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PERCEPTION AND SCIENTIFIC OBSERVATION: A CHALLENGE TO THE ASSUMPTION OF OBJECTIVITY

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ABSTRACT

PERCEPTION AND SCIENTIFIC OBSERVATION:
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This inquiry attempts to determine whether the assumption of objectivity usually made about scientific observation is justified. This is considered to be an important question because it is usually assumed that scientific knowledge differs from the rest of human knowledge precisely because it is objective. The belief in the objectivity of science is traced back historically to the beginnings of science in Western civilization. In a brief historical analysis, it is revealed that despite many important historical changes in our account of scientific knowledge, there are two aspects of scientific epistemology which remain constant. These two aspects are first, that scientific knowledge differs from the rest of knowledge in that it is factual and second, that the scientific method is the only appropriate way to distinguish fact from illusion.

This long standing tradition in the Western way of knowing presents a unique difficulty to anyone wishing to conduct an inquiry into the value of objectivity. The difficulty arises from the fact that most available methods of inquiry have the assumption of objectivity embedded in them.

There is, for example, a tendency for methods to be either analytical or experimental. This division is believed to reflect an underlying separation into subjective and objective. A method was sought which was not prejudiced by having the assumption of objectivity embedded in it. The method settled upon is called the method of anomalies, which is modeled after the thought experiment. Thought experiments were constructed to produce information anomalous to the assumption of objectivity.

Three such anomalies to the assumption of objectivity were produced by this inquiry. The first anomaly demonstrates the inseparability of logical and empirical elements in perception. Specifically, it demonstrates that the real and the illusory can only be separated after certain logical assumptions are made. This indicates that the logical assumptions are as fundamental to the definition of the real as are the sensory elements. The second anomaly demonstrates the influence of the whole on the experience of the parts in both scientific measurement and in normal perception. Specifically, it shows that concepts or constructs define and select the appropriate measurements to be taken, as well as those to be ignored. The third anomaly demonstrates that the qualitative experience in observation and perception is, in part, determined by the purpose of the observation or perception. This means that the act of measurement or perception in part determines the result of the perception or measurement.

On the basis of the anomalies generated by the inquiry, some important conclusions about the meaning of factuality are derived. A chapter is devoted to unearthing the various definitions and meanings of the word "fact" within existing theories of knowledge. When each of these definitions of fact is reviewed in light of the anomalies to objectivity, it is concluded that none of the available definitions of fact are able to deal with all three of the anomalies generated by the inquiry. On this basis, it is concluded that the notion of factuality is misleading.

It follows that an alternative epistemological unit is required to replace the notion of fact in our understanding of scientific knowledge. Such an alternative building block is described. In addition, a theory of scientific knowledge which is not based on the assumption of objectivity is outlined. In such a theory, other units of knowledge than logical and empirical elements would play an important role. Some psychological elements of the alternative theory of scientific knowledge, such as assumptions, whole and goals are discussed.

CHAPTER I

INTRODUCTION

In 1964, C.P. Snow published "two cultures", in which he distinguished between the literary man and the scientifically oriented scholar. His central theme was that these men represented the way in which scientific knowledge and literary knowledge were growing steadily apart. Each of the two cultures was evolving with separate values, methods and pictures of the world. At that time, the scientific culture appeared to be the more viable in terms of affording us solutions to our problems. Or, as Snow put it, it gave us "hope". No literary man who read Snow's "two cultures" could have escaped feelings of inferiority for not knowing the Second Law of Thermodynamics. Since that time there has been an increasing recognition that our social and ecological fabric required sensibilities which apparently are not available within the scientific culture. The promise that appeared to rest in science and technology in 1956 appears far less promising in 1970. The industrial might resulting from the application of science and technology is a threat to both man's social and biological continuation. But whether we see science as a positive or negative development in human culture, we cannot deny that putting science in its proper place intellectually and assimilating it into the general culture is a serious problem. It is a problem in both general sociology and in the sociology of science. Underlying this significant problem is a problem in the psychology of knowledge.

These sociological and academic effects behoove us to examine carefully the rationale for making a sharp distinction between science and non-science, as well as for elevating scientific knowledge to the pinnacle of human credibility.

One of the fundamental reasons for believing in science, as well as for distinguishing it from other types of knowledge, is our belief that science is objective or factual. We know scientific knowledge to be based on facts; facts in turn, are based on observations. Other aspects of human knowledge, such as cultural tradition and folklore, though they are based on experience, may call upon us for allegiance but not for the same kind of credibility that we give science.

It appears that we evaluate our knowledge according to our assessment of its origins. This means that we are using epistemological criteria for discriminating between science and other aspects of human thought. As indicated above, it is not sufficient to say, as is often said, that science is empirical while other forms of knowledge are not. Since empirical merely implies that knowledge is based on experience, no one will deny that contemporary religious and artistic wisdom is a heritage from past experience. To find the basis of the distinction which sets science apart we must learn to distinguish between kinds of experience. Only objective observations can give a proposition the stamp of scientific truth, and not all of experience is considered to be objective. It is not accidental that we speak of a scientist's laboratory experiences as observations while we speak of his daily experiences as perceptions. The words have a connotation of objectivity and subjectivity, respectively.

It is this separation of sensory experience into two qualitatively distinct classes which is the basis of the epistemological distinction between science and other kinds of human knowledge. Knowledge, scientific or otherwise, is not generated by each individual, but rather is passed on to us through language in a substantially accomplished form. To say that credibility of information depends upon its epistemological origins is merely to express the belief that we evaluate knowledge according to the basis we believe it has in experience. It is important to stress that our assessment of the experiential base for most of our knowledge is itself a belief since, as we said before, knowledge is given to us not directly through experience but as a second hand report of experience.

If this reasoning is correct, the separation of experience into subjective perception and objective observation is of the utmost importance. In the first place, it is one of the bases for distinguishing the scientific from the non-scientific. The belief in its objective origins is probably the most significant reason for the credibility status that science enjoys. In the second place, the belief in the distinction between these two kinds of experience is at the core of a methodological debate between Snow's "two cultures".

It is apparent that the assumption of objectivity plays an important role in distinguishing between scientific and non-scientific knowledge. It is therefore reasonable and desirable to conduct an inquiry to determine whether or not the assumption of objectivity is justified. A study of the history of the assumption of objectivity reveals that such an inquiry must first of all deal with a serious methodological difficulty. This difficulty arises from the fact that all currently available methods of inquiry have imbedded in them assumptions which preclude the possibility of using them to inquire into objectivity.

An inquiry into the assumption of objectivity is obviously a problem in epistemology. A historical analysis (Chapter II) reveals that the distinction between objective and subjective knowing is of such long standing that problems of epistemology and the disciplines for their study have been defined as though the distinction were, in fact, valid. If we examine the present state of affairs with respect to epistemological questions, we find two groups of scholars actively engaged in problems of epistemology.

Psychologists make use of scientific methods to study the epistemology of subjective experience. This is the study of the psychology of perception. The study of object observation is for the most part left to historians and philosophers of science. The psychology of perception and the philosophy of science are the modern inheritors of the epistemological question. Nevertheless within these disciplines there is no generally acknowledged need for an integration of these respective fields.

The reason they do not see such a need is that they have accepted the validity of the subjective/objective distinction. Nowhere is this more evident than in the case of the psychologist who places his faith in the objective methodology of science to reveal to him the nature of subjective experience. Though this may not necessarily indicate a difficulty since both the analysts of science and the psychologists may be justified in assuming the capacity of objective perception, it is a serious difficulty, for anyone who wishes to question that assumption. It would not be logical to inquire into objectivity with tools which have imbedded in them a prejudice towards objectivity. For this reason we find that none of the current methods available is suited for our inquiry.

Psychologists may be divided into two groups, neither of which is prepared to deal with an inquiry into objectivity. The experimental psychologist has the usual assumption of objectivity built into his method. The phenomenological psychologist accepts the distinction between objective and subjective, and defines his field of study in terms of the subjective. The other group of scholars who are interested in understanding objective observation as it takes place in science have also defined their philosophies and methods in such a way that they are committed to the assumption of objectivity. Scholars of science who take an empirical approach are themselves committed to the view that there are objectively observable facts about science. Students of science who are committed to an analytic method are for the most part also committed to the view that there is a significant analytic distinction between epistemic and non-epistemic perception and that it is not different from the more familiar objectivity/subjectivity distinction. It is not surprising to find that contemporary students of epistemology find themselves committed explicitly or implicitly to the belief that there is an essential difference between observation and perception. This results in the fact that the distinction between observation and perception is an implicit aspect of one of the basic assumptions of logical positivism. This basic assumption is that there is an essential difference between analytic and synthetic knowledge. Belief in this distinction is couched in a variety of terminology; such as formal and empirical, theoretical and factual, and conceptual and observational. Regardless of the terminology, this fundamental distinction of logical positivism implies a difference between observation and perception.

In Chapter III, a method is suggested which attempts to hold the assumption of objectivity in abeyance. This is the method of "thought experiments". Reasons are given for believing that this method makes no prior commitment to the analytic/synthetic distinction which is to say the distinction between subjectivity and objectivity. Instead it is committed to the view that so called facts get their meaning from the context of ideas and artifacts within which they are viewed. The method which is suggested differs from the usual empirical approach in that it does not propose to generate any new facts. It also differs from the usual analytical approach in that it does not rely on a normative analysis of language or logic. Instead, it attempts to give old facts new meaning by changing the context from that within which they are normally viewed.

The old facts which are appealed to are all relevant to the assumption of objectivity. The new context results from holding that assumption in abeyance. Chapter II indicates that the epistemic status of perception has been the subject of study by two different groups of scholars. Since both these groups accept the assumption of objectivity, they have conducted their inquiry into perception in "haughty isolation" from one another. The psychologist has generated facts about perception but has seen no need to incorporate these "facts" into his own methods of observation. This is undoubtedly due to the belief that a scientist's observations are a qualitatively different class of perceptions than the subjective experiencing of the subjects in psychological experiments. On the other hand, historians and philosophers of science who have analyzed the role of observation in science have for the most part ignored the work of perceptual psychologists.[1] These men have also seen no need to extend to the world of daily experience their analytical insights about the influence of perception on knowledge. The method proposed in Chapter III attempts to capitalize on the lack of cross-fertilization between these two fields of study. It is hoped that the new context for the old facts will develop from forcing a cross-

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[1] Notable exceptions are Kuhn (1962), Polanyi (1958), and Hanson (1958).

fertilization of these two independently developed sources of knowledge about perception.

If, as is maintained in Chapter II, psychologists and the scholars of observation have worked in isolation from one another because they both accepted the assumption of objectivity, then it would seem reasonable to expect that holding the assumption in abeyance will have the effect of bringing the two fields together. Bringing together facts from these two disciplines tends to create anomalies to the assumption of objectivity. Anomalies are seen as information which contradicts our normal expectations. They may, as has been indicated, be well-known facts which do not become anomalous until put in proper context.

Before we can bring facts from these two disciplines to bear on the assumption of objectivity, it is necessary to know what is meant by objectivity. Chapter IV offers an analysis of five important philosophies of knowledge currently attempting to explain how we know. The purpose in reviewing these positions is to obtain an understanding of the role of the assumption of objectivity within these various theories of knowledge. The analysis reveals that, though the belief that objective knowledge is possible is almost universally accepted by these various theories, the exact formulations telling us how objectivity is achieved differ significantly from one theory to the next. One characteristic which most theories of knowledge are revealed to have in common, and which demonstrates the almost universal commitment to the idea of objectivity, is the promotion of an indubitable within each theory.[1] By an indubitable is meant an element of knowledge which is given to us in such a way that we can have no doubt about it. The fact that each of the theories promotes an indubitable indicates that the belief in objectivity is very strong. There is also little agreement among these theories concerning how such indubitables become incorporated into knowledge. However, the lack of concensus concerning the way in which objectivity is achieved seems to indicate a need to examine the possibility that indubitables do not exist.

Chapter IV concludes that most theories of knowledge subscribe to the possibility of objective perception of indubitables. Such indubitables are generally believed to be objective in that we can be certain in our knowledge of them and confident in their independence of us. Having made explicit what is meant by the assumption of objectivity, the next step involves presenting anomalies which contradict the assumption. Chapter V presents information anomalous to the assumption of objectivity. Each anomaly consists of two sets of information. One of the sets is taken from the psychology of perception and the other is taken from the epistemology of

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[1] Transactionalism is the only exception.

science. The two sets in an anomaly consist of information which is widely accepted by the specialists in the domains which generated them. What is unique about their presentation in Chapter V is that they are paired together in a new context. That new context is the intention of our inquiry. This means that the pairing of two sets of information is based on the belief that each set has the same point to make about the assumption of objectivity. The combination of the three anomalies serve as a systematic method for compiling counterevidence to the assumption of objectivity. The purpose of compiling counterevidence is twofold. In the first place, it gives us a method for evaluating whether or not the assumption is justifiable. In the second place, the recognition of counterevidence will lay the groundwork for establishing the conceptual and factual requirements of alternative assumptions to that of objectivity.

In Chapter VI, an attempt is made to re-evaluate the status of the assumption of objectivity in view of the anomalies presented in Chapter V. In doing so, the various candidates for the position of indubitable which were revealed in Chapter IV are reviewed to see how each is affected by the anomalies. In Chapter VII, the broad outlines of a satisfactory resolution to the epistemological problems raised by the anomalies to objectivity are discussed. An attempt to identify as clearly as possible a positive alternative to the assumption of objectivity is connected to recent developments in both perception and philosophy of science.

Finally, Chapter VII attempts to present a picture of science which does not depend on the assumption of objectivity, and traces some of the implications of such a view of science. These implications are discussed in terms of three major changes that are suggested by the inquiry. The first is a revitalization of the psychology of knowledge. The second involves identifying some of the negative effects the assumption of objectivity had in preventing improvements in the psychology of knowing. Finally, alternative methods of conducting scientific inquiry are indicated.

CHAPTER II

IDENTIFYING THE ASSUMPTION OF OBJECTIVITY

Asking whether or not there is justification for distinguishing between subjectivity and objectivity is a question of epistemology. Having developed an interest in such a question, one naturally peruses the existing intellectual and academic specialties to find how such questions have been treated. To do so, the statement of the question must be slightly changed. It can be asked what segment of the intellectual community is interested in the role that the senses play in knowledge. When the question is stated in this way, the answer is immediate and, that is, that both the psychologists and epistemologists are interested in the role which the senses play in the acquisition of knowledge. Why then, we ask, do we distinguish between the epistemologists and psychologists?

The usual reason for distinguishing between a psychologist's and an epistemologist's interest in perception has to do with their various purposes and methods in undertaking that study. Presumably, epistemologists are interested in perception only to the extent that it plays a role in verifying knowledge, while psychologists are interested in describing the process by which sensory experience takes place. That such a difference in interest exists between psychologists and philosophers is no doubt true. Whether such a difference in interest is sufficient reason to justify the wide gap that exists between the methods and conclusions of these two groups at the present time is another question.

Hamlyn (1961) tells us that epistemologists are interested in perception only insofar as it can provide us with an indubitable unit of knowledge. Epistemologists are not interested in how the senses work; they are, instead, interested in finding an element in knowledge which can be given to us in such a way that we can have no doubt about it. Traditionally that way has been perception. Within contemporary epistemology, it is generally recognized that such perceptual indubitable units are not available to the average person but came to us only through the elaborate methodology of science. Debates over the epistemic status of perception are currently most viable and relevant in philosophy only within the philosophy of science. (Collins, 1967)

Psychologists, on the other hand, have little or no interest in establishing an indubitable unit of knowledge. They merely wish to explain how sensory experiencing takes place. The ontological reliability of such sensory experiencing is a secondary interest. In arriving at his "description of perception," the psychologist will rely on scientific methodology to establish the descriptive reliability.

Other than interest, the most obvious difference between epistemologists and psychologists is one of methodology. The philosopher of science who for the most part has inherited the problem of establishing indubitables works either with linguistic or historical analysis. The psychologist interested in describing perception relies mainly on experimental observation. The question seems to be, is this methodological difference between the two sufficient explanation for the lack of interchange between the two disciplines in questions of substance? Given that each group has its own methods and interest with respect to the problems of perception, should there be any cross-fertilization between the two fields? Epistemologists, after all, are not interested in the whole of perception but merely in the fact that some of our perceptions are ontologically reliable. Psychologists, after all, are not interested in abstract problems like ontological reliability since for that they need only trust a long tradition of laboratory sciences. This suggests that each group uses the same rationale (basis) for ignoring the other, and that this rationale is a stronger reason for keeping the two groups separate than either interest or method. The rationale is that both groups rely on the achievements of science as proof of the fact that objective perception is possible. The philosopher interested in epistemology considers it natural to look within the philosophy of science to establish indubitables since, after all, science has produced the knowledge in which we have the most confidence. Likewise, the psychologist can accept his methodology on the basis of its long tradition of success.

The separation between the psychologist's and epistemologist's interest in perception is very convenient in that it allows them to ignore one another's work. But it presents a serious problem to this inquiry because it reveals that both groups have already accepted as justified the assumption of objectivity. It is because the psychologist assumes that scientific observations are a different class of perception than those which he is studying that he can use observation as a tool for studying perception. It is because philosophers accept the objectivity of scientific knowledge that they reduce the problem of the indubitable to a problem in the philosophy of science.

In order to find an appropriate method for inquiring into the validity of the objective/subjective distinction we must look into some of the history of this distinction, since it is obviously one which antedates current methods of dealing with epistemological questions. Such an examination will assist us in evaluating currently available methods and in discussing the conceptual requirements of a method appropriate for an inquiry into the assumption of objectivity.

The division of the world into real and apparent, which is the same as its division into the veridical and illusory, antedates the current methods available for studying epistemological questions. The experimental method and the analytic method used by philosophers of science are relatively new approaches for conducting science. They have developed out of a tradition which based its presumptions of objectivity on one or another version of the distinction between real and apparent. Though many aspects of the account of scientific knowledge have changed drastically within the history of science, two principles emerge which have remained constant despite other important changes. The first of these, hereafter called the "principle of separation", involves a persistent belief that the world could be separated into real and apparent. The separation can either be stated in ontological terms, such as real and apparent, or in epistemological terms, such as veridical and illusory. The second principle to emerge, hereafter called the "principle of verification",[1] involves a persistent belief that what distinguishes real from apparent is scientific method. Let us now examine the views of some of the earliest proponents of a science based on observation so that we may see how these two principles of scientific epistemology have remained constant.

Aristotle believed that the sensible qualities of objects were not of the essence of the object, but were merely a means of differentiating the object. There is no doubt that to Aristotle, writing about epistemology and perception were synonomous tasks; therefore, one can understand much about his metaphysics by studying his perception theory. One can see how the two balance each other.

> By a 'sense' is meant what has the power of receiving into itself the sensible forms of things without the matter. This must be conceived of as taking place in

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[1]The reader must be warned against possibly confusing what is here called the "principle of verification" with the traditional positivists' verification principle. The first is here used only as a convenient tool for discussing some of the historical developments.

> the way in which a piece of wax takes on the impress of a signet ring without the iron or gold; we say that what produces the impression is a signet of bronze or gold, but its particular metallic constitution makes no differences in a similar way. The sense is affected by what is coloured or flavoured or sounding, out (sic) it is indifferent what in each case the substance is; what alone matters is what quality it has, in what ratio its constituents are combined. (Aristotle, 1952B, p. 656)

We can see that Aristotle distinguishes between real and apparent by noting that he did not believe that the sensible qualities were of the essence of things. It follows for Aristotle that not all sensible qualities are to carry equal significance in knowledge. In addition, his position is consistent with the "principle of verification" when he holds that science is uniquely qualified in telling us which sensible qualities carry significance. Aristotle's "principle of verification" relied upon consistency and coherence. Sensory experience was to be considered veridical only when it gave evidence consistent with our larger picture of nature.

The neo-Platonism prevalent in the 16th and 17th centuries did much to promote the notion that the world of appearance was a poor reflection which in itself could not be trusted to reveal the underlying unity and harmony in the universe. Kepler (1952) sought to show that the real qualities of the world were all quantitative and that the fundamental differences were of number and not of qualities. Though he did not refer to the difference between quantity and quality as primary and secondary, that distinction has persisted in the physical sciences to this day.

Galileo made the distinction between primary and secondary far more explicit and central to his work. He differed from Kepler in that he was interested in terrestrial science and was an atomist who believed the fundamental units to have only geometrical qualities. He made the distinction more explicit by clearly identifying the secondary qualities with the senses. These qualities were considered to be subjective and the primary qualities objective. We can see this in his own words.

> But that external bodies, to excite in us these tastes, these odours, and these sounds, demand other than size, figure, number and slow or rapid motion, I do not believe; and I judge that, if the ears, the tongue, and the nostrils were taken away, the figures, the numbers, and the motions would indeed remain, but not the odours, nor the tastes nor the sounds, which, without the living animal, I do not believe are anything else but names, just as tickling is precisely nothing but a name if the armpit and the nasal membrane be removed...
> ....
> This form of the primary-secondary doctrine in Galileo is worth a moments pause, for its effects in modern thought have been of uncalculable importance. It is a fundamental step toward that banishing of man from the great world of nature and his treatment as an effect of what happens in the latter, which has been a pretty constant feature of the philosophy of modern science, a procedure enormously simplifying the field of science, but bringing in its train, the big metaphysical and especially epistemological problems of modern philosophy. (Burtt, 1954, p. 78)

The distinction between primary and secondary qualities was a clear cut part of the scientific heritage to which Newton paid tribute. He was probably the first to apply the distinction explicitly to color. He makes it quite clear that to speak of rays of light as having color is a "vulgarity", for to speak "philosophically" and "correctly" the rays can only be said to have properties that give rise to the sensation of color.

Few did more to mold our conception of science than Kepler, Galileo and Newton, and it is clear that they were not alone in supporting the picture of the world which held man's daily experience to be largely illusory. That these and other important physical scientists hold this epistemological and metaphysical position meant that only pictures of the world concordant with that viewpoint could be considered scientific. There were also some important philosophers who expanded on this doctrine, though for them the consequences of the doctrine more often led to divergent world views.

The rationalism of Descartes rests in large measure on his belief that perception is not epistemic. He divided the world into the cognitive and extensive realms. The extensive realm was made up of primary qualities, which were not directly available to the senses. The cognitive realm was made up of secondary qualities, which gave rise to the world of appearance. To look upon the world through the senses and to rely upon this information was seen by Descartes as childish naivete. Only through the application of reason could one come to know the qualities of the extensive world.

The same division of primary and secondary qualities was maintained by Locke (1952) though this led, in his case, to empiricism. The primary qualities of things could be empirically determined because of their permanence, despite the variation in their secondary qualities; for example "take a grain of wheat, divide it into two parts; each part has still solidity, extension, figure and mobility: divide it again, and it retains still the same qualities. These are also the qualities which can be detected by more than one sense." (Locke, 1952, p. 298)

Combining the doctrine of primary and secondary qualities with an empirical philosophy is not quite as simple as it seems in the preceding quotation. We can see in the next quote from Locke, the precursors of Berkeley's and Hume's skepticism.

> Besides this ignorance of the primary qualities of the insensible parts of bodies, on which depend all their secondary qualities, there is yet another and more incurable part of ignorance, which sets us more remote from a certain knowledge of the co-existence or inco-existence (if I may so say) of different ideas in the same subject; and that is, that there is no discoverable connexion between any secondary qualities and those primary qualities which it depends on." (Locke, 1952, p. 316)

From Aristotle to Locke, we can see that those who believe in a science based on observation also believe in the "principles of separation and verification". The exact form of the "principle of separation" did, of course, differ in some important details. For example, Aristotle distinguished between these sensory qualities which displayed the natural order, and all the other qualities which were not significant differentia. The neo-Platonists believed that only these observables which displayed mathematic harmonies were real. These early distinctions became a more formalized version of the "principle of separation" in the doctrines of primary and secondary qualities which persisted until Berkeley and Hume, and which still persist among some practicing scientists. There are also variations in the way the principle of verification was expressed. For example, Aristotle believed that coherence and consistency were the methods by which science could distinguish true from false propositions while the neo-Platonists relied upon mathematic harmony as a criterion for truth. Among the believers in the primary/secondary distinction, there were those, such as Kepler, who believed that the mark of scientific method is quantifiability and those who, like Locke, believed that science dealt only with qualities which remained the same when perceived by any of the senses. Despite these many significant variations, it should be noted that all of these men believed that only certain aspects of human experience could be trusted and that only the special methods of science could distinguish these aspects from the rest of experience.

The next important group of theorists of science changed our account of scientific knowledge significantly, though they did not abandon the "principles of separation and verification". The important transition is marked by Berkeley and Hume's challenge of the primary/secondary distinction.

Berkeley took issue with the notion of primary and secondary qualities because of his epistemic principle, "esse est percipi". This marks a very significant turning point in the development of a distinction between subject and object. Berkeley laid to rest the argument that the real world is made up of more permanent qualities than the ones we are capable of perceiving. He did this on the basis of a principle of knowledge which asserts that all knowledge must ultimately be reduced to perceivables. We can see in this principle, the origins of the positivists' principles of verification. "I see this cherry, I feel it, I taste it and I am sure nothing cannot be seen, or felt, or tasted: it is therefore real. Take away the sensations of softness, moisture, redness, tartness, and you take away the cherry." (Berkeley, 1952, p. 117)

Hume made use of the phenomenalist reductionist principle to challenge the prevalent view of science which was current in his day. That is to say, he applied the "to-be-is-to-be-perceived" principle to many of the scientific ideas such as the Newtonian view of cause, time and space, and succeeded in challenging their credulity. Only those propositions which had an immediate connection with sensory experience were considered to be scientifically verified. All other propositions were considered to be tautological or nonsensical.

Hume's analysis of causality rests on a similar principle of reductionistic phenomenalism. Causality is an idea, which must be reduced to the constant conjunction of impressions if it is to be meaningful. In this connection it is interesting to note that Berkeley and Hume were both extremely critical of Newton's notion of absolute time and space. Their criticism of Newton's time and space were essentially no different than those adopted by the Vienna Circle.[1]

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[1] A more detailed account of reductionistic principles of verification used by phenomenalists is given on pages 73-81.

Before the British empiricists, the "principle of separation" was understood in such a way that certain sensory experiences were inherently subjective while others were inherently objective. The British empiricists maintained that all sensory experience is indubitable: the problem is to separate sensory experience from normal daily consciousness. In normal consciousness, language and other habits deceive us into attributing equal status to propositions which are very different in origin. Our frequent use of words, such as left and right, may mislead us into believing that these terms have a basis in reality, that is, similar to objects such as tables and chairs. In fact, "left" and "right" are organizational conventions, logical in origin, while "table and chair" are labels for a class of experience, empirical in origin. Newtonian time and space, according to Hume and Berkeley, are merely logical principles, like our number systems and have no basis in reality since they cannot be perceived. Science is the most reliable method for producing knowledge since it substitutes experiments for experience, and in doing so separates the formal from the empirical. It should be noted that the "principles of separation and verification" are still an essential aspect of this account of scientific epistemology. Verification, in this approach, means an immediate basis in sensory experience, best arrived at through scientific experimentation. "Separation," in this context, is between the formal and empirical as parts of experience.

Since the beginning of British empiricism, there have not been any significant changes in the overall view of scientific knowledge. British empiricism has developed into logical positivism. The many steps by which this evolution took place are covered in considerable detail in Chapter IV. But in essence there is little difference between Berkeley's "to-be-is-to-be-perceived" and contemporary, slightly more sophisticated principles of verification. The essential point which unites them is that one can distinguish objective experience by making certain that it has a basis in sensory experience. More recent versions have added the safeguards that these experiences must involve publicly observable and repeatable operations.

The "principles of separation and verification", despite the many variations in the manner of their expression, remain essentially unchanged aspects of scientific epistemology. It was suggested earlier that something more than interest and method separated epistemologists and psychologists in the pursuit of an account of knowledge. That "something more" is a prior commitment of both disciplines to the principles of separation and verification. Both groups of scholars accept that there is objective and subjective perception. Because of his interest in knowledge, the epistemologist can dismiss the psychology of perception since he is only interested in objective observation. He limits his interest to objective observation because it is here he believes that he will find a description of the process by which man establishes an indubitable. The psychologist, on the other hand, is interested in a description of man's use of his sensory capacities. He accepts as part of his scientific tradition the use of these capacities as a method of scientific inquiry, but he also wishes to understand their use in other contexts.

Commitment to the assumption of objectivity antedates the contemporary separation of epistemological interests into psychology and philosophy of science. How, then, can we rely on either of these methods as tools appropriate to inquire into the assumption of objectivity? Let us examine in turn the status of each discipline's capacities to deal with the question of objectivity.

It has already been indicated that by virtue of interest and methodology, experimental psychologists have only a secondary interest in the ontological veridicality of perception. That secondary interest is usually expressed in terms of demonstrating the conditions under which perception can be influenced by other processes. For example, psychologists are interested in demonstrating the influence of motivation in bringing about a misperception. Whenever they are involved in this secondary interest, psychologists rely on physicalistic methods to obtain the "correct" description of the stimulus and consider any deviations from this description to be an illusion. In doing so, they indicate their belief that the methods of physicalistic observation are qualitatively different from normal perception. As was pointed out before, this belief is also indicated by the fact that perception is studied by the method of observation.

Experimental psychology is committed to the subject/object epistemology primarily because experimental psychology developed in the late nineteenth century at a time when it was widely accepted as a credential of science that scientists were capable of making objective observations. The question as to whether or not objective observation was possible could not be asked once the answer had been presumed by the methodology. If we look at the current status of the psychologist's philosophy of science we find a division which parallels the subject/object position that we have repeatedly come across. The two predominant ideologies in contemporary psychology are behaviorism and phenomenology. These two camps are in agreement that there are two worlds - the subjective and the objective. On almost everything else, they are in substantial disagreement. (Koch, 1964)

That behaviorism and phenomenology agree about the subject/object split is very significant in understanding why neither school is prepared to deal with the subject of this inquiry. The purpose of this inquiry is to see whether there is any basis for making the assumption of objectivity. Such a question is obviously motivated by an interest in the psychology of science.

If this is properly a question of psychology, why is it that behaviorism and phenomenology are not prepared to deal with it? The answer seems to be that by virtue of accepting the separation between subject and object, each of these predominant schools of psychology have defined its subject matter around the question almost as though they were deliberately skirting the question. The behaviorist cannot evaluate the assumption of objectivity because he defines psychology so as to exclude anything which is not publicly observable. He wishes to study only man's behavior. In reaction to this point of view, the phenomenologist defines his subject matter so as to exclude anything which is publicly observable. He wishes to study man's experience. Questions about the psychology of science fall somewhere between these two. In particular the problem of assessing the role of objective perceptions seems to be outside the grasp of either of these points of view, even though it is clear that their present juxtaposition is somehow related to the assumption that such objective perceptions are possible. Skinner, (1953) Rogers, Koch, (1964)

For historical and conceptual reasons, the methods currently available within psychology are not well suited to deal with the question we are asking about objectivity. At least, this is so if we are speaking of these methods as isolated units. The method suggested in the next chapter for dealing with the assumption of objectivity will attempt to make use of "psychological" phenomena, but it will do so in a manner not consistent with either behaviorism or phenomenology. Before the psychological phenomena can be rendered useful to this inquiry, we must examine the other approach to epistemological problems, which is the philosophy of science.

It was indicated earlier that the separation of experience into objective observation and subjective perception has led to the development of two separate academic disciplines to deal with epistemological problems. It has also been indicated that because of historical and conceptual commitments to the assumption of objectivity, the first of these, experimental psychology, does not have methods appropriate to the purpose of this inquiry. We turn now to an assessment of the method currently available within the other academic discipline which deals with epistemic problems - the philosophy of science.

One of the reasons that contemporary philosophers who are interested in questions of epistemology turn to the philosophy of science is that they believe science has the only methods for establishing indubitables. The reduction of the field of epistemology into the field of philosophy of science constitutes a judgment about the justifiability of the assumption of objectivity. The question we must ask to determine whether there are methods currently available within the philosophy of science to deal with the assumption of objectivity is as follows: do the methods which philosophers use to study science have any inherent commitments concerning the epistemic status of perception? If they do, do these commitments preclude the use of these methods in this inquiry? The first thing which is of interest in answering this question is that philosophers of science are divided into two major groups on the question of method. These two groups are the empirical and the analytical philosophers of science. The first of these two groups consists of those philosophers who believe in an empirical approach to the study of science. According to this point of view, the study of science should itself be a science. Included within this group are some of the earliest and now classical contributors to our understanding of science. Notable among these are the work of Mills and Whewell. There is much in their account of science which contemporary students would consider analytic. For example, the two men debate prescriptive questions such as "what is the proper scientific logic of induction?" Nevertheless, the method which both men agree upon is empirical. They both seek to establish the correctness of their own positions by supporting them with historical "facts" within the history of science. Their commitment to an empirical approach seems to reveal their belief that scientific methods are capable of differentiating between subjectivity and objectivity.

Among the empiricist philosophers of science, the question of this inquiry - that is, whether or not objective perception is possible - is called the problem of semantics or empirical significance. (Stevens, 1939, Frank, 1957) A strictly empirical interpretation of the problem of empirical significance puts this problem squarely in the domain of experimental psychology. This is precisely the way scientists (as opposed to philosophers) who write about science tend to see the problem.

For example, Stevens (1939) has suggested that psychological research on problem solving and concept formation is ground-breaking work in understanding science. Helmholtz saw quantification as the distinguishing feature of science, but he saw in the rudimentary foundations of counting, a "psychological capacity" which is propaedeutic for quantification.

> If we scrutinize closely what is done in counting an aggregate or number of things, we are led to the ability of the mind to relate things to things, to let a thing correspond to a thing, or to represent a thing by a thing, an ability without which no thinking is possible. (Helmholtz, 1930, p. xiii)

The reasons psychologists have demonstrated little enthusiasm for this problem within the science of science have already been given. Even though there has not been any activity among psychologists attempting to integrate the problem of empirical significance into their research on perception, the reverse is not true. That is to say, some philosophers of science interested in the problem of empirical significance have attempted to incorporate some principles of perception in their account of scientific discovery. In recent years, there has been an increasing recognition among some philosophers of science that the psychological must be included along with the logical in order to obtain a genuine picture of scientific knowledge.[1] This is particularly true in giving an account of the "context of discovery" within science. (Kessel, 1968) The inclusion of psychological variables as well as the sociological factors

[1]Notable among these are Kuhn (1962), Hanson (1958), and Polanyi (1958)

among this new group's account of scientific discovery has had a very profound effect in shifting away from the traditional conception of scientific knowledge. It has forced a recognition that intuition, insight or creativity are more important factors in the progress of science than had previously been recognized. Formerly, an inductive prejudice as well as a desire to limit analysis to the logical and exclude the psychological presented a picture of discovery which has been called induction by enumeration. According to this view, scientific discoveries came about from inductive processes taking place in a series of repeated observations. This placed the burden of discovery primarily on laboratory observation. By bringing in psychological factors the new empirical scientists of science have reduced the role which laboratory observation plays in their account of scientific knowledge. They have, of course, not completely eliminated objective observation from their account of scientific knowledge. For this group to do so would run counter to their own methodology. According to this school, the role of observation is most significant in the "context of justification".

