess the vocabulary or concepts for describing the common features. There is nothing to say that they must be describable in terms of current neurophysiology. Nagel continues:

> They may be describable only in the terms of a future psychology whose form will in part be determined by the development of psychoanalytic theory. ...This interpretation makes Freud's mentalistic discourse about what he regards as a physical system comprehensible, and makes the analogy with visualization in physics acceptable, though not so close as might initially appear. Instead of inferring specific similar causes from similar effects, he infers similarity of causes in unknown respects from observed similarity of effects. (p. 16)

A difficulty with this view lies with one very significant assumption:

> It assumes that there is some definite objective character or disjunctive set of characters common to the states that are ordinarily grouped together by their similarity of appearance to consciousness (and their contextual and behavioral connections and significance.) ...(If we assume a conceptualization of these characteristics must await the development of a future psychology), to assume that an objective psychology, whose concepts refer to physical phenomena, will roughly preserve the distinctions and categories embodied in commonsense mental concepts, is to assume a great deal. (T. Nagel, p. 17)

Further one might rightly question the utility of retaining a theoretical term which currently has no independent characterization, but which in the future might be independently characterized, if one very significant assumption proves to be correct (even though there is no good reason to assume it will be). There would seemingly be little utility unless one has glimmerings of the future independent characterizations, and that is what this thesis proposes to be the case in contemporary psychoanalysis. There are many terms which seem to enter into the explicands of qualitatively different explicandum. This is seen in the experimental studies cited previously and also may be inherent in statements that refer to "clinical utility." That is,

one often hears statements like, "I know the theory is no good for a variety of reasons, but I find it to have great clinical utility. It helps me in working with patients." This is like Grossman and Simon's contention that anthropomorphic language can be clinically useful. What does this mean? It may mean that there is an unexplicated theory of clinical inference, and that the aforementioned psychoanalytic theoretical terms do have independent characterization in the clinical situation. Determining whether this is in fact true or illusory awaits a comprehensive study of clinical inference.

If we assume that the analogy of mentalistic-anthropomorphic imagery in psychoanalysis to visual imagery in physics is at least provisionally acceptable, then the next question appears to be: Does this view provide justification for theorizing about the central nervous system, or other physical systems, in mentalistic-anthropomorphic terms? So far it has been argued that these terms may in principle be used to refer to physical processes of which, at present, we are unable to form a physical conception. That is not to say, however, that a useful theory can be constructed using these terms. As was stated with reference to models in physics, these models are helpful but they are not sufficient to make a theory. The concepts represented by the model must be represented more formally in terms of mathematical or logical relationships to one another. It is these formal relationships and not the phenomenal model which makes a theory useful. T. Nagel therefore asks:

> Why should it then be expected that our understanding of the brain can be advanced by theorizing with phenomenal concepts of a mentalistic type? Desires and aversions, pleasures and pains, intentions, beliefs, and thoughts certainly provide very useful explanations of what people do. But is there any reason to expect that further refinement and systematization of these explanations will yield a theory of how the central nervous system operates? (p. 20)

Further, even if a mentalistic explanation succeeds in explaining certain behaviors there is no reason to believe that it is "true". That is, one may correctly say that a person behaves as if he is unconsciously following certain rules, but it may be questionable if he is in fact really following them.

This deserves two comments. First, it is not true that phenomenal concepts have been shown to provide useful explanations of behavior. One might interpret that to be Schafer's contention as well as perhaps the contention of those who believe in the development of a 'pure' clinical theory. At this point this contention is far from proven. Second, many psychoanalytic theoretical terms are non-phenomenal, for example, structure and neutralization, even though they may have anthropomorphic referents as do force and energy.

