# IV

## THE PERSONAL DIMENSION IN SCIENTIFIC METHOD

Like knowing, scientific method is not a set of rules which, if followed, guarantees results. Instead, it functions like an art where the rules are maxims. Like an art it is creative. Its creativity extends beyond the discovery of the proper objects of the various sciences, to the modification of scientific method itself. As Kuhn repeatedly affirms in his The Structure of Scientific Revolutions, many of the most profound scientific achievements coincide with the implementation of novel means of experimentation or the transformation of ideals of what constitutes a scientific result in the particular field. Galileo's views on the mathematization of nature symbolize the emergence of modern science. Lavoisier's quantitative method established him as the father of modern chemistry. Psychology and sociology are in crisis because they have yet to agree on an approach which will unify the various methods and correlative metaphysical views currently in use. Methods also develop. We do not identify modern physics with the work of Galileo alone, nor modern chemistry only with the foundational work of Lavoisier. Likewise, if we accept Kuhn's analysis of scientific development, the current crises in psychology and sociology may be the preliminary stages of the emergence of a unified science in both fields. Though I think there are powerful reasons to conclude that such unification will not occur in the near future, if at all. (indeed, Kuhn does not go so far as to conclude this either), the various schools of thought will develop within the range of their methods,

The dynamic character of science is relatively obvious. That science has developed rather than declined is also generally accepted. That objective ideals have been instrumental in science's development is also beyond dispute. Stressing objective tests with results in principle accessible to all, dispassionate inquiry, exact results, and so on did much to separate science from common sense and nonsense, establishing a socially accepted body of knowledge which has proven itself in its theoretical and practical fruitfulness. However, it is questionable if the ideals which have worked so well for physics, chemistry, and to some extent for biochemistry, will be equally successful by themselves for other areas of biology, animal and human psychology, and sociology. It is questionable if the objective. ideals of the exact sciences are faithful to the' actual performance of the scientist in the development of any science. Science may have progressed despite its inadequate self-knowledge, and we may be at the point, especially in the human sciences, where the ideal of science in general needs to be transformed from the objectivist ideal to a personal one if we are to continue to attain significant results.

In short, the development of science depends on scientists. Far from being trivial, this implies that the dynamic developmental nature of science rests on the progressive development of knowledge in the person. The person is also the creative source of transformation in science. If scientific methods are more than mere techniques, then personal acts of knowing, not fully specifiable in terms of methodological precepts, are necessary for scientific development. An adequate philosophical theory of scientific method must account for these acts. In this chapter we will explore those areas where personal involvement is necessary in scientific method £or Polanyi. In turn, this will provide a foundation for understanding his theory of the structure of the scientific community and the nature of scientific progress in the next chapter. In the subsequent chapters we will see how personal knowledge is necessary for the adequate development of the biological and the human sciences.

## 1) THE PERSONAL CONTEXT OF LOGIC

For most of the twentieth century there has been an emphasis on the logical analysis of science. The logical positivists were extremely influential, and much contemporary work is being done in the shadow of their thought. Also, individuals such as Sir Karl Popper, Ernest Nagel and Carl Hempel have made logical analysis central to their philosophies. I do not propose to engage in a detailed comparison of Polanyi's thought with theirs, but it will be helpful to situate Polanyi with regard to this general trend in terms of a discussion of his view of the personal context of logic. This will provide a convenient manner of mediating between the results of the previous chapters and the concerns of the present. It will demonstrate not only the validity, but also the necessity, of investigating personal know ledge if we want to develop a complete account of science. The major failing of many modern philosophers is not found in their doctrines, but in their neglect of personal involvement in knowing.

Logic seems to offer the promise of not merely being objective know ledge, but of being an objective method, Very generally and simply, logic is concerned with the validity of arguments, If we reason logically, then, if the premises of our argument are true, the conclusion should also be true. However, logical validity is distinguished from truth per se, for it is possible to argue validly from false premises to fa1se conclusions. If we consider truth as independent of the person we can understand the attraction of logic as an objective method. If we start with true premises, with the aid of an explicit method which retains its efficacy and objectivity independently of any particular person, we can arrive at true results which are also independent of any particular person. It may be tempting to suppose that we have completely and adequately transcended ourselves, or that we have fully objectified reason itself, or at least have come to some sort of "logical reconstruction of the general form of reasoning.