For the purpose of this inquiry, even the limited commitment represented by the belief that objective perception is possible in the "context of justification" is more than can be incorporated into the method. For as soon as any element of the assumption of objectivity makes its way into the method of the inquiry, then we are using it as a tool when what we want is to make it the subject of inquiry. There is no doubt, however, that this inquiry was inspired by the successes realized from the incorporation of psychological factors into the epistemology of science by such men as Kuhn, Hanson and Polanyi. It is reasonable to expect that the influence of psychological factors could potentially change our account of justification as much as this new group of empiricist philosophers of science have changed our account of discovery. The method of this inquiry must incorporate the spirit of these accomplishments while excluding the commitment to an empiricist's account of justification.

As indicated above, there are two schools of philosophy of science. They are divided on the question of method. We now turn to the second of these, which is the analytical school.

Proponents of the analytic methodology argue that public objects, such as science, are fundamentally normative structures consisting of symbolic language which is controlled by regulative principles. (Ayer, 1952; Carnap, 1966 and Nagal, 1961) The appropriate method for determining the correct use of this symbolic language does not rest upon an examination of its actual usage. The rules of the game of chess are examples of such normative structures and they determine what a correct chess move is. We do not say, for example, that children unaware of the rules of the game, who are playing with chess pieces, are playing chess. By the same token, we cannot rely on our observation of individual scientists at work correctly to define for us what is science.

Though this is an accurate presentation of the analytic point of view, it is by no means one to which all analytic philosophers would assent. A notable recent exception is revealed in the work of Harris. The traditionally analytic point of view, Harris argues, (MacKinnon, 1968-69) has generated a methodology which involves interpreting science by fitting a reinterpreted residue into a predetermined epistemological framework. Rather, he suggests, the philosopher must use the evidence afforded by the sciences to provide a comprehensive and coherent conception of the universe, and examine the methods actually employed in scientific investigation to discover standards for the reliability of this knowledge.

The various schools of analytic philosophy differ with respect to methods of analysis and epistemic commitments. In discussing models of the assumption of objectivity in Chapter IV, these schools are discussed with respect to their epistemic commitments in particular. At this point in the discussion, it will suffice to say that these schools maintain that science is the knowledge that is produced in the form of language. The philosophy of science, therefore is the clarification of the meaning of these languages by means of either exhibition or replacement analysis. (Korner, 1966; Ayer, 1952)

The word analytic is confusing because its precise meaning varies from school to school. For the logical atomists (Russell, 1943 and early Wittgenstein, 1922), it meant essentially to reduce all propositions to atomic sentences, the subject and predicates of which were objects and their properties in the real world. Analytic philosophers of a positivist conviction (Ayer, 1959) wish by means of sorting to clarify all propositions into three classes: the analytic, the synthetic, and the nonsense categories.

At first glance, it would appear that we have at last struck upon a method which is explicitly non-empirical and would therefore be most appropriate for this inquiry, since we are attempting to avoid methods which have an inherent prejudice towards objectivity. This, however, is not the case. As we pointed out above, since the time of Berkeley the principle of separation has been between analytic and synthetic aspects of experience, rather than between inherently objective and subjective elements of sensory experience. The analytic philosophers of science accept the analytic/synthetic distinction. They believe that an accurate understanding of science can only be achieved if work is done in both aspects. The scientist with his experimental techniques generates synthetic propositions but the ordering and clarification of such propositions is best achieved by individuals trained in linguistic and logical analysis. Consequently we find that contemporary analytic philosophy of science is committed to both the "principle of separation" and the "principle of verification", which is to say, it accepts the assumption of objectivity.

Nowhere is their commitment to the epistemic soundness of the analytic/synthetic distinction more evident than with respect to the question which is of central concern to this inquiry - that is, "what is the epistemic status of perception?" The majority of analytic philosophers of science maintain that there is an essential analytic difference between perception and scientific observation. Not only do they deny the relevance of psychological perception to knowledge but they maintain that the whole problem of perception is a metaphysical delusion.

For example, Ayer (1952) maintains that it is an accident of our language that we cannot refer in a grammatically correct manner to the qualities of an object without first using the grammatical fiction "thinghood"; that is, the subject of a sentence is a thing on which its qualities are predicated. This accident leads us to believe erroneously that the qualities are related to the thing rather than being the mere sum of all the qualities. The problem of perception is born from this linguistic delusion, since distinctions between real and apparent are made and create a metaphysical substrata.

> Thus the problem of showing how statements about material things are related to observation-statements, which is, in effect, the traditional problem of perception, might be thought to require for its solution that one should indicate a method of translating statements about material things into observation-statements, and thereby furnish what could

> be regarded as a definition of a material thing. But, in fact, this is impossible; for, as I have already remarked, no finite set of observation statements is ever equivalent to a statement about a material thing. What one can do, however, is to construct a schema which shows what sort of relations must obtain between sense-contents for it to be true, in any given case, that a material thing exists; and while this process cannot, properly speaking, be said to yield a definition, it does have the effect of showing how the one type of statement is related to the other. (Ayer, 1952, p. 24)

The kind of schema recommended by Ayer will be examined more carefully in the discussion of the behaviorist-positivist model in Chapter IV.

Collins (1967) has developed a schema which is aimed at demonstrating the logical independence of knowledge and perceptual claims. Though an exposition of this schema is too technical for the present context, the following points are made to demonstrate this his schema depends entirely upon language analysis and says nothing of perceptual abilities. Essentially Collins attempts to show that perception verbs are used with propositional statements, but this does not indicate that the verbs mean both perceptual and knowledge claims in the same context. In the claim, "he sees Mount Monadnock", we have essentially a perceptual claim since it is possible to make the claim about someone who has never heard about Mount Monadnock and therefore is not making a judgment "that that is Mount Monadnock". There appears to be a combined perceptual and knowledge claim in the following construction, "he sees that that is Mount Monadnock". Collins argues that these can be separated. There is a knowledge claim which can be logically separated from the perceptual claim by pointing out that it could not be attributed to anyone who did not know about Mt. Monadnock, even if it could be shown that that person is perceiving Mt. Monadnock. Insight into the essence of his argument comes from understanding his claim that the proposition that "Seeing is believing" is not analogous to "swimming is exercising". Doing a little seeing is not doing a little believing, as doing a little swimming is doing a little exercising. Collins concludes from this that even though perception influences knowledge, and knowledge influences perception, and that those influences can be demonstrated experimentally, this is no basis for asserting the inseparability of the two. The distinctions made by Collins and Ayer are not necessary conclusions from an analytic approach to the problem of semantics. They have embedded in them more than the analytic method as a presupposition. They follow in the tradition of Wittgenstein in assuming a representative theory of signs. Wittgenstein believed that the modern alphabet is essentially the same kind of sign system as was hieroglyphics. There is another analytic philosopher who presents us with a contrasting view to the notion that signs are representative.

Hanson distinguishes between symbols and signs, arguing that signs stand for referents while symbols characterize them. The atomic, protocol, or observation statements in science are not, as Wittgenstein (1922) maintains, maps or signs of the phenomena, but are more complex symbolic characterizations. It is not possible in Hanson's view to separate the knowledge claim from the perception claim, since the basic statements of knowledge give rise to alternative perceptions.

We shall see later that this is a debate between those who hold logical constructs to be fictional conveniences, as does Ayer, and those, as Hanson, who holds constructs to be "whole" principles of organization.

With the exception of Hanson, the large majority of analytic philosophers have disassociated the problem of observation from the problem of perception. They see no need for an integration between their methods and the evidence collected by experimental psychologists. In addition, the sharp distinction which these philosophers make between analytic and synthetic propositions reveals an explicit belief in that "principle of separation" which in turn reveals an implicit belief in the capacity of science to make objective observations by which to produce synthetic knowledge.

We have seen that one of the oldest and most persistent aspects of scientific thought can be understood in terms of two principles. The "principle of separation" which maintains that only certain aspects of experience can be trusted and the "principle of verification" which maintains that scientific method is the only way available for discriminating those aspects of experience which can be trusted and those which cannot. The "principle of separation" preceded the formation of our contemporary academic specialities; therefore, we developed a discipline with methods and interests for each of the two kinds of experiences that the seperation created. The philosophy of science would answer the most important question in philosophical epistemology which was, how does man establish an indubitable unit of knowledge, while the psychology of perception would give us a description of man's ordinary subjective experience. Even with each of these separate disciplines of epistemology there were other dichotomies which once again paralleled the "principle of separation," so that psychology divided into behaviorism and phenomenology and philosophy of science divided into analytic and empirical approaches.

Not all the developments in these fields militated against joining scientific epistemology and the psychology of perception. We noted above, for example, the work of a few philosophers of science who brought psychological factors to our understanding of the logic of discovery. There is also some evidence that many analytic philosophers are realizing that a combination of analytic and empirical techniques must be used to give an account of epistemology. A good example of this is available to us in the work of Harris. There is, unfortunately, no counter balancing evidence that psychologists are becoming more aware that certain analytic and normative processes, such as language, must be incorporated into their study of perception. There is no doubt that psychologists have for a long time recognized the influence of language on perception. They have not learned from analytic philosophers to analyze language as a set of publicly available prescriptions governed by normative regulations.

We have seen that these two disciplines have, as Hirst (1959, p. 24) puts it, dealt with these questions "in haughty isolation from each other", and we have tried to argue that there is more than just interest and method which has kept the two disciplines apart. That "something more" is the assumption of objectivity. What, therefore, can we learn about the conceptual requirements of a method to be used to inquire into the assumption of objectivity? The first thing to be learned is that there are immediate methodological benefits which can result from merely holding the assumption in abeyance. We can see this by examining what happens to the two major objections which have kept psychology and scientific epistemology apart, once the assumption of objectivity is held in abeyance.

From the discussion so far, it can be seen that the relationship between epistemology and perception is logically and historically ambiguous and controversial. Since this study intends to show that perception is epistemologically relevant in science, at least two philosophical distinctions between perception and epistemology must be dealt with. The first of these is that raised by Hamlyn (1961) in which he says that the purposes of epistemology and the science of perception are divergent. Epistemological philosophy is prescriptive and aims at justifying claims to knowledge. Psychology of perception is descriptive and aims at understanding the conditions which give rise to certain experiences. This distinction is one which most psychologists would readily accept and is one of the reasons which has kept perceptual scientists out of the business of developing sign theories for the epistemology of science. It seems, however, that the distinction must ultimately fail since it is precisely in the activity of perception that the normative and the experiential become fused. This is true in the ideographic sense that an individual in the activity of perception labels his experiences according to linguistic categories, and thereby shapes the experiential by way of norms. The normative and experiential also come together in the nomenthetic or public sense that language becomes fused to phenomena in science. The original intention of perceptual science to describe has lead to a description which involves necessarily normative epistemic considerations. In other words, some of the most important conditions to give rise to certain experiences or percepts are linguistic, and therefore normative.

Besides the distinction between prescription and description there is the claim made by Ayer (1952) and Collins (1967) that perception is a metaphysical problem, and therefore by definition falls into the realm of nonsense, or at least has nothing to do with the claim to knowledge. As was implied in a previous section, proponents of this view already are relying on a form of perceptual atomism which is by no means universally accepted. When Ayer claims that those who propose the perceptual problem are being deluded by language into thinking that the subject of propositions is a real substratum giving rise to its predicated qualities, he reveals his phenomenalism, which after all is a theory of perception. It is by no means clear from studying experimental psychology of perception that the primitives of experiences, or the givens are what Ayer calls "sense content". Nor is it clear that man has the capacity to simply and directly "represent" these sense contents in atomic sentences. It is preferable not to prescribe any limitations on what description will ultimately reveal, or to elevate a particular epistemological commitment to an epistemological prescription.

The method we choose will have to overcome the analytic/synthetic distinction: that is, it must somehow combine normative information with experimental information without assigning one to the realm of ideas and the other to the realm of facts.

CHAPTER III

THE METHOD OF ANOMALIES

At this point, a method which is neither analytic nor experimental is required. It has been demonstrated that the currently existing methodologies for dealing with questions of epistemology tend to be either analytic or empirical. We have already seen that the almost universal acceptance of this distinction militates against an inquiry into the question of objectivity. It does so by virtue of the fact that current methods have imbedded in them a commitment to the assumption of objectivity. The method which will be used makes no inherent distinction between analytic or synthetic, subjectivity or objectivity, veridical or illusory. In the context of the inquiry, perhaps the best name for this method would be the method of anomalies. This name suggests a new method, but in fact it is a method which is modeled after a scientific method which goes back to Galileo. This is the method of "thought experiments." (Myers, 1968) The method of anomalies requires a considerably detailed explanation but it will be helpful to begin by expressing some broad outlines and the major thrust of the method.

The method of anomalies provides a systematic way of producing counterevidence to an assumption which is the subject of an inquiry. Broadly speaking, there are five steps which must be accomplished. In the first place, an assumption of some significance in the current structure of knowledge must be identified. The next step requires making this assumption explicit. This involves reviewing the role the assumption plays in the various theories relevant to the domain in which the assumption is made. Having made the assumption explicit makes it possible to hold that assumption in abeyance and thereby collect anomalies to the assumption. These anomalies challenge the credulity of the assumption. The next step involves seeking an alternative to the assumption which will hopefully conclude the inquiry by making possible the last step in which the difficulty raised by the anomalies is resolved.

In order to examine the question as to whether or not the subjectivity/objectivity distinction is valid, we must set it aside in the method we use for examining the question. No matter how peculiar the method suggested appears to be, its peculiarity is a necessity, for to get rid of it would necessitate accepting a method which would assume in advance an answer to the question being asked. A natural question which will be asked of anyone who claims not to be working in either the analytic or synthetic realm, is: What realm are you working in? Kant had an answer to this question; he would have pointed to the realm of the a priori synthetic. Though Kant's a priori synthetic is an appropriate answer to this question, it is not one which this author had in mind when he chose this method. But if one thinks of Einstein's thought experiments in which he proposed physically impossible events, such as physicists riding elevators through empty space in order to get at conceptual consequences about the real world, one can easily see that this work is important despite the fact that it is neither analytic nor synthetic.

Perhaps the best known example of a thought experiment was the one given by Einstein[1] in which an elevator containing experimental physicists is first dropped in free fall in a gravitational field and then pulled at a constantly accelerated speed by a supernatural force in an inertial field. Einstein demonstrated that the physicists inside would confuse a gravitational field for an inertial field and vice versa. It should be noted that Einstein came to this conclusion by the method which analytic philosophers claim can only be arrived at through linguistic analysis. (Ayer, 1952) In other words, he proved that the distinction between inertia and gravity is analytic, not synthetic. He demonstrated that phenomenally you cannot distinguish between the two kinds of forces. This important conceptual clarification was done by pushing the meaning of the concepts inertia and gravity to the limit of their implications.

The method of thought experiments differs from the analytic method in that it depends upon propositional consequences. In other words, thought experiments aim to make conclusions which are relevant to the real world. They depend upon an "if-then" series of propositions rather than a definitional analysis.

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[1] This idealized experiment is described in detail in Einstein and Infeld (1938) pp. 214-222.

> ...thought experiments are similar to empirical experiments in that the results of both must be interpreted by means of propositions having either theoretical or factual status. Just as the "facts" supposedly revealed by a physicist's experiment are established by the experiment only if the notions of electricity, optics, heat, and so on governing the construction and use of the experimental apparatus are correct, so the philosopher's interpretation of the results of his thought experiments is acceptable only if the propositions he employs in his interpretation are acceptable. (Myers, 1968, p. 191, 192)

The discussion of Einstein's work at this point is presented only as an example of the fact that not all good scientific inquiry can be easily classified as analytic or experimental. Particularly in the context of discovery it is becoming more clearly recognized that fanciful imagination is an important part of scientific fruitfulness.

The reason so much stress has been placed on the non-analytic and non-experimental aspect of this method is that the specific assumption which we wish to examine in the inquiry is one which is generally used to justify the distinction between analysis and experimentation. Having made this point clear we must examine the various steps in the method to see first how they work in general, and secondly how they apply to this particular problem. The first step in the method of anomolies involves identifying an assumption. To understand this step we must begin with an examination of the role that assumptions play in knowledge in general.

Webster's tells us that an assumption is anything taken for granted, a presupposition. All knowledge systems, be they empirical or formal, make use of assumptions as a necessary beginning place. The important point to be noticed is that they are not subject to empirical test, but are used as a background for testing propositions of lower order of generality. Myers states this proposition very clearly when he says,

> It is evident that while science is self-correcting in respect to the system of belief it produces, it is not self-correcting, in respect to those beliefs such as that in the existence of reals which the doing of science presupposes. If, contrary, to the fact, these presupposed beliefs were wrong, then the practice of science would not correct them. Hence science does not provide an example of how someone working within a framework of material and methodological assumptions can critically examine the framework without standing outside it. (Myers, 1968, p. 184)

It is because assumptions are not subject to empirical tests that other methods are necessary for conducting inquiry into knowledge which already exists.

According to T.S. Kuhn (1962) it is characteristic of normal science that a certain cluster of achievements based on common assumptions form a defining network. This network, which he calls a paradigm, is accepted by most scientists as the source of questions to be worked on. Within such a paradigm there is no means of questioning the basic assumptions which are the starting place that came prior to the achievement of facts. Kuhn includes in his definition of paradigms, theories, laws, instrumentation and application which grouped together create a coherent research tradition. This tradition is what must be learned to become initiated into the science as one of its practitioners. The viability of the system rests on two characteristics. The first is that the paradigm as initiated is unprecedented enough to attract followers. Secondly, it is open ended enough to leave some problems unresolved. The course of normal science is characterized by Kuhn as one in which the unresolved problems of the paradigm are worked on in a "puzzle solving" fashion, but during which there is no questioning of the fundamentals of the paradigm. (Kuhn, 1962)

A paradigm is defined as a pattern, example or model and it is this broad sense of the term which is important in the present context. Most historians of science would consent to the existence of paradigms as just defined, even though they might disagree with the definition and interpretation by Kuhn.

It should be noted that historians do not agree that the notion of paradigm is applicable to all sciences. Watson (1967) notes that psychology is an example of science in which one can find lacking many of the characteristics of a paradigmatic science. There is no basic agreement among psychologists about theories or methods. Besides this ideological provincialism, Watson points to significant national differences in psychology as evidence of the fact that the first paradigmatic revolution has not yet arrived in psychology.

For the purpose of this discussion, however, Watson's objections are not critical, since he substitutes for paradigms the notion of prescriptions. In doing so he differentiates for us between those aspects of the Kuhnian definition and the broader definition referred to above, making it clear that some form of assumptive structure is needed in a science as a set of guiding principles. Watson says of his prescriptions,

> The overall function of these themes is orientative or attitudinal: they tell us how the psychologist-scientist must or should behave. In short they have a directive function. They help to direct the psychologist-scientist in the way he selects a problem, formulates it, and the way in which he carries it out." (Watson, 1967, p. 435)

For the purpose at hand, which is to demonstrate what the role of assumptions in science is, the differences between prescriptions and paradigms are not significant. It can be seen that in either case a form of a guiding principle which is obtained as part of a scientist's training, and can be traced through a period of time in history, is being taken for granted by the individual scientist in his research.

It is clear that there is agreement on the proposition that all sciences have imbedded in them certain assumptions and that those assumptions are not subject to the self-correcting influence of inquiry. They are above those influences and therefore one must stand outside the science in order to question them. The full and complete impact that such assumptions have on the knowledge which is generated under them is simply not known to us. But there are two important insights about the influence of assumptions which have been identified. The first of these two insights is that assumptions play a very significant role in defining the problems and selecting the issues which shall be investigated within a discipline. The second insight into the influence of assumptions is less definite but probably more important: it appears to be desirable to eliminate as many assumptions as possible. There is some evidence that suggests that some great scientific breakthroughs have occured as a result of removing old assumptions, rather than by adding new information.

Let us now turn to an examination of the roles which assumptions play in defining the problems within a domain of knowledge.

It is in the realm of presupposition and assumption that we find the source of problem definition. For this reason science must have methods for looking within itself.

> Real progress in any science comes not merely by "adding to" existing knowledge, but by becoming aware of our assumptive worlds, conscious of their inadequacy, destroying and disintegrating them, and then rebuilding them in the constant search for more adequate formulations. (Cantril, 1949, p. 375)

In this connection the transactionalists have attempted to develop a systematic method by which one can come up with new questions in science. This is done in the belief that problem definition is as significant, if not more significant, than problem solution. Heisenberg agrees that this is a problem in the physical as well as the social sciences. He maintains that we have developed experimental methods to generate and evaluate solutions but that we have inherited from the Greeks a propensity for an intuitional approach for generating and evaluating the validity of questions. Many contemporary writers, (Bridgman, 1949; Stevens, 1947; Ayer, 1952) have taken the position that the principles of operationalism constitute one set of criteria for discriminating meaningful from nonsense questions, while the meaning of operationalism is intimately related to our notions of objectivity. The discussion of operationalism as a method for evaluating questions will be postponed. It is our view that operationalism offers no systematic method for generating questions.

The method of challenging assumptions by the means of anomalies, which is roughly modeled after the thought experiment, is a method which will yield significant new questions. An example which we have already discussed at length is the set of new questions which become evident from the reintegration of the epistemology of science and psychology of perception. Questions about problems which these two questions have in common are not evident as long as one assumes a difference between objective observation and subjective perception. For example, it is for the most part not recognized that the data language problem in philosophy of science and the behavioral indicator problem in the psychology of perception are essentially the same problem. In experimental perception there is probably no greater difficulty than the fact that the experimenter must rely on some kind of verbal or behavioral indicator to evaluate the phenomenological. No definitive solution to this problem, is yet available, which can assure an experimenter that two subjects responding to the same words or other behavioral indicators are experiencing the same phenomenological qualities. This problem, which is as familiar to psychologists, has an exact counterpart in the philosophy of science. In this context, the problem has to do with the relationship of language to phenomena. The philosopher of science must study the language of scientists and assume that the language is an adequate indicator of the phenomena which the scientists study. Notice the similarity in the quotation below to the familiar problem in perceptual psychology.

> There is no disagreement on the phenomena to be studied, but when one comes to tell what these phenomena are, or how they are to be categorized, then the strife begins. (Kattsoff, 1957, p. 27)

The belief that there is agreement on the phenomena under study but no consensus as to how to tell what the phenomena are about or how to categorize the phenomena, is parallel to saying that certain perceptual experiences are taking place without responses from which they can be inferred. If all the evidence one has to go by is in fact public knowledge, then it must consist of the telling and categorizing of the phenomena by the scientific community; otherwise, we are forced to maintain that we cannot express what we are in fact agreed upon.

In addition to suggesting new and important questions, there may well be a more direct benefit from challenging existing assumptions. In recent years some writers have emphasized that many of the significant changes which have taken place in science came about, not from adding new knowledge to what was already known, but from destroying something which had been taken for granted within science.

Agnes Arber (1964) is one such writer who acknowledges that basic assumptions are necessary and inevitable. She believes that we have opened new areas for actual investigation by reducing these assumptions to the smallest possible number.

> As a single example, from biology, of something which was long held, without adequate reason, to come into the category of the given; and to be thus immune from inquiry, we may recall the affirmation that the leaf is a basic unit of the plant body in the angiospersms. So long as this was assumed, any effort to understand the morphology of the leaf was forbidden; the leaf was a concept which one could not, as it were, get behind. When, however, the ban was lifted, and the leaf lost, the privileged position accorded to it as an organ sui generis, the way was opened towards interpreting it. (Arber, 1964, p. 81)

More evidence that it is desirable to eliminate as many assumptions as possible comes to us from Polanyi (1967). He has investigated the logic of negation and he holds that many important scientific breakthroughs are cases in which problems arising from a certain assumption are eliminated by negating the validity of the assumption rather than by solving the problem. Among these are the Principle of Inertia, the Second and Third Laws of Thermodynamics, the Theory of Chemical Elements, the Principles of both special and general Relativity, the Principle of Indeterminacy, and the Pauli Principle. Taking only one of these as an example, certain ideal conditions which were assumed in mechanics led scientists for many years to believe in, and in fact work on, the construction of perpetual motion machines. The fact that no one is presently engaged in solving this problem does not mean that the problem has been solved. It means that the impossibility of the problem has been raised into a universal principle in the form of the Second Law.

If Polanyi and Arber are correct in assuming that by reducing the number of existing assumptions we are making scientific progress, then it means that challenging assumptions is desirable and that we should try to develop systematic methods which allow us to challenge assumptions.

Having gained an understanding of the significance of assumptions in knowledge we may now turn our attention to the next step in the method. That next step involves making as explicit as possible the assumption which is the subject of inquiry.

After having said this much on the significance of underlying assumptions, it is disappointing to say that very little has been written and presumably this means little is known about how one identifies such an assumption. As these assumptions are related to science they tend to be propositional; that is, they tend to be assumptions about the nature of things. Classic examples of such assumptions, so blatantly identifiable post hoc, are the assumptions of absolute time and space. A further example is that described by Arber concerning the status of the leaf. Not all assumptions related to science are of this form however. At least one set of assumptions in science is concerned with the abilities of scientists, rather than the subject matter which they study. Among these are the assumptions scientists make about their power of observation. It has already been mentioned on several occasions that these fall within the domain of the psychology of science.

No matter what the assumptions are about, the fact that they are so difficult to identify can be understood when we consider that they have an implicit and an explicit aspect. The explicit assumptions in a science are always available for challenge. A good indication of this is the fact that Hume (1952) and Berkeley (1952) as well as Newton himself (Michelmore, 1962) challenged the assumption of absolute time and space. They were not successful because their challenge was explicit and they did not carry the challenge through all of its implications. Einstein, on the other hand, not only challenged this assumption, but showed us what difference it made. Hopefully, this dissertation will succeed in justifying the belief that explicit assumptions are available in the formal structure, and that we can understand their meaning by carrying them out to their logical conclusions.

Implicit assumptions make their influence felt in the perceptual, rather than in the logical, realm. Changes in explicit assumptions can be made without a change in implicit assumptions. The consequence of doing so is to produce oddity. It has often been said that only a few men truly can understand Einstein's theory of relativity. This claim rests upon a belief that only a few men can treat physical propositions on an exclusively logical level. Having mastered the mathematics allows one to calculate consequences which are logically independent of absolute time and space. When we convert the mathematics to description, we involve a language which is based on the implicit assumption of absolute time and space. The consequences must necessarily seem to us impossible because we cannot imagine them.

The seeming impossibility of some of the relativistic conclusions is due to the fact that at the level of "common sense", and therefore perceptually, we are Newtonians. The same kind of discrepancy applies to theories of perception and philosophies of science. As professional scientists, psychologists may conclude various things about perception which lead us to think that everyday perception is not veridical. Nevertheless in his everyday experience the psychologist operates on the assumption of perceptual realism. Likewise scientists and philosophers of science may take positivist and other positions which deny any metaphysical claims on reality, but in their everyday lives as well as in the laboratory they operate as realists. The assertion that assumptions can influence perception is made on the basis of much perceptual research. The most direct evidence for this point is provided by Ames and his colleages. Some of this research will be examined in detail in Chapters IV and V. At this point, Ames' words are sufficient to communicate the relationship between implicit assumptions and perception.

> That it is our perceptual world and not our abstracted world that is most basic and is directly related to our purposes is disclosed by the fact that in a concrete

situation it is our perceptual response (sensations) and not our abstracted concepts that determine our actions (distorted ROOM). (Ames, 1960, p. 14)

In the case of the distorted room, the assumption that "this is a room" does not have to be made explicitly by the subject, it is immediate in his perception. Furthermore, changing the implicit perceptual assumption requires more than merely telling the subject that it is a distorted room. One must tell the subject how the room is distorted; that is, one must carry it out to its experiential consequences. Identifying an underlying assumption means more than pointing to its explicit name, such as the subject/object split or absolute time and space. It means getting a feel for the experiential consequences of making such an assumption.

At this point, it is of crucial importance to consider the use of anomalies in challenging assumptions. To do this we must clarify the meaning of the term anomaly and the term challenge.

Webster's defines an anomaly as anything which deviates from the general rule, while Kuhn, to whom we are indebted for the idea of anomalies, sees them as facts which are contrary to the existing paradigm. Kuhn himself is unhappy with being forced by linguistic convention into calling anomalies facts as opposed to theories.

Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the

> anomalous has become the expected. Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed, until the scientist has learned to see nature in a different way - the new fact is not quite a scientific fact at all. (Kuhn, 1962, pp. 52-53)

The first question which we must consider is one of factuality. Are anomalies simply experimental facts which cannot be accounted for by existing theory? Most observers will agree that not all experimental results which cannot be accounted for by existing theory are to be considered anomalous in Kuhn's sense of the word. The majority are attributable to error and most are ignored.[1]

Three contemporary philosophers of science who have pondered the problem of scientific discovery are important in understanding the factuality of anomalies. They are Popper, Kuhn and Feyerabend. These men hold complex and often conflicting views concerning the evolution of scientific ideas, yet each is identified with a particular tenet which is relevant to this question.

Popper's views with respect to the problems of induction are of special importance in an analysis of the role of perception in science, but this will be taken up in a later section. The part of Popper's philosophy which is relevant to this question is his emphasis on the deductive logical consistency of scientific knowledge systems. He briefly states his position as follows:

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[1] A great deal of relevant literature concerning the disposition of counterevidence is available on this point. Notably the work of Polanyi, Duhem, Popper and Feyerabend.

> My point of view is, briefly, that our ordinary language is full of theories; that observation is always observation in the light of theories; and that it is only the inductivist prejudice which leads people to think that there could be a phenomenal language, free of theories, and distinguishable from a "theoretical language"; and lastly that the theorist is interested in explanation as such, that is to say, in testable explanatory theories: applications and predictions interest him only for theoretical reasons--because they may be used as tests of theories. (Popper, 1959, p. 59)

That facts do not stand up by themselves is important in understanding anomalies. Kuhn has indicated his belief that crisis in the evolution of science results from the experience by certain scientists of anomalies to the traditional paradigm. We must keep Popper's perspective in mind when using the word anomalies, for it would make no more sense to believe that bits of information against a theory can exist independently, than to believe that they can exist independently to support the theory.

Kuhn himself is not clear on this point. He says on the one hand that the discovery of oxygen by Lavoisier was anomalous to the existing chemical paradigm. He goes on to recite that if the criterion of discovery is the isolation of the gas, then it predates both Priestly and Lavoisier. If, on the other hand, the criterion be the understanding of the chemical qualities of a gas then Lavoisier's attribution of acidic and atomic characteristics to the gas means that he fails to discover it. He goes on to show that no experiment in any specific time or place can be isolated in which oxygen was discovered. The term discovery is in part to blame because it leads us to believe that oxygen was experienced the first time through some exclusively inductive process. As Kuhn (1962) points out there is evidence to suggest that Lavoisier was dissatisfied with the entire existing paradigm and that this dissatisfaction was essential to his discovery.

If anomalies are not ontological contradictions bumped into by scientists engaged in the evolution of normal science, then we must seek an alternative set of dynamics to account for the crises that occur in the evolution of science. It is Feyerabend (1961) who provides such an alternative by suggesting that scientific progress takes place during crisis periods while an abnormal pluralism is flourishing. Normal science to Feyerabend is monistic and dogmatic and if it is not challenged for a sufficient period of time then it will become metaphysical. With Feyerabend, the role of anomalies is considerably diminished. The view held here falls somewhere between Kuhn and Feyerabend. Anomalies are neither ontological stumbling blocks to an otherwise monistic theoretic bliss, nor mere fallout of theoretical debate. We must conclude, therefore, that at the present time there is no clear cut answer as to whether or not anomalies are simple experimental facts.

There is another question which arises out of the way. From reading Kuhn one is lead to believe that anomalies arise at a time just before revolution to a new paradigm. It is the present writer's view that old facts explained by the existing paradigm can become anomalous if offered in the right context. "The principle of inertia, the law of conservation of energy were gained only by new and virginal thoughts about well-known experiments and phenomena." (Einstein, Infeld, 1938, p. 96)

With these considerations in mind, let us consider a definition of what is intended here by the word anomaly. An anomaly consists of phenomena viewed in a context which suggests that our expectations with respect to the behavior of the phenomena are in error. As an example, consider Einstein's thought experiment. None of the facts concerning the behavior of the physicist's experiments in the elevators is new facts; none of the facts is in themselves surprising or anomalous, but the total effect of seeing these facts under conditions where we expected other facts, gives rise to our suspicion that something is awry.

If one has been successful in identifying an underlying assumption and understanding its implications, it should be possible to produce anomalies merely by asking for counterevidence to the assumption. Challenging the assumption means looking at the anomalies as they fit under the conditions prescribed by any paradigm or model which makes use of the assumption. In this way we can make comparisons between various models' ability to deal with the phenomena. Scientific models are positive generators of facts. We tend to see the models with the facts they have produced. In this situation, we try to make them deal with the facts which are relevant to their domain, but not necessarily produced by them. Here we can look again at Einstein's thought experiments as an example of this technique. He often used facts produced by terrestrial physics in contexts, ideal or real, which were not part of the conditions Newton assumed in his model. The famous example in which clocks travelling at different speeds are found to keep a different time relies on the fact that the original context which was assumed by Newton has been changed.[1] This produces a fact about these two different time pieces which Newton should be able to account for, since his model purports to have general laws about time. Hopefully it can be demonstrated that in the field of perception, various models have generated facts which appear to fall within the same domain but, in fact, these models are very often generating anomalies for other models of perception. By challenging, we mean that we are making demands on the model which we would expect it to be able to fulfill if it were an adequate model to account for the domain of which it is a model.

Once the assumption of objectivity as it exists in the various models of perception has been challenged, we should be able to detect the conceptual requirements of an adequate alternative to the assumption of objectivity. Such alternatives could involve different kinds of solutions. An alternative assumption to that of objectivity might be made which does not have the problems which are revealed by the challenge to objectivity. Adjustments might be made in other aspects of the models so that they no longer depend on an assumption in

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[1] This idealized experiment is described in detail in Einstein and Infeld (1938), pp. 214-222.

this area. This is the same as saying that we might find that the assumption did not play an important role. Finally, we might conclude that the assumption of objectivity is justified under certain conditions and not under others. Whatever the case might be, the alternative will have to be explored in terms of its consequences for the resolution of problems raised by the challenge.

In summary, it will be beneficial to review the five essential steps in the method of anomalies.

The method of challenging assumptions by means of anomalies does not fit into the usual classifications of analytic or synthetic. This is so because the usual division presumes objectivity which is the subject of the inquiry. This method resembles the thought experiment in that it makes use of already known facts to explore the implications of combining these facts with assumed conditions.