We have left aside for the moment the continuing question of what is meant by a mentalistic or anthropomorphic explanation. Chomsky, (1968, quoted in T. Nagel) who uses mentalistic terminology freely, states:

> It is an interesting question whether the functioning and evolution of human mentality can be accomodated within the framework of physical explanation, as presently conceived, or whether there are new

> principles, now unknown, that must be involved, perhaps principles that emerge only at higher levels of organization than can now be submitted to physical investigation. We can, however, be fairly sure that there will be a physical explanation for the phenomena in question, if they can be explained at all, for an uninteresting terminological reason, namely that the concept of 'physical explanation" will no doubt be extended to incorporate whatever is discovered in this domain, exactly as it was extended to accomodate gravitational and electromagnetic force, massless particles, and numerous other entities and processes that would have offended the common sense of earlier generations. (p. 22)

T. Nagel raises the question of how a mentalistic theory would have to develop so that it would come to be considered a physical theory. He importantly stresses that a theory from which the mentalistic character can be removed without loss of explanatory power is essentially not mentalistic. What brands a theory as mentalistic or anthropomorphic is that its explanatory value cannot be recaptured in a non-anthropomorphic version. Mentalistic descriptions and explanations have to be understood by assuming the point of view of the subject of the mental states and processes referred to.

Therefore, it is not the words that are used but the theoretical context into which they fit that determines whether an explanation is anthropomorphic. For example, to speak of 'the rage of the maternal introject' may be non-anthropomorphic, while to speak of 'tensions between ego and id' may be, if the former fits into a theoretical context that provide it with independent characterization and implicit definition, and the latter relies on identification with the structures mentioned. To restate this, for an

explanation to be non-anthropomorphic it must meet two criteria. First, the terms in the statements must fit into a theoretical context that implicitly defines and independently characterize them. Second, the explanation must meet acceptable criteria for scientific explanations[1] and not appeal to empathy, identification, etc. for their validity.

Whether any set of current psychoanalytic propositions that could be called a theory (e.g. dream theory, theory of defense, etc.) meet these criteria is open to dispute. Certainly Hartmann, Kris, and Lowenstein's de-anthropomorphization of the structural theory is open to question. However, there is no evidence for Schafer's assertion that anthropomorphism is inherent in

> ...propositions concerning functions that set aims, rank order other functions, or discharge themselves or discharge their own non-instinctual energy. In being represented as self-activating and self-regulating, autonomous functions are being implicitly portrayed as independent symbol utilizing minds that can make themselves up. I see no way around this anthropomorphic implication. (1976, p. 109)

Others have seen ways around this anthropomorphic implication. Peterfreund (1971), for example, in his work on information and systems theory has suggested feedback loop self-regulation. That Schafer pays no attention to this sort of work and that others such as Klein (1976) rather perfunctorily dismiss it, is perhaps indicative of certain a priori

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[1]For a discussion of the criteria for scientific explanations the reader is referred to Nagel (1961), chapters 2, 3 and to Sherwood (1969) for a different view. Some aspects of deductive explanations were previously discussed.

assumptions about what a psychoanalytic theory should be like. Of particular concern here is the assumption that psychoanalytic theory should not be or cannot be like the "natural sciences"; that it must deal with purpose rather than cause; meaning rather than motives; whys rather than hows; and the like. The next chapter will examine some aspects of this assumption.

Chapter Summary

This chapter primarily discussed extra-theoretical criticisms of psychoanalytic metapsychology. Several of these are obviously extra-theoretical and say nothing about the scientific status of the theory or formulation under scrutiny. Other criticisms were discussed which, while not obviously extra-theoretical, were found to depend on certain a priori philosophical assumptions. Finally one criticism was presented which does not appear to be extra-theoretical. This criticism is that metapsychology is anthropomorphic. While this criticism is more difficult to assess than generally acknowledged, several approaches for assessment of the criticism have been discussed.

It was stressed that it is important to keep in mind that the function of a theory is to explain certain phenomena, and that losing sight of this may lead to metaphysical discourse in the guise of scientific discussion. If the data that psychoanalytic propositions were formulated to explain are lost sight of then as Wachtel (1969) has pointed out, "...there is a danger that a poor formulation may be replaced

not by a more adequate one, but by a reluctance to encompass the observations that led to the formulation in the first place" (p. 652). This chapter attempted to show that many recent criticisms of metapsychology have failed to consider the data that led to the original formulations.