That logic is a partial objectification of reasoning Polanyi does not dispute. Indeed, he considers it a cultural achievement of great value for organizing expression, clarifying thinking, and gaining new knowledge. For example, he notes that deductive reasoning can lead "to a new conception wholly implied by our original conception, yet different from it", and that logic can formalize this procedure. "(A) proof does more than denote its subject matter; it brings it about." But it is not to be supposed that this happens impersonally. There are at least three ways in which tacit knowing enters into this formalizable process. Tacit knowing enters into the selection of premises. This was recognized by Aristotle, who distinguished mediate an immediate results. Mediate results were conclusions of syllogistic arguments. Immediate knowledge yielded the premises. However, this knowledge was not immediate in the sense of being obvious. Nor was it like the law of non-contradiction which we can.' riot doubt without jeopardizing the validity of our full range of mental activity. There was mediation on the part of the mind. For Polanyi this mediation is the activity of tacit integration in understanding and responsible judgment. Second, tacit knowing is operative in the transformations which occur in deriving a valid conclusion. Now it is true that for the trained mind many logical operations can be performed with such fluidity that it may appear that the person is simply following the rules, However, the trained logician or the mathematician does with ease what most of us do with the utmost difficulty, If there are rules to be applied, still they must be selected from a set of rules, If certain rules are to be applied in certain situations, still the situations must be recognized, Finally, the transformations themselves are guided by an overarching understanding of the result to be achieved or at least by the anticipation of such a comprehensive result, In other words, the proof should hang together, and it is the intelligibility of the proof which is grasped with relative ease by the trained mind, In short, as performed by humans, logical proofs are creative. It is not immediately obvious that the conclusion is a proposition equivalent to part or all the premises, and it demands an act of insight to determine which rules are to be applied in which situations to yield which results, an act of which machines are, apparently, incapable.

Now, if logic itself can be fully understood only if placed within the context of personal knowing, it should follow that any knowledge which is "logical" can only be understood as knowledge in the same way. In short, insofar as knowledge is logical it demands a personal appraisal to recognize its objectivity. However, this is a result which can be conceived as applying only to the realm of the apriori. Is it possible to have any purely objective empirical knowledge? From the discussion of his epistemology it is clear that Polanyi does not think so. It remains to show that there is neither a purely objective method nor purely objective knowledge in science. The results of the previous chapter imply that the human mind is not purely objective. That is, there is not a fully specifiable set of rules which, if rigorously followed, will yield knowledge as the rules of logic, if rigorously followed, will yield valid arguments. To know human knowing, then, is to know a set of informal activities. Though the human mind is not "purely objective" in this sense, we could still hold out the hope that we can develop a purely objective method which would result in scientific knowledge. Though the mind is a set of informal acts the method would be formal, However, if the scientific method is something that people do, then there are points, as in logic, where formal operations rest on informal acts. For Polanyi, insofar as this is the case a purely objective method is not possible.

However, as the results of the previous chapter indicate, Polanyi's rejection of what I have termed "pure objectivity" is not a rejection of objectivity in every sense. There are three points Polanyi stresses concerning objective knowledge. First, a theory is "objective knowledge insofar as it is not I, but the theory which is proven right or wrong when I use such knowledge." Such is the case with a mathematical theory which yields the wrong calculations concerning some set of events. Second, the theory per se exhibits an independence of the vicissitudes of subjectivity. It need not change as I change. Finally, the theory's independence of "internal" experience can be generalized to its independence of individual' "external" experience. Hence, the space and time of Einstein abstracts from the relations to us which are suggestive of absolute space. Copernicus's theory displaces the importance of common, but particular, daily experience of the heavens, and in general, theories in natural science are considered true independently of the particular places and times of individuals. However, as was shown in the last chapter, it would be absurd to suppose that they can be accepted as true outside a personal context. Polanyi states

> Thus, when we claim greater objectivity for the Copernican theory, we do imply that its excellence is not a matter of personal taste on our part, but an inherent quality deserving universal acceptance by rational creatures. We abandon the cruder anthropocentrism of our senses--but only in favour of a more ambitious anthropocentrism of our reason.