Challenging assumptions is important because assumptions are taken for granted by the usual methods of inquiry. Paradigms or prescriptions are examples of sets of guiding assumptions in science. They generate the questions scientists ask and the problems for which they seek solutions by means of empirical methods. The reduction of the number of assumptions seems to be another good reason for challenging assumptions since there is evidence that some scientific progress came directly from the negation of assumptions.

Identifying an underlying assumption means more than being able to call its name or to isolate it within the formal system. It offers an opportunity to trace its logical and experiential consequences. Identifying the assumption explicitly is an analytical problem. Identifying the assumption implicitly means demonstrating the concrete differences the assumption makes in any given system.

Demonstrating the concrete differences that an assumption can make means dealing with phenomena as well as with theories. In this method the phenomena which are used are called anomalies. An anomaly consists of phenomena viewed in a context which suggests that our expectations with respect to the behavior of the phenomena are in error. Challenging assumptions in this context means making demands on the assumption, forcing it to deal with material which is contradictory to it, but relevant to its domain.

The result of challenging the assumption is an assessment of the adequacy of the assumption and the development of conceptual requirements for an alternative which would be more adequate than the assumption being challenged.

CHAPTER IV

MAKING THE ASSUMPTION OF OBJECTIVITY EXPLICIT

The purpose of this Chapter is to examine the assumption of objectivity as it fits into various theories of perception and observation. To accomplish this, five different models require exposition. These models are complexes of philosophical and psychological theories of empiricism. It is obvious that not all aspects of these models can be dealt with in this context. Our primary goal is to see how each defines objectivity.

Two kinds of perceptual theory must be reviewed in order to get a proper understanding of the prevalent models of perception which are available. They are philosophical prescriptive theories and psychological descriptive theories. The two kinds of theories combine to form a model of perception. The prescriptive theory is the more general of the two. It supplies us with rules or minimal requirements which any of the descriptive theories must meet. For example, the prescriptive realist sets up requirements which must be met by any description but he will permit any description that meets the standards. Five such models will be developed: they are the realist, the positivist, the configurationist, the neorealist and the transactionalist.

It will become obvious that not all of these five models addresses itself directly to the assumption of objectivity. Furthermore, to the extent that they do so, their formal definitions differ, as do their emphases on certain aspects of these definitions. If we are to maintain the criteria of synonamous definitions, we will undoubtedly find that all of these five models make different assumptions about objectivity.

Webster's defines objectivity as "an objective state or quality, or objective reality," objective is further defined as "1. of or having to do with a known or perceived object, not a mental image or idea. 2. being, or regarded as being, independent of the mind; real 3. concerned with the actual characteristics of the thing dealt with rather than with the thoughts, feelings, etc." Two aspects of this definition are worth emphasis. The first is the feature of independence,[1] objective knowledge is independent of the knower. The second feature is implicit in all the definitions, but is not stated. This is the feature of certainty. When something is objective, we are sure of its truth and meaning. We hold the classical definition of objectivity to imply independence and certainty. Each of the five models which follows will be examined in order to answer these questions: Does the model allow for the classical definition of objectivity? In what ways does it differ from the classical definition of objectivity? How is the objectivity that it does allow, achieved?

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[1] The feature of independence is discussed in more detail on pages 148-150.

A realist is one who believes that there is an existential world which is independent of us and external to us and that this world can be known by us. Early naive forms of realism held that objects conveyed images directly to our minds through some ethereal medium. Though this picture is still consistent with much of our daily experience, it is seldom taken seriously by philosophers or scientists. One of the earliest forms of realism to be taken seriously was the dualistic position described in Chapter II as the doctrine of primary and secondary qualities.

Locke's (1967) theory of perception is an example of an indirect realism since it holds that only part of what we perceive can be attributed to the object independently of our perceiving it. Certain qualities, such as color, are only in us and are dependent on the senses. Other qualities can be apprehended by several senses and, therefore, they are held to be in the object. This view was widely held until very recent times. (Montague, 1965) One of the major reasons for believing it was that it paralleled the qualitative/quantitative distinction in science.

> From the belief that the nonquantitative qualities of objects are ineffective and useless, it is but a short and tempting step to the belief that they are not really objective attributes at all, but merely subjective effects which are produced upon the mind of the observer, and which exist only therein. The dualistic theory makes this step possible, for, according to that theory, the objects presented in direct perception have their locus with the percipient, and are in no sense numerically identical with their extra-organic causes. Then, too, if the secondary or nonquantitative qualities have ceased to be welcome in the realm of physical causes, they have

> to be, out of mere decency, as it were, provided for in an asylum, and what more natural than to regard the mind itself as that refuge? Since the secondary qualities are restricted in their effects to the perceiver's own processes, it seems appropriate to think of them as essentially and exclusively mental in the nature, and hence as of interest to psychology rather than to physics. (Montague, 1965, p. 197)

The primary/secondary position has a complex description of the perceptual process which holds that the qualities which we experience are only partially representative. The perception of color, for example, has some aspects which are primary; that is, they are a function of a feature of the incoming light stimulus. Such qualities can be expressed quantitatively in terms of duration and intensity. Other aspects of color perception are due to the effects of light on the sensory system. An example of such a secondary quality is hue. We have already quotedLocke to the effect that he was in doubt that we could ever discover and describe the systematic relationships between primary and secondary qualities.

One theory which attempts a description of the relation between external and internal qualities is Johannes Müller's theory of specific nerve energy. (Allport, 1955; Boring, 1942) It holds that the qualities one is aware of are due to the nerve which has been stimulated, not the object that stimulated the nerve. Müller himself held that a nerve could be stimulated by any kind of stimulus and still give rise to the same conscious quality. His followers, however, modified the theory in such a way that nerves were also specific as to the kind of stimulus which would activate them. Helmholtz' theory of hearing which holds that each discriminable tone has its own specific nerve, and Herring's theory of color-vision which postulates a similar recepter specificity, are still generating experimental research. (Allport, 1955, Boring, 1942)

Let us now turn to our questions concerning the status of objectivity which implies the certain knowledge of qualities independent of the knower. According to this dualistic position, objectivity is achieved by making a sharp distinction between scientific observation and daily perception. Scientific methods allow us to distinguish the quantifiable qualities from the subjective ones. In normal perception there is a confounding of the objective and subjective qualities. Scientific observation must develop special techniques for reducing knowledge to quantitative variables or qualities. Those aspects of experience which are due to primary objective qualities demonstrate their independence and certainty by virtue of the fact that they are amenable to quantification. The objective world which we come to know through the use of scientific observation is therefore different from the world of daily experience.[1]

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[1] Other forms of realism will be treated in subsequent sections. Russell's critical realism is better understood within the context of phenomenalism, while perspective realists such as McGilvary and Gibson are treated later as neo-realists because of historical and philosophical differences.

Currently, logical positivism is by far the most influential philosophy of science among scientists and philosophers alike. In fact, for many, it encompasses all of philosophy. Logical positivists are in agreement on two fundamental tenets. The first is that there are only two kinds of knowledge, synthetic and analytic. Synthetic knowledge consists of facts acquired through observation, and their validity is determined empirically. Analytic knowledge consists of logical operators by means of which facts can be related to one another. The proper use of analytic knowledge is determined by analytic methods. Analytic knowledge is tautological since all analytic propositions are ultimately reducible to the form A is A. Even though all their implications are not intuitively obvious, -- for example, all the mathematical derivations of a set of axioms are not intuitively obvious -- they are necessarily deducible from such axioms.

The second fundamental tenet to which all logical positivists are committed is that the meaning of a proposition is the method of its verification. A proposition can only be meaningful if it is verifiable in principle. Verifiability is always defined in terms of observations. It is over the precise meaning of observation that logical positivists are divided into phenomenalists and physicalists. Phenomenalists believe that the referents of observation are sense data, while physicalists believe that the referents of observations are physical objects. The phenomenalist position is the older form of radical empiricism dating back to Berkeley. Among its most notable advocates were: Schlick, early Carnap, Moore, Ayer, Bergmann, Godel, Waismann, Feigl, Price and early Russell. Physicalism was developed somewhat later and in part as a reaction to the ambiguity of verification principles as seen by the phenomenalists. Physicalism ultimately became the basis for the unity-of-science hypothesis accepted by most members of the Vienna Circle. Among its most important representatives are late Carnap, Feigl, Frank, Reichenbach, Hempel, and Sellars.

The phenomenalist is one who believes in Berkeley's maxim "to be is to be perceived." The phenomenalist believes that there is only one thing we can be sure of, only one indubitable: the existence of sense data. All knowledge must be based on sense data if it is to be viewed as empirical knowledge. It is often said that phenomenalism is a theoretically neutral position. It is probably better to say that phenomenalism asserts nothing about metaphysics and very little about epistemology. That very little, however, is by no means neutral. How little phenomenalism implies is made explicit by H.H. Price:

> It may be worthwhile to mention explicitly a number of things which we are <u>not</u> committed to.
> 1. We are not committed to the view that sense-data persist through the interval when they are not being sensed. We have only to admit that they exist at the times when they are being sensed.
> 2. We are not committed to the view that several minds can be acquainted with the same sense-datum. We have only to admit that every mind is acquainted with some sense data from time to time.

> 3. We are not committed to any view about what is called "the status" of sense-data in the universe, either as regards their relations with other types of existent entities. They may be events, or substances, or states of substances. They may be physical; i.e. they may be parts of, or events in, material objects such as chairs and tables or (in another theory) brains. They may be mental as Berkeley and many others have held. They may be neither mental nor physical.
> 4. We are not committed to any view about their origin. They may originate as a result of processes in material objects, or of mental processes, or of both. Or again, it may be that the boot is on the other leg: it may be that they are ultimate constituents of the universe, and material things (perhaps minds as well) may be just collections of them; in which case they "just are", and have no origin and no explanation, since every thing else is explained by reference to them. (Price, 1959, pp. 113-114)

This statement certainly represents the minimal statement of the phenomenal position. But even at this minimal commitment, it is necessary to disagree with those, such as Ayer and Price, who maintain that all perception theories are phenomenal. The most notable exception to the "sense datum is primitive" notion are the gestalt psychologists and the neo-realists, such as J.J. Gibson. These two groups assume that perception is primitive and do not agree with the phenomenalist presumption that perception is a more complexly organized meaningful awareness which is problematically related to a more primitive sense data. Despite these exceptions, however, phenomenalism does allow a very wide range of positions, both philosophical and psychological, concerning the problem of perception. It is precisely because seemingly imcompatible positions, such as realism and idealism, can agree on the phenomenal test of reducing all things to impression, that a phenomenalist like Ayer can maintain that the difference between these two positions are nothing more than linguistic preferences.

In order to understand the role of objectivity in these various models, it is not necessary to compare contrasting phenomenal positions. Whether or not the realist or idealist is correct metaphysically will not make any difference if he is at the same time a phenomenalist. The test of propositions for the phenomenalist involves reducing propositions to denotable sense data. It is this which gives a proposition its meaning. Whether sense data are signs of external objects or internal ideas is a separate question not itself subject to the principle of verification, and according to some, it is therefore an erroneous question. The search for the meaning of objectivity in this model will require that we examine carefully all of the available formulations of phenomenal verification principles. This will give us the prescriptive or philosophical theory of perception. We will also find that each of these can be elaborated descriptively.

Berkeley rejected the notion that any qualities could be said to be primary, by which he meant existent in matter. The same arguments which proponents of the primary/secondary view used to dismiss secondary qualities, like heat, are also applicable to primary qualities, like figure and extension. As Berkeley puts it, the same "eyes from different stations", or "eyes of different texture" will apprehend figure and extension differently. (Berkeley, 1952, p. 415) For this reason the substantive material qualities have no greater claim to an independent existence than do the secondary qualities. This does not preclude a belief in a science based on observation. "...by a diligent observation of the phenomena within our view, we may discover the general laws of nature, and from them deduce the other phenomena..." (Berkeley, 1952, p. 434)

According to Hume, the test for any complex idea involves examining its definition and determining the simple ideas of which it is composed. If there is still ambiguity concerning any of the simple ideas we merely have to bring them to the final test, which is to revive the impressions of which the idea is a faint copy. "The most lively thought is still inferior to the dullest sensation." (Hume, 1952, p. 455)

In Berkeley there is an implied distinction between sensation and perception. When we say we hear a coach we are really inferring that the sound we hear is made by a coach. The sound in this example is the specie of given which contemporary phenomenalists accept as sense data. The coach is an interpretation of the sense data. These would be referred to today as cognitions. Within his notion of impressions there is no clear distinction between perception and sensation.

In most of his applications of the test of reduction there are at least the seeds of distinction between sensation and perception. When Hume treats "power" as he did "cause", he says that each man is "conscious" of the power by his will to move his limbs.

> A man, suddenly struck with palsy in the leg or arm, or who had newly lost those members, frequently endeavors at first to move them, and employ them in their usual offices. He is as much conscious of power to command such limbs, as a man in perfect health is conscious of power to actuate to any member which remains in its natural state and condition. But consciousness never declines. Consequently, neither in the one case nor in the other, are we ever conscious of any power. (Hume, 1952, p. 472)

In the question we can see that Hume uses two senses of the word conscious. Certainly in the case of a palsied man there are some immediate impressions which he experiences as the loss of power. It requires a close, diligent analysis to realize that the experience of power is but the influence of past experience on present impressions. It is consistent with Hume's position to say that the sense data or impressions combine with past experience or ideas to result in a percept of power.

The conception that perception is an interpretation of sensation is the fundamental phenomenalist model. It has persisted since Berkeley in various forms of psychological theories and until recently as a tenet of positivism.

One of the earliest psychological theories to be elaborated experimentally, which fits under the rubric of phenomenalism, was Wundt and Titchener's structuralism. In particular, Titchener's core-context theory had many features in common with contemporary phenomenal definitions of meaning.

Titchener's core was pure sensation, and this was defined by the structuralist in terms of elements. It was experimentally described in terms which were similar to those used by philosophers of science, such as E. Mach, F.E. Moore, Ayer and Russell, in describing "patches of colors". They were "here, now", having particular shapes and extensity. Psychologists will recognize in G.E. Moore's description below reminiscences of the introspective subjects' diligent attempts to avoid the stimulus error.

> I hold up this envelope, then: I look at it, and put it down again. Now what has happened? We should certainly say (if you looked at it) that we all saw that envelope, the same envelope; I saw it, and you all saw it. And by the it, which we all saw, we mean an object,...But now, what happened to each of us when we saw that envelope? I will begin by describing part of what happened to me. I saw a patch of a particular whitish colour, having a certain size, and a certain shape, a shape with rather sharp angles or corners bounded by fairly straight lines. These things; this patch of a whitish colour, and its size and shape I did actually see. And I propose to call these things, the colour and size and shape, sense-data, things given or presented by the senses - given, in this case, by my sense of sight. (Moore, 1965, p. 98)

The description given by Moore dates back to approximately 1910 when he introduced the notion of sense data into the mainstream of British philosophical thinking. The criteria of sense data had already been applied in a thorough reexamination of Newtonian physics by Ernst Mach. At the same time that the structuralist psychologists were trying to develop an experimental description of sensory elements, Mach was insisting that all scientific concepts be reducible to sensory elements. In addition, it is clear that Mach was in agreement with the structuralists that a trained individual was required to distinguish between sense data and everyday perception. (Hill, 1961)

This new form of radical empiricism anticipated much of what was later to be called operationism. It represented a no nonsense application of a verification principle that Mach applied to rid physics of metaphysical speculation. Such speculation made its way into science in the form of forces and substances for which there was no sensory basis. Many writers have since paid tribute to Mach for a conceptual clean-up job without which an Einstein would not have been possible. (Capek, 1961; Frank, 1957; Einstein and Infeld, 1938)

At this point, a curious involution takes place in the development of the status of the perceived object. In the philosophy of science the physical object is distinguished from the material object while in psychology the object of perception is distinguished from the stimulus. Russell and Wittgenstein developed their theories of description which involve logical maps of the external world. The elements in these maps have varying degrees of certitude beginning with the only true empirical knowledge in the form of atomic facts, which though they cover a small range of generality, they are the only things of which we can be certain. Such atomic facts can be reduced to sense data. They can be represented in the form of sense data statements which are denotable in form; for example "Black patch passes red patch now," can be contrasted with the less empirical descriptive form which would be a pointer reading. Less reliable but useful logical forms such as constructs, e.g. time and space, are reliable to the extent that they can be supported by atomic facts. They must however remain hypothetical. Physical objects such as tables and chairs are descriptions; their status is logical, not ontological. The existence of material objects (tables and chairs) are hypothetical. At the same time, a similar development took place in the psychology of perception. A percept finds its meaning in the context of associations to which it gives rise, not in its external context. Titchener was willing to relinquish the problem of meaning to the logician. The stimulus gives rise to sensory elements, not to a picture of itself.

The early phenomenalists as well as the structuralists in psychology, made a clear and firm distinction between objects and sense data. Objects were the products of perception, and perception was a process of interpretation of sense data. By means of careful analysis or introspection, the perceived object could be reduced to the sensations of which they were interpretations. For the analyst philosopher of science this meant developing a sense data language which would be closer to the truth than the object language. It would be closer to the truth because credibility is a function of how close a statement is to the epistemic root, which is sensation. It is curious to find that phenomenalist philosophers debate the question, "is a sense-datum language possible" late into the early 1940's, (G. Bergmann, 1954; Ayer, 1959) without ever considering the fact that the structural psychologists had made an experimental attempt at just this question. The phenomenalist philosophers write with a great deal of confidence that the development of such a language is in fact possible. But an examination of the structuralists' work suggests that the problem with phenomenalism is not a linguistic shortcoming but a perceptual one. Most critics of phenomenalism argue that the phenomenal languages seem artificial. The history of psychology suggests that a more fundamental criticism is that the phenomenal account of experience is artifactual.

Late forms of phenomenalism, such as represented in the writing of Ayer and Bergmann, use softened forms of verification principles in which it is recognized that propositional statements are verifiable in principle if some kind of experience is pertinent to determining their credibility. Such statements tend to be circular and the meaning of the term experience becomes more ambiguous. It is perhaps because of some of these logical problems that more recent positivists have abandoned phenomenalist verification principles as a criterion of meaning.

Let us see how the phenomenalist construction deals with the problem of objectivity. The word objectivity is inappropriate to the entire way of thinking involved in phenomenalism; in fact, the word object has a different referent for the phenomenalist than it does in its classical definition. As we have seen, an object to a phenomenalist is an interpretation, the product of a language which is already one step removed from the indubitable, that is experience. Objectivity in the classical sense, we have defined as assuring certainty and to the phenomenalist that is the function of sense data, not the function of objects. Despite this peculiar semantic involution, the meaning of the classical term objectivity is important to the phenomenalists.

Phenomenalists are not in agreement concerning the question of independence. Russell, (1965) for example, insists that sense data are independent of the observer, but Price (1965) argues that this is a metaphysical position. What the phenomenalists are in agreement on is that sense data have intersubjective reliability. What the sense datum language allows one to do is to make actual the potential experiences which it describes for any particular observer. This is very close to a pre-behaviorism description of physicalism. It describes in any particular situation what to look for in order to verify. Later forms of physicalism get rid of the phenomenological language, and describe what to do in order to verify. Just as phenomenalism is associated with experience, the later form of positivism which is usually called physicalism, is associated with behavior. We will return to this theme after we obtain a clear picture of the phenomenalists' relationship to objectivity.

So far we have seen that objectivity for the phenomenalist differs from the classical definition in that it substitutes intersubjective reliability for independence. The other important characteristic of objectivity is the notion of certainty. The fundamental motivation for the phenomenalist movement was in the promulgation of this notion more than any other derived from the empirical tradition. G.E. Moore (1965) demonstrated his commitment to certainty by describing sense data as the one thing about which we cannot possibly be mistaken. This also seems to be the reason Mach chose this form of radical empiricism with which to eliminate many concepts in physics. It is also the dedication to verifiability, as the definition of meaning, which continues the logical empiricist tradition even though it abandoned phenomenalism.

For the most part, logical positivists abandoned the attempt to reduce protocol statements to sense data statements. The impossibility of building a sense data language as well as the likelihood that most scientists would not use it if it did succeed, made phenomenalism the one aspect of early logical empiricism that has almost disappeared. It was replaced by the thesis of physicalism.

Meaning was for the physicalists, as for the phenomenalists, defined in terms of verifiability. The essential difference between phenomenalists and physicalists is in the nature of data language and phenomena or what we have previously called factual reference or empirical significance. Physicalists insist that data language be the language of physical objects in the Newtonian sense of object, and that the referent of this data language be physical objects. Physical objects here refer to macro-objects and different physicalists use different terms to designate them: Carnap calls them "observables", Reichenbach calls them "concreta". We should be careful to distinguish between the meaning of the word physical in this context, and the meaning of physics as a subject matter: for example, in microphysics the phenomena under study must be translated into the physical language which is composed of physical objects capable of entering into our experience. Most physicalists argue that this object language is the natural language of observation. The physicalists come a great deal closer to the classical definition of objectivity than did their predecessors, the phenomenalists. They replace the intersubjectivity criteria of the phenomenalists with the publicity criteria. Since for the phenomenalist the primitives were sense data, intersubjective reliability was the best that could be hoped for. By making the object the referent to which all other things must be reduced, the physicalist is able to re-establish the independence of observation.

The obvious psychological parallel to this development is behaviorism. Boring (1942) says of the union of behaviorism and positivism that "they could have sensation and eat it too." The method of verification in science, as in perception in psychology, can be completely externalized and given an independent referent. The discriminated response and the physical definition are attempts to banish the subjective in the two domains of perception and observation.

There can be and usually is a substantial difference between the way things appear and the way things are: therefore, there must be a difference in the way in which each of these is verified. The process of perception can itself be actually described by relying on people's behavior measured physicalistically as an index of their abilities with respect to the stimuli. From such a description, it is learned that the discrepancy between the stimulus object and the object of perception is due to perceptual constancies which tend to reduce the amount of heterogeneity in the perceived object when it is viewed from all its perspectives.

There is in this an element which remains consistent with the interpretation thesis of the phenomenalists. In the behaviorist account of perception we find reference to terms such as signs, cues or clues, which give rise to a percept which is more like the thing being perceived than the sensations giving rise to the perception. Thus, looking at a dime from a few feet away one's perception of its size is more consistent with its actual size than with the retinal image of its size. The retinal image in this description serves as a cue to the distal object. These kinds of considerations do not apply to objective observation, that is to physicalistic description, because they are public and repeatable.

In a later chapter we will consider the interpretation problem that is to be found, even within physicalistic description. It will suffice to say at this point that in a physicalistic description there are analytic conventions whose relationship to fact is not clear, and there are physical objects which are amenable to more than one physicalistic description.

We can see that the classical meaning of objectivity is essentially restored by the physicalists. The referent of our descriptions is independent objects. Certainty of our descriptions rests on the criteria of denotability, publicity, and repeatability.

Though the large majority of logical positivists, among scientists and philosophers alike, can be placed into either the phenomenalist or physicalist camp, there is at least one philosopher who is closer to logical positivism than to any other recognizable philosophy of science: Carl Popper (1959) who disagrees with both phenomenalists and physicalists. He is an empiricist in that he believes that our knowledge must be rooted in observation, but he believes that verification or any form of induction necessarily gets bogged down in psychologisms. Logical rigor requires that we avoid the psychological, and this requires a deductive empiricism. According to this view, science operates by making bold general assertions which can only be evaluated in terms of their ability to generate "falsifiable" propositions. Freud's theory is a poor one, not because it cannot be verified, since it offers evidence at every turn, but because it cannot produce propositions which can be falsified. Compare to this the concrete propositions generated by Einstein's relativity which could easily have gone wrong.

Popper has concluded that we cannot rely on objective perception to confirm hypotheses. At first glance, it would appear that Popper has abandoned the assumption of objectivity; in fact, he continues to maintain that we can rely on observations in instances of disconfirmation but not in instances of confirmation. There are psychological reasons why this solution to the problem of perception is not satisfactory and these will be dealt with later. (see pages 199-200)

We have seen that one of the elements which united physicalists and phenomenolists was their belief in the principle of verification. Meaning for both of these groups was obtained by discovering the way in which a term could be verified. They differed in their belief concerning the ultimate reduction that would result from verification. The phenomenalists argued that the primitive indubitable was in the private experience of sense data, the physicalist arguing that verification would reduce to publicly observable physical objects. The group we are about to consider is in agreement with the phenomenalists insofar as they believe that the primitive elements of knowledge are in the private experience of the individual. For this reason the group is usually referred to as the phenomenologists. The phenomenologists, however, do not fall into the logical empirical tradition since they do not accept the division of all knowledge into analytic and synthetic; rather, they are the descendants of Kant in maintaining that certain principles of mental organization are innate, just as Kant maintained that Euclidean geometry and space and time were a priori synthetic. The phenomenologists did not hold meaning to be dependent on verification. Certain objects are held to have meaning without even the possibility of verification. For example, we are able to imagine golden mountains and bucolic unicorns, and they have meaning for us in that they are capable of entering our phenomenological experience through imagination. Such objects are said to subsist, rather than to exist, and subsistent objects are meaningful even though they are not subject to verification. (Spiegelberg, 1960)

It has been said that phenomenology is inherently anti-scientific in that it has traditionally been associated with existentialism and philosophers such as Nietszche and Kierkegaard. This negative association is not a fundamental problem for our own position. Most of these early existentialists were not systematic writers and they did not object to science because of any belief that science was fundamentally in error; rather, they held that science was ethically undesirable. In addition, the phenomenologists who were interested in the problem of factual reference were scientists. They were not, however, empiricists. All empiricists wish to reduce meaning to a specific set of sensations. Husserl objected to this because of his belief that meaning can only be grasped intuitionally. Take, for example, the meaning of a verbal statement; it cannot be said to be discoverable in the utterance since no two utterances can ever be the same. The meaning of a statement can be uttered in many different ways. Husserl argued that to try to reduce meaning either to objects or to a specific sense experience is like maintaining that the meaning of a sentence is in its utterance. (Spiegelberg, 1960)

The phenomenologist philosophy of science is different from all we have thus far covered in that it attributes discovery to insight. Though this is not an empiricist position, it is not necessarily anti-scientific since it does maintain that in the context of justification some empirical testing is used.

Let us now consider that aspect of phenomenology which is most central to the subject of inquiry. The phenomenologists believe that the primitives are whole perceptions which cannot be reduced to a more elementary form; therefore, configurations are to phenomenologists what sense data are to phenomenalists and physical protocols are to physicalists. Though the psychological description of such perceptual configurations began with Ehrenfels and was continued by the gestalt psychologists, the idea that the primitives of experience involved complexly organized patterns was present among philosophers who antedated gestalt psychology.

Leibnitz's theory of monads held that mental criterion and worldly entities matched one another as a pair. Each such monad was a complex whole, therefore an object in the real world gave rise in experience to its mental counterpart in what is at least a superficial anticipation of the gestalt principle of isomorphism.

William James thought that the greatest error in empiricist philosophy was that it based all of knowledge on little bits of experience which he pointed out are never actually found in consciousness. Consciousness flows, it is remarkably unified, and there is nothing in it like the disjointed bits which the empiricists are appealing to. (James, 1890)

Perhaps the most important historical figure in configurationism is Brentano. Brentano was not a self-avowed phenomenologist, but his thesis of intentionality was a starting point for his students, among whom were Meinong and Husserl. The thesis of intentionality maintains that in every awareness there are two elements, the presentation and the object. The presentation is a mental act such as sensing, feeling, thinking and perceiving. The object is physical and independent of the act as an external object, but in awareness it is necessarily bound to the act of awareness. The thesis maintains that no object can make its way into awareness without an accompanying object. Brentano with his thesis of intentionality has established the nature of what is to be considered primitive in knowledge for the phenomenologists, and these primitives are neither elementary awareness (sense data) nor independent objects of awareness (physical objects), but perceived objects. Perception is impossible without an object, and objects are unknown unless accompanied by perceptual acts. (Spiegelberg, 1960)

Edmund Husserl, referred to as the father of phenomenology, developed the method which would lead to a true intuitive science. According to Husserl, empirical science could only lead to relative truth because it distorted the perceptual primitives by ignoring their individuality and classifying them on the basis of group characteristics. His method of bracketing resembles Descarte's method of doubt, but it is intended to apply to experience rather than to reason. To come to the essence of a perceived object, one must remove from the mind all categories, classes and general descriptions. On viewing the work before me, for example, I must restrain from descriptions based on categories such as desk, table, and furniture, and accept the perceptual object as it is. This alone can yield an "objective" and "essential" knowing of the thing. Such a program of scientific description, though many have deemed it desirable, is obviously impossible since the essential objective descriptions cannot be linguistic. Because our very language assumes categories and classes, it is an obstructing device. It should be obvious that the gestalt psychologists who inherited the phenomenological tradition did not accept this radical point of view on science, but they did accept perceptual insight as the essential mechanism of learning and discovery.

The gestalt psychologist made by far the most important contribution to the configurationist point of view. Their contribution falls into this category because they accepted the criteria of primitive configurations; that is, they accepted primitive perceived objects and sought as their goal a description of the perception of such perceived objects. Perhaps the most significant contribution of the gestalt psychologists was not in their writing but rather in their collection of perceptual demonstrations which serve them as doubled-edged pedagogical instruments. They both demonstrated the influence of organizational principles on what is perceived, and at the same time, served as an example of the significance of intuitive discovery. Whenever one wants to explain the meaning of prägnanz, proximity, closure or any other of the whole-part determinisms, it is best to turn to specific examples of each case rather than use verbal description.[1] The student will respond with an immediate understanding of the principles and an increasing understanding of the meaning of insight.

The gestalt tradition followed Brentano in asserting that the perceived object differs from the object. This difference results from the influence of the perceptual process. Much of gestalt work was an attempt to describe the precise nature of such influences. The gestalt principles of organization are just such descriptions. "The whole determines the perception of the parts," is probably the most famous of such principles, and Ehrenfel's example of the melody is probably the most famous demonstration of that principle. How is it, Ehrenfels asks, that we can hear the same melody

[1] A more detailed and illustrated example of whole-part determinism is discussed on pages 150-156.

played in two different keys, if we attribute the perception of such melody to the physical dimensions of sound. It is not the individual note that shapes the melody, but the melody that gives perceptual meaning to each note. Thus it is that these organizational principles are characteristic of the perceptual fields such that there is always a discrepancy between perceptual experience and stimulus geometry. (Köhler, 1967)

The discrepancy between stimulus and perception is not, as the behaviorist describes it, a learned mechanism of efficiency (constancy hypothesis), but it is an innate characteristic of the organism which is the basis of intuitive learning. The gestalt psychologists point to several experiments in which lower forms of life such as chicks and infant monkeys, as well as near neonate humans, demonstrate that they also perceive as though they are subject to the principles of organization.

Having said that the gestalt psychologists subscribed to a description of perception that elevates illusion to a universal principle, and subscribed to a thesis of intentionality that makes perception its object; it will seem curious to conclude by saying that the gestalt psychologists were realists and believed in a form of verifiability. It is precisely on this point that Merleau-Ponty (1963) is critical of the gestalt psychologists. According to Ponty's account, they discovered form but then made the mistake of putting form in nature. The gestalten were elevated to ontological patterns of organization and Köhler, for example, sought examples of physical gestalten. These patterns became elements in the environment -- i.e., stimuli essentially no different in their causal effects on consciousness than the behaviorists' stimuli.

> We do not think that the notion of Gestalt is pursued to its most important consequences either in those materialist conclusions or in the mentalist interpretation which we indicated at first. Instead of wondering what sort of being can belong to form and, since it has appeared in scientific research itself, what critique it can demand of the realist postulates of psychology, it is placed among the number of events of nature; it is used like a cause or a real thing; and to this very extent one is no longer thinking according to "form." As long as one sees the physical world as a being which embraces all things and as long as one tries to integrate behavior into it, one will be driven from mentalism, which maintains the originality of biological and mental structures only by opposing substance to substance, to a materialism, which maintains the coherence of the physical order only by reducing the two others to it. In reality, matter, life and mind must be understood as three orders of significations. But it is not with the help of an external criterion that we will judge the alleged philosophy of form. On the contrary, we would like to return to the notion of form, to seek out in what sense forms can be said to exist "in" the physical world and "in" the living body, and to ask of form itself the solution to the antimony of which it is the occasion, the synthesis of matter and idea. (Merleau-Ponty, 1963, pp. 136-137)

This form of realism differentiates between the significance of scientific truths and argues that pattern discoveries are more significant than that filling in of detail which is the confirmatory end of the scientific business.

Two noted philosophers of science, Hanson and Polanyi, have applied gestalt psychology to the problem of scientific observations. Both of these men emphasize the creative aspect of scientific discoveries and spend much time in rebutting the idea that discoveries take place inductively. In the following quotation we can see the strong gestalt influence, both in giving a whole-part deterministic description of perception and an intuitional account of discovery.

> Perceiving the pattern in phenomena is central to their being "explicable as a matter of course". Thus the significance of any blob or line in earlier diagrams eludes one until the organization of the whole is grasped; then this spot, or that patch, becomes understood as a matter of course. Why does Mars appear to accelerate at 90° and 270°?--(P) Because its orbit is elliptical--(H). Grasping this plot makes the details explicable, just as the impact of a weight striking clay becomes intelligible against the laws of falling bodies. This is what philosophers and natural philosophers were groping for when they spoke of discerning the nature of a phenomenon, its essence;[2] this will always be the trigger of physical inquiry. The struggle for intelligibility (pattern, organization) in natural philosophy has never been portrayed in inductive or H-D accounts." (Hanson, 1958, p. 87)

In strong opposition to the logical empiricists, these configurationist philosophers of science place a great deal of value on the logical constructs. It will be recalled that, in their disdain for metaphysical speculation, the majority of logical empiricists strove for a language, the elements of which were either synthetic or analytic. All synthetic elements were to have phenomenal or physical referents: otherwise; they were to be dismissed as nonsense. One set of elementary terms was especially problematical to this program and that was the set which makes up the subjects of propositional sentences; for example, the subject "pencil" in: "The pencil is yellow, pointed and cone shaped." or the subject "body" in: "The body had a mass of ___ and a velocity of ___." To the logical empiricist, be he phenomenalist or physicalist, the status of these subject terms is ambiguous. For the phenomenalist it is a construct referring to the interpretation of the sense data. To the physicalist it is a construct which combines or interprets the measures of the object. For neither is it an underlying essence. For the logical positivists, these terms are incomplete description, less essential to synthetic truth than the predicate statements. As we have seen above, Ayer believes that we are forced to use such subject terms merely because of an accident of our language.

In sharp contrast, the configurationist sees such subject terms or constructs as the very essence of science. Constructs are not primarily an underlying substrata which gives rise to secondary qualities which are the predicate statements, but instead they are an organization principle discovered through insight, thereby giving meaning to the predicates. Without these constructs past results could not be interpreted and future research could not be organized. What Polanyi says below about the subject term organ applies to all such subject terms, not simply to teleological ones.