CHAPTER IV

SOME CONSIDERATIONS CONCERNING CAUSAL AND TELEOLOGICAL EXPLANATIONS IN PSYCHOANALYTIC THEORY

This chapter will discuss some of the issues relevent to the position that psychoanalysis cannot or should not be a natural science. There are many complex issues involved in this position and all those who hold it do not do so for the same set of reasons. Therefore this discussion must necessarily be selective. Additionally since excellent critiques of certain aspects of this position have been written by Rubinstein (1976) and Sherwood (1969), this chapter will focus on aspects that have not been extensively discussed, in particular teleological versus causal explanations in psychoanalysis.

It should be noted that there are those who argue that any science of human behavior is logically impossible. This argument generally relies on the premises that each case is unique, that factors cannot be isolated, and therefore no generalizations are possible; or that the subject matter of pyschology cannot be subject to the rigors of the scientific method. This argument has been advanced not only against the possibility of a science of psychology but against the possibility of a science of history as well. The premises

underlying this argument have been vigorously criticized by Nagel (1961), Sherwood (1969), Rescher (1970), and many others. Among contemporary critics of psychoanalytic metapsychology, no one would seem to be a consistent supporter of this position. Although at times it appears that Schafer (1976), is contending that any psychoanalytic explanation of behavior is impossible, for the most part he seems to be arguing that a different sort of explanation from those used in the natural sciences is appropriate. He, and others before him, have termed this understanding rather than explanation. More will be said about this later.

This chapter will examine some aspects of what Sherwood has called "the thesis of the seperate domain" (1969, p. 37). This view holds not that scientific explanations of human behavior are impossible but that they will differ from explanations in the natural sciences in important ways. It is proposed that a major difference lies in the concepts used to explain the subject matter. Whereas natural sciences invoke causes, mechanisms, functions, and the like as explanatory concepts; a science of human behavior (i.e. psychoanalysis) invokes meanings, purpose, reasons, intentions, etc. The position that psychoanalysis is or should be this later type of science has been strongly advocated by Klein (1970, 1976), Schafer (1976), and Gill (1973, 1976; Pribram and Gill, 1976). They contend that the clinical theory of psychoanalysis is at least potentially self-sufficient, and that metapsychology which attempts to be a natural science is

either clinically irrelevant, obfuscating, or impossible.

Writing before Klein stated his position, Sherwood's (1969) examination of the "thesis of the separate domain" focuses mainly on philosophical works. It is interesting that Sherwood finds adherence to "the thesis of the separate domain" to be rare among psychoanalysts. He writes that "apart from a few such instances, it seems fair to say that this position has been an 'outsider's' argument, a defense put forward by philosophers who are favorably disposed to psychoanalysis" (p. 126). Sherwood focuses on proposed distinctions between movements and actions and between causes and reasons. He concludes that while there may be good logical grounds for these distinctions, they do not necessitate a division of science into two separate domains. Further, "This sort of separation would be a 'bad' way of 'saving' psychoanalysis from the pressure of facing up to the rigorous standards developed in the natural sciences " (p. 184).

Rubinstein (1976) interestingly points out that Klein's position is not a new one, even within psychoanalysis. He states that it has its roots in a claim formulated by Dilthey that the understanding of humans and their affairs is different from explanation in the natural sciences in that the former must be understood in terms of meaning and the latter in terms of cause and effect. Rubinstein examines a similar position in the work of the psychoanalyst Imre Hermann. Rubinstein concludes that a strictly clinical theory, though possible, is an incomplete theory; and that such a theory is not only compatible with but in fact requires a natural science metapsychology for

for completion. He further states that the difference between views of man presented by metapsychology and be clinical theory, "is not the difference between causal understanding and understanding in terms of meaning, but in the way man is seen by us, as a human organism or as a person" (p. 262).

Two Traditions of Explanation

Extending far back in the history of ideas is what Von Wright (1971) has called the two traditions of scientific explanation. Lewin (1935) has termed that one Aristotelian and the other Galileian, though Von Wright proposes that the latter extends back to Plato. Depending on one's definitions, the contrast between the two traditions has been variously characterized as causal versus teleological, and mechanistic versus finalistic. For the purposes of this discussion the labels causal[1] and teleological will be used.