Knowledge is also objective in that it is of a transcendent reality. This means that Polanyi does not make the mistake of the idealists and claim that reality would not exist if there were not a knowing subject, a transcendental ego, or group of transcendental egos. His philosophy only implies that reality would not exist for us if we were not knowers, not that it would not exist in itself. Thus, the objectivity of knowing is for us recognized in personal judgment. The objectivity of judging is grounded in itself; that is, it is self- accrediting. In other words, the conditions for knowing the object and the conditions for the existence of the object do not need to coincide in the general case. Since knowing does not create the object, for Polanyi the object is impersonally given. Thus, when I refer to reality as transcendent or independent I mean that it does not need a knower to exist. Though the fact that personal knowledge involves both subjectivity (in the non-Polanyian sense) and objectivity is paradoxical, if we recognize that commitment is self-transcending the paradox is not as mystifying. I shall now turn to the main topic of this chapter, showing how an account of scientific method includes an account of the personal in science.

## 2) OBSERVATION

Scientific observation is an activity of personal knowing. Because it is a specialization of human perceiving, it is a skill. Its operation and significance is influenced and often determined by scientific theory. This is because its content is theory-laden either in fact or in anticipation. Observation takes place within the context of a theoretical viewpoint, or at least the anticipation of one. It will be most efficacious, then, if I introduce three meanings of "theory-laden" which are operative in various philosophies of science. It will then be possible to determine in which way theory influences observation for Polanyi and in what manner the "data" of observation are theory-laden for him. This will make it easier to see how observation is a skillful operation of personal knowing.

A first distinction is between observation and theory. Unfortunately, Polanyi does not offer a definition of theory. I shall introduce a minimal notion of theory which I do not think will prejudge any issues concerning the interpretation of Polanyi's philosophy and which is not incompatible with it. A developed theory involves the use of technical terms. There is an attempt to use these terms univocally and exactly. A developed theory elucidates correlations discovered in nature. These relationships are between the aspects, things or events to which the technical terms refer. So in Newtonian mechanics we have the technical terms "force", "mass" and "acceleration". They were taken to refer to something real, though now we tend to think of most scientific correlations as merely probable. The second law of motion correlates the three. But there are also observations. In science it is useful to have an exact language to describe the content of observing. Thus, in mechanics the observational language is primarily mathematical. In some sciences the exact language of observation and the language of technical terms referring to the object of study can coincide. Such is the case in the study of the cell. Some of the elements of the cell which are related to one another are directly observed. However, in physics, for example, the problem of "theoretical entities" is most acute. These "entities" manifest themselves indirectly. Because they are not immediately given it becomes a problem of whether we are merely correlating measurements or are actually discovering correlations which exist between unobservables. I wish to bring up this problem at this point merely to indicate that there can be a difference in observational and theoretical language. An electron is not "directly" observed, but the measurements of it are. The observations can be described without reference to electrons. But at the same time, there is a sense in which they cannot. This brings me to the three meanings of "theory-laden".

The first meaning of the theory-ladenness of observations is relatively unproblematic. It is the fact that the validity of observations as scientific depends on theory. It is this use of theory which Nagel refers to when he writes of the borrowed laws of a theory. Certain instruments may be used in a science, but the laws of the science do not govern the use of the instruments. Other theories may account for the relation of the instrument to what it measures, and, hence, ground the judgment that the results obtained by the instruments are valid or objective or scientific. The theory which grounds the use of the instruments or experimental techniques need not consist entirely of borrowed laws. Thus, the use of cloud chambers in physics has its validity grounded by certain theoretical views about what is being studied in the cloud chambers. Likewise, the use of dyes and radioactive tracers in biology is valid insofar as it does not significantly alter what is to be studied. The judgment that it does not depends on the theory concerning what is to be studied.

A second sense of theory-laden is that the significance of the observation is determined within the context of the theory under investigation. It should be remembered that the observation is independent of the theory in that the theory does not determine (in the sense of determinism) what is going to happen. and, therefore, what observations we are going to have. This independence of observations depends on the independence of reality from theory. The ideal per se is not the real. To understand this meaning of theory-ladenness we must specify further the meanings of observation and significance.

In Chapter II we saw that a perceived content is constituted by a tacit integration. In such perceiving both the elements of the gestalt proper and the background are subsidiaries of the focal object. In perceiving no sense data are "given". Polanyi states that

> ••• it is very difficult to discover any such primary sensations which are given previous to our interpretation of them … (T)he moment we notice a thing, say by sight, we perceive it as something. We usually perceive it as being at some distance and as forming part of something else or standing out against other things as its background.