> The philosophy of behaviorism does not contest the duality of body and mind, but it assumes that all mental performances can be fully specified without referring to any mental motives. Could this be true? Consider an analogy. All textbooks of physiology refer to organs and the function or organs, and in spite of frequent solemn declaractions that such

teleological conceptions are unnecessary and indeed objectionable, no one has yet published a textbook of physiology that does not speak of organs and their functions. For the biological functions or organs can be known only as coherent wholes. This also applies, of course, to the motions forming a skillful performance, or an act of intelligence; these can be identified only as parts of their meaningful coordination. To describe, as behaviorists claim to do, workings of the mind without relying on the guidance of mental motives is as impossible as it is to describe physiological events occuring in an organ without being guided by the observation of its coherent physiological functions." (Polanyi, 1968, pp. 34-35)

The emphasis that the configurationist philosophies of science place on the role of scientific constructs marks an important transition in our account of scientific discovery.[1] The positivist tradition viewed constructs with disdain since they seem to mark the point at which metaphysics made its way into science. More recent configurationists, such as Polanyi, Hanson, and Kuhn, attribute most of the progress in science to the discovery or creation of such constructs. According to these writers, these constructs are simple units discovered through insight which change the course of science. They point to Kepler's ellipse, Galileo's pendulum, and Newton's gravity as the kind of constructs which, despite their relative simplicity, change the course of physics in an irreversible manner.

All of the configurationists are subject to Merleau-Ponty's criticism. Like the gestalt psychologists, Hanson and Polanyi

[1] We have already discussed some of the background to this transition on pages 37- 38.

diverge significantly from the traditional scientific epistemology. They do so in that they give a non-inductive account of discovery and hold that pattern discoveries are more significant than detail discoveries. In addition to this they wish to apply a coherence criterion to the meaningfulness of scientific propositions. Köhler believed that truths are hierarchically arranged, and that those at the top are more meaningful. A similar position is held by the configurationist philosophers of science. We can see in the quotation that follows a faith in the future of science that dramatically differs from the logical empiricists point of view.

> Once the recognition of anticipatory powers in science establishes a conception of reality transcending tangible things, we might be able generally to acknowledge higher entities, intangible and yet real--as real as matter and yet meaningful. We shall recognize thus a cosmic hierarchy in which man has once more his own place." (Polanyi, 1967-68, p. 196)

Despite these many deviations from the epistemological paradigm, Ponty's criticism is appropriate. The configurationists have redefined the nuggets of experience, as well as the account of their meaning, but as to their validity the empirical test of correspondence remains the traditional mark of science. Once more we quote Polanyi to demonstrate the faith configurationists place in correspondence.

> What we mean is that the thing will not dissolve like a dream, but that, in some ways it will yet manifest its existence, inexhaustibly, in the future. For it is there, whether we believe it or not, independently of us, and hence never fully predictable in its consequences. (Polanyi, 1967-68, p. 191)

In contrast to the logical positivists, truth and meaning are separate issues for the configurationists. While the truth and meaning of a proposition are the method of its verification for the positivists, the configurationists maintain that while truth depends on verifiability, meaning depends on context. A proposition gets its significance by finding its place in the larger whole.

The meaning of objectivity for the configurationist is very close to the classical meaning. As is evident in the quotation above, he believes in independence of external real objects. In addition, he believes that verification makes a thing certain, but that it does not make it meaningful. Therefore without essentially redefining objectivity the configurationists deprive it of much of its significance.

Thus far we have described epistemological "isms" which are familiar by virtue of a long tradition. What will presently be referred to as "neo-realism" does not have as clear and unified a traditional position. Several important philosophical strains arising out of a reaction to positivism in the philosophy of science, and relativism in the theories of perception, have resulted in a group of thinkers who share several common characteristics without necessarily paying tribute to a common label. Among the earliest such movements in philosophy was the American and British new realism, as well as certain forms of critical realism. In the psychology of perception cybernetic and information theorists were reacting to the subjectivity implied in much of the relativistic theories which had been prompted by the gestalt psychologists.

The most important characteristic of neo-realism with respect to the assumption of objectivity are its externalization of the object of perception and its active view of the perceptual process. We will discuss both of these. There are, however, other characteristics that the neo-realists have in common which are not so central to this thesis. The most striking of these is their acceptance of common sense, ordinary language and daily experience as a standard form of knowledge, against which to evaluate science and philosophy. Consistent with this, and in sharp contrast to the positivists and behaviorists they show no disdain for metaphysical concepts. They make free use of terms such as reality and nature. They have an affinity for the language of information and communications, and spend little or no time clarifying the meaning of the concepts couched in this language. Among the early philosophical leaders of this group are M.R. Cohen, T. Nunn and E. McGilvary. The most outstanding psychological contributors to this point of view are Gibson and Attneave. Within the philosophy of science, Duhem's work is very important, but current writers such as Harris and Hawkins have more of the superficial characteristics of neo-realism.

The positivists' relegation of metaphysics to the realm of nonsense and the behaviorists' relegation of consciousness to the status of epiphenomena can be viewed as a process of internalization. Simply by telling us that belief in reality is metaphysical, the positivists did not rid us of our "psychological" need to speculate, which was a hindrance to progress. By the same token when behaviorial psychologists chose to ignore the world of private experience they did not rid the individual of his awareness of it, but justified their choice by claiming that nothing that happened there could make any difference.

Positivists treated metaphysics and consciousness in a very similar manner, applying their principle of verification as the only criterion of meaning. They found that both metaphysics and phenomenological awareness were meaningless. This means that the language of the ordinary man, as well as the ordinary language of philosophers and scientists, is largely nonsense. In addition, since it accepted the verifiable scientific description as the true and meaningful picture of the world it means that most of human experience falls into the world of illusion. In the realm of individual experience a parallel disdain of phenomenology means that the world of perception is largely the product of internal and subjective manipulation of unknowable proximal stimuli. The objects of awareness can be viewed as internal perceptions. These perceptions can be considered the products of internal mechanisms, either learned constancies or innate principles of organization, as much as they are the products of stimulation.

Historically, the trend in positivism and behaviorism had been to view the world as much simpler than it was experienced in consciousness. The secondary qualities with which former realists enriched subjective awareness had disappeared: but other mechanisms such as constancies, also had been introduced which organized and enriched proximal stimuli. The real world was made up of energies and bodies, as described by physicists. The textures, colors and complex configurations of awareness were enrichments upon the energies arriving at the receptors. The neo-realists are united in reversing this situation. The objects of awareness are in the real world. The process of perception has them as objects. Whatever modifications perception makes upon its objects, it is in the direction of impoverishment, not in the direction of enrichment.

One of the men who was most influential in reversing this trend was T.P. Nunn, who held that all aspects or qualities of an object were entirely independent of our perceiving them. In his view, experience is more veridical than our ideas. The different shades of yellow one can see in a buttercup are properties of the flower, just as we experience them. According to this view, ideas are poor oversimplifications of the experience they represent. (Passmore, 1968)

We can refer to this trend as a process of externalization. Ever since the beginning of the study of perception there had been a continually increasing tendency to attribute the qualities of experience to internal (inside the perceiver) mechanisms. A good early example of the process of externalization can be found in McGilvary's (1965) work on perspective realism. McGilvary argues that contrary to earlier philosophies, relationships are real: a geometrical object, for example, has many perceptual faces depending on the perspective from which it is viewed. Traditionally it was thought that perspective was an inherently subjective notion. McGilvary makes use of projective geometry to calculate various perspectives of a cube, and argues that none of these perspectives is dependent on a viewer; therefore, they are as much a feature or property of the cube as its Euclidean dimensions.

If perspective is real, why not texture, prägnanz or all the constancies which make up the behaviorists' apparent world. This is precisely the thesis that Gibson offers as the only hope for a true stimulus-response psychology.

> The first would be a psychophysical theory of the correspondence between variables of stimulation and qualities or dimensions of experience. Its chief novelty would be the explicit postulate that for every quality of experience there is a discoverable variable of stimulation. This means not only that the sensory qualities, so called, have stimuli, but that all the qualities of surfaces, edges, slopes, and shapes have stimuli, and that all the qualities of motion, action, and causality have stimuli, and that all the qualities of persons, groups, institutions, words, and symbols have stimuli. Its method would be the psychophysical experiment with optimal conditions for discrimination. Admittedly, the isolation and control of the stimulus variables for most of the latter qualities would be fantastically difficult. In this sense, the theory might never be fully verified. It is, however, simply the extension of the accepted theory of sensory experience to spatial and social experience and it provides a straightforward program of experiments. (Gibson, 1951, pp. 104-105)

We can see the same process of externalization in Hawkins' reasoning:

> My criticism of the way of thought that is common to Kant and the relativist is that it arises from a broken-down analogy. Perception is not perception of a received-mental-object, problematically related to a real object that we do not perceive. The proper counterpart to the received message in communication is not the object of perception, but the perception itself, the psychological act - whole object is, precisely, the physical object of perception. (Hawkins, 1964, p. 51)

The externalization of the object of perception as it is advocated by the neo-realists is not without its difficulties. The most important of these difficulties is understanding the ambiguous residual of the internal object, what Gibson calls "stimulus information" or "invariances in experience" and what Hawkins terms it "message". In order to understand this we must look at their description of the perceptual process.

The neo-realists are united in viewing perception as an active process leading to an achievement. The originator of this view of perception was Gilbert Ryle. Ryle, however, did not agree with Gibson, Attneave and Hawkins, these men having devoted more time to the psychology of the problem than to the philosophy of the problem. Ryle did not agree that seeing or touching referred to any mental or physiological capacities of the organism. For Ryle these were merely achievements in the same sense as is implied by the terms "winning" or "catching" as in the phrases "winning a race" or "catching a ball". To the psychologist Gibson, achievements implied activity leading to the achievement, and in his The Senses Viewed as Perceptual Systems, the subject matter under study were composed of the activities of listening, seeing and touching, and not the senses of touch, audition and vision. Only in sensation do the five senses operate as separate passive receptors. These are merely the energy receiving systems which bring stimulus energy into contact with the nervous system. In perception the entire body works as a single system in processing stimulus information, the order embedded in stimulus energy. Stimulus information can go through several transformations without losing its significance and it makes use of stimulus energy only as a carrier in the same way that the telephone uses currents of electricity. Contained in the message is information which comes from the object of perception, not from the energy which carries the message from the object. In this way, all the riddles of constancy and other discrepancies between proximal and distal stimuli are resolved.

The retinal image of a coin held at a slant a short distance from the eye is for sensation an ellipse, but for perception it is circular. Prior theories have tried to explain this by introducing some form of mediator between sensation and perception. Gibson (1966) merely held that the information of circularity is present in the retinal image. It is a higher order invariant and it is invariant with respect to all the rest of the stimulus information entering into perception at that time. For it must be remembered that perception involves not just the eye, but other information available to the perceptual system about the perspective of of the eye with respect to the coin. Such information is given by certain invariant ratios existing in optical array.

In the case of the coin, the invariance which serves as information for the perception of circularity is, according to Gibson, "ratios and other invariances in the optic array" (Gibson, 1966, p. 306). It is safe to assume that Gibson means by ratios, relationships such as the mathematical relationship between radius and circumference. These ratios need not be conscious. Assuming that Gibson's notion of invariance in the optic array is an adequate substitution for the constancy hypothesis, there still remains a difficulty with the example of the coin. It just so happens that slanted coins seen from a distance are like railroad tracks seen from a distance; that is, they are cases of <u>incomplete</u> constancy. According to both the constancy hypothesis and Gibson's invariance hypothesis, the tracks should not appear to converge at all and the coins should appear completely circular. In fact, experiments demonstrate that perception falls somewhere between complete constancy and an exact reading of the optical or retinal image. Gibson attributes the incompleteness of the constancy to a special case of sensation intruding on perception due to the learning of perspective.

> Some sorts of visual sensation, especially linear perspective, are very obtrusive, the more so when attention has been educated to it by having learned to draw pictures. The result may be the illusory appearance of foreshortened surfaces and decreasing size with distance. When this attitude is adopted, the information for the <u>slant</u> of the coin becomes a sensation of elliptical shape and the information for the recession of distance becomes a sensation of

angular convergence to a vanishing point. I do not know of any good evidence to show that animals or young children are subject to these illusions of perspective. (Gibson, 1966, p. 307)

Unlike the behaviorists and the gestaltists the constancy or invariance of perception is not problematically related to sensation. It is immediately present in the stimulus information and has its ontological existence in the message which is the ordering of stimulus energy. Unlike the behaviorists' constancies such invariances do not have to be learned by the associating of sensations, although they must be discriminated.[1] Gibson agrees with the gestalt psychologists that the ability to detect such invariances is present in young children and animals. (Gibson, 1966)

When the senses are used as a perceptual system, the perceptual activity involves the processing of information which is brought to the senses. In the ordering of stimulus energy, the perceptual process decodes this message and refers to its object which is external to the body. This reference to the object is best understood if one thinks of the telephone analogy in which the message is what is understood even though it is attributed to the sender. This view of perception emphasizes veridicality and is in marked opposition to most of the work that was being done in perception when Gibson promulgated it. Most of the neo-realists take it as a matter of course that our ordinary experience is correct in

[1]Most learning theorists would not accept the distinction that Gibson makes here between discriminating and learning by association. Nevertheless it is presented here since this is an attempt to express his theory as he sees it.

assuming that we perceive things as they are. To put it in Hawkins words, "The sensory channels are too well matched to the normal message source to allow any consistent decoding of message sequences that violate the basic customs of nature." (Hawkins, 1964, p. 51) Hawkins goes on to argue that there are two convincing reasons for believing that nature's encoding is well matched to our decoding.

> The first fact is that the set of possible messages from nature is much larger than the set that nature actually uses. This is demonstrated by the ease with which, in artificial laboratory situations, perceptions can be disoriented and judgment misguided. (Hawkins, 1964, p. 49)

While this first argument is most often used to support the opposing point of view, Hawkins seems not only to have assumed a veridical decoder but also a cooperative encoder. Further, Hawkins argues that our decoders are not tuned to all possible sorts of messages, but only to those nature is in the habit of providing.

Finally we arrive at the question of scientific observation. How, if at all, does scientific observation differ from ordinary perception? In any of its essential aspects it does not. According to the neo-realists, veridical perception is always a capacity that man can employ. All of the perceptual research which has accrued in demonstrating the influences of motivation, purposes and selective attention does not demonstrate that perception cannot be veridical. For example, Gibson distinguishes between literal and schematic perception. Literal perception is veridical in that it depends on the stimulus which is independent of the subject. Schematic perception demonstrates that attention and poor perceptual conditions such as are used by most researchers in social perception, can and do distort the stimulus. Gibson does not address himself to the question of scientific observation, but it is clear that the objective observation, as classically defined, is possible under what Gibson calls "literal perception". (Gibson, 1951)

Knowing that the perceptual process is capable of both literal and schematic productions we merely have to enhance those conditions which produce literal perception and minimize schematic perception. This is what we do in the process of measurement. "Every measurement involves a particular finding or set of findings, certified by perception and independent of wish or bias." (Hawkins, 1964, p. 86)

At first glance it would appear that the meaning of objectivity for these neo-realists has been fully restored, but upon closer analysis their concept of objective perception deviates from the classical concept of objectivity in several important ways. The first deviation from the traditional concept of objectivity in the neo-realist epistemology is related to the externalization of the object of perception. Early neo-realists, such as T.P. Nunn, repeatedly emphasized that reality is more diversified in its properties and perspectives than is our perception of it. The neo-realist account of scientific observation has imbedded in it an element of this problem. The early new realists would criticize it because it inherently glosses over many of the potentially discriminable properties in the perceived object. This is in essence Husserl's criticism. He held that in scientific description the use of data language to represent observations -- impoverishes reality. (Spiegelberg, 1960) Nowhere among the more recent writers included above as neo-realists can we find a clear discussion of this issue. In fact, there are only hints that the perceptual oversimplifies the actual, and never is there discussion of the implications of this kind of misrepresentation in scientific epistemology.

We can see the residue of this problem in those few places in which writers like Gibson and Attneave fall back on the principle of economy, rather than the principle of correspondence, as a criterion of truth. Gibson is clear in maintaining that his is a theory of strict psychophysical correspondence.

> The test is simple: does a specific variation in the observer's experience (or behavior) correspond to a variation of the physical stimulus? Although this experiment has seldom been applied to what are traditionally called perceptions, it can and should be performed. (Gibson, 1950, p. 62)

But, on the other hand, when Gibson is forced to explain how perception yields a message more closely identifiable with the distal than the proximal stimulus, he falls back on the concept of invariance. All the incoming information is processed to produce the most invariant percept possible. This is similar to Attneave's argument that perception reduces uncertainty by producing the most "economical" picture of the stimulus based on the available information. Economy and correspondence seem to be quite different criteria. Whether they can be compatably mixed in the fashion suggested here is not a question with which these writers deal. It would seem clear, however, that the criterion of economy is one which deviates from the strict correspondence implied in the independence feature of the classical definition of objectivity.

In passing, it is instructive to repeat an interesting reversal in perceptual theories that is related to this problem. It was pointed out earlier that, before the neo-realists, the tendency within the field of perception was to accept the physicist's description of the stimulus and to view the process of perception as one which takes advantage of past experience in order to bring to consciousness a very complex and highly enriched representation of the stimulus. It would appear that, by externalizing the object of perception, the neo-realists have inherited the mirror image of the problems that are inherent in the constancy hypothesis. They began with an extremely complex stimulus situation containing so much variation that the process of perception must reduce its complexity in order to bring to consciousness a rather constant picture of the world. Neither of these pictures seems to render the use of correspondence criteria possible in the philosophy of science.

The second important difference between the neo-realists viewpoint and the classical definition of objectivity is in their belief in a hierarchy of factuality. In this we can see their connection to the ordinary language analysts with whom they share an affinity for common sense. For Gibson anything that can be measured is a fact, which can be measured. Higher order variables, such as the dimensions of a human smile may someday be measurable, but whether we can successfully incorporate this meaning of factuality with what is usually connoted by the facts of physics seems doubtful. Gibson himself says that there are facts at all levels of "sensibility". That this is true as a common sense proposition few will question, but the fact that the level of their existence depends on human sensibilities would ban many such facts from the realm of science. Most scientists would agree that the range of observables to which man has been able to bring his senses has and will continue to expand, but the range of sensibilities which man may bring to scientific observation is prescriptively limited.

What is here called transactionalism has even less of an explicit tradition than the neo-realists who were discussed in the previous section. Philosophically this position has been called pragmatism or instrumentalism, but the two most important contributors to what is called transactionalism pay no homage to this tradition and are more scientific than philosophical. In particular this refers to Werner Heisenberg and Adlebert Ames, whose work in quite different sciences led them to very similar conclusions with respect to the problem of observation and perception.

There is a very significant and important link between philosophical pragmatism and psychological transactionalism. Pragmatic transactionalism is the one school of philosophical psychology which received both its initial philosophical expression and its later psychological amplification in the United States. Pierce was the first to develop it as a philosophy, and James as a psychology. Later Bentley and Dewey refined the philosophy and Ames, and Cantril made the psychology more concrete.

Transactionalists are united in agreeing that the primitive of knowledge is a relationship between instrument and the given;instruments include such things as physical apparatus, overt and covert operations, and constructs and assumptions; the givens can only be known in the way they relate to any of these instruments. The primitive element of knowledge is a transaction in which one becomes aware of the consequences of his actions under this set of initial conditions. The percept or the observation is the relationship between what the perceiver or observer brings to the situation and what is given by the situation. This relationship, although it is not a clearly defined unit, is to transactionalism what object protocols are to physicalism, sense data is to phenomenalism, gestalten are to configurationism, and information to the neo-realist.

William James offers the following pragmatic edict:

> ...to attain perfect clearness in our thoughts of object...we need only to consider what effects of a conceivably practical kind the object may involve - what sensations we are to expect from it, and what reactions we must prepare. (James, 1969, p. 411)

This is the pragmatist's counterpart to the positivist's verification principle. The positivists, whether physicalists or phenomenalists wish to use sense data or protocol objectivity to establish that the idea was valid because of existential independence. Pragmatists not only doubt the possibility of establishing correspondence, they also assert the undesirability or at least irrelevance of doing so. In that they hold concepts to have meaning independent of their existential representativeness, they are similar to the configurationists. The concept guides experience and therefore influences consequences. We can demonstrate the irrelevance of the correspondence by using James' famous argument for the belief in God. Justification for the belief in God does not come as an answer to the question, "Does God exist?" but rather to the question "What difference does the belief in God make to the human experience?" If today were the last day of human existence then the belief in God would truly be meaningless, since concepts have no retrospective significance. But since it is not man's last day, a continued belief in the existence of God makes a significant difference in the perspective which molds future experience. No doubt James would have used this argument as in fact some of his philosophical descendents have against the positivist disdain for metaphysics.

In his analysis of perception, James also came very close to the position of the contemporary transactionalists in psychology. Concepts are useful abstractions which have their influence on perceptions by selection, organization, and anticipation. In certain contexts he refers to these as "supposals" which have a self-fulfilling influence upon perception and which can only be evaluated in actual instances of carrying out a purpose.

F.C. Scheller, is one of the best known pragmatists, and was in fundamental agreement with James' view. Scheller and Alfred Sidgewick developed a pragmatic logic, which in sharp contrast to that of logical empiricists involved considerations such as cognitive satisfaction and task completion. Most logicians would consider these factors to be psychological. Scheller's concept of reality is strikingly similar to that of contemporary transactionalism because it is both subjective and evolutionary. Reality, as well as logic and methodology are by no means defined for all time, but are subject to change with man's condition. All of these usually static terms are seen as variables dependent upon man's purpose. (Scheller, 1912)

An evolutionary and psychological view of the analytic aspects of knowledge is one of the hallmarks of pragmatism. Mathematics, logic and conceptual requiredness are empirical in origin in the broadest sense of that term. None is clearer on this point than Dewey. All such analytic considerations are based on prior successes. One of the best examples of the evolutionary nature of analytic knowledge comes to us from Bentley and Dewey's analysis of the evolution of what is to be considered a satisfactory scientific theory. Not only does the content of our explanations progress with scientific success, but also there is a change in the very nature of what we consider to be a satisfactory explanation.

Bentley and Dewey (1949) review the nature of explanations in the history of physics and abstract three types of explanations which have evolved. They are given here in historical order, which represented not just change but improvement in their view. Taken together, they provide one of the broadest philosophical definitions of transaction and do so in a contrasting context. In addition, they demonstrate that these functionalists would never be satisfied with any static definition of logical necessity, so typical of the positivists. The three kinds of explanations are:

> SELF-ACTION: where things are viewed as acting under their own powers.
> INTER-ACTION: where thing is balanced against thing in causal interconnection.
> TRANSACTION: where systems of descriptions and naming are employed to deal with aspects and phases of action, without final attribution to "elements" or other presumptively detachable or independent "entities," "essences," or "realities," and without isolation of presumptively detachable "relations" from such detachable "elements". (Bentley and Dewey, 1949, p. 108)

In the transactional point of view, the repeated emphasis upon the "non-detachability" of any of the elements of knowledge is one of the factors which distinguishes transactionalism from the previous four theories of knowledge that we have examined. In each of the others, the indubitable was characterized by what Dewey calls detachability and what we have called independence. The search for an element of knowledge which we could attribute to nature, rather than to ourselves, was abandoned by the pragmatists. This represents the important transition in the perspective on knowledge. Despite all the differences which exist between the other theories of knowledge that we discussed previously, the fact that they all have proposed an indubitable means that the fundamental task of the creators of knowledge is to read the book of nature. When the pragmatists abandoned the hope of isolating a detachable element, they established a completely new perspective on knowledge. The creators of knowledge, according to the pragmatist point of view, are not reading the book of nature; instead, they are charting a course of human achievement. One of the most important characteristics of the transactional position held in common with the neo-realists, is an active view of perception. But, in the context of transactionalism, the activity is an essential aspect of the relativity of perception. Bentley and Dewey have stressed the significance of this point within the framework of scientific experimentation. Dewey urges that we substitute the word "taken" for the word "given" when referring to the observables of an experiment, emphasizing that man is not a passive recipient of information but an active resolver of problems. Man is in no position, nor should he want to be, of ever apprehending the pre-cognized givens. The objects of inquiry are the products of previous inquiries both in the private and the public sense.

In accordance with the new perspective mentioned above, inquiry is conducted into problems of knowledge, not problems of nature. The need for inquiry is the result of noting inadequacies in what we already know. As Dewey puts it, it is work to be done "usually at the edge of knowledge." (Bentley and Dewey, 1949) We are conducting our inquiries into anomalies that exist in the present state of knowledge. Therefore even the problems we work on are molded by past knowledge. The methods or instruments we will use for working with the problem were once the object of inquiry for someone else. Dewey includes among instruments, concepts, meanings, overt and covert operations, and apparatus. The results of an inquiry are "taken" by all of these instruments, not given by nature. Our knowledge is to be evaluated, not according to its ability to represent nature, but according to its reliability in achieving human purposes.

The development of a descriptive psychology of perception based on the pragmatic philosophies presented above was undertaken by Adlebert Ames and his followers, Cantril and Ittelson. Following in the gestalt tradition they rested their case primarily upon a set of experimental demonstrations, which it was Ames' genius to construct. For purposes of discussion here we will refer to the distorted room experiment.[1]

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[1]Detailed description for the construction of distorted rooms as well as descriptions of typical observations made in the room are given in Ames. pp. 182-196)

Such demonstrations carry the same amount of conviction as do the gestalt displays, but they are more elaborate and the principles of their construction are more explicit.

The distorted rooms are built on the principle that several different actual configurations can give rise to the same retinal image. The walls in the trapezoidal room appear rectangular because one assumes walls to be rectangular. If this is the case than any piece of furniture placed in the trapezoidal room will appear distorted. This creates a situation in which a distorted room appears normal, while making its normal contents appear distorted. Here we have in essence the first important principle of transactional perception, which is that assumptions influence what is perceived. The second principle has to do with the role of action. The way to come to see the room trapezoidally is by acting with respect to it. If a subject tries to catch a ball which he has bounced off the various surfaces of the distorted room, he will at first fail to catch the ball. With practice his catching will improve, and his perception of the room will gradually shift. Take the same subject immediately to a room which was constructed rectangularly and he will perceive it trapezoidally. Only with additional practice of action will his perception return to that of a rectangular room. The second principle is, therefore, that assumptions and purposes are intrinsically related to one another. The assumptions one initially made concerning room did not come from the correct perception of all rooms in prior experience, but from the set of purposes one has had with respect to similarly constructed rooms. That our present assumptions with respect to rooms are dependent on purpose, and not the similarity of room constructions, can be demonstrated by considering the fact that Ames' distorted rooms would have entirely different consequences for a blind person or for an acoustical engineer.

We are never afforded the opportunity of perceiving the world without any assumptions. This is probably why Husserl's new science did not flourish.

> The fact that the world can be apprehended in accord with various schemata implies that there exists one world which does not depend for its existence on how it is differentiated and that schematization creates some, but not all, of the differences in the world. However, nothing follows concerning the possibility of apprehending the world in a pre-schematic, or pre-linguistic way. (MacKinnon, 1968-69, p. 126)

The transactional description of perception corresponds to Bohr and Heisenberg's description of observations in quantum physics. This correspondence is of the utmost significance and creates the basis for a new epistemology. Ames' description of perception is that of an individual bringing a set of assumptions to a set of givens; what he experiences is the relationship between the assumptions and the givens. The individual is never afforded a view of the givens independent of any assumptions. He can obtain alternative views by bringing to the same givens alternative assumptions. Bohr and Heisenberg's descriptions of a measurement procedure in quantum physics, are cases in which the measurement obtained is not attributed to the phenomena, but to the relationship between the phenomena and the apparatus.

Just as Ames says that what you see is a function of how you look, Bohr and Heisenberg say that what you obtain is a function of how you measure. This is not a solipsistic position: it merely denies the possibility of direct apprehension.

Both instances are analogous to one of the favorite transactional analogies, in which it is said that the world is only viewed through transformation lenses. The transformations of the lenses can never be separated from that which is apprehended with them, and what is apprehended can never be known independent of the transformations. In the case of the perceptual demonstrations, the rooms perceived are as much a function of the assumptions made about what is perceived as they are a function of the visual display. In the case of the quantum measurement, the numbers obtained do not refer to the phenomena, but to the relationship between the phenomena and the apparatus. Heisenberg's apparatus and Ames' assumptions have their analogy in the transformation lenses. We can see the parallels between Ames' view of sensation, and Bohr and Heisenberg's view of measurement, in their own words,

> We can no longer speak of the behavior of the particle independently of the process of observation. As a final consequence, the natural laws formulated mathematically in quantum theory no longer deal with the elementary particles themselves but with our knowledge of them. Nor is it any longer possible to ask whether or not these particles exist in space and time objectively, since the only processes we can refer to as taking place are those which represent the interplay of particles with some other physical system, e.g. a measuring instrument.
> ....
> Thus, the objective reality of the elementary particles has been strangely dispersed, not into the fog of some

new ill-defined or still unexplained conception of reality, but into the transparent clarity of a mathematics that no longer describes the behavior of the elementary particles but only our knowledge of this behavior. (Heisenberg, 1958, p. 15)

We conceive that we cannot directly know external events; that our senses and sensations and ideas give us, at most, a prognosis of their significance. What is known can never exactly correspond to the immediate occasion. At best their relationship can only be a similarity or analogy. The degree of this similarity may vary, but we can never know the inherent nature of objects and their relationships as such. .... The fact that WE CAN NEVER KNOW what we commonly consider as facts and truth is rather appalling. It is as if nature had set about to create a super hoax with humans, like donkeys chasing the "feed bag." But we are in that situation as long as we insist on believing that knowledge of the constitution of things as such and their relationships, is the final goal. (Ames, 1960, p. 4)

The status of objectivity from the point of view of transactionalism is curiously muddled. This is so particularly if one relies exclusively upon Ames and Heisenberg as spokesmen. For example, Ames, who persistently maintains that we can never know the essence of things independent of our purposes and assumptions, nevertheless distinguishes between subjectivity and objectivity.

Both the subjective and the objective worlds are essentially humanistic in their essence. The difference between these two worlds is not that one is subjective and humanistic and that the other is objective and nonhumanistic, but that one is uniquely personal to the individual experiencing it, disclosing those aspects of nature that can be experienced by him and by him alone; while the other discloses the humanistic aspects of nature that can be the common experience of all men. These aspects must be impersonal, that is, exclusive of individualistic significances (cf. modern science)." (Ames, 1960, p. 8)

This position is similar in some respects to the publicity criterion of physicalists. It separates private significances from public significance. One further complication with respect to the transactional position has to do with what Ames may mean in the quotation above when he refers to "all men". This is puzzling since Cantril, his closest colleague, through whom all of Ames' work is known, explicitly promotes the notion of cultural relativity in which assumptions, purposes and language define reality differently for different groups.

Heisenberg's view of objectivity is no more clear than is Ames'. On the one hand he says,

> While political ideas may gain a convincing influence among great masses of people just because they correspond or seem to correspond to the prevailing interests of the people, scientific ideas will spread only because they are true. There are objective and final criteria assuring the correctness of a scientific statement. (Heisenberg, 1958, p. 194)

On the other hand he does not identify scientific propositions with reality,

> Furthermore, one of the most important features of the development and the analysis of modern physics is the experience that the concepts of natural language, vaguely defined as they are, seem to be more stable in the expansion of knowledge than the precise terms of scientific language, derived as an idealization from only limited groups of phenomena. (Heisenberg, p. 185)

An assessment of objectivity from this perspective reveals that, at least with respect to the feature of independence, objectivity in the classical sense is not acceptable. Precision and accuracy can be achieved and through these we can be certain of reliable results. But reliable results in this context have a different meaning since they do not refer to the phenomena independently of the conditions under which the results were obtained. There is still an "if-then" quality to the knowledge, but the "if" refers to something one can do in order to obtain certain results and never allows one to know essences.

Objectivity is also different with respect to the criterion of certainty; in both the quantum situation and with respect to any of the perceptual demonstrations, what is known or perceived is in principle expandable on the basis of ways of knowing. We have always recognized that as our instruments of observation improve, knowledge expands; but this was seen as due to the limitations of our senses. The transactional position maintains that there is no one description which is correct, proper or complete with respect to a set of givens. The limits of what is to be perceived are not inherently bound in the stimulus, so that with respect to certainty we can assign only a probability to a particular percept arising from a combination of assumptions and givens. Similarly, in the quantum mechanical experiment, descriptions are not of phenomena, but of the relationship between phenomena and other physical systems; therefore, the limits of description are not inherent within the phenomena, but depend on man's ingenuity in devising means of actualizing the phenomena's potential. All we can hope to do is assign a probability to a given outcome given a particular set of initial conditions. Transactionalists, therefore, do not accept the finality that is implicit in the goal of description. Since any particular description involves assumptions and goals, as well as givens, no description is ever considered to be the correct description. Instead, descriptions can be given a probability of occuring, given a set of initial conditions and a purpose. Since any of these factors may change with the human condition, finality is impossible.

In this form of transactionalism, there is an important change with respect to the notion of certainty. The change can be best understood in terms of a change of interpretation of the meaning of probability. Prior to the development of quantum physics and the principle of indeterminacy, probabilistic prediction systems were used in classical physics but in a very different way than they came to be used in quantum physics. Probability theory was employed as the appropriate mathematical system to make predictions about physical systems in which there were technical limitations preventing the accurate measurement of that physical system. For obvious technical reasons, an analysis of the kinetic motion of gas particles cannot rely on the direct measurement of individual molecules in the gas. Therefore, probability theory was used to estimate the parameters of interest on the basis of the behavior of large groups of gas molecules. This application of statistical inference is used to overcome instances of human ignorance due to technical limitations. The principle of indeterminacy changes the interpretation of the role of probability theory by changing the status of the measurement problem from that of a technical problem to that of a logical problem. In the quantum physical experiment, the impossibility of measuring the trajectory of an individual particle is not a technical problem but a logical and physical impossibility. Therefore, the use of probability inference techniques is an inherent aspect of "knowing" in this domain. It is not a temporary substitute, eventually to be replaced. We can either interpret this ontologically or epistemologically. That is, we can say that there is either an inherent element of chance in nature or an inherent element of ignorance in knowledge, but we can no longer interpret it as a matter of technical error, which, at least in principle, could be overcome.

Thus we see that both of the fundamental features of the classical definition of objectivity have been replaced in the transactional account of knowledge. The search for the independent in the form of the indubitable given of knowledge was abandoned, making knowledge relative to man's purposes. The feature of certainty was also abandoned by elevating indeterminacy from the technical realm to the logical realm. For this reason, it is correct to say that the transactional theory of knowledge does not accept the assumption of objectivity. It is because of this that many of the methods and experimental demonstrations developed by the transactionalists have been incorporated into this inquiry.