It has frequently been argued that teleological explanation is unscientific, illogical, or the like; or that anything that is valuable and scientific about it can be encompassed by causal explanations. For example, Eacker (1965) dismisses explanations that invoke reasons or purpose. He argues that reasons and purposes are not observable. To say that a person did something for a reason is simply to say that he did it, since what is available for observation is that he did it and not his reason. He continues that

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[1]Here the term causal is used in its loosest sense to refer to explanations that maintain that phenomena occur in a determinate order where the cause proceeds the effect.

an explanation that invokes purpose in behavior implies that the behavior is to be explained by future consequences rather than by what occurred in the past. Eacker writes, "it is still enigmatic; how can something that has yet to occur, a future event, possible influence something that is now occurring, a present event" (1975, p. 44). He concludes, "that such explanations are both untestable and teleological" (p. 45).

Eacker's dismissal of teleological explanations can be faulted for two reasons. First, his criticism of reason and purpose as non-observable appears to rely on a realist perspective. As has been previously stated there is no logical reason that these or other 'mentalistic' terms cannot function as theoretical terms. Second, although how the future can influence the present may be enigmatic, it is neither illogical nor absurd. Chein (1972) has pointed out that to contend that the nonexistent future can influence the present is no more illogical than to contend that the non-existent (i.e. no longer existing) past can influence the present. Braithwaite (1968) makes a similar point and states that, ". . . many non-teleological laws of nature, e.g. Newton's laws of mechanics, are symmetrical with respect to earlier and later times occuring in the laws: they state that the present is determined by the future just as much as it is determined by the past" (p.337).

Nagel (1953, 1961) is perhaps the most prominent proponent of the position that teleological explanations can be

reformulated to take the form of nonteleological[1] explanations and visa versa, ". . . so that in an important sense teleological and nonteleological explanations are equivalent" (1961, p.403). Nagel adopts as the distinguishing criteria of teleological explanations the occurrence of such phrases as 'the function of,' 'the purpose of,' 'in order that,' 'for the sake of,' or any similar expression that indicates a means-ends relationship.

A teleological explanation, Nagel contends, is a condensed explanation, containing at least one and usually several unstated assumptions. For example, a statement of the form, 'The function of A is P' assumes that there is a class of systems S with a certain organization of components C such that when S is in environment E, the presence of A will insure P. In many cases there is the additional assumption that unless P occurs, S will not continue to carry on its characteristic activities.

Many psychoanalytic explanations are stated in means-ends or teleological form. For example, Freud states, "Symptom-formation, then, does in fact put an end to the danger situation" (1926, p.145). This may be restated as, 'The function of symptom-formation (A) is to end the danger situation (P);' and reformulation as follows: When a human being (S) with a certain psychic structure (C) is in an environment such that there is a danger situation (E), the

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[1]Nagel uses the term nonteleological. However, the term causal defined loosely (cf. previous footnote) could be substituted.

occurance of symptom-formation (A) will insure the end of the danger situation (P). Again it is tacitly assumed that unless the end of the danger situation occurs, the human being will not be able to carry on its characteristic activities.

What if the occurrence of symptom-formation insures the end of the danger situation, but so do alternative processes not requiring sympton-formation? (That is A is a sufficient but not a necessary element in the set of conditions for P to occur?) Would the statement 'The function of symptom-formation is to end the danger situation' still qualify as a satisfactory explanation? Nagel (1961) calls such explanations unsatisfactory. In his view, given the assumed boundary conditions S,C,E, in order for 'The function of A is P' to be a satisfactory explanation A must be a necessary condition for the occurrence of p.

Nagel's viewpoint, if accepted, has important implications for psychoanalytic explanations. Many psychoanalytic explanatory statements can be stated in the form 'The function of A is P' where it is clearly not intended that A be considered a necessary condition for P. Returning to the example, it is usually held that not only symptom formation but also sublimation, or alloplastic behavior may 'put an end to the danger situation.' What do we make of such explanations?