However, this is an act of interpretation which differs from an act of discovery. While sense data are not given as objects for integration into gestalts, perceptual gestalten can be "given" as objects for the inquiry which leads to the discovery of intangible, intelligible patterns. Thus, in the simplest case, if we consider the observation to be a gestalt, then the significance of the gestalt will be determined by the theory which accounts for it, or interprets it, or explains it. This interpretation will be in terms of an intelligible pattern. Thus, in Chapter II I distinguished between the facial configuration and its meaning. In science this kind of distinction can be even more pronounced, for the gestalts can be a series of readings on an instrument and the pattern can be a mathematical equation. The significance of the reading is determined in terms of the mathematical equation.

The third sense of observations being theory-laden is that the perceiving of contents can be influenced by our understanding. Our understanding can function as subsidiary in our constitution of a gestalt.

Elements can be perceptually significant depending on their intangible significance. For example, if we are sitting in a train which is at rest and the train next to us begins moving, we often spontaneously perceive ourselves as moving. Yet, if we come to understand that we are not moving our perception can change. To a certain extent we can select a different gestalt. By integrating some stable object into our background we can perceive ourselves as at rest and the other train as moving. Examples are numerous. A person who understands more about a certain situation will notice more. Consider a scientist watching a bug cross a table. If he has studied insects, he will notice many more things than the layman. This will involve having a series of gestalts which the layman conceivably could have, but does not. Polanyi gives the example of a doctor reading an x-ray. He will observe patterns which the untrained eye cannot see. Consider Pasteur examining the residue from fermentation for the presence of yeast. Without some prior understanding one would hardly expect to notice certain gestalts in that monochromatic mass

To illustrate these three meanings I shall return to the problem of observation in physics. The observation of the electron is indirect for its electric charge can be measured, but we do not directly experience the electron. It is theory-laden in the first sense because the validity of the readings (as relatively accurate readings of what we want to measure) depends on the theories about the instruments and what is measured. They are theory-laden in the second sense because their significance is determined by their being placed in equations which define the electron. The observations in this sense cannot be described without reference to electrons. But then there is the third sense of theory-laden in which the observations can be described without reference to electrons. We can consider the observation simply as the visual gestalt and describe it by saying that the big hand is on the six. This observation is theory-laden because the fact that we have it and the way that we have it is influenced by our anterior understanding.

From the above analysis we can readily understand Polanyi's stress on skill in observing. As our theoretical abilities progress so will our observational abilities and vice versa. Skillful observation is a prerequisite for, and is oriented to, the higher activities of scientific method, specifically discovery and confirmation. Polanyi adds that

> The part of observation is to supply clues for the apprehension of reality; the process underlying scientific discovery. The apprehension of reality thus gained forms in its turn a clue to future observations; that is the process underlying verification.

But the development of theoretical thought also contributes to the development of observing as a skill. I cited previously an example from Polanyi of a doctor viewing an x-ray. The experienced doctor notices much more than the novice.

Also, like any other skill, observing relies on physiological processes, which can be considered part of our native endowment. The combination of this factor with others such as interest, knowledge, and so on, results in different people possessing the skill to different degrees. There is thus an irreducible personal element in perceiving. Often the differences between people are unsystematic. Observations may differ: within a certain range. Thus, Polanyi gives the example of the Fuegans visited by Charles Darwin and the Beagle. The natives did not notice the Beagle lying off shore. Here we have an instance of cultural differences influencing the range of "normal" perceiving. There is a set of different observations made by the Fuegans which differ· among themselves unsystematically, but which differ from those of the Europeans in a more systematic manner. Then~ there are examples of strictly systematic differences in perceiving among members of the same community. Polanyi cites the example of the astronomical assistant Kinnebrook whose observations of celestial motions differed systematically from those of his superior.

These reflections concerning the skillfulness of observing in science show that observations are theory-laden for Polanyi in the third sense of theory-laden. The fact that observations can be theory-laden in this manner illustrates the personal character of observing, for it is my body and my interest, knowledge, and values which influence what I perceive.