CHAPTER V

THREE ANOMALIES TO THE ASSUMPTION OF OBJECTIVITY

We have already explained what is meant by an anomaly in our section on method. In the following material, the anomalies are constructed with a definite plan in mind. As indicated previously, one of the fundamental reasons for separating perception and observation was based on the assumption that observation is objective and that perception is subjective. To go along with this distinction would be to prejudge the question of objectivity: therefore, much care has been taken in selecting anomalies which can be exemplified in what might be clearly recognized as coming from each of these two domains. This is why the first anomaly deals with geometry and illusions, the second deals with reversible figures and a simple problem in physical description taken from the history of physics, and the third deals with the problem of equivalent configurations in perception and complementary description in quantum physics. The examples selected have been paired off in this manner in the hope that parallel abstractions could come from each half of the pair, thereby making the same point with respect to the assumption of objectivity.

The anomalies result from two steps which have already been discussed in some detail. The first step involves the bringing together of two currently separated fields of epistemological research. If, as argued previously, the assumption of objectivity is what keeps philosophy of science and the psychology of perception separated, then the effect of holding that assumption in abeyance is to allow us to see their commonality of interests. This is why each anomaly consists of information that has been collected from each discipline and brought together to bear on one problem. The second step involves the selection of items which, in the combined context of the two epistemological disciplines, appear to be counterevidence to the assumption of objectivity.

Each of the three anomalies is designed to make a different point about the objectivity of observations. The first anomaly is counterevidence to the assumption of objectivity in that it demonstrates that what is observed depends in part on assumptions being made by the observer. The second anomaly attempts to demonstrate that the observed is influenced by patterns of organization brought to it by the observer. Finally, the third anomaly attempts to demonstrate that what is observed is influenced by the goals of the observer.

In 1955, Floyd Allport thought it was necessary to begin his textbook on perception by giving a clear and simple demonstration of the difference between subjective and objective reports. He believed that we could abstract from this simple situation some general principles for distinguishing between these reports. The principles were to be applied to reports concerning more complex subjects such as are usually studied in psychology, wherein the distinction between objective and subjective is more difficult to make.

Allport presented the simple geometrical illustrations that are shown in Figures 1 and 2. He held that we can distinguish the objective from the subjective by observing the methods the subject uses in arriving at his reports.

Allport asks two questions of the subject, looking at Figure 1. The first question calls for a subjective answer. It is, "How does Figure 1 look to you?". The subject merely glances at the figure and replies that Figure 1 appears to be two straight horizontal parallel lines. The second question Allport asks of his subject is, "What are the facts about Figure 1?" This calls for an objective answer. The subject takes a ruler, which is a straight edge, and compares the straight edge to both lines. He then measures the distance between the two lines at various points along the lines. The subject then replies that the facts about Figure 1 are that it contains two straight lines, and that these lines are parallel. Though the subject gives the same reply to both questions, it is clear that he has used a different method to arrive at each answer. The significance of the difference between these methods becomes clear only when the two methods are applied to Figure 2.

> Here we see lines like those of Figure 1, but in a different setting. If the reader will not repeat his two observations exactly as before, the point can be readily made; and in making it we shall come at last to a true understanding of the problem of perception. First we ask, as before, the phenomenological question: "How do the lines appear?" The reply will probably be, for both the upper and the lower figure, that they look CURVED. For the upper figure the report will probably

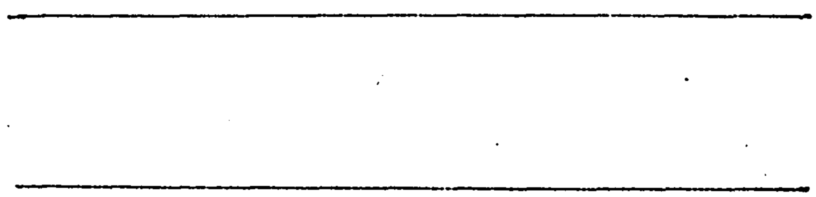

FIGURE 1

ALLPORT'S NON-ILLUSION STIMULUS

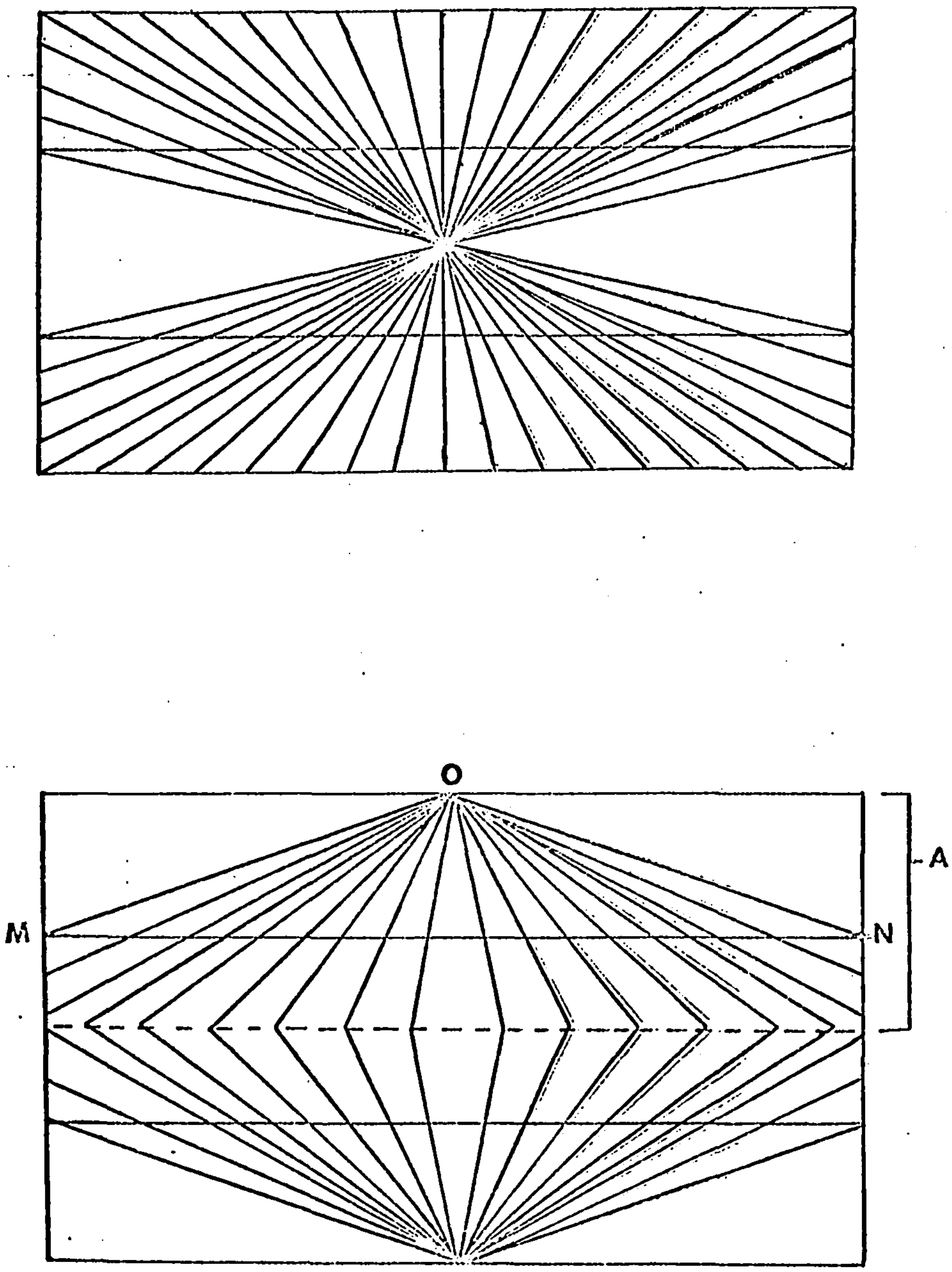

FIGURE 2

ALLPORT'S ILLUSION STIMULUS

be that they look closest together at the extremes and furthest apart at the center, while for the lower figure they look closest together at the center and divergent at the extremes. Now we ask for the observation corresponding to the physicalistic question: "What are the facts regarding the lines?" If the reader will again use the straightedge or ruler for testing both the upper and lower figures, thus again bringing in denotation, he will probably report that the lines in both cases are STRAIGHT and PARALLEL. Clearly then, phenomenological and physicalistic procedures may differ not only methodologically BUT IN THEIR REPORTED RESULTS. The two methods of observing give answers that disagree. It cannot be altogether true that things look as they do because they are what they are.
....
Moreover, if a large number of observers went through the same two procedures for Figure 2 we would doubtless find this same difference of outcome in all the observations. Each type of description is in this case general for a population of observers and follows its own laws. It is evident that pursuing the objective method and satisfying the criteria of denotation, relative observer-freedom, and publicly performable operations may lead to an experience content that is different from that produced by the phenomenological method with its failure to satisfy these criteria. The question of whether objectivity-criteria are to be used or not; therefore becomes significant. One of these methods cannot be substituted for the other without running the risk not only of eliminating the type of experience pertaining to first, but also of changing the report. Phenomenological descriptions must be used to state how the lines "look"; but if we want to know the "facts" about them, we must place our reliance upon the physicalistic account. (Allport, 1955, pp. 38-40)

It appears that this is one simple situation from which we can abstract some clear cut principles for discriminating between objective and subjective reports. This is possible only if we accept that the description the subject gives as an answer to the question "What are the facts about Figure 1 and 2?", is in fact an objective description. By objective, we assume Allport means that the description depends on the stimuli and not on an arbitrary bias that the subject brings to the stimuli.

We will demonstrate that in fact the description "straight and parallel" in response to either Figures 1 or 2, has imbedded in it arbitrary subjective biases brought to the situation by the observer. In addition, it will be shown that another answer can be given to the question "What are the facts about Figures 1 and 2?" besides the answer "straight and parallel lines." This alternative answer deviates as much from "straight and parallel" as does the phenomenological reply, but nevertheless satisfies all of Allport's criteria for objectivity.

The bias which we refer to is the assumption of an Euclidean geometry along with certain conventions of measurement and certain other assumptions about physics. There is nothing inherent in the stimuli of Figures 1 and 2 to justify any of the assumptions that are implicit in the Euclidean description given by Allport. We will demonstrate that alternative assumptions about geometry, physics and conventions of measurement will give us a description of these same figures, that is just as objective as the one given by Allport, but is nevertheless different. We will then be forced to face the question, "How can there be more than one objective description of these stimuli?"

Since the publication of Lobachevakii's <u>New Elements of Geometry</u> (1955) it has been well known that Euclid's geometry is only one of many geometries or theories of relations. Since antiquity it had been assumed that Euclid's was the only geometry and geometers have concentrated on problems within that systematic body of knowledge. In particular, proving the fifth postulate, which is the axiom of parallels, is a problem which had especially interested geometers. It is said that Gauss, Bolyai and Lobachevakii concluded at about the same time that a geometry was possible which did not assume the axiom of the parallels was in fact possible. For a long time, such non-Euclidean geometries were considered logical curiosities, of interest only to mathematicians and logicians. It was not until Einstein used it as the physical geometry appropriate to the theory of Relativity that non-Euclidean geometries became widely accepted as candidates for physical descriptions. It was at this time that the non-Euclidean descriptions were recognized as possibly more "real" than Euclidean descriptions. (Aleksandrov, 1963; Reichenbach, 1958)

To say that formal, purely mathematic geometry is a convention simply means that its terms are arbitrary symbols organized on the basis of logical consistency. Several such logically consistent systems can be constructed. The elementary definitions of a geometry are similar in many respects to the dictionary definitions of languages: therefore, it is possible to construct an Euclidean, non-Euclidean dictionary which gives the non-Euclidean definitions that most nearly approximate their Euclidean counterparts. (Table I is borrowed from Poincaré, and shows what such a dictionary would look

SPACE:	portion of space situated above the fundamental plane
PLANE:	sphere cutting the fundamental plane orthogonally
STRAIGHT:	circle cutting the fundamental plane orthogonally
SPHERE:	sphere
CIRCLE:	circle
ANGLE:	angle
DISTANCE BETWEEN TWO POINTS:	logarithm of the cross ratio of these two points and the intersections of the fundamental plane with a circle passing through these two points and cutting it orthogonally, etc.

TABLE I

ALTERNATIVE DEFINITIONS OF ELEMENTS IN LOBACHEVAKII AND EUCLID'S GEOMETRIES like.) The use of such a dictionary allows us to make logical translations from one geometry to another, only in principle. In actuality, such translations require the complex mathematical theorems of Riemannian geometry.

As long as non-Euclidean geometries were not involved in describing the physical world, the possibility of such geometries was not considered significant. The philosophical significance of the Theory of Relativity was in demonstrating that not only were alternative geometries possible, but alternative conventions of measurement were also possible. This meant that the non-Euclidean geometries could be applied to the real world, including even those situations which could be equally accurately described by Euclidean geometry. Basic measurement conventions were shown to be definitions rather than empirical truths. Non-Euclidean descriptions of space could only be applied to the real world if we constructed new definitions of measurement which would allow us to make the appropriate observations. We suddenly discovered that, since the geometry had made its way into the instrumentation of measurement, many of the Euclidean verities were true only by definition. Three different sets of assumptions are involved at arriving at our conventions of measurement. All of these are arbitrary definitions for which it is possible to construct logically consistent alternatives. The first of these which is most obviously relative is the conceptual definition of units. Basic units such as feet and meters are arbitrary conventions and we can devise methods for translating from one such system to another. The second of these involves the physical coordination of the defined units. This is a matter of building instruments which are coordinated to the scaling system devised. Certain assumptions about physics are involved when we define a foot rule on the basis of its rigidity and the stability of its shape.

The third set of definitions has to do with the rule for taking measurements. For example, when measuring distance in the ordinary Euclidean approach, we assume that the unit of measurement remains the same when transported from one plane to the next, and we therefore add equal values for each adjacent application of the instrument. It is possible to devise other rules which are as logically acceptable as those we are accustomed to using. Such rules must be stated in terms of a simple regulation which applies to each time and place a measurement is made. In the usual Euclidean approach such a rule simply states that an equal unit is added, let us say, one foot, each time the foot rule is placed exactly adjacent to the previous measurement. An alternative might be a rule which calls for a reduction in the size of the unit as a function of either the time or the place of the measurement. This alternative would call "constant" in length a unit ruler which from the point of view of Euclidean geometry would be said to shrink in size. In other rules the size of the unit could vary as a function of the distance from a defined point. The important point is that the combination of the possible rules of measurement is practically infinite and the selection of any particular rule is a matter of convenience.

It is precisely because these definitions are coordinated to physical objects like instruments of measurement that we must be careful not to mistake the definitions for empirical facts. That a foot rule remains a foot long is not something we can test, rather, it is something we have defined. If two foot rules have the same length at point A and then are transported to point B, we must find that they are the same length at point B or assume that one of them, or perhaps both of them, are no longer foot rules. We do not reject the definition for to do so would be to disrupt all previous measurements; rather, we assume the deviant rule to be an exception.

> Thus we could arrange measuring rods, which in the ordinary sense are called equal in length, and laying them end to end, call the second rod half as long as the first, the third one a third, etc. Such a definition would complicate all measurements, but epistemologically it is equivalent to the ordinary definition, which calls the rods equal in length. In this statement we make use of the fact that the definition of a unit at only one space point does not render general measurements possible. For the general case the definition of the unit has to be given in advance as a function of place (and also of the time). It is again a matter of fact that our world admits of a simple definition of congruency because of the factual relations holding for the behavior of rigid rods; but this fact does not deprive the simple definition of its definitional character. (Reichenbach, 1958, p. 17)

A geometry, therefore, cannot be applied without first developing conventions of measurements appropriate to it. Riemannian geometry has provided the mathematical abstractions necessary to enable mathematicians to describe non-Euclidean relations. Most of us must accept the fact that such non-Euclidean geometries and metrics are logically consistent since we are unable to internalize the conventions of measurement and the logic of the geometry in the same way we have done for Euclidean geometry. Generally speaking, when we are faced with a problem in geometry we test the correctness of the logical steps in the problem by visualizing or drawing the consequences. Thus, for the axiom of the parallels we would draw a straight line with a point next to it and "see" that only one straight line can be drawn through that point which is equidistant at all points with the one line. Our ability to do this rests upon our implicit knowledge of the definition of the elements of Euclidean geometry such as lines, points, planes and parallels; also, it rests on the implicit knowledge of the conventions of measurement to be used to test the straightness of lines and the distances of lengths. For this kind of implicit knowledge about Euclidean definitions we have no non-Euclidean counterparts. Mathematicians who have developed non-Euclidean counterparts for these definitions through the use of Riemannian analytic geometry claim that they gradually develop an intuitive grasp of such non-Euclidean relationships. For the sake of those of us who do not have the mathematical facility, mathematicians have devised a method of depicting non-Euclidean relationships in Euclidean diagrams. Perhaps the most famous of these diagrams is given in Figure 3. It was first developed by Klein, who with

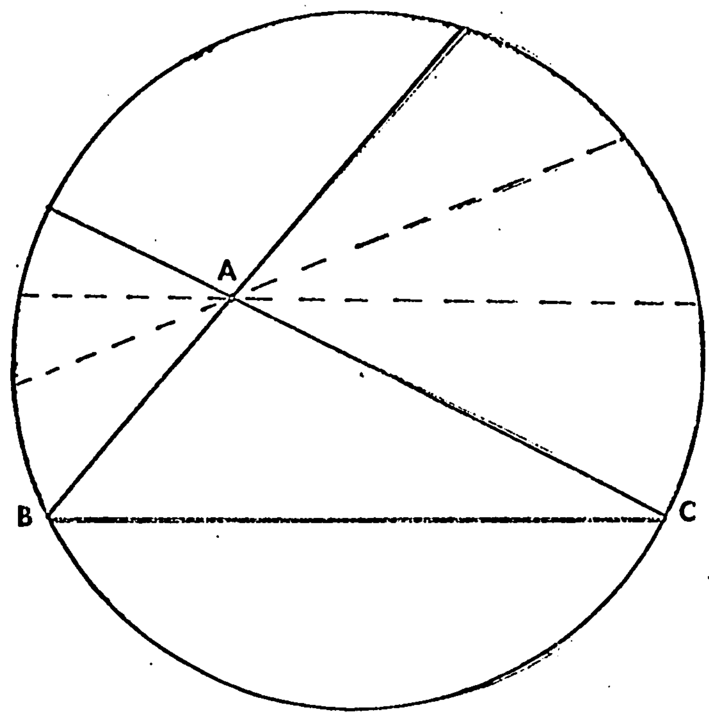

FIGURE 3

KLEIN'S DEPICTION OF A NON-EUCLIDEAN SPACE

Poincaré did much to amplify the significance of the development of non-Euclidean geometry to non-mathematicians. (Hilbert, 1952; Poincaré, 1953)

The circle forming the boundary of Figure 3 is also the boundaries of a Lobachevakii space. Since in Lobachevakii's geometry straight lines are geodesic, they do not travel infinitely as in Euclidean geometry. We may therefore represent the entire non-Euclidean space within this circle. If we limit ourselves to the space within the circle it can be seen that more than one straight line can be drawn through a point which does not intersect another straight line some distance from that point. To understand how we can limit the space by a circular boundary it must be understood that distance within the space is measured by means of a non-Euclidean convention.

Mathematicians have worked out complex formulae which describe in Euclidean terms the non-Euclidean metric for measuring the distance between two points. Mathematicians tell us that the consequence of this metric is that the unit of measure gets smaller and smaller as it approaches the periphery of the space. This simple statement of the effects of the metric has been seized upon as a heuristic device for the communication and depiction of non-Euclidean spaces.

Rather than explicating the complex mathematical functions which represent the conventions of non-Euclidean geometries, Poincaré (1953) Klein and Reichenbach (1958) make use of a universal force which affects the instruments of measurement in the same way the mathematical functions affect the conventions of measurement. In order to simplify the situation, Poincaré even reduces this force to something as simple as heat. Using this heuristic, we can explain how the circular boundary of Figure 3 could never be reached by anyone taking measurements from within the space. Poincare merely postulates a decreasing amount of heat emanating from the center of the space. This mysterious heat affects all bodies, immediately placing them in thermal equilibrium. As we look upon it from outside the space we can see that the observer from within the space who is making observations as he moves toward the periphery of the space is gradually decreasing in size. His instruments also decrease in size as do the steps he takes in walking toward the periphery. It becomes obvious that the observer within the space will never reach the periphery.

Reichenbach has addressed himself directly to the visualization or graphic representation of non-Euclidean spaces. In this he makes use of Poincaré's technique of projecting 3 dimensional curvilinear Euclidean spaces onto planes which yield 2 dimensional non-Euclidean spaces. Using this technique we can describe Allport's stimuli as they would appear in a non-Euclidean space. Figure 4 depicts the projection of a 3 dimensional Euclidean hemisphere into a 2 dimensional positively curved non-Euclidean space. Figure 5 shows the 2 dimensional non-Euclidean surface. We will follow Poincare by describing the non-Euclidean convention

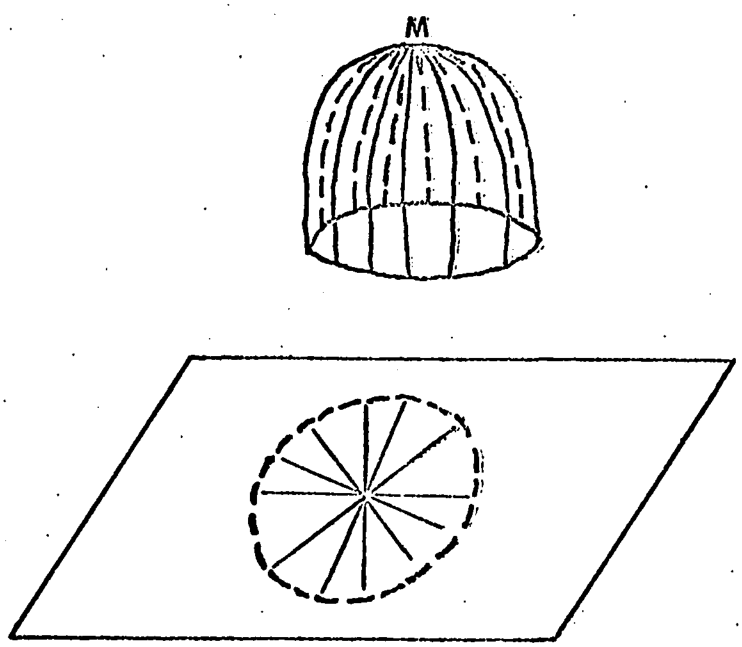

FIGURE 4

THE STEREOGRAPHIC PROJECTION OF A
3 DIMENSIONAL CURVILINEAR SPACE ONTO
A 2 DIMENSIONAL NON-EUCLIDEAN SURFACE

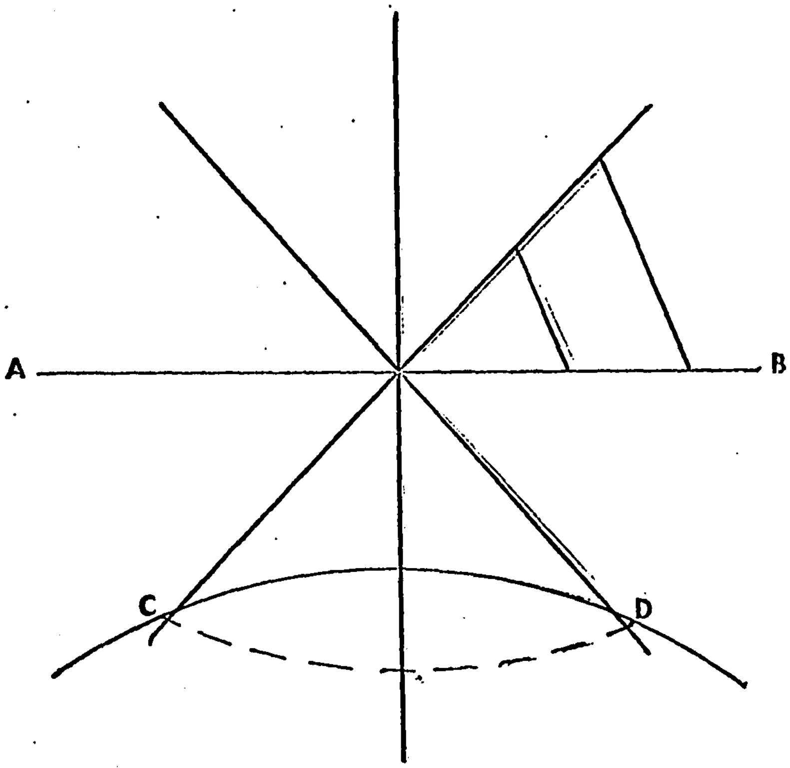

FIGURE 5

2 DIMENSIONAL NON-EUCLIDEAN
SPACE OF POSITIVE CURVATURE

of measurement appropriate to this surface in terms of a universal force which influences instruments within this space. The force expands all tangential lines and it increases in strength as one approaches the periphery. The force does not influence radial lines. This means that if a rigid rod were held perpendicularly against the radial AB in order to draw an equidistant CD, that the rod would increase in length in moving from the midpoint towards A and B, since as it travels from the midpoint towards either A or B it becomes increasingly tangential. If we wish to connect the points CD in this space, according to the Euclidean definition, the straightest line between CD is the dotted line. Since the dotted line CD is farther removed from the midpoint it carries the rod through that portion of space of maximal influence of the universal force; therefore, the full length of the rod can be laid down fewer times along dotted CD than it can along solid CD. Thus, we have a non-Euclidean description wherein straight and parallel do not coincide, and it is in this sense that the axiom of the parallels cannot be demonstrated in this non-Euclidean space. (Reichenbach, 1958, Hilbert, 1952)

The non-Euclidean description appropriate to Allport's stimuli in Figure 1 is obviously different from both the phenomenological and the Euclidean description. Despite this difference, the non-Euclidean description satisfies all of Allport's standards of objectivity, and therefore it stands on an equal footing with his own physicalistic description. He gives us three standards of objectivity, all of which are to be applied in evaluating the method that a particular observer uses in achieving a description. According to Allport, if the three standards of objectivity are met, the object of description can be said to have an existence apart from us and belongs to the world of the natural sciences. The three standards are: the degree of observer involvement, denotability and operationism. Of these three, the most difficult standard to apply is the first one.

Is it not clear what Allport means by subjective involvement. For example, in the problem of describing the stimuli in Figure 1, is it more or less subject involvement when the observer makes explicit the role that geometry and conventions of measurement play in the description which he gives? We are all more willing to rely on a non-mathematical intuitional application of Euclidean geometry than on a non-Euclidean geometry. It is therefore proper to say that a Euclidean description is more subjective because it relies on psychological rather than on logical analysis? Despite this fundamental ambiguity in the standard of observer involvement it seems clear that the differences in observer involvement between the Euclidean and non-Euclidean descriptions are trivial ones that are due to cultural prejudice.

The standard of denotability means, according to Allport, coming into physical contact with the object. In Figure 1, Allport means by denoting, placing a ruler in physical contact with the figure. It should be clear that according to this criterion the Euclidean and non-Euclidean do not differ at all. What differs with respect to rigid rulers is the manner in which they are coordinatively defined and the rules for their application, not their coming into physical contact with the stimulus.

The third of Allport's standards of objectivity is operationism which can be subdivided into the criteria of publicity and repeatability. By publicity is meant that the process of observation must involve operations such as those undergone with the ruler, which can be observed by anyone present. Repeatability means that such operations would yield similar results when carried out at a future time. With respect to the criterion of operationism it should be obvious that the Euclidean and non-Euclidean descriptions of the stimuli in Figure 1 are on equal footing.

We are forced to say that there is more than one physicalistic description of the stimuli of Figure 1 which meet the criteria of objectivity. Allport would probably retort that even though this is true we have only managed to complicate the meaning of physicalistic description, and have not understood his central thesis which is an attempt to differentiate the phenomenological from the physicalistic. He could at this point maintain that the Euclidean and non-Euclidean descriptions combine to define the objective methods for the physicalistic description of the stimuli in Figure 2, and that these physicalistic descriptions form a class still essentially different from the phenomenological. To make this point, Allport would probably rely on the distinctions he made with respect to Figure 2, which is, according to Allport, a phenomenological illusion. To refer to this stimulus as an illusion means that the phenomenological deviates from the physicalistic and is therefore a distortion of reality. This is the case if one accepts the Euclidean description as the only legitimate physicalistic description of either the stimuli in Figures 1 and 2. It can be seen that if we use the non-Euclidean physicalistic description as our "true" or "real" description of the stimuli, then the phenomenological description of Figure 1 becomes an illusion and in the relevant segment of Figure 2 the phenomenological and physicalistic correspond.

To demonstrate this we have drawn dotted lines through the stimuli in Figure 2 in such a way as to reduce Allport's illusion to its fundamental elements. From this demonstration, it can be seen that the apparent curvature of the parallel line is always in the same direction. The ends of the straight line appear to curve towards the point of origin of the radiating lines. Explaining this illusion requires an accurate description of the segment A since the more complex illusion is made up of various combinations of this simple segment. We can superimpose segment A of Figure 2 onto a positively curved non-Euclidean 2 dimensional surface such as the one given in Figure 5. If we assume that the line MN (Figure 2) is a straight line, then it will curve as does the dotted line CD (Figure 5). Having done this we can see that the physicalistic non-Euclidean description of the segment A (Figure 2) corresponds to the phenomenological description. All of the straight lines radiating from point O in Figure 2 can be superimposed upon the non-Euclidean space without any change in their apparent or physicalistic dimensions since radials are not affected by the universal force in this non-Euclidean space. It might be objected that the line MN (Figure 2) is a parallel rather than a straight, but in order to make this objection one must bring in Euclidean geometry, since in non-Euclidean geometry straights and parallels never coincide. It would be possible to construct a non-Euclidean surface of negative curvature in which solid CD (Figure 5) would be the straightest, while dotted CD would be the equidistant. Constructing such a surface would merely require reversing the position of the pole and the circumference of the hemisphere being projected in Figure 4. In such a non-Euclidean space, the universal force would reduce the size of all tangential lines as a function of the distance from the midpoint.

If we review each step that was taken we can see that the word illusion makes sense only in a very narrow application. Figure 2 demonstrates an illusion if our standard is Euclidean geometry, and Figure 1 represents an illusion if our standard is non-Euclidean geometry. The error in Allport's analysis is characteristic of most of the work done on illusions in that it seeks to establish a hierarchy of description in which the physicalistic narrowly construed is held to be the standard against which all the descriptions are to be judged. Allport claims his comparison is between the phenomenological and the physicalistic. Our demonstration shows that Allport is contrasting the phenomenological with the common sense view, which is certainly different from the physicalistic.

The fact demonstrated above, that the phenomenological can sometimes be Euclidean and sometimes be non-Euclidean, suggests that a description of phenomenological geometry might give us standards by which to evaluate other types of description. The beginnings of the kind of research which might lead to a phenomenological geometry is indicated by Luneburg's (1948) work. Luneburg and his associates have demonstrated that the geometry of binocular vision can take on different kinds of metrics and for certain circumstances non-Euclidean metric is assumed. Non-Euclidean metrics, are associated with the perception of a large array of stimuli at a substantial distance from the subject. (Hardy, Rand, Rittler, Blank, Brider, 1953)

The demonstration given above and Luneburg's preliminary research seem to indicate that what we call illusions are phenomenological experiences for which we have not yet identified an appropriate geometry and metric. Furthermore, it suggests that the separation of phenomenological and physicalistic descriptions into two qualitatively distinct fields of experience is unjustified.

We have seen in our review of various perspectives concerning objectivity that the notion of independence is one of the basic standards by which objectivity can be established. The idea of independence can be expressed in both the jargon appropriate to perception, and that appropriate to observation.

We assume a feature to be a part of the stimulus when all subjects can discriminate it by means of verbal or other responses. By the same token we assume an observation to be factual when repeated public operations yield similar observations. We have already seen that the manner in which such observations are made is a point upon which there is less agreement than there is upon the belief that factual observations can be made.

The most important feature of factuality is independence. When we assert something to be a fact we mean that it in no way depends upon us. Feyerabend (1965) refers to the independence of facts as the "autonomy principle", which he says is one of the most basic implicit assumptions of contemporary science. The autonomy principle does not insist that the "discovery" or generation of facts is independent of theory or of the observer's cognitions, but that once they are established they remain facts forevermore and become an essential unit of knowledge within the discipline.

T.S. Kuhn (1962) concurs with Feyerabend in holding that the most important assumption of the traditional epistemological paradigm is the belief that facts have an independent existence which, once established, makes them stable elements of knowledge. According to Kuhn, this leads to a theory of history of science in which knowledge is considered cumulative. In this view theories merely serve to generate the building blocks of knowledge which are the facts.

In this section, we wish to examine the phenomena of patterning, or as the gestalt psychologists call it, form perception. In doing so we will look at an example of form perception taken from the science of perception and one taken from the history of physics. It is hoped that the reader will see parallels between the two examples to be given. The purpose in examining patterning is to demonstrate that perception and observation are similar in that they select from the given only certain features, and that these features are selected on the basis of a pattern brought to the situation by the observer. In the field of perception such patterns are usually referred to as predetermining tendencies. With respect to scientific observation, these patterns are theoretical concepts. The implication which will have to be examined subsequent to the discussion is that the line between logical and empirical or theoretical and factual is difficult, if not impossible, to draw.

Figure 6 is the stimulus of a reversible figure which can be seen either as a goblet or as two profiles. Literature in perception is replete with examples of such reversible figures, yet for all practical purposes they remain curiosities. "There is not much history to the reversible perspective because the fact was obvious and no one has been successful in offering a plausible explanation of it." (Boring, 1942, p. 271)

In the example of the goblet, we can see the fundamental argument of the configurationists: the whole determines the perception of the parts. If one begins with the whole "goblet"

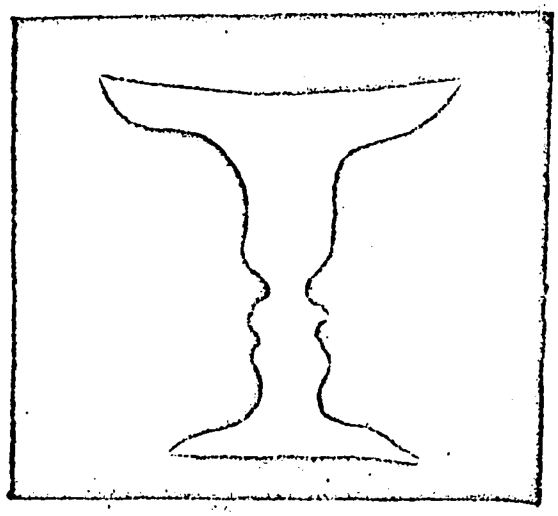

FIGURE 6

REVERSIBLE FIGURE

then one can see its stem, base and rim in Figure 6. On the other hand, if one begins with the whole "profiles" one can see the noses, lips and brows. In this example we find the basis of the argument that facts are dependent on some kind of pattern. This becomes clearer if we consider the "whole" as a hypothesis which can be tested. If this is a goblet then one should be able publicly to denote its rim, stem and base. Is it possible to establish that this is in fact a goblet? The answer must be affirmative but by the same token it also must be said that we can demonstrate by publicly denoting noses, lips and brows, that it is in fact two profiles.