Though Nagel is not altogether clear on this point, he seems to propose that if there are alternative processes A,B each of which is a sufficient condition for the occurrence of P, then there must be determinable differences in the boundary conditions such that under conditions S,C,E, A is a necessary

condition for the occurrence of P; and under conditions (S,C,E)' B is a necessary condition for the occurrence of P. In accordance with this interpretation, statements of the form 'The function of A is P' where A is known not to be a necessary condition for P, might better be characterized as incomplete rather than as unsatisfactory. Thus we could say that 'The function of symptom-formation is to end the danger situation' is an incomplete explanation; requiring for completeness a specification of the conditions under which only symptom formation and not sublimation, nor alloplastic behavior, nor anything else will 'end the danger situation'.

It is important to recognize that there is a significant a priori assumption underlying the characterization of these explanations as incomplete. That assumption is that they are potentially capable of being made complete. Although there is no logical reason to suppose that theconditions under which A is a necessary condition for the occurrence of P cannot be discovered, neither is there a logical reason to suppose they can be. Whether they can or not is an empirical and not a logical question.

It might also be worthwhile to consider the possibility of probabilistic teleological explanations of the form 'The function of A or B is P' where the probabilities of A or B being a necessary condition for P are each specifiable. Though Nagel (1961) discusses probabilistic deductive explanations, he does not consider probabilistic teleological explanations. However, there is nothing obviously illogical about such explanations and they may warrant further examination.

It is sometimes asserted that goal directed or self-regulating behavior is unique to living systems and that teleological explanations are uniquely suited to explain the behavior of such systems. Nagel (1953, 1961) and others have analyzed certain systems of this type and have concluded that the goal-directed and self-regulating features can be accounted for by nonteleological explanations. The full argument is too complex to be reproduced here. However, in general, the goal-directed or self-regulating features of such systems is accounted for by some combination of causal systems arranged in such a way as to provide negative feedback. A simple example of a system of this type is a thermostatically controlled heating system.

A large literature now exists concerned with the relevance of feedback regulated systems to the explanation of human behavior. Much debated is the question "When, if ever, does creating a model that simulates human behavior count as explaining that behavior?" Fodor (1968) suggests that this question is a special case of the more general question: "Under what circumstances does the ability of a theory to account for the relevant observational data make that theory true?" (p. 146). Viewed in this light the answer to the question whether simulating a behavior really explains that behavior will frequently depend on one's view of the cognitive status of theories and not be resolvable by appeals to logic or fact.

In any case, though not without flaws, Nagel's argument that teleological and nonteleological explanations are in all

important senses equivalent is a powerful one. If accepted it would not necessitate that all explanations containing the terms purpose, reason, goal, etc. be reformulated. According to Nagel teleological and nonteleological explanations differ with regard to emphasis and perspective in formulation. Teleological explanations focus on the contributions of the components of an organized system to the maintenance of the system's usual way of functioning. They are thus particularly suited for the study of directively organized systems, e.g. people. However, if accepted Nagel's argument brings into serious question the claim that psychoanalysis and the natural sciences use radically different explanations and thus fall into separate domains.

Explanation of Intentional Action

It is probably fair to say that a clear statement of the logical grounds on which psychoanalytic clinical theory is or should be divorced from natural science cannot be found in the works of Klein, Gill, or Schafer. Although in their discussions they uniformly contrast the explanations of natural science metapsychology which invoke cause, mechanism, and the like, with the explanations of clinical theory which invoke purpose, reason, etc.,it is probably also fair to say that they would find a Nagel-like reformulation of clinical theory unacceptable and that something essential would be lost in the reformulation.

It is proposed that central to Klein's and Schafer's positions is the notion of the person as agent acting intentionally. (Gill's statements of this matter are too

sparse to comment on.) Klein is quite explicit on this matter. He writes, "The central objective of psychoanalytic clinical explanation is the reading of intentionality; . . . symptoms became intelligible as exemplifying an aimful solution" (1976, p.26). To Klein meaning and significance are related to intentionality in a way that is not altogether clear. In many cases it appears that the meaning or significance of a behavior is equivalent to the perception of its intention. For instance he writes, "When an analyst sees a pattern of behavior functioning as a 'defense' for example, he is stating the significance of a pattern of behavior; he is making a statement about its purpose or intention" (1976, p.52).