However, they are also theory-laden for Polanyi in the second sense. Above I cited two manners in which observations are determined to be significant. The first concerns the meaning of observations as "clues for the apprehension of reality." They become meaningful within the context of that apprehension. They then have an intangible meaning. Secondly, they can be significant as confirming instances, evidence for a theory. They then have a meaning within the context of verification. Now in both these cases it is an act of personal knowing which constitutes them as meaningful. Polanyi claims that "In both processes there is involved an intuition of the relation between observation and reality." In the first case it is an act of understanding and in the second a responsible judgment. The claim that they become meaningful because they are understood in an insight or intuition integrating them within a theory distinguishes Polanyi from the positivistic tradition in which theoretical terms gain their meaning from their relation to observational terms. For Polanyi it is the reverse, Observations and, hence, observational terms, gain their meaning from theories which integrate them. However, the meaning of observations is not uniquely determined by theory nor is theory uniquely determined by observations. Polanyi's theory of the logical unspecifiability of tacit integrations explains why it is possible to have a variety of theories explaining the same observations. Likewise, many perceptions have commonsense meanings, which means that they are interpreted, though this interpretation does not have the status of a scientific theory. This means that observations have an independence from theory, just as perception is independent of understanding. However, this does not mean that there is a set of neutral observations accessible to all men, for we have seen that cultural differences, training, and anterior understanding, knowledge, values and interest will influence what and how someone perceives something. Yet that

> perception will still maintain some independence from other acts, for it is a unique integration relying on its subsidiaries, but not completely conditioned by them. Also, the intelligible meaning of the perception is not evident in the perception as perceived. It is discovered in the understanding of the perception. Thus, theory exhibits an independence of observations for they are subsidiaries in an act of understanding. Because the focal "object" is logically unspecifiable in terms of its subsidiaries, the basic terms and relations of the explanatory system would not be definable in terms of the descriptive technical terms used to fix the data nor in terms of observations. They subsist "on their own," precluding the positivist reduction of laws to a neutral observation language or any observation language at all.

Now, the meaning of observation terms can also be independent of particular theories. First, the meaning of observation terms is often established to a certain extent before a theory is discovered, and they can retain this meaning after the theory is replaced. An example of this is mathematical measurements. The fact that something is measurable is an interpretation, but it is an interpretation which is compatible with a number of possible theories and which will not be rejected simply because these theories may be. Thus, we can partially understand the insistence that new theories must account for the observations for which older theories accounted. These observations can be independent of the particular theories and, therefore, the old theory's observation language at some point is commensurable with the observation language of the new theory. Again, the simplest example is to consider two mathematical theories which assume that the realities they are trying to understand are mathematizable. This does not mean that competing theories will be completely commensurable for there will be points where their interpretations of the observations diverge. However, it does mean that Polanyi is not open to many of the charges brought against Feyerabend based on the interpretation that on his view the observation languages of competing theories are completely incommensurable, because the meaning of the observations is determined solely by the theory. In its strongest form this view entails that either competing theories have no meaning in common, or if they do, that it is relatively trivial. This would mean that is it impossible for two theories to contradict each other, thereby eliminating one means of deciding between them.

> One of the epistemological claims on which Feyerabend bases his view is that "Experiences arise together with theoretical assumptions, not before them, and an experience without theory is just as incomprehensible as is (allegedly) a theory without experience." However, on Polanyi's view this is

> comprehensible. Animals certainly have experiences without theories. While with humans experience and theoretical assumptions can in fact develop together, it is possible to perceive something without knowing what it is, and it is possible to have a similar perception after I know what it is. Also, we question our experience, so it is in some Sense "given" for questioning, just as it is "given" for verification in science while experience for Polanyi is not an "unchangeable fundament" and while'" he may agree with the spirit of Feyerabend's assertion that "Galileo invents an experience that has metaphysical ingredients", the experience does not have these ingredients by virtue of being an experience, but by virtue of being understood, though the fact that we understand certain things will allow us to have experiences we may not have had, had we not understood them.

One of Feyerabend's main points, however, is that when people appeal to experience, they often appeal to experience as interpreted in a certain manner without adverting to their own interpretation. For them the facts may be considered as there for all to observe. Polanyi also rejects this naive realist interpretation of facts. If we appeal to facts, we appeal to more than our experience, and the presuppositions of our factual analyses should be investigated.

To summarize, perceptions are independent because perceiving is an act distinct from understanding. Perceptions and observations are theory-laden because they are understood. Observations need not derive their meaning completely from a particular theory, but in that theory may retain part of the meaning they had prior to the articulation of the theory. They may also retain this meaning after the theory has been rejected. This means that the meaning of observations in two competing theories need not be completely incommensurable. However, insofar as the observations gain meaning by being integrated into a theoretical framework, they are theory-laden in the second sense of the term. Polanyi did not address the problem of theory-ladenness as thoroughly as this, but I find this interpretation implied by his epistemology and by statements he has made about scientific facts.