The most difficult question seems to be whether we interpret the pattern or whole as something inherent in the stimulus, or as a factor brought to the situation by the subject. The opposing points of view with respect to this question will be reviewed in a subsequent section.

In order to demonstrate that this example is not a psychological problem pertinent only to perception, let us look at another example from the history of mechanics. This example (Figure 7) is borrowed from Kuhn and it demonstrates that two scientific descriptions of an object have no necessary overlap in the attributes they wish to measure. When viewing a body in constrained fall, the Aristotelian measured the weight of the stone, the vertical height to which it had been raised, and the time required for it to achieve rest, and he considered such measurements an adequate description of the phenomena. Galileo's description of the pendulum

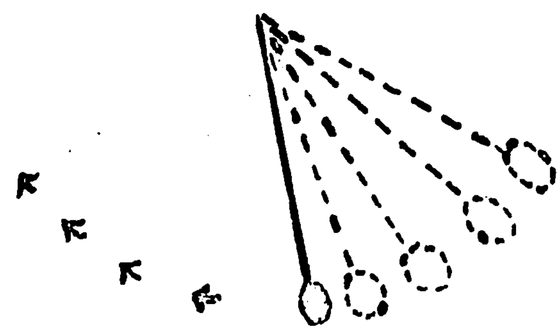

FIGURE 7

ALTERNATIVE VIEWS OF A
SWINGING BODY

ARISTOTLE

describes Figure 7 as a body in constrained fall

He measures:

1. the weight of the stone
2. the vertical height to which it had been raised
3. the time required for it to achieve rest

GALILEO

describes Figure 7 as a pendulum

He measures:

1. weight
2. radius
3. angular displacement
4. time per swing

required the measurement of weight, radius, angular displacement and time per swing. It is obvious that the dimensions being considered by each observer with the single exception of weight, are not interchangable. If we consider the total sets of measurements made in describing a phenomenon, then we are forced to say that one is incorrect, or that they are different phenomena, or finally that neither fully represents the phenomenon. (Koyré, 1939, 1943)

The difficulty presented by both of these examples is the identification of the independent element in observation or perception. Once we are operating from a particular theoretical construct it is impossible to disprove it unless we are provided with an alternative theoretical construct. One could have made a thousand observations on bodies in constrained fall and never have concluded that they were in fact pendulums. The larger concept is a prerequisite to the empirical test. The questions one asks about the phenomena are determined by these constructs. It does not make sense, for example, to ask "Where is the goblet's nose?" Before one can start looking for the noses, he must know that they are a possibility, and such knowledge depends on the assumptions he is making about the array or stimulus before him.

There are other ways in which the examples given above can be interpreted. It can be said that the individual measures taken are the objective facts which remain stable over time and that the pattern is an interpretation of these measures. According to this account the measurements cannot be challenged.

This point of view is consistent with phenomenalism in which the primitives of knowledge are individual sense data. Though this position can be defended with respect to many kinds of descriptions, it is fallible with respect to the kinds of situations in which more than one objective description is available. Given the parameters selected by Aristotle, one could not arrive at an interpretation equal to Galileo's pendulum anymore than one could make profiles out of bases, stems, and rims.

The examples presented above are anomalous to the idea of objectivity since it is impossible to separate the elements which are independent of the observer from those that the observer brings to the situation. This is particularly significant if one accepts the fact that the most important steps forward in science are made through the creation of such constructs such as that of a pendulum which gives us insight or a new way of looking at something.

The idea that constructs guide us in the selection of the significant parameters to be measured is not a new one. We have expressed this in our discussion of the configurationist's view of science. What is important to notice in this context is that it occurs at the perceptual level. It is not entirely an intellectual matter. Furthermore, there are parallels between the anomaly and the one presented before it concerning the appropriate geometry to be used in describing the stimulus. In each case, we are given seemingly contradictory descriptions of a single stimulus or object, and yet it is impossible to assign preference to one or the other of these by using any of the criteria of objectivity. Again we are faced with the question, "How can there be more than one objective description of a single thing?"

In this section we will look at examples in which the qualities of perception and the properties of observation are demonstrated to be the function of a relationship between the observer and the observed. It might be argued that this is precisely what we have already examined in the cases of the alternative geometries and reversible perspectives; however, as we will see later the cases of the geometries and the perspectives are subject to various interpretations, some of which are not consistent with a relativistic point of view. The three examples which we wish to examine are also subject to various interpretations. We will present them here as they were interpreted by the men who created them. They are, a specific example of a transactional demonstration, an examination of I. Kohler's prismatic lens experiment, and an experimental exposition of indeterminacy relations in quantum mechanics. These will be presented from the points of view of Adlebert Ames and his colleagues and Werner Heisenberg and Bohr respectively. In discussing the experments we will be presenting nothing which is essentially new. The benefit in doing so should derive from the context of combining the perspectives of observation and perception and from noting parallels between Ames' and Heisenberg's contri-

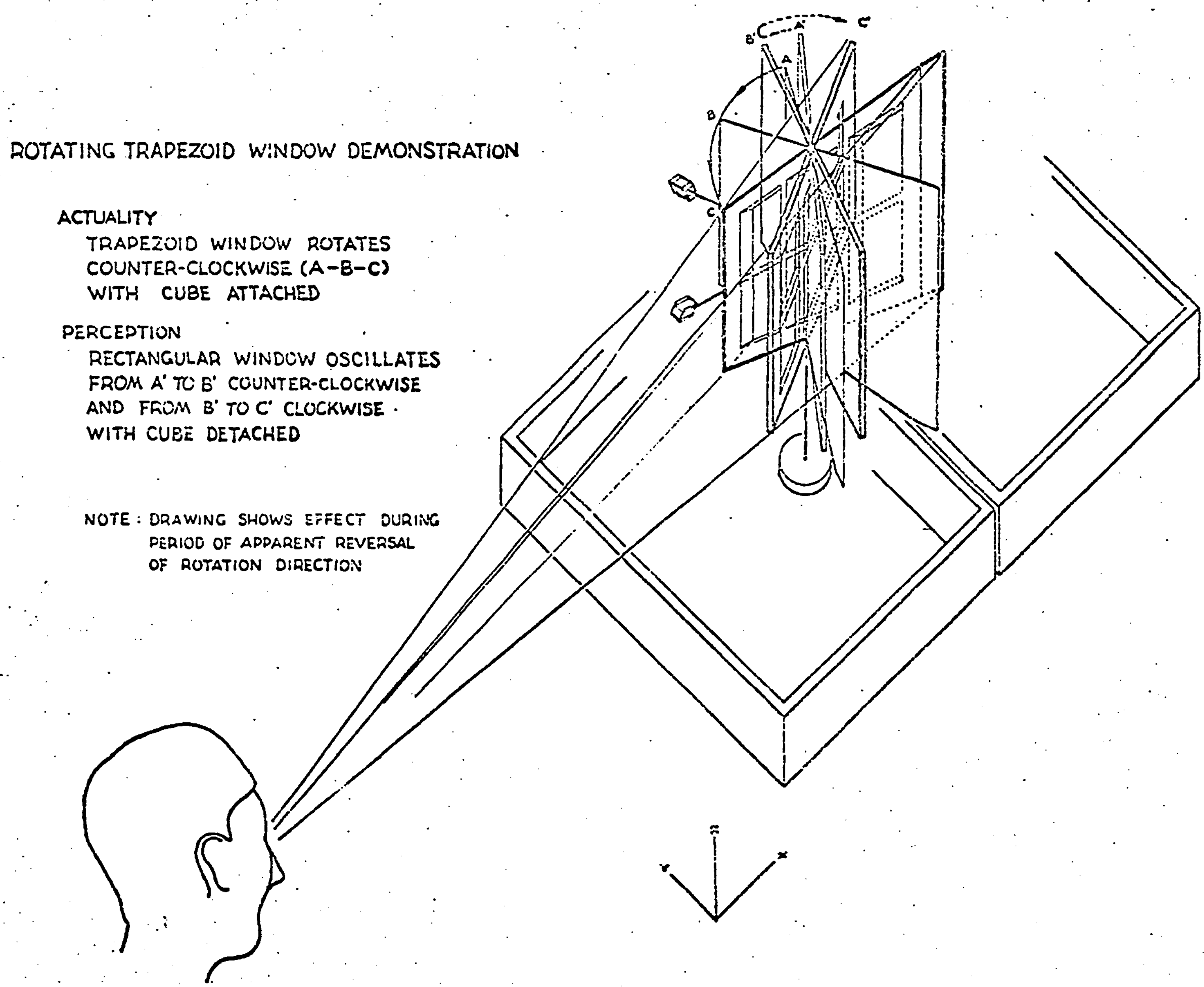

FIGURE 8
AMES' ROTATING TRAPEZOIDAL WINDOW

butions. Finally, it is important to remember that the purpose in reviewing these phenomena is to see how they can be incorporated into our assumptions concerning objectivity.

To understand the basis of all of Ames' demonstrations requires understanding what Ames means by "equivalent configurations." In Figure 8 we have depicted a stimulus which has equivalent configuration values as a trapezoid or as a rectangle viewed from a particular angular perspective. In many respects what we have here is a simple reversible figure. This particular example of equivalent configuration is the basis of several of the Ames' demonstrations, the most famous of which is the rotating trapezoid.

In the rotating trapezoid demonstration we can see how Ames employs the equivalent configuration principle to demonstrate the role that assumptions play in perception. When the trapezoidal shape is shaded to appear as a window frame, the assumption that it is rectangular gains strength. When this frame is rotated on its vertical axis and viewed monocularly it appears to be an oscillating rectangular window frame, rather than a rotating trapezoid.

The use of the phrase "appears to be" requires clarification. Allport (1955) describes the rotating trapezoidal window as a "dramatic masterpiece of ambiguous stimulation". When we combine phrases such as "ambiguous stimulation" and "appearing to be" it might be supposed that the subject is in doubt about what he sees, and that phenomenologically his experience is fuzzy, unstable or unclear. But this is not at all the case; if the subject assumes the rectangular shape and he views the stimulus monocularly, his description will remain stable and definite. He will unambiguously perceive an oscillating rectangular window frame.

The stability of this perception can be demonstrated by attaching to the trapezoidal shape other objects which will rotate syncronously with it. If a rigid tube is attached to the apparatus, as in Figure 8, the tube will appear to rotate rather than oscillate. The assumption of rectangularity will persist in making the window oscillate. At first, subjects will assume the two to be pliable and under this condition will report that the tube "bends" around the window in order to accomplish its rotation. The role of cognitive factors can be further demonstrated by informing the subject that the tube is rigid. Given this information the subject will report that the tube appears to be cutting through the window frame when the window and tube are seen to be moving in opposite directions. When a small cube is attached (Figure 8) to one end of the frame we get an even more dramatic result. The subjects' report that the cube moves in a rotary motion, seeming to detach itself during part of its motion, and proceeds to move with no apparent source of support for half of its rotation.

It has been said of this stimulus that it is a masterpiece of ambiguity. Certainly, this observation cannot mean that the subject is in doubt or is unclear whether the window is oscillating or rotating. When this stimulus is viewed for the first time from a perspective of equivalent configurations by a subject raised in Western culture, he always reports, immediately and without doubt, that the window oscillates. We must conclude that Allport means by "ambiguous" to refer, not to the experience of this stimulus, but to his knowledge of the experimental history. In fact, Ames has demonstrated that knowledge of the two perspectives or of the experimental history of the stimulus will not in itself significantly influence the probabilities of either experience. This means that even if a person knows that he is viewing the Ames' rotating trapezoid when he views it from the perspective of equivalent configurations, he will unambiguously perceive an oscillating window frame.

The dramatic reports given as a result of attaching the tube and cube are dependent on the weight or probability valuation that the subject holds with respect to the assumption of rectangularity. Similarly, how close one approaches the optimum degree of equivalence configuration is a function of the viewing conditions. The optimum conditions specify a monocular viewing of the rotating trapezoid from 20 feet away. The subjects' weighting of the rectangularity assumption can be influenced. Ames has indicated that oscillatory motion can be perceived when the rotating trapezoid is a simple cut-out of uniform color and shading, but under these circumstances viewing conditions must be closer to the optimum, and even then the oscillatory effect is more tenuous. The addition of mullions, panes and shading adds weight to the rectangularity assumption by taking advantage of the familiarity of windows.

> In his past experience the observer, in carrying out his purposes, has on innumerable occasions had to take into account and act with respect to rectangular forms, e.g., going through doors, locating windows, etc. On almost all such occasions, except in the rare case when his line of sight was normal to the door or window, the image of the rectangular configuration form on his retina was trapezoidal. He learned to interpret the particular characteristic retinal images that exist when he looks at doors, windows, etc., as rectangular forms. Moreover, he learned to interpret the particular degree of trapezoidal distortion of his retinal images in terms of the positioning of the rectangular form to his particular viewing point. These interpretations do not occur at the conscious level, rather, they are unconscious and may be characterized as ASSUMPTIONS as to the probable significance of indications received from the environment. (Ames, 1951, p. 14)

There is cross cultural evidence which supports Ames' contention that such assumptions are culturally determined. In an experiment conducted by Allport and Pettigrew (1957) it was shown that the degree of susceptibility to the perception of oscillatory motion of the rotating trapezoidal window was a function of the individual's past experience with rectangular forms, especially windows. It was of course impossible to get a population of subjects who had no experience with rectangularity, but at least two of the groups in this experiment were young Zulu herdsmen with little formal education. Zulu culture is such that rectangular forms are not to be found among their artifacts. Zulu homes, windows, and doors are all constructed with curved lines. These subjects were tested with the trapezoidal window in a building of western construction, and thereby assuring that they had had at least one actual contact with rectangular windows. Nevertheless, in order for these subjects to report oscillatory motion, all the conditions had to be optimal, and they reported oscillatory motion less frequently than did western acculturated Zulu boys of their own age. The difference between the western acculturated and Zulu acculturated boys in the frequency of their perception of the oscillatory motion in all but optimal circumstances was statistically significant at the .001 level of significance.

Two important points must be emphasized in order to understand Ames clearly. The first one is that all perceptions are probabilistic, and therefore correspondence is between experiences and not between experiences and reality. Because the Ames demonstrations seem to be contrivances under which the individual is not operating normally, there is a tendency especially noticeable in secondary source accounts of Ames' work to view one of the perceptions as an illusion and the other as correct. Ames (1960) himself is hostile to this interpretation.

The second point to be emphasized is that the probabilistic nature of perception is not revealed in the perception itself. It is for this reason that the tendency referred to in the last paragraph seems so natural to us. Each perception has the same quality of certitude or the same degree of "sureness" as any other in experience until such time as an alternative is available. It is for this reason that the term "ambiguous stimulus", to refer to the Ames' demonstrations, is misleading. The ambiguity is known only to those who are familiar with the two views. Ittelson (1960) can help us to clarify this point.

> How can anyone act in the face of the probability of failure? Action implies certainty. If the perceptions on which actions are based necessarily prove to be to some extent imperfect, and if our actions will inevitably be unsuccessful to this extent, we might eventually reach a condition where we would hesitate to act at all. The answer to the question is to be found in the nature of the perceptual process--when the predictive reliability of a perception becomes high enough we act "as if" we were dealing with certainty. Even if the predictive reliability is low, so that we lack a sense of surety, when we finally do act we must act with certainty if we are to have any chance of being successful. One aspect of perceiving, then, is the creation of certainty out of uncertainty or probability. Every perception is an act of creation; every action is an act of faith. Every action is based on the belief that highly probable events are certain events. It is an act of faith in the reliability of one's own assumptions, of faith in oneself. Accordingly one product of perceiving is our creation of a world of FUNCTIONAL absolutes. At any given moment, these functional absolutes are treated as if they were certain but, concurrently, are held open for modification. (Ittleson, 1960, p. 38)

The meaning of the term "interpretation" in our discussion of Ames' work may be the source of some confusion. Many secondary sources view Ames' demonstrations and his functional analysis as a continuation of the Helmholtz "unconscious inference tradition." (Allport, 1955) There are aspects of this tradition which are inconsistent with Ames' (1960) own interpretation of his work. Generally speaking, unconscious inference implies that in sensation there are the raw qualities of experience which are subject to the various interpretations by perceptual processes. The problem with this analysis is in identifying what qualities are a function of sensation, and what modifications are attributable to perception. Though it is not explicitly stated in many places there is the implication that the sensations, or that which is interpreted, contain the more fundamental qualities, and that the modifications imposed on these by perception are organizational and do not effect the basic qualities. This view of the matter is particularly applicable to the notion of equivalent configurations. When we have equivalent configurations it seems that in the stimulus the basic qualities such as color and size are given and that the process of interpretation is one which influences their organization. Ames (1960) himself does not subscribe to the distinction between sensation and perception, and holds that all the qualities of perception are "interpretations" or " prognostic directives". It must be admitted, however, that Ames' own demonstrations are not sufficient evidence for dismissing the notion that basic qualities are given in sensation.

A review of the many perceptual demonstrations developed by Ames and his colleagues at the Hanover Institute reveals that the kinds of configurational equivalencies Ames worked with were in the broadest sense geometrical. That is, the features of stimulus array, which he demonstrated could be perceived in more than one way, tended to be holistic and organizational. It is possible to interpret the Ames' demonstrations as instances of the perceptual reorganization of of more elementary sensory input. For this reason it is desirable to look for another source of experimental information which demonstrates that other than organizational characteristics are the function of perceptual interpretation.

Ivo Kohler (1964) conducted an experiment which relied on a principle that resembles Ames' equivalent configuration; it demonstrated that at least one so-called "basic quality" is not given in sensation, but is as much the function of interpretation as are organizational qualities. In this experiment Kohler wore prismatic lenses for a long period of time. The effect of wearing such lenses is well known. When they are first put on, the lenses distort the subject's perception in a direction consistent with the optical transformations. But with continued use under conditions in which the individual is involved in activity, the visual perceptual processes compensate for the optical transformation and render the transformation "normal". Upon removing the transformation lenses, there is a prolonged period of aftereffects which distorts the visual perception in exactly the opposite direction from the optical transformation of the lenses. For example, if the lenses worn distorted straight vertical lines so as to curve them to the left, then the aftereffect of removing these lenses would be to perceive straight vertical lines as curved to the right. Presumably, the reason for this opposite effect is that the optical input is still being transformed or interpreted as though the person was wearing the lenses, and what was a compensating transformation now becomes the source of distortion.

The specific kind of transformation which is of interest to us here is one which deals with color perception. The reason for our interest in color perception is that color is usually considered one of the "basic qualities" which is given us by sensation. In this respect, color is sharply distinguished from such qualities as curvature and texture, which seem to be more subject to interpretation. In this experiment Kohler's prismatic lenses diffracted light so as to produce small rainbows of color. Much to Kohler's own surprise, he discovered that there was a complementary aftereffect which gave him the experience of seeing the opposite end of the rainbow along the contours of white objects whenever he removed his prismatic lenses.

The diffracted color bands seen upon putting on the lenses for the first time came as no surprise. They were fully predictable from our knowledge of optics. The discovery of the complementary color after-effect was absolutely unpredictable. This kind of aftereffect simply would not be anticipated from our knowledge of color after-effects, and in fact it is inconsistent with all existing theories of color perception. We quote directly from Kohler's (1964) diary to capture some of the excitement that he experienced in this serendipitous discovery. The account refers to Kohler's experience at the time when he removed his lenses and gave them to someone else to look through. The two men compared one another's perceptions of the room, Kohler's companion gave a description commensurate with what is known about the prismatic distortions which occur prior to a period of adaptation while Kohler's description results from having worn the lenses for a long period and therefore are after-effects from adaptation.

> Finally, he casually mentioned that the edges had colored bands. He was referring to the vertical beam on the right side of the window, which was illuminated by the light on the ceiling and stood out in sharp contrast from the rest of the window, which was dark. I asked him what the color was, but before he told me I clearly noticed--without spectacles--that it was a YELLOWISH-RED, belonging to the longwave extreme of my spectrum, and that it appeared on the outermost edge of the white crossbars. I was so struck by this observation that I seized the spectacles and put them on myself. Lo and behold, the edge glimmered in a beautiful, saturated blue. I tried this experiment over and over again, until late into the night, on all kinds of objects. The result was always the same. Whenever I looked at an edge without the spectacles, I saw a dim yellowish-red or bluish-violet band: as soon as I looked at it through the spectacles, the edge took on a complementary hue. (Kohler, 1964, pp. 68-69)

These after-effect colors were complementary in several ways to the ones induced by the prismatic lenses. The first and most important of these complementarities was between the ends of the spectrum seen as an after-effect and with the lenses. The color bands seen with the lenses were at the short end, while those seen as an after-effect were at the long end of the spectrum. The optimum conditions for seeing the color bands were also complementary opposites. With the lenses, the color bands were most conspicuous under bright sunlight illumination, while the after-effect bands were seen most conspicuously at dusk or under artificial illumination.

Kohler was surprised to find that there were these color after-effects because such an after-effect suggested that the color perceptions were subjective. Our notions concerning color perception rely on a correlation between biochemical activity in the retina, and the frequency of light stimulation. The reason Kohler's findings are anomalous is that they could not be connected to the frequency of light stimulation unless the transformation came between the source of stimulation and the retina, as was the case with the prismatic lenses. Kohler then naturally hypothesized that the adaptation effects were to "prismatize" the lens of the eye in order to counterbalance the optical diffraction of the prismatic lenses. Such an effect on the lens of the eye would account for all the phenomena observed. Kohler devised an experiment to test this hypothesis. He merely put himself in a room in which the only light source was incandescent sodium which produces light which is homogeneous and cannot be diffracted.

> The results of this attempt were unequivocal: even in the homogeneous sodium light, I saw WITH or WITHOUT spectacles the SAME color bands which in normal daylight I could see only WITHOUT spectacles. This evidence clearly indicated that the phenomenon in question was a purely subjective one, that is, a specific kind of afterimage. (Kohler, 1964, pp. 69-70)

The significance of these findings and their connection to Ames' work rests in their similarity to Ames' principle of equivalent configuration. What Kohler shows us is that for a perceptual quality as basic as color there can more than one kind of stimulation; that the experience of color depends as much upon what the individual brings to the situation as upon the situation itself. It also makes explicit that assumptions may be viewed as unconscious mechanisms. The prismatic adaptation of the visual perceptual system is analogous in Kohler's experiment to the assumption of rectangularity with respect to trapezoidal retinal images of windows. The only difference is that it is easier for us to believe that we can have perceptual assumptions about geometry, than it is for us to believe that we can have such assumptions about hue.

With each anomaly that we have discussed so far, we have tried to develop parallels between material taken from perception, or the psychological realm, and material taken from other science, or the physical realm. Finding such parallels is important to our central theme: perception and observation are essentially the same kind of activities, and one set of principles should be capable of encompassing them both. The physical parallel to Ames (1960) and Kohler (1964) is given to us by Heisenberg (1958A, 1958B) in his famous indeterminacy principle and by Bohr (1962) in his principle of complementarity. These two principles of quantum physics are the essence of what is referred to as the Copenhagen interpretation. The Copenhagen interpretation is an account of quantum mechanics which, for the vast majority of physicists, is at the present time the only acceptable account. We will accept the Copenhagen interpretation without delineating its relationship to earlier more classical interpretation, such as Born's (1960), or what some would consider presently competing interpretations, such as DeBroglie's (1960) hidden variable interpretation. It will suffice to say that the Copenhagen interpretation is the only available account which does not require the addition of special constructs for each case of quantum phenomena. The hidden variable theories in their present development have not been successful in presenting one account for all the various phenomena, but must resort to the use of unparsimonious special rules. (Capek, 1961; Jammer, 1966; Putnam, 1965)

Our concern with indeterminacy and complementarity does not rest on an intrinsic curiosity about the physics for which they were developed to deal. Instead we are interested in these principles because they represent a fundamental change in the epistemology of physics. This fundamental change is conceptually equivalent to the changes urged by the transactionalists in the field of perception. Indeterminacy and complementarity suggest the same kind of transition for our notion of "property" in physics that the transactionalists wish to bring about in our notion of "qualities" in perception.

It is necessary to begin at the level of phenomena in order to understand how indeterminacy and complementarity combine to resolve the problems inherent in quantum mechanics and also to change our epistemological account of properties. As a starting point, we will present two experiments which give us seemingly anomolous information concerning the nature of elementary particles.

The first of these is Einstein's (1905) interpretation of the photoelectric phenomenon. In a typical photoelectric experiment a source of ultraviolet light which is mono-chromatic is shown upon a metal surface. The result is that electrons are given off by the metal. The older wave theory of light could have predicted that the light energy would increase the energy levels in the metal, and consequently that electrons would be emitted. The problem with the classical interpretation was that the energy of light was held to be a function of the light intensity; consequently, a classical prediction was that as one increases the intensity of the light the emission of electrons begins at an intensity threshold and proceeds to increase in proportion to the light intensity increments. Such a prediction does not square with experimental evidence that no intensity of red light could produce the photoelectric phenomenon, while very low intensities of ultra-violet light would produce electron emission. Einstein related the connection between energy and frequency to a similar relationship that Planck had noticed in black-body radiation. He demonstrated the accuracy of Planck's formula $E = hv$, wherein E is the energy of the light, or photon, and v is the frequency of the light wave, while h is Planck's constant. In other words, Einstein was saying that light was made up of particular photons, each having an equal amount of energy, and that that energy is a function of the frequency of the light, not its intensity. The intensity of the light governed only the number of photons carried by a light wave. Given that the photons were of sufficient energy to cause electron emission in metal, then an increase in their number or in the intensity of the light would increase the number of electron emissions. However, below a certain frequency an increase in intensity would not produce any photoelectric emission.

What is most important at this point is that in Einstein's solution $E = hv$ there is a curious mixture of parameters taken from classical mechanics and classical optics. Energy (E) which is a parameter of particles in classical mechanics, is said to be a function of frequency (v), which is a parameter of waves in optics. The photoelectric effect clearly demonstrates that light comes in quanta, particles of energy, but the value of such particles can be determined by observing one of light's wave characteristics.

The seeming contradiction in $E = hv$ is amplified when we consider another phenomenon of quantum mechanics, which is the interference of electrons. Elementary units of matter were classically considered to be particles, but in deBroglie's (1940-42) interpretation of the interference of electrons we find that such elementary particles as electrons are associated with a wave characteristic which is frequency.

Interference was first demonstrated by Young. (Silva, 1969) In this simple experiment Young pierced two small holes in a screen and light shone through these two openings. When one of the openings was covered up, the pattern of light that was projected on the surface behind the screen was homogeneous and circular. When light was shown through both openings, the projected pattern which resulted contained bright and shaded striped areas. Young (Silva, 1969) interpreted this to mean that light was a wave and that the projected pattern resulted from the cancelling and amplification which occur when waves interfere. What deBroglie was interested in explaining was that a similar interference pattern can be obtained by projecting electrons through two slits onto a surface which is capable of showing scintillation points resulting from electron contacts. His problem was exactly reversed from that of Einstein's in the photoelectric phenomenon. Einstein (1905) started with the knowledge of a wave characteristic of light and wished to account for its seeming particle behavior; deBroglie (1940-2) began with the knowledge of particular matter and wished to explain seeming wavelike behavior. deBroglie concluded that for an electron with the mass M there would be associated a wave with the frequency which can be determined by the formula $hv = mc^2$. From this he was able to calculate the wave length of any given particle. We are faced with a situation in which parameters classically associated with waves can be used to calculate the values of parameters usually associated with particles, or vice versa. It is this dilemma which the principles of indeterminacy and complementarity clarify, and in so doing change the epistemological status of properties. How can it be that light behaves like a shower of particles and elementary particles behave like waves? How can seemingly contradictory properties such as wave length and energy both be descriptive of the same thing?

Heisenberg (1930) arrived at his principle of uncertainty by means of a thought experiment. He realized that in experiments such as those in which interference patterns were obtained from a scattering of elementary particles there was an inherent conceptual difficulty in which the classical language of optics and mechanics was being mixed together in a way simply not feasible in classical physics. In his thought experiment Heisenberg tried to trace the trajectory of an elementary particle in the same way trajectories are traced in classical mechanics.

We can extend his thought experiment to apply to the interference experiment already described. In describing the trajectory of a particle, two parameters are necessary, position and momentum. We can calculate the position of a particle from knowing the probabilities of its arrival at a particular point on the scintillation screen. deBroglie's equations allow us to calculate precisely the probabilities of such scintillations, and therefore through retrodiction we can establish where a particle must have been if it went through the slit and caused this particular scintillation. But if we wish, at the same time, to know the parameter momentum of that particle we must attach a device to the slit which is attached to the framework by an elastic connection; so that as particles go through the slit some of them will collide with our measuring instrument and give us an estimate of their velocity. If we do this we will find that we have destroyed the interference pattern on the scintillation screen. The loss of that interference pattern, however, makes the measure of position impossible. It may appear that this is unique to the situation, but physicists have come to realize that this is in fact an example of a principle which is true for all complementary parameters in quantum mechanics. The principle is simply that one can never obtain an accurate measure of both position and momentum at the same time. Stating the principle positively, we can say that the measuring of one of the pair to arbitrary accuracy necessarily reduces the possibility of equally accurate measurement of the other of the pair. (Frank, 1954; Silva, Lochak, 1969)

The reader must be warned against a very common misinterpretation since we are all subject to what Putnam (1965) calls the assumption of "no disturbance." "The measurement does not disturb the observable measured--i.e., the observable has almost the same value at an instant before the measurement as it does at the moment the measurement is taken." (Putnam, 1965, p. 83)

In making the assumption of no disturbance we are led to believe that before measuring either position or momentum, a particle existed which had both position and momentum, but that by virtue of taking one of the measurements we disrupted the other. This is a metaphysical position which by virtue of the principle of indeterminacy could never be subjected to an empirical test. Heisenberg (1958B) makes very clear that for an elementary particle to possess both position and momentum, at equal degrees of accuracy, is a mathematical as well as a physical impossibility. The formalism of wave mechanics does not allow for the description of a packed wave, which is what high accuracy in both parameters would imply.

Bohr's (1962) principle of complementarity is an extension of the logic of indeterminacy.[1] It maintains that seemingly contradictory properties may be ascribed to the same system because they are experimentally exclusive of one another. Wave and particle characteristics which are intuitively contradictory are in fact complementary, since no experiment can be devised which allows you to measure a wave characteristic accurately which does not, at the same time, preclude the accurate measurement of a particle characteristic. Jammer (1966) makes clear the relationship between this notion of complementarity and Heisenberg's principle of indeterminacy.

> For the interaction between the object of observation and the agency of observation--which interaction in accordance with the quantum postulate cannot be neglected as it could in classical physics--makes it

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[1] Jammer (1966) offers documentary evidence to counter the popular notion that Bohr took directly from Heisenberg's principle, merely extending his logic. Bohr did not have the interest in formalism that Heisenberg had and the historical evidence suggests that Bohr had considered complementarity as a solution to the dual nature of light before he became familiar with Heisenberg's indeterminacy. Nevertheless, it is still legitimate to say that indeterminacy and complementarity are logically the same principle. Whether it is expressed by saying that indeterminacy is a logical deduction from complementarity, or that complementarity was generalized from indeterminacy is a moot point. We will see in the discussion that Jammer promotes the idea that Bohr got his notion of complementarity from William James.

> impossible to separate sharply the behavior of the atomic system from the effect on the measuring instrument whose behavior must be expressed in classical terms. By combining the atomic system with different classically descriptable devices one may measure complementary variables, and by expressing the results of these measurements in classical terms one may describe an atomic system in terms of complementary classical pictures. (Jammer, 1966, p. 348)

It is important to note what happens to the notion of physical property, according to Bohr and Heisenberg. Which of the two contradictory properties will be observed depends on the instruments and the decisions of the observer as much as on the system being observed. There are similarities between what we have said here about properties of experimentally observed physical systems and what we said before about the qualities experienced in the perceptual experiments of Ames and Kohler. In all of these cases it is impossible to separate factors in the observer from factors in the observed. Another interesting similarity between the aforementioned observations is that they all seem to be cases of complementarity. The simplest example for intuitive appreciation is the case of the reversible figure. In each of these cases we are led by common sense after experiencing the complementary properties; in the case of the goblet and the profiles we ask, "But what is it really?" We must be careful not to confuse common sense with scientific procedures, for as Putnam (1965) says about the Copenhagen interpretation of quantum mechanics, experimental evidence must be preferred to intuitive conviction.

CHAPTER VI

THE EFFECTS OF THE ANOMALIES ON THE ASSUMPTION OF OBJECTIVITY

One way to evaluate the significance of the anomalies presented in the previous chapter is to review the five models presented in Chapter IV in order to assess how each deals with the anomalies. An alternative, and hopefully more economical approach, is to deal with the notion of objectivity directly. We have seen that there are two features of objectivity; these are independence and certainty. Chapter IV presented alternative ways which have been proposed to establish independence and certainty. In this section we will take each of these independently and see whether the anomalies give us any reason for preferring any of these over the others.

The most important method for establishing the independence of knowledge has been to establish an epistemic root, wherein certain elements of knowledge have been specified to be independent. These roots or independent elements are usually called facts, and a fact is established by means of objective observation. Not all proponents of independent knowledge wish to rely upon observed facts for establishing credence. There are some exceptions among philosophers of science; most notable among these is Karl Popper, (1959) who has suggested an alternative to a reliance on objective perception for establishing independence. Before we can deal with such alternative suggestions we must review the several methods of establishing factuality through observation. As was indicated in Chapter IV there is a great deal of agreement concerning the proposition that there are facts, but not as much agreement as to how it can be established that any particular proposition is a fact. There also is agreement concerning the notion that facts are established through observation, but the exact meaning of observation differs for each of the five models presented in Chapter IV. Each seeks to establish an indubitable, a primitive of experience about whose independence there can be no doubt.

Lockean realism, which divides experience into primary and secondary qualities, is not an epistemology which is taken seriously by philosophers and psychologists today; but in an alternative form it is still a viable epistemology for many practicing scientists. As was pointed out in Chapter IV, one of the most appealing aspects of the primary/secondary distinction, is that it parallels the often made distinction between quantifiable and nonquantifiable variables. According to this distinction scientific progress is made by extending quantitative techniques, since it is by quantifying variables that we establish their independence from the observer. In other words, this form of realism maintains that we establish the primitives of experience by their quantifiability.

Using quantifiability as a criterion of independence is based on the common sense assumption that our number system is either neutral or empirical. There is growing agreement that such number systems are in fact relative conventions. It was demonstrated in Chapter V that the geometry one uses is neither neutral nor empirical and that the choice of a geometry makes a difference in the description obtained. Advancements in our description are therefore not dependent on improvements in the realm of observation alone. Non-Euclidean geometry gave Einstein (Polanyi, 1967-68) a new way of looking, not a better look at the universe. The differences in the descriptions obtained, using Euclidean and non-Euclidean geometries, cannot be ascribed to empirical progress alone.