Schafer (1976, p.199) rejects the systematic use of the terms intention and intentionality first because the terms are nouns and in his view subject to abuse, and second because the term intentionality has a long philosophical history and carries unwanted connotations. He finds the verb form "to intend" and the adverb "intentionally" unobjectionable if used to refer to consciously formulating reasons or goals for actions. However, for the purposes of this discussion we can disregard the conscious implications of "intentionally". If this is done, acting intentionally is equivalent to Schafer's notion of acting for a reason. The notion of a agent acting for a reason is clearly central to Schafer's position. He writes:

> Using action language, one no longer explains behavior and mental processes in terms of the forces of psychodynamics or the influence of underlying motives. Instead, one answers why-questions in terms of reasons. Essentially, in giving reasons for particular actions, that is in explaining them, one restates these actions in a way that makes them more comprehensible. (1976, p.210)

Further, in a way that is similar to Klein, Schafer relates reasons to meaning. He states, "An action is called that by virtue of our attributing meaning to it, and to state its reasons is one way of stating what that meaning is, just as to state its meaning is one way of stating what its reasons are" (1976, p.231).[1]

We can now examine some of the ways in which intentional action can be understood and ask if its use as an explanatory concept requires the acceptance of a separate domain of explanation. Before proceeding it should be noted that there is a large literature in phylosophy pertaining to what has been called "the problem of 'intentionality'" (Sellars, 1958, p. 507). Fortunately for the present discussion it will be sufficient to distinguish three alternative interpretations of the notion of intentional action.

1. Intentions are causes of actions. - According to this interpretation, certain teleological explanations of goal-directed behavior are viewed as reducible to causal explanations in which the intention is the cause and the action is the effect (Braithwaite, 1968). As is the case with the causal explanation, it would be meaningful to inquire as to the cause

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[1]There are undoubtably other senses in which the term meaning is used in psychoanalytic discourse. Most appear to designate a phenomenon meaningful if it can be placed in a causal nexus. To others a phenomenan is considered meaningful by virtue of its signifying, symbolizing, or communicative function. Ricoeur's (1970, 1974) hermeneutic approach appears concerned with meaning in this sense. Rycroft's (1966), Home's (1966) and Szasz's (1961) notion of psychoanalysis as a semantic theory implies a similar notion of meaning. Shee Shope (1973) for a discussion of Freud's concept of meaning.

of the cause. For example, the statement, "The person raised the window in order to lower the temperature" could be rendered in causal form as, "The person's intention to lower the temperature caused him to raise the window." One might then inquire as to the cause of his intention. Viewed in this light an intention might either be a dispositional term or a mentalistic theoretical term of the type previously discussed.

It appears, however, that this causal interpretation of intentional action is similar to Nagel's analysis of teleological statements of the form 'The function of A is P', and makes similar boundary assumptions. The above example could be restated as 'The function of his window raising was to lower the temperature.' In the case of the functional interpretation it is assumed that boundary conditions are either known or discoverable such that when those conditions occur his window raising will be a necessary condition for temperature lowering. In the case of the causal interpretation it is assumed that boundary conditions are either known or discoverable such that when they occur the intention to lower the temperature will cause (or at least precede with determinable probability) the action of window raising.

It is evident that in most untrivial cases we do not know the conditions under which particular behavior will occur, nor under what conditions a particular behavior is a necessary condition for a particular consequence. In lieu of knowing those conditions we may characterize explanations of intentional action as incomplete. Again, however, underlying this

characterization is the priori assumption that those conditions are knowable.

It should perhaps be noted that either to specify the conditions under which a behavior will occur, or to specify the conditions under which a particular behavior is a necessary condition for a particular consequence would be to formulate a law about behavior. (In the first case the law would be of the form 'if A then B', and in the second case it would be of the form 'if not A then not B'). It has been asserted by some that there are laws of behavior (particularly historical laws) but they are complex and are yet to be formulated; others have asserted that there are such laws but they are too trivial to deserve explicit formulations. Again neither position can be proven a priori, and lacking empirical justification, remain statements of belief.