Observations are also theory-laden in the first sense for Polanyi. This means

> ••• that the numbers giving longitudes, elevations, and times which enter into the

> formulae of celestial mechanics are not facts of experience. The facts are readings on the instruments of a particular observatory.

They are scientific facts insofar as they fit into theories by which we accredit the validity of the results of the instruments. It is because their scientific factualness results from their being placed in a theoretical context that they are not facts of experience. This can be generalized to most measurements.

We have seen that personal judgment is the decisive epistemological act for Polanyi. Our reflections on the theory-ladenness of observations lend support to this conclusion, if only to show that observation alone is insufficient to establish full contact with reality. Given the third sense of theory-ladenness we may conclude that there will often be some degree of uncertainty regarding observations insofar as observing is biologically based. Not only can the senses be deceptive, but different physiologies may give rise to differences in perception. Since perceiving occurs within a context partially constituted by understanding, responsible judgment and valuing, and that context influences what will be perceived, observation alone does not establish contact with reality. Finally, and most importantly, since observations are theory-laden in the first two senses, what is being observed is determined not through observation alone, but also through understanding. That the understanding is correct is established in judgment.

Additionally, the fact that observations constitute a manifold, the diversity of which we cannot control, lends credence to Polanyi's claim that we cannot "establish complete intellectual control over experience in terms of precise rules which can be formally set out and empirically tested." At this point we can at least conclude that if such control were possible, it would not be established on the level of perceiving and observing. This conclusion, correlated with the fact that there is an independence of observation from theory, partially undermines the Laplacian ideal of attaining a complete, exact knowledge of the present which would permit deductions of all future and past events. As we shall see later, the ideal of complete knowledge is effectively refuted by evidence of emergence in nature. However, we may conclude at the present time that exact observations are difficult to obtain given that we are embodied knowers. Most values are not exact, but are approximations. Even if they could be made, the ideal still would not be fulfilled. Because there is an independence of theory and the content of observations, theories are subject to confirmation through observation. It is this divergence between observing and theory which precludes certain predictions; that is, prediction where we have no reasonable doubt that the predicted value will be the actual value. As Polanyi notes, no formulas can foretell the actual readings on our instruments." Because the readings in fact often diverge from what is predicted, a personal judgment is required to determine effectively the significance of the readings.

It may be objected that our inability to attain complete and exact knowledge of a situation cannot effectively refute an argument based on the ideal of the attainment of such knowledge. Laplace's view concerns what would obtain in an ideal situation. Because the ideal situation does not occur, does not mean that the consequences of his argument are invalid. In a strict sense this is correct. Thus, to refute Laplace effectively requires an affirmation of the unsystematic element in nature. I will discuss that in following chapters. In addition, the fact that knowledge of the initial situation has proven extremely difficult to obtain for the reasons I have given is itself evidence for a lack of system in the universe.

## 3) UNDERSTANDING, EXPLANATION AND FACTS

Polanyi's argument against a comprehensive formal method extends into his account of explanation. Polanyi does not have an adequate account of explanation, though he does have much· to say about the role of theoretical thought and its relation to acts of knowing. First, I shall outline the general direction in which Polanyi thinks that an account of explanation should go, contrasting his approach with Carl Hempel's. Second, I shall formulate as best I can a notion of science as explanatory within the context of Polanyi's thought, using some clues he has provided, and borrowing from the similar philosophy of William Whewell. I shall then draw upon the discussion of the abstractness of explanations in Pierre Duhem's The Aim and Structure of Physical Theory and David Bohm's Causality and Chance in Modern Physics to elucidate Polanyi's argument that scientific knowledge exhibits an indeterminacy in its inception and its application.

In his last work, Meaning, Polanyi characterizes explanation

> ••• as a particular form of insight--an insight that relieves our puzzlement through the establishment of a more meaningful integration of parts of our experience, achieved through the subsumption of a natural law under a more general law.

It is problematic' that explanation has been identified with insight. It is more proper to suppose that the explanation is the content of the insight. The insight is the integrating which yields the "more meaningful integration." While it is also proper in common speech to identify insight with the content of understanding, this is not the meaning of insight which Polanyi uses in the discussion which provides the context of the above quotation. This transposition of meaning can be confusing. However, Polanyi's point seems to be that there is a particular type of insight which yields scientific explanations, as opposed to insights which lead to the acquisition of skills or other integrations.