The discussion of the role of patterning in Chapter V suggests another difficulty in relying on quantifiability as a criterion of independence. The variables selected to be quantified are not selected by standards which are inherent in the system of quantification. Even if it is agreed that Aristotle's and Galileo's descriptions of a swinging body are equally objective, because they are both quantifiable, it would be impossible to maintain that the variables quantified are independent of the observer's conceptual system.

As we have seen, logical positivists subscribe to two fundamental tenets. The first of these is that there are only two kinds of knowledge, analytic and synthetic, or formal and empirical. This distinction is one which both phenomenalists and physicalists agree to. The second fundamental tenet of logical positivism is the belief in a principle of verification. Phenomenalists and physicalists are not in agreement on the exact nature of verification. We shall, therefore, deal with the first of the two fundamental tenets and leave the discussion of verification for later. Each of the three anomalies casts doubt on our ability to separate the formal from the empirical aspects of knowledge. Philosophical criticisms of the analytic-synthetic distinction based on logical and linguistic analysis are available from Quine (1953) and Putnam (1962). Putnam (1962) tells us that among the most convincing evidence for the logical empirical distinction is the abundance of everyday examples. Statements such as "Bachelors are all unmarried males." are clearcut instances of analytic statements, which sharply contrast with statements of the type "There is a book on the table." Quine (1953) and Putnam (1962) give linguistic and logical arguments why such examples are not correct, despite their intuitive appeal. The three anomalies presented in Chapter V can be seen as counter examples to the belief in the analytic/synthetic distinction.

In the first anomaly we demonstrated that the description of a space depends as much on the geometry and coordinative measurement conventions as it does on observations. The two descriptions of a curvilinear 3 dimensional Euclidean space and the 2 dimensional non-Euclidean space of positive curvature, depicted in Figures 4 and 5 on pp. 141 and 142 could be complementary descriptions of the same real space given that the instrumentation of the non-Euclidean coordinative definition of measurement were physically available. The reason physicists can choose between one or another geometry is because they elect to keep coordinative definitions constant. Such coordinative definitions are arbitrary; it is not possible to establish them empirically. This means that the selection between geometries is a logical impossibility, not a technical impossibility.

> There is an impossibility of making measurements which is due to the limitation of our technical means; I shall call it TECHNICAL IMPOSSIBILITY. In addition, there is a LOGICAL IMPOSSIBILITY of measurement. Even if we had a perfect experimental technique, we should not be able to avoid this logical impossibility. It is logically impossible to determine whether the standard meter in Paris is really a meter. The highest refinement of our geodetic instruments does not teach us anything about this problem, because the meter cannot be defined in absolute terms. This is the reason why the measuring rod in Paris is called the definition of a meter. It is arbitrarily defined as the unit, and the question whether it really represents this unit has lost its meaning. The same considerations hold for a comparison of units at distant places. Here we are not dealing with technical limitations, but with a logical impossibility. The impossibility of a determination of the shape of a surface, if universal forces are admitted, is not due to a deficiency of our instruments, BUT IS THE CONSEQUENCE OF AN UNPRECISE QUESTION. The question concerning the shape of the surface has not precise formulation, unless it is preceded by a coordinative definition of congruence. What is to be understood by "the shape of a real surface"? Whatever experiments and measurements I make, they will never furnish a unique indication of the shape of the surface. If universal forces are admitted, the measurements may be interpreted in such a way that many different shapes of surfaces are compatible with the same observations. There is one definition which closes the logical gap and tells us which interpretations of our observations must be eliminated: this task is preformed by the coordinative definition. It gives a precise meaning to the question of the shape of the real surface and makes a unique answer possible, just as a question about length has a unique meaning only when the unit of measurement is given. It is not a technical failure that prevents us from determining the shape of a surface without a coordinative definition of congruence, but a logical impossibility that has nothing to do with the limitations of human abilities. (Reichenbach, 1958, pp. 28-29)

It is often said that the differences between Euclidean and non-Euclidean descriptions of the same space are not significant differences, but are of the same order as the differences between meters and feet. Within the science of physics the debate over this question is still not resolved. Those who maintain that geometries are mere conventions are charged with the responsibility of making available translation rules which will demonstrate that the differences between geometries do not make a difference in physical geometry. As it is, in order to maintain a single set of coordinated definitions of measurement, it was necessary for Einstein (Reichenbach, 1958) to use non-Euclidean geometry in his Relativity description of gravity. To extend the Euclidean geometry of Newton's universe would have required a change in our definitions of measurement and this would have presented complications far more disruptive than a change in geometry.

Leaving the problem of physical geometry for the physicist to resolve, we are still faced with the epistemological consequences of the fact that Euclidean descriptions are not unique. The most important of these consequences is that the Euclidean physicalistic can no longer be relied upon as a standard for realistic description. The fact that such Euclidean descriptions are not unique physicalistic descriptions means that there is no justification for giving them primacy over phenomenological descriptions since the latter may come to encompass all of the former.

The anomaly of the pendulum versus the body in constrained fall is also an example of our inability to draw the line between facts and ideas. If we maintain that Aristotle's description of a body in constrained fall and Galileo's description of a pendulum are both factual, then we are forced to admit that some factual descriptions are somehow better than others, even though they both maintain the same degree of objectivity.

If, on the other hand, we wish to draw the line between facts and ideas at an even lower point we may choose to say that the terms "pendulum" and "body in constrained fall" are ideas which represent interpretations of the facts. Accordingly, the facts are viewed as the measurements taken. The obvious difficulty with this account of the matter is that the two ideas are not dealing with the same facts.

If we followed this logic to its natural conclusion we will still have to conclude that "body in constrained fall" is probably the best interpretation to refer jointly to the parameters in which Aristotle was interested. Consequently, we still do not have a basis for preferring one description over the other.

The anomaly of the pendulum not only demonstrates the difficulty of drawing the line between analytic and synthetic, but it also violates the other basic tenet of positivism, which is that factuality is established via a verification principle. According to the positivist's verification principle the meaning of a proposition is discoverable in the method of its verification. The method of verification for either the Galilean or Aristotelian descriptions involved taking certain measurements, but it is not correct to say that these measurements alone define the meaning of "pendulum" or "body in contrained fall". In order to take the right measurements in the right order, one first must have the idea that selects the relevant parameters. When looking at a swinging body, no number of observations of its weight, the height to which it had been raised, and the time required for it to achieve rest, would ever lead to the conclusion that what is being observed is a pendulum. The difficulty in reducing the verification or perception to a set of physically observable operations, is that the ideas which select and organize the operations are not themselves inherent in those operations. It follows that verification principles can only be used to account for confirmation, they cannot be used to account for discovery or learning. There are more difficulties with the verification principles than the fact that they cannot account for the context of discovery. These other difficulties differ for physicalists and phenomenalists, and, therefore, will be considered later. At this point we wish to limit our comments in such a way that they apply to all positivists, whether they be physicalists or phenomenalists.

Nowhere is the distinction between formal and empirical more clearly violated than in the case of microphysical measurement. The instrument or apparatus in an experiment is a physical representation (a coordinative definition) of the experimenter's formalism, just as is a ruler in the simple measurement situation. The Copenhagen interpretation holds that in principle there is always interaction between instrument and system in the quantum physical situation. Whether one measures a wave or particle characteristic depends on the instrument one uses. Complementarity further indicates that if you choose to actualize a wave characteristic you thereby automatically eliminate the possibility of actualizing a particle characteristic. They are mutually exclusive, yet complementary descriptions.

The data in a quantum experiment is in principle different from that which we usually think of as data. Data traditionally are attributable to the system under investigation. In the quantum situation, the data refer to the relationship between the system and the instrument. A change in instruments means a change in the knowledge of the system. This means that you cannot separate discussion of the phenomena from discussion of the instruments by which they are studied. The phenomena have no specifiable characteristics of their own, but can be viewed only as they relate to other systems.

The three anomalies are also directly relevant to the two prevalent points of view within positivism about the nature of the primitives of observation and perception. The phenomenalists are in agreement that an observer must be present so that there can be sense data, but any normal observer with the right training will be able to verify a scientific observation. This belief is obviously based on the idea that sensation, if not perception, is the same for all "normal" human beings. The first difficulty with this point of view rests in the assumption that someplace in the complex perceptual apparatus of man there are some fundamental qualities which stand in a one to one relation or perfect correspondence with their source of stimulation. The search for such elements of consciousness has long been abandoned by psychology, but the belief in their existence is still strong, particularly among theorists who distinguish between sensation and perception. The phenomena most anomalous to this point of view is that which was given to us in Kohler's (1964) experiment with prismatic lenses. Frequently, in the writings of phenomenalists, "patches of color" are used as examples of sense data. Kohler's (1964) experiment demonstrates that color cannot be used as an example of sense data, since the experience of it can arise from more than one set of physical conditions. It does not depend exclusively upon a perfect correspondence to the frequency of light arriving at the eye. Phenomenalists attempt to refute the argument from hallucinations by holding that they are ideographic peculiarities. In Kohler's (1964) experiment, as in Ames' demonstrations, (1960) we are not dealing with the perceptions of madmen, but clear cut demonstrations that sensation is influenced by experiential and conceptual factors which may be shared by all the individuals in a culture, thereby influencing what each of them experiences in the same way without being attributable to a direct correspondence to the source of stimulation.

The case of Euclidean geometry is a good example of a culturally acquired set of descriptive biases, which is so widespread in Western culture that many thinkers, such as

Kant (1963), attributed them to man's native intelligence. The ability to visualize or in other ways rely on psychological capacities in dealing with Euclidean geometry, means that Euclidean assumptions are to most of us perceptual mechanisms, operating at the same level as Kohler's diffraction mechanism in the prismatic lens experiment.

If we are prepared to admit that the experience of sense data, such as color, can be influenced by past experience, then how do we evaluate sense data with respect to the criterion of independence? In the first place, phenomenalists were never committed to the notion that the sense data were independent of the observer. They were merely committed to the notion of intersubjective reliability. As was pointed out in the last paragraph, this implies that similar stimuli give rise to similar sensations, even though phenomenalists need not commit themselves to this metaphysical position.

If this option is excluded and phenomenalism is defended in terms of intersubjective agreement, then we have the difficulty of defining what is meant by "normal observer". When Kohler's (1964), colleague, looking through the prismatic lenses, saw the violet end of the spectrum everywhere Kohler (1964) saw the red end, do we say that one of these men is a normal observer, and the other not? Or perhaps, we will maintain that neither observer was normal. Either of these possibilities seem defensible, but each has the consequence of placing cultural limitations or sociological brackets around our scientific propositions. This seems to run counter to the initial reasons for resorting to sense data as the indubitable.

In Chapter IV we pointed out that, for the most part, logical empiricists abandoned sense data as the primitive of knowledge in favor of physical objects. The physicalists do not avoid many of the problems of logical positivism by going from sense data to physical objects. In the first place, physicalists tend to assume that there is no need to explain what a physical object is; but the case of the swinging stone demonstrates how the same physical object can be described physicalistically, in more than one way, with very different outcomes. We have also shown that there is no sense in appealing to the distinction between measurements and interpretations of measurements, since it is the interpretation that governs which measurments will be taken. Furthermore, the same logic can be carried one step further to demonstrate that each measurement is based on certain complexes of assumptions and coordinative definitions, as was demonstrated in our discussion of geometry. The case of the pendulum is particularly relevant to this point since it later became part of a coordinative definition of time. The interplay between ideas and instruments is striking when we think of Galileo, noting the regularity between his own pulse and time per swing of the pendulum. The evolution of the concept of time was such that pendulum clocks would someday be used to establish the regularity of the human pulse.

We have seen that another suggested primitive was an intuitive configuration. Kant (1963), who was the father of the intuitive configuratist's tradition, believed that the basis for such intuition was a priori synthetic knowledge. Among the elements of Kant's (1963) a priori synthetic knowledge was Euclidean geometry. By a priori Kant (1963) meant that geometry, as well as other parts of the a priori synthetic were not learned from experience, but were part of man's innate abilities. For Kant the a priori synthetic ordered and organized sensation into perception; therefore, in order to identify the a priori synthetic, experience was necessary, since its influence could only be detected in experience. By synthetic, Kant meant that this intuitive innate capacity told us something about the real world in much the same way that geometrical axioms led to specific knowledge about real triangles in the physical world.

The discovery and elaboration of non-Euclidean geometry is a serious setback to Kant's (1963) notion of the a priori synthetic, particularly if it is not possible to establish any logical or physical basis for preferring Euclidean geometries to non-Euclidean geometries. Because many neo-Kantians maintained that the intuitive visualizing of Euclidean geometry demonstrated its claim to primacy, Poincaré (1953) and Reichenbach (1958) were very concerned to demonstrate that non-Euclidean relationships could be visualized by those who are familiar enough with the consequences of its axioms.

Kant's a priori synthetic asserted that the origin of geometry was not empirical, but that geometrical knowledge was synthetic. This assertion means that empirical techniques would allow us to demonstrate that the geometry is in fact a physical geometry. There are in these relationships beliefs which are similar to those held by the gestalt psychologists. According to the gestalt psychologists, the principles of organization were innate and immediate, but according to the principle of isomorphism they were also a phenomenon influencing the natural environment. For this reason, the same principles of organization influenced the stimulus (physical gestalten), the neurological processes (physiological gestalten), and perception (phenomenological gestalten).

> As THINGS, phenomenal objects have definite properties. Apart from their resistance to distortion we have encountered their impenetrability, and their inertia, according to which bigger objects move more slowly than smaller ones. This correspondence between phenomenal and real things is, according to our theory, not primarily a matter of experience--although we do not deny that experience may influence thing properties--but the direct result of organization. Psychophysically, the process distributions which correspond to perceived things must in several respects be similar to physical things, and therefore we must, on the basis of isomorphism, conclude that behavioral things have autochthonously characteristics similar to real things. Here, as in so many other fields, a purely empiristic theory is bound to run in a vicious circle. Our theory avoids not only this but at the same time a Kantian apriorism. (Koffka, 1935, p. 305)

The experiments of Ames (1960) and Kohler (1964) suggest that the principles of organization are not innate or natural, but dependent upon experience. The discovery of perceptual learning, as exemplified in the adaptation phenomena in Kohler's (1964) experiment, and the cultural dependence of assumptive worlds in Allport's (1957) experiment, have effects on the gestalt principle of isomorphism that are similar to those that the discovery of non-Euclidean geometry had on Kantian a priorism. The belief in a priorism and isomorphism were abandoned, but the belief in intuitive spatial organization and perceptual insight remained.

> Once the principle was established that the forms of perception--and by extension the forms of belief--were mind-dependent and not determined by the intrinsic character of things perceived, it was only necessary to add that these forms were themselves a function of psycho- and socio-dynamic development, and one went from Kantianism to all the verities of subjectivism, relativism, and cultural determinism that have at once plagued and enriched modern philosophical thought. (Hawkins, 1964, pp. 48-49)

Configurationism as a philosophy of science was promulgated by Köhler (1967) as a method to achieve superior knowledge, but Köhler (1967) explicitly accepted that physical gestalten contained their own pattern of organization. This pattern was not imposed upon the phenomena. Using this approach, Köhler (1967) would have a criterion for preferring certain physical descriptions over others, since it was possible to give accurate descriptions that lacked insight into the physical gestalt. Such insightful descriptions could be distinguished by virtue of their internal harmony and by virtue of their consistency with the rest of acquired knowledge.

More recent configurationist philosophers of science, such as Hanson (1958) and Polanyi, (1967-68, 1958) have abandoned the principle of isomorphism, viewing the patterning as the influence of the conceptual on the observational. These patterns are seen as evidence of conceptual creativity. As we pointed out previously, Hanson (1958) and Polanyi (1967-68, 1958) both subscribe to the notion that only the details subsumed under the pattern are subject to empirical test. Polanyi (1967-68, 1958) in particular maintains that patterns in the history of science can only be evaluated on the basis of consistency and coherence criteria. Polanyi (1967-68, 1958) has collected examples of scientifically sound research which have been ignored or denied by various disciplines because their results do not fit in to the prevailing paradigm.[1]

The configurationist philosophers of science tend to agree that the meaning of consistency and coherence is to be defined by the standards by which an explanation is accepted. If this is the case then there is a sense in which the Copenhagen interpretation is an indication of a change in our fundamental notions of consistency and coherence. The principle of complementarity in fact elevates inconsistency to the level of understanding. Certainly in classical physics the properties of objects must be stable in space and time, and cannot be inconsistent with one another. According to complementarity, properties only exist when they are being observed, and therefore need not be consistent with one

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[1] One example, familiar to psychologists, is the experimental work on ESP by Rhine. (Polanyi, 1958)

another. Polanyi (1958) and Hanson (1958) do not tell us how such shifts in our notions of coherence and consistency can take place.

The configurationists have forced the recognition of the phenomena of patterning in both the realms of perception and observation. It is impossible to ignore the fact that our knowledge contains these somehow irreducible units with stable organization and impelling perceptibility. It is also impossible to ignore that through his creative activity man has been able to augment the number of such units. Despite this contribution, there are some very important omissions in the configurationist's account of these irreducible units. A nativistic a priorism has lost its credibility, and because of this loss the mechanism of intuition inevitably gets replaced by sociological and psychological compulsions, which run counter to Kantian rationalism and gestalt naturalism.

We have reviewed four candidates for the position of independent indubitable: primary qualities, sense data, physical objects, and configurations. We now turn to an assessment of the neo-realist candidate--information. Information was defined in Chapter IV as an invariant set of relationships existing among a set of measurements.

In Chapter IV we concluded our discussion of neo-realism by remarking that there were conceptual differences between the kind of objectivity implied by the independence criteria and the kind of objectivity which is promoted by Hawkins (1964) and Gibson (1966). Despite Hawkins' (1964) claim that the message is entirely independent of the receiver, and Gibson's (1966) claim that his is a theory of strict psychophysical correspondence, it was pointed out that both men resort to the criterionof economic description and to a hierarchical arrangement of factuality. In their view, the use of the criterion of economy in no way violates the independence of the facts, since economy merely refers to the most reasonable arrangement of all the incoming information and it can be defined in terms which are entirely measurable in that information. Even if we assume their own prediliction for the jargon of information theory, such a view of the criterion of economy is only justified if the set, to which the incoming information belongs, is known. The most clearcut demonstration of this set dependence is the contrasting descriptions which result from using different geometries. Within either geometry, economy criteria can easily be shown to rest entirely upon observable parameters; however, for this to be the case, the geometry and the conventions of measurement must be known in advance.

Neo-realist philosophers of science, such as Hawkins (1964), do not accept the relativity of geometry. According to their point of view, the apparent conventionalism, is due only to the limitations of contemporary knowledge.

> The view set forth here is that physical geometry is neither a system of self-evident truths legislating for the universe, as some philosophers may have thought, nor an explication of mind-dependent forms of perception, independent, as such, of any empirical tests. It is, rather, a part of our empirically tested and testable knowledge of nature; but it has, in the description of nature, a special role and a special

> kind of priority. Spatial properties are involved in any demonstrative reference to physical realities, and Kant was right in saying that we do not identify phenomena as physical except in consequence of their spatial ordering. We do not first identify things as physical and only afterward find that they have spatial location. But metric geometry cannot be derived, as Kant supposed, from our "A PRIORI forms of intuition." Its theorems can be tested with high precision under the given conventions of measurement, and these conventions themselves rest upon judgments that the growth of knowledge may confirm or cast into doubt. (Hawkins, 1964, pp. 55-56)

Reichenbach (1958) and Hawkins (1964) disagree concerning the nature of the conventions of measurement. According to Reichenbach (1958), these are coordinative definitions; therefore, it is logically impossible to confirm them empirically. Hawkins (1964), on the other hand, maintains that empirical knowledge can support or cast into doubt the conventions of measurement. The analysis presented here assumes that Reichenbach (1958) is correct, at least with respect to physical geometry; but even if Hawkins (1964) is correct, the points made with respect to the use of Euclidean physicalistic descriptions as criteria of reality in psychological experiments are still valid. Neo-realists are merely asserting that someday we may be able to confirm or disprove our conventions of measurement. Until that day comes to pass the experimental psychologist has no criteria for preferring one physicalistic description to another.

Gibson's (1966) analysis of illusory perception nevertheless rests upon the tacit acceptance of Euclidean assumptions. With respect to simple geometrical illusions, such as the one in Figure 2, Gibson (1966) maintains that the stimulus contains conflicting sets of information. His analysis of simple geometrical illusions is essentially the same as that which he gives for reversible figures. About the goblet figure Gibson says,

> In the absence of texture and parallax, the information for edge-depth or superposition has been arranged to specify two opposite directions of depth. There are two counterbalanced values of stimulus information in the same "stimulus". The perception is equivocal because what comes to the eye is equivocal. (Gibson, 1966, p. 247)

Equivocation over whether this is a goblet or a pair of profiles might be due to the fact that the stimulus is equivocal. This does not clarify how equivocal information can be considered independent of the observer, since not all reversible figures are spontaneous. The case of the pendulum is a good example of two sets of equivocal information which do not reverse spontaneously. Before Galileo, no one saw the pendulum set of information values and, after Galileo, no one accepted the older Aristotelian description. In the case of the geometrical illusions in Figures 1 and 2, Gibson (1966) would maintain that the phenomenological stimulus is at a different level of sensitivity than the geometrical level. The lines appear curved because the stimulus information produced by the combination of lines is different than the stimulus information produced by two lines alone. According to Gibson's definition, however, every variation in experience must be matched to a measureable variation in the stimulus. On this score the non-Euclidean description of Figures 1 and 2, given in Chapter V, lends support to Gibson's (1966) hypothesis.

The Kohler (1964) and Ames (1960) experiments lend support to Gibson's (1966) notion that invariant relations are what the individual perceives. They contradict the idea that these relationships exist in the stimulus independently of the observer. Ames' (1960) reliance on the principle of equivalent configurations demonstrates very clearly how certain invariances, for example between the trapezoid and the rectangles viewed from certain perspectives, give rise to certain invariant sensations. The Allport (1957) cross cultural study suggests that the perception of these invariances relies upon past experience. Kohler's (1964) experiment shows that more than one set of information variables can give rise to color perception, depending on the perceiver's set. We must conclude, therefore, that terms such as invariance and information are only meaningful within a larger context. That larger context refers to the assumptions and axioms within which the incoming bits of information are incorporated. This is why in Gibson's (1966) test we measure the variations in experience first, and then see if we can find corresponding measurements in the physical realm.

If we maintain the order of presentation used in Chapter IV, we should next consider the transactional model. The transactional model does not fit into the present scheme since it does not promote a candidate for the position of "indubitable". We will, therefore, consider the transactional model as one which suggests a method of evaluating knowledge that is an alternative to those which rest upon indubitable observations or facts. As was suggested earlier, Karl Popper (1959) is a philosopher who wishes to avoid the psychologisms of the type presented in Chapter V. He therefore does not wish to rely upon confirmatory observations to evaluate scientific hypotheses. He is a logical positivist who wishes to substitute a principle of falsification for the usual principle of verification. According to Popper, (1959) the only acceptable scientific propositions are those which are capable of falsification. We can trust our observational capacities when it comes to disconfirmatory evidence. In evaluating a proposition we select the hypothesis which has the greatest possibility of falsification and believe in it in proportion to our inability to disprove it.

This dissertation is based on the idea that psychologisms are unavoidable in scientific verification. Why not, therefore, accept the falsification solution? The answer is a psychological one. Essentially it is that scientific propositions of the most fundamental type, which seem to be at the crossroad of breakthroughs, are simple concepts which channelize psychological processes in a positive manner such that they tell you what to look for. By simple concepts we mean the creation of new entities, such as Galileo's pendulum and Kepler's ellipse. There is nothing inherent in these concepts to suggest the relevant falsifiers. The disconfirmatory evidence comes only after an alternative concept is available to generate this evidence. Once more we turn to the example of the goblet profiles; there is nothing in either of these terms to suggest the alternative perspective. If we follow the "if, then" logic, which is associated with disconfirmation as well as confirmation, we can never go from one of these terms to those parts of the other term that would really give us good counterevidence. For example, if Popper (1959) is correct we should be able to say if this is not a goblet then it will have a brow or nose. In essence then we agree that disconfirmatory evidence is less subject to the psychologisms that Popper (1959) wishes to avoid, but that it is these same psychological factors which make disconfirmatory evidence so rare, unless there is a competing theory.

Finally, we examine the method which the transactional model will use to cope with the difficulties presented by the three anomalies. As we said previously, transactionalism does not accept the idea of an observational indubitable. Instead it views both observation and perception as relativistic processes. Perceptions or observations are not evaluated in terms of their correspondence to the real world, and theories and ideas are not evaluated in terms of their correspondence to known facts; rather, perceptions and assumptions are evaluated in terms of their functional utility. This criterion implies that there must be a goal, an end which man wishes to accomplish which can be used to evaluate percepts and ideas. In all instances of descriptive complementarity, the description preferred depends upon what we wish to accomplish. In addition to the tolerance for multiple descriptions, which is built in to this approach, there is the implication that no description is ever complete. This is so because perceptions or descriptions arise out of the relationship between assumptions and givens, and they are evaluated in terms of purposes. The givens limit what will be perceived only insofar as certain assumptions are disproved, but we can never anticipate from this knowledge what future assumptions will be confirmed. Only man's ability to come up with new goals and assumptions can ever give us a measure of the limits.

The Bohr-Heisenberg interpretation of quantum mechanics has many characteristics in common with Ames' view of perception. Both emphasize that what is observed is a function of what the observer does as well as the system being observed. Heisenberg (1958A, B) also concurs with Ames (1960) that a future experiment may reveal other observations with the same system, and that these new revelations will depend on the goals the experimenter brings to the situation.

> His final conclusion that the probability formulae of quantum mechanics include a reference not only to the kind of experiment which prepared the state, but also to the kind of experiment which is ultimately envisaged. By this he means that the development of the wave function does not describe a process occurring independently of observation, but that it represents rather a set of incomplete potentialities which need to be completed by a future action of measurement. (Heelan, 1965, p. 43)

The transactionalists differ from all the other positions in one more important characteristic. This has to do with the feature of certainty. Epistemological positions which promote the notion of an indubitable admit the possibility of error in description. The probability of error increases with the complexity of the descriptive task. In this sense, the distinction between observation and perception is on a continuum of probability. Perception has a higher probability of being in error than does observation.

In contrast to this point of view the transactionalist indeterminacy position maintains that probability is inherent in description and not a measure of technical proficiency. The probabilities in the quantum mechanical situation are irreducible. They express our knowledge in terms of the likelihood that a particle will appear at a given point under a given set of initial conditions. The similarity between this view of probability and that expressed by Ittleson (1960) in Chapter V is too striking to be accidental. In neither case does the acceptance of probability as an inherent feature of our knowledge mean ambiguity. If we say that the goblet and the profile are descriptions of equal probability, given certain initial conditions, it means that when one of the two is being viewed it is an all-or-nothing perception. Our knowledge can only refer to a series of events, not to the absolute outcome of one event. In classical terms this is an indication that more work is needed, since probability is a measure of ignorance, but in quantum mechanical terms, our knowledge is basic even though it cannot make predictions about the position and momentum of a particular particle. Certainty and independence, therefore, are criteria of objectivity only in a model which insists on absolute indubitables.

We have repeatedly emphasized the significance of the notion of indubitables in both fields of perception and scientific epistemologies. It is therefore not sufficient to say that transactionalists abandon the notion of indubitables in their account of perception and description. In beginning our examination of the transactional substitute, we mark an important transition in the course of the inquiry. For just as it is not sufficient to show counterevidence to the existence of indubitables, it is not sufficient to collect counterevidence to the assumption of objectivity. What remains to be done is the exposition of a viable alternative. The first task in arriving at this alternative involves getting an understanding of what transactionalists substitute for the notion of indubitables. We will refer to this transactional substitute as "devices". It should be pointed out that those whom we are calling transactionalists are not self-avowed members of any school, and in the case of the towering figures--Ames and Heisenberg--there was not even mutual recognition of their epistemological similarities. Nevertheless, the two men have come to similar conclusions concerning the process by which we acquire knowledge. Let us now turn to an analysis of the similarities in their understanding of observation and perception that led us to coin the term "device".

It is interesting to note some of the similarities among Ames' assumptions, Bohr and Heisenberg's instruments, and Kohler's central aftereffects. In the first place, each is a device which the knower has come to use on the basis of prior successful experience. Another expression which could be substitued for device would be heuristic function. The choice of the word device is clearly appropriate to apply to Bohr and Heisenberg's instrument. It is obvious that apparatus such as thermometers and interferometers are mechanical devices which are not themselves the object of knowledge, but are standards used to establish the existence of a property in the object of inquiry. Such devices play a unique role in connecting cognition to perception. As we saw on pages 135-136, 181-182, instruments of measurement are coordinative definitions and, as such, contain a blend of analytic and synthetic elements. It is in these instruments that the prescriptive elements of knowledge, such as our numerical and logical systems, fuse with the physical elements of our knowledge such as rigid bodies used in the construction of rulers. The ruler is a good example. The intervals represented by the numerical markings on its surface are determined analytically, and on the basis of the scale we wish to use. The choice of a rigid, stable material is not arbitrary but, based on our experience with this material, we conclude that it will satisfy the prescriptive rules of the numerical system.

Coordinative definitions, in many respects, represent the state of human knowledge at any given point and can be applied to make contemporary experience consistent with past experience. These instruments also have been called operational definitions. (Bridgman, 1949). It has become recognized in recent years that operational definitions have both an analytic and a synthetic element, and that the interaction of these two elements in a single measurement operation means that we are bringing past knowledge to bear on present experience. The combined analytic and synthetic status of these elements means that in some ways they act as prescriptive rules and in other ways as empirical facts. With respect to any particular experimental situation, the coordinately defined instruments are prescriptive rules which are not questioned. In the long run, however, experimental evidence may come to change the coordinative definition.

The idea that improvement in the realm of observation is the mark of scientific progress tends to overshadow the most important fact about how such improvements of observation comes about. There is the implicit assumption that the changes in theory result from an improvement in the precision of our observation. Agassi (1968) has reviewed this question carefully and concludes that the reverse is actually true. As long as we are working within a particular theory the operational definitions define both the variables to be measured and the level of precision which is desirable. It is not until a competing theory arises that changes in the operational definitions may require that scientists make even more precise observations.

> Thus, when an observer highly increases the degree of accuracy of observation while referring only to one current theory, his observation may be useless (sic) if the fit is judged good enough, or useful if he calls the fit into question--quite in accord with the above observation of the relativity of degrees of

> accuracy. For instance, when the atomic weight of oxygen was deemed close enough to 16 there was neither any point in increasing the accuracy of the measurement of its deviation from 16 to further decimals nor even any point in isolating its isotopes to find the more precise atomic weight of the predominant isotope. Things changed drastically, of course, when 16 was not good enough any more because nuclear physics should yeild the exact deviation from this weight (when the mass of a proton is taken to be 1). (Agassi, 1968, pp. 289-290)

If Agassi's analysis is correct, when viewed historically an increase in precision really is a change in the theoretical realm, not in the factual realm. For example, we could say that an increase in our precision of observation of pendulum clocks allows us to decide in favor of Newton's ($\underline{a = const\ 1/r^2}$) over Galileo's ($\underline{a = g = const}$). This is so because, according to Newton's formula, pendulum clocks at various altitudes should, over long periods of time, become discrepant with one another, while according to Galileo's formula they should not. To attribute the resolution of this problem to the more precise observation of the behavior of pendulum clocks is misleading because the competing operational definitions must come before the observations can be made. Accordingly, Agassi (1968) argues that the increased precision is a result of a change in the operational definitions, and not an initiator of that change. We do not change our notions of acceleration because we observe the discrepancy between the clocks; rather it is instead the other way around. Once a decision is made about which is the preferred operational definition, then it dominates the construction of measurement instrumentation with respect to future observations.

The application of the term "device" to Ames' assumptions and Kohler's central aftereffects may not seem as clear as an application to Bohr's and Heisenberg's instruments. Ames' assumptions and Kohler's central aftereffects are a means of bringing past experience to bear upon immediate perception. But applying the notion of device to these particular situations brings an additional insight which carries our understanding beyond the traditional explanation of these perceptual effects. For the most part, learning theorists have tried to subsume Ames' "assumptions" and Kohler's "central aftereffects" under the concept of perceptual set. A perceptual set predisposes the organism to have certain experiences. The precise nature of this predisposition is a function of past experience with that particular set of stimuli. Sets, therefore, are exclusively empirical in origin.

If we substitute for this notion of perceptual set the concept of device, as delineated above, we gain the additional insight that there is an analytic or prescriptive aspect or, in psychological jargon, a cognitive aspect, at work in producing these perceptual effects. How else could we possibly explain the functioning of these devices in situations in which the perceiver has had no experience, or in situations where the bulk of his experience runs counter to the effect of the device? In the case of Ames' assumption of rectangularity, for example, the bulk of the subject's optical experience runs counter to the effect of the assumption; and yet the assumption persists in having its effect in all of its relevant situations. If the device was based entirely upon descriptive experience, then we should be set to see trapezoidal windows, not rectangular ones. The assumption of rectangularity is not based on a large number of sensory experiences of rectangular windows, but on the cognition that windows are rectangular. If this device were based entirely upon sensory learning, then it would work in exactly the opposite direction. That is, since the majority of optical displays of windows have been trapezoidal, the device should work in such a way as to convert non-trapezoidal optical windows to trapezoidal ones. Concretely, this would mean that we should have no difficulty in seeing Ames' rotating trapezoid as a rotating trapezoid; but a rotating rectangular window frame would appear to us as an oscillating trapezoid. This simply does not occur. There is, therefore, a prescriptive element which modifies what we experience so as to be consistent with what we know, regardless of the number of experiences we may have had to the contrary. To state this in terms of operational definitions, we merely mean to say that not all operational definitions are given equal weight in our awareness.

It is not accurate to say that all publicly observable operations are of equal significance. The usefulness of an operation depends upon whether or not the operation can be incorporated analytically, not synthetically. Of all the potential ways we might measure time, operationally we select those which most closely approximate our logical schemes and ignore those for which we cannot create convenient prescriptive rules.

We have seen that Ames' assumption of rectangularity demonstrates that these heuristic devices cannot be based entirely on sensory learning; this is so because the great bulk of sensory experiences with windows give rise to a trapezoidal optical display. An examination of Kohler's device also suggests that sensory learning is an insufficient explanation. Kohler's "central aftereffect" works in all situations in which one would expect a diffraction pattern if there were a physical basis for it. It is not that the device works only in those situations in which the perceiver has had sensory experience with the diffracting lenses; rather, it applies to all optical input. Kohler himself refers to the central aftereffect as a mnemonic which acts more as a "sorting" rule than as a predisposition of the sensory apparatus. (Kohler, 1964)

We have seen, therefore, that perceptual devices have an analytic aspect with respect to any one experience. They are definitional. In addition to an analytic aspect, such devices appear to have a range of convenience built into them. The perceptual device of Ames and Kohler are experienced by the subject as being "out there" in the stimulus precisely because they only affect some of the stimuli, leaving others seemingly untouched. The assumption of rectangularity affects the windows in a room while not affecting the perception of circular table tops. The whole from which the perceiver is working appears to determine whether or not a device will be used. This is most obvious in the case of experimental devices of measurement. The instrument is only applicable to a specific set of phenomena. This is why no one thought to build an interferometer for particles of matter until their discreteness was in doubt.