2. Intention is in the eyes of the beholder. According to this view certain actions are classified as intentional when they are constituent actions of a goal-directed or self-regulating system. In this interpretation it is assumed that goal-directed and self-regulating behavior can be explained causally. Thus intention becomes a category of perception. Self-attribution of intention is considered no differently. Self-attribution of intention may influence behavior, but this is explained in terms of a more complex feedback loop: "Every output of the organism in turn becomes a new input information" (Peterfreund, 1971 p. 239).

In that this interpretation assumes that what is perceived as intentional action is in fact caused action, it must bear the same criticisms as the first interpretation. That some systems that appear to be acting intentionally can be explained in causal terms, is not proof that all such systems can be so explained.

However, an additional problem surrounds this interpretation of intentional action. Even if we accept the view that intention is a matter of perception, we are left without an explanation of why certain actions are perceived as intentional. Certainly not all goal directed or self-regulating systems are perceived by everyone as acting intentionally (e.g. a car with a governor).

In many cases, it may be appropriate to dismiss a phenomenon as a matter of perception and a problem for psychology. For example, proponents of the doctrine of emergence might argue that though a chemist might be able to predict certain properties of a newly developed polycarbon molecule by knowing its chemical structure, he could not predict how it would smell. A chemist might appropriately reply that chemistry contains no expressions for olfactory properties and thus cannot predict to smells.[1] He might continue that while the explanation of many properties of this molecule are problems for chemistry its smell is a matter of perception and a problem for psychology. Similarily a cyberneticist may dismiss the problem of intention so as to continue working on

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[1]See Nagel (1961), Chapter 12, for a discussion of the issue of emergence.

machines unimpeded by the notion. However, what is a psychologist to say? It does not seem that he can say properties of persons are problems for psychology, but intention is a matter of perception and not a problem. Perhaps those who seem to think that the problem of intention is obviated by calling it a problem of perception assume that developing a psychology of perception will be an easier task than developing a psychology of intentional action. Once again, this is an a priori assumption.

3. Intention and action are conceptually or logically connected. - According to this interpretation the intention to do something cannot be defined without reference to its intended result, which is often referred to as the object of the intention. Thus 'the intention to lower the temperature' differs from all other intentions, and it would be meaningless to speak of an intention without making reference to it object. The actions which bring about an intended result appear to form a linked chain. The unity of this chain of actions is not provided by a causal connection, but by their subsumption under the same intention. Further, the chain of actions that bring about the intended consequence say little if anything about the intention and may vary greatly. For instance if the person who intended to lower the temperature found the window stuck he might open the door, turn off the air conditioner, smash the window, or perform a variety of other rational or irrational actions including abandoning the intention. If the person in question smashed the window or did something of the sort, an outside observer might have difficulty guessing the intention.

Von Wright (1971) suggests reserving the term teleological for explanations in terms of intention. He terms explanations which are couched in teleological terms but transformable to causal explanations, quasi-teleological. (e.g. The purpose of the thermostat is to maintain a constant temperature.) Teleological explanations do not specify the necessary and sufficient conditions for the occurrence of an action. Rather an action is "understood" in terms of its intention. Continuing with the about example, if one were to ask why did the person smash the window, answering that it was his intention to lower the temperature would be a teleological explanation. Similarily, Freud's statement that "Symptom-formation, then does in fact put an end to the danger situation" (1926, p. 145), which was previously seen as an incomplete functional or causal explanation might be restated as a teleological explanation. Restating it would require the assumption of an agent so that it would take the form, "The action of symptom formation results from the agents intention to end the danger situation."

Explanations of this sort are clearly similar to the types of explanations Klein and Schafer offer in terms of intentions, reasons, meanings, and the like (cf. pp. above). Yet there appears to be nothing about these explanations that make them incompatible with causal or 'natural science' explanations. It seems perfectly reasonable to attempt both to discover the cause of an action and also to understand the intention of the agent of the action. However, either to maintain that all teleological explanations will eventually be transformed into complete causal or functional explanations,

or to maintain that some class of actions can never be explained in causal or functional terms is to assert a belief that is neither empirically nor logically justifiable.

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