Polanyi has pointed to a fruitful area of investigation. For example, if our perplexity is resolved through having an insight, then it may be possible to understand the nature of scientific insights through an analysis of scientific puzzlement. This would help define the subject area of science (or of the particular sciences in a more refined investigation) and thus what counts as an explanation in science. However, Polanyi appears to have philosophical reasons for not doing a detailed analysis of this type. He points out "that being puzzled implies a selective judgment." There are some things which scientists think it is worthwhile to be puzzled about and others which should not be investigated. These appraisals rest on the premises of science operative at the particular time. The premises of science cannot be fully specified for Polanyi. It may seem that this prevents us from arriving at a general theory of scientific puzzlement, since we cannot assemble the elements for getting the insight into the general form of these elements. I think that this problem can be circumvented by concentrating on the acts which give rise to any premises and investigating their general structures. However, this is another approach to the problem which Polanyi does not pursue.

What distinguishes scientific explanations from other explanations and from other contents of insights in science is that scientific explanation subsumes "a natural law under a more general law." Since Polanyi deliberately conflates the act of insight with explanation there are two aspects to a scientific explanation, the act of insight and the expression of the insight. Now, Polanyi seems to accept the deductive-nomological model of explanation insofar as this is the general form of the expression of the insight. At least his language in Meaning would indicate this. He characterizes Hempel's and Nagel's views of explanation as "subsuming a natural law within a more general law of which it is a special case," Then he states his view of explanation as an integration "achieved through the subsumption of a natural la.VI under a more general law," Since he uses virtually the same expression to characterize one aspect of his understanding of explanation and his understanding of their formulation of the theory it seems reasonable to conclude, that he accepts their theories of explanation as being at least partial accounts. What he objects to is the supposition that this theory is an exhaustive account of the matter. In particular, it fails to account for the fact that we accept a particular account as an explanation. In other words, it does not provide an exhaustive account of the criteria for determining what constitutes a relevant explanation. Some of the criteria can be specified, but not all, for not all the criteria are formal. That Hempel's view of explanation is inadequate is easily shown, That this in adequacy rests on his oversight of conscious activities can be concluded if one agrees that the criteria immanent in human intelligence guide us in our discovery and acceptance of explanations.

First, let me distinguish two meanings of "relevance". In one sense we may speak of an explanation being relevant in that it is an answer to a question which is puzzling us; for example, a particular "why-question", Thus, a psychological theory of human motivation may be relevant as part of an explanation of why a particular person smiled in a particular context, but it would not be relevant if we were trying to understand why a certain element combines with another element. Hempel's theory does provide some criteria for determining relevance in this sense. But there is another sense in which a particular form of expression is not relevant because it does not constitute an explanation at all. It is a well-accepted thesis that the deductive-nomological model does not adequately distinguish between what constitutes an explanation and what does not.

In its simplest form the deductive-nomological, or covering law, model of explanation is as follows. What is to be explained is expressed in the explanandum sentence. It is the explanandum phenomenon. For it to be explained it must be deduced from the set of explanans sentences. The set as a whole is termed the explanans. These sentences are of two types, statements of general laws and sentences expressing initial conditions. Explanation of the explanandum is equated with its deduction from the explanans. This also means that it is "subsumed under those laws."

This model works for some cases, but not for all. Bromberger and Scriven have criticized this model of explanation as being too broad. It admits examples which satisfy the criteria, but which we would not claim were explanatory. I shall provide a simple counterexample of Bromberger's here and have provided a more sophisticated and lengthy counterexample of his in the notes. Bromberger states:

> All of Cassandra's predictions always come true. (Cassandra is a computer.) Yesterday Cassandra predicted that it would rain today. But obviously that is not why it is raining.

Thus, the deductive-nomological model does not provide absolute criteria for determining what is an explanation and what is not.

However, it does provide some criteria for determining what is a relevant answer to a particular why-question. The rule that the explanandum must be deducible from the explanans limits the range of possible explanans.

As I shall show, the lack of sufficient formalized criteria is not the deficiency of particular theories, but is a limitation placed on theories in general. They must be applied, and in the application personal knowledge is intrinsically involved. In this instance we cannot derive sufficient criteria from the general form of expression for determining what counts as an explanation in a particular case and what does not. We must take into account the tacit powers of the scientist. Specifically, what will relieve his puzzlement must be specified. However, only he can know this, and then only incompletely and he can only know it after he has the insight or set of insights. Thus, an account of discovery is intrinsic to a complete account of explanation.