The final and perhaps most important aspect which these devices seem to share is their dependence on purposive behavior. Both Ames and Kohler emphasize that the building up of their devices depends on successful action. Ames repeatedly demonstrates that the way to bring about modifications in the perceptual assumptions a person is making is to make him do something. Activity which leads to counterevidence to the assumption will eventually force changes in that assumption. This can be seen simply in the case of the trapezoidal room. As long as he is assuming a rectangular room, a subject attempting to bounce a ball off the various surfaces in the room will make inappropriate, and therefore unsuccessful movements in trying to catch the ball. Kohler, as well as other perceptual psychologists who make use of transformation lenses, has repeatedly demonstrated that compensatory adaptation devices only evolve if the subject uses the lenses while trying to successfully mobilize in the environment.

It is not as easy to demonstrate the role of purposive behavior in the development of scientific instrumentation; however, the fact that we refer to the apparatus in scientific experiments as instruments suggests that they are a means of accomplishing a goal.

These "devices" are as close as the transactionalist comes to offering a conceptual unit to replace the traditional indubitables of epistemologies based upon the "principles of separation and verification." This inquiry leads us to believe that the assumption of objectivity and, therefore, the "principles of separation and verification", leave much to be desired in the way they account for our scientific knowledge. We will rely on transactional devices as one of the building blocks for an alternative account to be considered in the following Chapter.

CHAPTER VII

SEEKING AN ALTERNATIVE TO THE ASSUMPTION OF OBJECTIVITY

The three anomalies demonstrate that descriptions and perceptions are influenced by three aspects of knowledge which the perceiver or describer brings to the situation to be perceived or described. These three elements are: assumptions, wholes and goals. Recognizing that this is the case constitutes a rejection of the assumption of objectivity.

To say that science is not objective calls for one of two reactions from the reader. The first reaction is that of denial; an analysis which concludes that science is not objective must be fundamentally in error since it runs counter to the bulk of our experience and common sense. The second reaction, probably less likely, would be to assent but, at the same time, to dismiss it. The thinking behind this reaction would involve falling back on the obvious discrepancy between science and other kinds of knowledge. It is to say that the analysis was successful in debunking but that this is an empty victory since, as it turns out, objectivity never did form the essential difference between science and non-science.

The first of these two reactions will undoubtedly find some measures of support since there may well be technical errors in the expositions of the anomalies in particular. Technical errors are not likely to determine the fate of the inquiry. In fact, more work should allow us to determine the correctness of the conclusions. That is to say, if this method is valuable at all, the success or failure of the inquiry should not depend on the examples of the anomalies in Chapter V, but should be capable of generating others like them.

The second reaction which would admit the conclusion but not the relevance of the inquiry is more damaging as a criticism of the inquiry. For that reason, we shall devote the remainder of this Chapter to understanding the implications of the inquiry and try to state in positive ways the consequences of accepting its conclusions. Perhaps the most important reason anyone might have for dismissing the inquiry and its conclusions would be that one of the consequences which follows from the conclusion seems to be impossible. That seemingly impossible consequence is that, by getting rid of the subjectivity/objectivity distinction, we have now completely lost the basis for distinguishing between science and non-science. Few modern thinkers would accept that there is no difference between science and non-science. Yet our inquiry began on the premise that it was objectivity which distinguished science from non-science. If we conclude by saying that objectivity is not possible, then we must find another basis for distinguishing science from non-science. We have seen that there is a long-standing tradition which can be traced at least as far back as Aristotle. This tradition remains essentially true about contemporary theories of knowledge which make a firm distinction between science and other aspects of knowledge. We discussed this tradition earlier as the "principle of separation and verification." According to the principle of separation, only parts of experience can be trusted and, according to the principle of verification, only science can identify those parts. Accordingly, the scientific method relies on the acquisition of prediction and control in order to establish the objective truth of a proposition. How do the conclusions of this inquiry affect this traditional model for scientific knowledge? They indicate that the criteria of verification, for example prediction and control, are a measure of success rather than a measure of objectivity.

Scientific descriptions, like perceptions, are human achievements; not passively received messages from nature. To say that scientific method can distinguish success from non-success, rather than distinguishing objective from non-objective, may at first glance appear to be a very small accomplishment. In actuality, it represents a very significant step. There are at least three major changes which the conclusions of the inquiry suggest. The first and most important is the revitalization of a field of inquiry which had been laid to rest by the assumption of objectivity. This field is the psychology of knowledge. Formerly, inquiry into the psychology of knowledge was, by prescription, bound to be fruitless since the principle of verification gave us normative standards which defined for all times the standards by which acceptable knowledge must be judged. Under this system, improvement in our methodology could only come from two directions: from improvements in our logical tools of analysis such as probability theory and mathematics, and from improvements in our technical apparatus of observation. The psychology of knowing was prescriptively defined. Since this was assumed to be fixed for all times, inquiry into the question was bound to be fruitless.

The second important change to result from this inquiry has to do with the identity of three important factors in the psychology of scientific observation which were heretofore prohibited by prescription. Under the assumption of objectivity, scientists were not only unaware of certain psychological factors influencing their observations, but they were trained to ignore the possibility of such factors. In this inquiry, we have seen the influence that assumptions, wholes and goals can have on perception. The influence of these psychological factors on perception was presented in a negative context. This was done deliberately in order to show some of the problems with the assumption of objectivity. There is also a positive aspect to the knowledge obtained in Chapter V, since what is demonstrated there is that we can become aware of the influence of these psychological factors. There is no need to relegate those to the realm of non-conscious influences. In fact, we shall attempt to advance the thesis that those influences were relegated to a non-conscious status only by conscious and deliberate prescriptions which scientists and others promote in the process of education.

The third important change results from the fact that this inquiry represents the viability of a methodology which is neither analytic nor experimental. There are reasons to believe that methods similar to this one could be fruitfully employed in almost any science. In particular, there are reasons to believe that this method will be especially applicable to the study of the psychology of knowledge. If in fact the methods for the construction and use of knowledge cannot be exclusively prescriptive, then there is room for improvement in this domain. On the other hand, there is no denying that knowledge is at least partially prescriptive. The appropriate method for studying knowledge will, therefore, have to include both analysis and observation without giving preference to one or the other.

We have already discussed the influence that the whole has on the perception of the parts. The many consequences that result from the conclusions of the inquiry about the assumptions of objectivity are all "parts" of a "whole" new picture of science. In order to properly communicate the meaning of each particular consequence, we must begin by presenting the whole to which it belongs. This will be done by making use of two analogies. One is the analogy of puzzle-solving which we will use to represent the "whole" picture of science when viewed under the assumption of objectivity. The other analogy will be that of map-making. We shall use the analogy of map-making to represent the "whole" picture of science viewed without the assumption of objectivity.

As long as we could make the assumption of objectivity, we assumed that the fundamental job of science was to discover facts. These facts, we assumed, represented small pieces of indubitable information about reality or nature. The thing which primarily characterized science was its reliance upon a method which could establish the factuality of a proposition. Though we readily admitted that there were many difficulties surrounding the enterprise of scientific knowledge making, these difficulties we thought would be resolved by the eventual accumulation of the relevant factual information. The analogy of puzzle-solving is only appropriate if we assume that, in this particular case, the pieces of the puzzle are not all available and that before we can put it together, we must discover each piece. Facts, therefore, are like the pieces of a puzzle and science is the method for the discovery of these facts. How the facts relate to one another as well as the relevance of any one particular fact may be theorized about but a final answer must await the discovery of all the facts. Consequently, all facts are assumed to be of eventual relevance and, therefore, of equal significance. Similarly, the accuracy of our theorizing concerning any local area within the puzzle is a function of the number of facts available in this area. The puzzle-solver is involved in the processing of information, the bits of which are given by objective observation.

If, on the contrary, we cannot assume the independence and certitude of any observations, then it follows that facts cannot be clearly identified. Consequently, what science is engaged in appears to be more like the art of map-making than that of puzzle-solving. The good map-maker must have precise methods of observation and he must keep a close guard against misrepresenting any of his observations. However, none of his observations is considered an independent unit which he must somehow accommodate. In addition to paying attention to the faithfulness of his observations, his methods must somehow incorporate three other aspects of knowledge besides the experiential. First, he must recognize the role of assumptions and conventions. This means that he must bridge the gap between the subject and object of knowledge. As a good map-maker, he considers the user of the map as much as he does the producer. Second, he must consider the influence of the whole on the parts of the map. The meaning of both observations and symbols can only be found in the relationship of a particular segment to the whole. Third, the map-maker must consider the goal or the purpose for which the map is being constructed, for example, an aviator's map will differ significantly from a trucker's map of the same region. The map-maker, therefore, is seen as a producer, as well as a processor of information.

The first thing which becomes obvious from comparing these two analogies is that they are a set of complementary pictures of a kind familiar to us in this inquiry. Goblets or faces, pendulums or bodies in constrained fall, wave or particle, and, finally, puzzle-solving or map-making--each pair represents complementary wholes which significantly influences our awareness of the parts. It is not possible in any case to absolutely establish one over the other. However, as is clear in the case of some of these, one may have the predominance over the other for long periods of history. In the training of scientists in our institutions of higher education the puzzle-solving picture of scientists has been so predominant as to virtually exclude the map-making picture of science.

At this point, we wish to change the course of our inquiry. Until the beginning of this Chapter, the focus has been on a narrowing down of an evaluation of the assumption of objectivity. This was done in order to demonstrate the inherent weakness is an assumption of objectivity. Having accomplished this, we are now required to examine the implications of abandoning objectivity.

Understanding the implications of abandoning the assumption of objectivity will require looking at three kinds of changes in our view of science. The first of these involves the identification of proscriptions now existing in scientific culture which are based on a puzzle-solving account, rather than a map-making account of science (or the origins of our knowledge). The second involves changes in requirements or assumptions that must be made if knowledge is to be called scientific. The third involves changes in the essential methodology of science. It is perhaps not correct to use the word change to refer to these sets of recommendations. It might be better to call them complementary or alternative courses of action. They may be considered complementary because the proscriptions and prescriptions, the requirements and methods, of a science will vary, depending on whether one views science as map-making or as puzzle-solving. Our inquiry into objectivity has been undertaken in order to present the case against the assumption of objectivity, and to the extent that it has succeeded it may be viewed as supporting to the map-making picture. Understanding the implications of the study, therefore, requires that we understand the consequences of this point of view. Not all of the epistemological and methodological consequences of these two divergent views of science can be dealt with in a single inquiry. We shall attempt to contrast the complementary sets of consequences which follow from our analysis of the anomalies in Chapter V.

As was stated above, the first and most important consequence of accepting the map-making picture of science is that it makes possible a psychology of science. Map-making, unlike puzzle-solving, has no prescriptively, non-psychological entities in it. This means the kind of human creativity which is possible differs in each of these pictures of science. As long as the belief in objective facts prevailed, their primacy as the most significant elements in our knowledge forbade any psychological influences from entering into the shaping of these facts. In recent years, the role of creative thinking has played an expanding role in accounting for the discovery of facts in the puzzle-solving picture. Finding the pieces of the puzzle required elaborate and intelligent schemes. Establishing the correctness of a discovery, however, remained a non-psychological enterprise. The few writers who recognized psychological variables in the context of justification put the process of confirmation through a long series of interpretations. These writers tried to establish the verification and falsification principles which could assure that the facts did not involve any psychological determinants in their make-up. Even if creativity was admittedly an important aspect of discovering facts, it could have nothing to do with what the facts told us about nature. Analogously, we would say that it is permissible to give credit for the genius of finding a piece of the puzzle but it is assumed that the shape of the puzzle piece is independent of the finder.

The kind of creativity which the puzzle-solving picture calls for admits to only two kinds of improvements. We can improve by extending the reach of our senses. This is accomplished by the proliferation of scientific instrumentation. This kind of improvement should increase the number of facts we discover by expanding the area within which we can search for the pieces. The other way in which we can improve according to the puzzle-solving picture, is by the use of logic and better formal arrangements. The organization, manipulation, and storage of the facts is done within logical systems which are theories. Theories may vary in their logical exactness from the rigor exemplified by geometry to the ambiguity of psychological and sociological theory. Proponents of this view differ as to how precisely we make progress theoretically, though most agree that the number of facts available in any particular area influences the exactness of our theories. Psychological variables may be important in arriving at theories but they are prescriptively forbidden from being influential in the realm of factuality.

In essence, we are saying that the assumption of objectivity prevents any improvement in the psychology of science. That which defines science is its non-psychological method of establishing facts. This single capacity to establish facts is the one thing which must remain fixed in the puzzle-solving picture of science. Logicians may improve our manipulation and interpretation of facts, and technicians our ability to observe them, but the fundamental method of knowing the facts is fixed.

We will postpone the discussion of the subject matter and method of the psychology of science as it would appear in what we are calling the map-making picture. This discussion will come more appropriately after a discussion of the limitations placed on psychological factors existing in the current picture of science. In the puzzle-solving account, there is always, as we have previously documented, the promotion of an indubitable fact. These facts are like the pieces of the puzzle. As we have already said, methods for accounting for the discovery of such facts have run the gamut from induction by enumeration (Mills) to induction by intuition (Frank) to, finally, the creativity of human genius (Einstein). Likewise, the methods for verification or confirmation have run the gamut from sense-data to physical protocols and, finally, to falsibiability potential. Regardless of which set of these alternatives is preferred by any particular philosopher or scientist, the central question is always one of factuality. This means, in effect, that while there may not be agreement about how facts are obtained, there is agreement that they are indeed obtained. Whatever the currently popular principle of verification happens to be, be it phenomenal, physicalistic or operational, it is believed to be a measure of the truth. It is probably correct to say that currently the criteria of verification are prediction and control. Presumably, if we establish prediction and control over a phenomenon, then that is a sufficient criterion for asserting that our knowledge is factual. That assertion is equivalent to asserting that we know the truth about that particular phenomenon.

Let us contrast to this picture one in which the account of scientific knowledge must proceed without the assumption of objectivity. As we said above, if we do not believe in the factuality of any assertions, then we must provide an alternative account of what it is that the principles of verification do for us. What exactly do we establish by the experimental verification of a proposition, if not its truth? The alternative suggested by the abandonment of objectivity is that verification is a measure of success, rather than a measure of truth. Prediction and control are a measure of the fruitfulness of our constructions rather than the faithfulness of our representations. Once more it is important to recognize that success and truth are complementary pictures which are so broad in their significance that neither can be absolutely preferred over the other. Nevertheless, it is important to realize that these differences are more than linguistic--they are substantive. Understanding the substance of the differences between the two alternatives requires an examination of many aspects, only some of which are touched upon in this inquiry. Among these important differences which can be identified as a result of this inquiry are three negative proscriptions at work in the present scientific culture. These proscriptions are explicit and implicit patterns of avoidant behavior trained into the scientific mind because of its acceptance of the assumption of objectivity. These proscriptions are an essential part of the "hard-nosed" attitude which is considered essential in a good scientist. They are only negative proscriptions if the conclusions drawn from this inquiry are in fact correct. Their effect on the product of science is diminishing, only if one assumes that science is engaged in marking successes, rather than in discovering truth. Few will doubt that these proscriptions do exist; many may argue that they are justified. It is important to notice, however, than an exact identification of the proscriptions can only come about if we recognize the contrast between the two views.

The first of the three proscriptions is that which proscribes against a consideration of goals. The proscription evolves naturally from the belief that science discovers truth rather than achieving successes. If science is engaged in discovering factual truth, then it would be nonsense to ask "truth for what?" However, if science is engaged in achieving successes, it is not nonsense to ask "success at what?" If our method reveals facts, then we need only be concerned with the proper application of this method and rest assured that the outcome eventually will be significant. In addition to the fact that it is illogical to question the significance of truth, the evaluation of performance by means of established goals is associated by the scientific community with engineering and other applied fields. Therefore, the proscription against goals not only is logical but it promotes purity.

The great difficulty which arises as a result of the goal proscription is that there is a tendency to evaluate all work of science methodologically. We cannot question the values of the pieces of the puzzle being brought forth. Their quality is presumably all equal. We can only criticize the quantitative productivity of various methods. Since the one area in which scientists are open to criticism is method, they are certain to keep this fixed. They are more willing to let the method prescribe what they will study, than let what they are to study prescribe the methods appropriate to its study. It is on this account that Wertheimer was so critical of science.

> We go from the world of everyday events to that of science, and not unnaturally assume that in making this transition we shall gain a deeper and more precise understanding of essentials. The transition _should_ mark an advance. And yet, though one may have learned a great deal, one is poorer than before. It is the same in psychology. Here too we find science intent upon a systematic collection of data, yet often excluding through that very activity precisely _that_ which is most valid and real in the living phenomena it studies. Somehow the thing that matters has eluded us. (Wertheimer, 1959, p. 1)

At least in some instances, the goals of a science have been allowed to change beyond our recognition because the product of science could only be evaluated methodologically. Psychology is an example of a science in which this is the case. The original goal of psychology presumably was to explain knowledge, experience and behavior. It must be admitted that psychologists had been applying a methodological criterion to their accomplishments which very often means accepting something far less than this original goal called for. We shall see that if science is construed as a method for achieving success, then the role of goals is returned to a central position in the psychology of science--one which permits other than methodological criteria for the evaluation of scientific accomplishments.

The second proscription of science which evolves from the assumption of objectivity is that which calls for ignoring the whole. According to this view, progress in science is achieved from the discovery of facts. How facts relate to one another within any particular discipline is the problem of theoreticians. The status of theoreticians in science is almost deliberately ambiguous. In the training of our scientists, little or no systematic attention is paid to the explicit development of theoreticians.[1] There is a mysticism prevalent around the question of theory within science. Great theories are the result of genius. Men like Newton, Einstein, Freud, Marx, are held up as examples of creative genius. The training with respect to the work of these men differs significantly from the training scientists receive with respect to the work of great experimenters. Great experiments are held up as examples which the initiate can someday imitate. We study their general character and analyze them into a systematic body of knowledge which we call methodology. Methods become separated from their discoveries. The study of theory and theorizing is far less systematic. Theories tend to remain associated with the name of their creators. Though the initiate is trained to imitate in the methodological realm, he is trained only to admire in the theoretical realm. This is consistent with objective epistemology since it would be nonsense to imitate creativity and genius.

Within any particular area of scientific inquiry, placing the facts into meaningful relationship is the problem of theory. But placing the disciplines in relationship

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[1]The conspicuous exception to this is physics. The development of theoretical physics as a specialty within the discipline indicates that this science more than any other has abandoned the puzzle-solving picture of science.

with one another, which after all is the next largest whole, seems to fall completely outside the realm of science. If psychologists are hard-pressed to understand the relationship between all the facts within psychophysics, let them try to relate psychophysics to personality, psychology to sociology, and psychology to all other disciplines. Whose problem is this? In theory, one supposes that this is the problem of philosophy but in practice it becomes a problem of administration. If we assume that science is a process by which we discover objective facts, then there is no need to worry how academic departments are created and dissolved since eventually the facts will emerge independently of such administrative decisions. If, however, we assume that the parts of knowledge being generated are influenced by the whole from which they come, the scientists can no longer afford to ignore the sociology of science.

The third and final negative proscription is that which calls for deliberately ignoring the subjective in the act of knowing. As long as we believe ourselves to be engaged in the objective description of reality we believe it desirable to eliminate all subjective elements from our descriptions. In the past this was accomplished by using descriptive languages with the greatest amount of logical explicitness. It is this aspect more than any other which makes quantifiability desirable. In recent years, there has been increasing awareness that these logical systems of description, such as geometry, are conventions. Though they are public entities, they are nevertheless subjective insofar as they are something which the observer brings to the observed. They are also arbitrary insofar as there is nothing about the object which enables us to establish a preference for using one convention rather than another. Though this conventionality of descriptive languages has been recognized, its most important implications have not been delineated. In recognizing that data language is conventional, it becomes important to establish criteria according to which we may prefer one convention over another.

In principle at least, it is no longer a question of how faithfully one convention, rather than another, represents the facts. Conventions are selected on the basis of convenience, not on the basis of truth. The next question is of the utmost significance--that is, "convenient for whom?" The way our scientific knowledge has evolved a class of experts in any given domain is necessary in order that the knowledge in that area can be understood. As long as this expertise was a natural outgrowth of the complexity of the facts, then it could not be questioned. But, if in fact the language of scientific knowledge is selected on the basis of convenience, this means that the user of the knowledge is being incorporated into the criterion for the selection of convention systems. There is a sense in which Einstein recognized this when he chose a non-Euclidean geometry in order to preserve our conventions of measurement. To do the opposite would have meant a far more disruptive effect on our picture of the world. There is a sense also in which Bohr recognized this point when he insists upon the fact that we must represent the facts of quantum mechanics in the language of classical physics, not to preserve a faithful representation of the facts but to maintain a connection between the two fields of knowledge. There are in these positions the seeds of an obligation which have not yet been recognized. If scientists do in fact have a choice in the assumptions and conventions which they make and use in describing the aspect of the world which they study, then is it not their obligation to amplify the degree of freedom available to them and make explicit the criteria they use in choosing their conventions? This would mean ultimately that the producers of knowledge must not only take an interest in what the knowledge is about, but they must take into account those who are to profit from that knowledge.

There would appear to be two criteria for evaluating the worth of a particular contribution. The first is ontological consequences; that is, according to the improvement that it affords in prediction and control of relevant areas. We measure a scientific contribution epistemologically in terms of the number of men who can incorporate the knowledge into their lives. The epistemological criterion increases in significance as the scientific domain in question comes closer and closer to man. We are at the present time reaping the fruit of having ignored this epistemological aspect of our knowledge in areas such as ecology, sociology and psychology. By using only our ontological criterion, we have produced a class of experts who in turn have produced a great deal of knowledge about each of these domains. But since they paid no attention to the user of the knowledge, the potential significance of their accomplishment was wasted. What we need in these areas is knowledge <u>for</u> man, not knowledge <u>about</u> man. The producer of knowledge must take into consideration the potential user of knowledge.

We have already pointed out that abandoning the assumption of objectivity leads us to the conclusion that science is involved in achieving successes, rather than in discovering truths. One of the most significant differences between these two pictures of science is that we can have levels or grades of successes, but we cannot have levels or grades of truth. This realization is of the utmost significance since it is helpful in resolving one of the problems which arose repeatedly in this dissertation; namely, how does one establish a preference among equally objective descriptions.

From the point of view of the history of science this is a theoretical problem, not a practical one. There is no doubt that we did come to prefer Galileo's pendulum to Aristotle's body in constrained fall. There is equally no doubt that we came to prefer Newton's ($a = const\ 1/r^2$) to Galileo's ($a = g = const$). The question is: how did we establish this preference? As long as we believed we were involved in verifying truths, there was no principle by which we could discriminate between alternative constructions of the same verifiable phenomena.

If we recognize that that the processes of perception and description are achievements instead of representations of fact, then it is possible to accept a principle of gradations among perceptions and descriptions with respect to a particular set of givens. We may term this principle the "inclusiveness of the goal." That is, we can grade the level of descriptive achievement by reference to the inclusiveness of potential achievements which it allows us to accomplish. We prefer pendulums to bodies in constrained fall because pendulums allow us to accomplish more in a greater number of diversified settings than do bodies in constrained fall. Any particular description is valid to a degree and can never be evaluated by itself. Levels or gradations imply a process of rank ordering which requires at least two competing descriptions. This is why the establishment of descriptive value is a theoretical task, not an experimental one. All experiments can ever tell us about Galileo's ($a = g = const$) is that it is essentially correct as far as it goes. One would never think to look for differences due to altitude until one is given the competing ($a = const\ 1/r^2$). We can grade the two formulas for acceleration with respect to potential accomplishment afforded rather than with respect to factual validity.

Another example of the use of the principle of goal inclusiveness to grade alternative constructions is provided by clinical psychology. A client complains of aversive reactions to sexual situations. This has led in her case to the dissolution of two marriages and is threatening to destroy a third. The Freudian construct of frigidity is available as a description of the clinical condition. If we limit our goal to that of understanding the client's marital sexual relations, the construct of frigidity would seem appropriate, though limited. But, if we wish to include behaviors outside the domain specified by this construct, then we find that the concept is not generally useful.

A preferable construct would be one which would account satisfactorily for the range of behaviors referred to by the diagnosis of frigidity, and at the same time be capable of shedding light on other aspects of the client's behavior. One such alternative assumes that the client avoids situations in which she is to be evaluated on the basis of her physical performance. Using this concept we find that she has similar aversive reactions to sports which are similar to her responses to sex. It is also noted that she has high standards of performance in those areas in which she does engage herself. We find that she is extremely successful in those things which she does undertake and avoids situations in which she does not meet her own standards of performance. In some respects this conception is similar to the clinical diagnosis of frigidity, but it has a much wider range of applicability. It allows the therapist to recommend preliminary therapeutic steps outside the range of sexual behavior, the aspect of the person's frigidity that is most threatening. The client is given an opportunity to learn that the consequences of failure at playing golf are not as threatening as she had thought. Once she has overcome her avoidance behavior to sports, she has the basis in her experience to begin moving with confidence in other areas of physical performance. With respect to this example, the avoidance construct is preferable to the frigidity construct, not because one is true and the other is false but because one is more inclusive of potential accomplishments than is the other.

This method for evaluating scientific propositions is offered as a substitute for the various principles of verification proposed by those philosophies of science which adhere to the assumption of objectivity. In combination with the notion of devices which we discussed previously, it produces a picture of science significantly different from those available in the objectivist tradition. According to this picture, science is engaged in producing devices which are evaluated according to their ability to actualize potential. Whenever two devices are competing within a domain, we prefer that which includes the widest range of potential accomplishments.

Finally, we return to the first and most significant implication of abandoning objectivity; namely, that a psychology of science is provided a most important role in the expansion of knowledge. As was indicated earlier, the assumption of objectivity asserted that the fundamental method of knowing for all science was fixed for all time. That method was the establishment of facts by means of observation. We could improve our processes of knowing in all respects except this one. It will help to return to our analogies and say that, in the puzzle-solving picture, we can improve the process by any method which will improve the processing of the pieces. According to this view, the scientist is only a processor of information. We may improve by extending the range of his senses and the logical systems for storing the information, but nothing must be allowed to interfere with the essential pieces as they are given to us by reality.

In the map-making picture, the agent of knowing is involved in producing information. According to this picture, assumptions, wholes and goals, as well as the experimental actualization, all influence the information produced. This latter picture points to the reinterpretation of many of the traditional methods of science in addition to indicating the explicit use of methods which, until now, have operated covertly in science. According to this map-making picture, an experiment is a demonstration of the actualizing potential of certain concepts. It does not give us a measure of truth, but it does give us a measure of success. Consequently, experiments are not the only method by which scientific awareness is extended. We must develop methods which allow us to understand how assumptions and conventions influence our scientific awareness of the world. Perhaps, the best example of these methods is the "thought experiment" which allows us to compare the contrasting influences of alternative sets of assumptions on our awareness of a particular domain. This dissertation is an example of such a "thought experiment." There is reason to believe that in physics at least, "thought experiments" played a crucial role at every major turning point. The thought experiments of Galileo, Einstein and Heisenberg seem to come at those points in the history of physics when the most basic of assumptions were being questioned. This method makes possible a form of knowing which laboratory experiments cannot develop. It makes possible an awareness of "what is being omitted," and it allows us to produce information from "nothing." Producing information from "nothing" can only be accomplished by the manipulation of theoretical entities. It cannot result from experimental observation. We have already discussed an example of this kind of awareness when we discussed the criterion for preferring one construction over another within a particular domain. In that discussion we noted that our rejection of Aristotle's body in constrained fall and Galileo's acceleration was not based on a substantive error in their formulations, but on the basis of a comparitive omission in their formulation. It is because Aristotle said nothing about time per swing and Galileo said nothing about altitude that we eventually preferred competing constructions which did include these omitted variables. Becoming aware of these omissions requires bringing an alternative set of expectations to the observation. Such expectations are governed by the constructs we bring to the observation situation. This is another way of saying that within any particular construction there is nothing to tip the observer off about its limitations. Only when an alternative set is provided can we become aware of what a construction does not do for us.

Since this particular dissertation demonstrates the influences of assumptions, wholes and goals on awareness, it suggests that the deliberate manipulation of any of these can improve our awareness within a particular domain. We have already noted that the prevalent conception of science prescribes against the explicit considerations of such factors as assumptions, wholesand goals, and proposes no method by which we can systematically study their influence. This inquiry is thought to be an example of a scientific enterprise which bears the seeds of a new method which will allow us to examine the influence of psychological factors--in our scientific knowing of the world. In calling for an explicit consideration of wholes and goals in particular, it seeks to establish a counterbalancing force of the utmost significance in contemporary science. The direction of this force only begins to emerge from this particular inquiry but its significance is so important that it must be delineated despite its inexactness. The methods suggested by this inquiry aim at achieving an integration rather than further differentiation within science. They place the stress on the inclusiveness or breadth of applicability of scientific findings, rather than on their certainty. These methods also suggest that a systematic approach to theoretical work is as feasible as a systematic approach to experimental work. All of which is to say that methods are available to remove the process of knowing from the realm of prescriptive philosophy and place it in the arena of scientific inquiry where it may undergo progressive change.

CHAPTER VIII

SUMMARY AND CONCLUSIONS

In Chapter I, it was asserted that the subjectivity/objectivity distinction underlies what C.P. Snow has called "the two cultures." In particular, the assumption that man can make objective observations by using scientific methods is one which is generally made and which is central to understanding the relationship between scientific and non-scientific knowledge. All of this is given as justification for this inquiry, which attempts to establish whether or not we are justified in assuming the capacity for objective observation. This question is seen as central to an eventual understanding of the psychology of knowledge.

Chapter II briefly reviewed the history of this problem. This analysis revealed that two principles of scientific epistemology have remained constant throughout the history of science. The first is the principle of separation, according to which human experience can be separated into subjective and objective experiencing. Specific expression of this principle has changed during various periods of scientific history. At times, there was a preference for expressing it in ontological terms, such as primary and secondary qualities, while at other times the principle of separation was expressed in epistemological terms, such as analytic and synthetic.

The second principle of scientific epistemology to emerge is referred to as the principle of verification.

According to this principle, the scientific method is held to be the only one by which one can distinguish the veridical from the illusory aspects of experience. In general, changes in the manner of expression of the principle of separation were accompanied by appropriate changes in the manner of expression of the principle of verification. That is to say, the specific criteria by which a genuinely scientific procedure was identified changed along with the way in which we separated objective from subjective experience.

The most important conclusion to emerge from the examination of the background of this problem is that the assumption of objectivity has played such an important role in molding our Western ways of knowing that most of the existing credible methods of inquiry have the assumption embedded in them. Contemporary disciplines suited to deal with epistemological questions tend to adopt experimental or analytical methods of inquiry, and the distinction between these kinds of methods are themselves based on the assumption of objectivity. Since the assumption of objectivity is the subject of this inquiry, it was concluded that methods which are based on the subject/object distinction would not be appropriate here.

In Chapter III the method of anomalies was described. This method was deemed appropriate for this inquiry since unlike most other available methods it is neither analytic nor experimental. It is based on the method of thought experiments in which facts, theories and assumptions about initial conditions are viewed together in order to discover new relationships. Anomolies are described as old facts seen in a new light. Their utility is in their ability to demonstrate to us the experiential consequences of particular assumptions. In the context of this particular inquiry, the method of anomalies is particularly useful since it allows us to hold in abeyance the assumption of objectivity. This in turn allows us to bring scientific epistemology and the psychology of perception together, since the assumption of objectivity is what held them apart. This is accomplished by asking that theories of scientific observation be capable of accounting for the facts of perception, and that theories of perception be capable of accounting for the facts of scientific observation.

In Chapter IV, models of perception and observation were reviewed in order to understand the role of the assumption of objectivity in the various theories of perception available. Five broad models of perception and observation were identified. They were realism, logical positivism, configurationism, neo-realism, and transactionalism. It was found that all but the transactional model subscribed to the assumption of objectivity. Each of the other models promote the belief that under special circumstances man is able to make objective observations and that knowledge obtained in this manner can be considered factual. Despite the agreement concerning the belief in the existence of facts, each of these models promotes a different description of the nature of facts and the process of observation.

Chapter V presented three specific anomalies to the assumption of objectivity. Each anomaly consists of a combination of information derived from two sources: scientific epistemology and the psychology of perception. The first anomaly demonstrates the inseparability of logical and empirical elements in perception. Specifically, it demonstrates that the real and the illusory can only be separated after certain logical assumptions are made. This indicates that the logical assumptions are as fundamental to the definition of the real as are the sensory elements. The second anomaly demonstrates the influence of the whole on the experience of the parts in both scientific measurement and in normal perception. Specifically, it shows that concepts or constructs define and select the appropriate measurements to be taken, as well as those to be ignored. The third anomaly demonstrates that the qualitative experience in observation and perception is, in part, determined by the purpose of the observation or perception. This means that the act of measurement or perception in part determines the result of the perception or measurement.

In Chapter VI the impact of the anomalies on the assumption of objectivity was assessed by taking each of the definitions of fact (which had been identified in the various models of Chapter IV) and determining how each definition is affected by the anomalies. It was concluded that none of the definitions of fact are capable of dealing with all of the anomalies. This was interpreted to mean that the idea of factuality is misleading, and led to an attempt to describe an alternative epistemological unit called "devices." Devices are forwarded as an alternative building block for a theory of knowledge which would not be based on the assumption of objectivity. In defining this unit, we depend a great deal on the transactional perspective which we found to be the only existing model that did not depend on the assumption of objectivity.

Chapter VII concludes the inquiry by depicting a view of science which does not depend on the assumption of objectivity. This picture of science differs from the more traditional view in two basic ways. It substitutes for the notion of facts the concept of devices and it replaces objective truth with relative success. Facts were held to be independent and certain. In sharp contrast, devices are demonstrated to depend upon three sets of psychological variables: assumptions which we make about their domain, the influence of "whole" patterning tendencies of knowledge already existing in the relevant domain and the goals we wish to achieve in the particular area. The criterion of objective truth does not allow for intermediate degrees of truth, but the criterion of relative success allows for a variety of degrees of value.

These basic changes which result from having abandoned the subject of objectivity also suggest some important methodological changes. It suggests a need to expand our methods for dealing with theoretical problems. Methods such as the thought experiment and the method of anomalies are put forward as examples of fruitful non-experimental methods. Abandoning objectivity also places new responsibilities on the scientist. It calls upon the scientist to make explicit his assumptions, wholes, and goals. He must develop criteria for selecting among these psychological variables when a plurality exists. He must develop some epistemological standards to complement his experimental criteria of success. In addition to pointing to a need for such epistemological standards, Chapter VII suggests a set of possible standards which follow from this inquiry.

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