Bromberger's theory of explanation yields problems similar to Hempel's. He states that

> Our analysis, then, does not segregate good answers from poor ones, only correct ones from incorrect ones.

For Polanyi it is the personal appraisal of the scientist which distinguishes the appropriate from the inappropriate explanations. Because it is a creative act, we will never acquire an exhaustive set of rules for determining what counts as an explanation, or a good explanation. At some point the scientist must interpret the rules, and that interpretation finds its criteria in our natural mental powers. Michael Scriven adumbrates a "subjective" theory of explanation.

> What is a scientific explanation? It is a topically unified communication, the content of' which imparts understanding of some scientific phenomenon. And the better it is, the more efficiently and reliably it does this, i.e., with less redundancy and a higher over-all probability.

He appeals here to the quality of understanding as a criterion of what determines an explanation. However, he apparently does not \"want to accept all the implications of this approach. He notes that "Understanding is not a subjectively appraised state any more than knowing is; both are objectively testable and are, in fact, tested in examinations." On the contrary, I think that the fact that understanding can be tested is evidence for the fact that objectivity involves subjective appraisal. We must suppose that someone drew up the examinations which are a test of understanding and that their understanding was not subject to a test. How were their understanding of the test and the students' comprehension of it objectively established? If the claim is that it is subject to tests of another kind, the problem simply recurs. How is it determined that the person's understanding fulfills the criteria of objectivity? In short, we are faced with a variation of the argument that any appraisal in terms of rules is personal, for, in the long run, there are no rules for applying rules. Thus, if the objectivity of the test is appraised by someone in accord with rules, there is a personal appraisal on the part of the person appraising, The problem Scriven is faced with is that of accepting his capability of arriving at objective results on the basis of his own "subjective" mental powers, I shall return to this problem when I discuss confirmation in science.

Polanyi refers to William Whewell's philosophy of science with favor and it is in Whewell's notion of colligation of facts and the consilience of inductions that we find some approximation to the thrust of Polanyi's notion of explanation. First of all, for Whewell, there is a distinction between theory and facts. A theory

> "...may be described as a Thought which is contemplated distinct from Things and seen to agree with them; while a Fact is a combination of our thoughts with Things in so complete agreement that we do not regard them as separate ….

Whewell identifies induction, for the most part, with discovery. His account of induction is remarkably similar to Polanyi's account of tacit integration. Scientific induction provides us with a new content which unifies the facts. If this unification, or the casting of the facts into a new relationship, is true, then it is accepted as a fact also. Such an induction is the colligation of facts. Now, there are different kinds of facts. Each kind is arrived at by induction. However, the different kinds of facts can be related to one another by induction. In this case we have the consilience of inductions. Thus, at one time terrestrial mechanics was considered as a theory about a set of facts distinct from celestial mechanics. However, Newton brought the two kinds of facts together in his mechanics, which manifests, therefore, the consilience of inductions.

Likewise, when Polanyi discusses scientific explanation he stresses the integrative aspect rather than the deductive or any other logical aspect. He would seem, then, to be proposing a theory similar to Whewell's. However, it is not as complete. As I noted, an explanation involves the subsumption of a natural law under a more general law. However, is the first natural law an explanation? If it is, it must be the explanation of another natural law. But then, how do we get to the point of explaining our experience? Surely our experience is not a natural law and it is what we set out to explain in the first place, Polanyi needs, then, a notion of "subsuming experience" under a natural law, or a subsuming of correct, but partial common-sense understanding of experience under a natural law, He needs, in short, a view of the colligation of the facts as broad as Whewell's and applied to a theory of explanation, His theory of tacit integration is broad enough, but its implications are not fully developed, His view of explanation does encompass the consilience of inductions, but even here it is partial. What is a natural law for Polanyi? He fails to provide a definition, and this has contributed to the fostering of misunderstandings of his philosophy of science, especially his views concerning reduction, emergence, and hierarchies in nature,

There is a remarkable similarity in Polanyi's and Whewell's views of the nature of a fact. For Polanyi insight is an act of indwelling. Because it is an indwelling, if correct, it is the assimilating of the object, relation, or integration to ourselves; that is, to our mental powers. The fact of correct insights, then, would be the ground for assuming a "correspondence between the structure of comprehension and the structure of the comprehensive entity which is its object" in scientific knowing, Less generally, it means that theoretical thinking - in this case scientific explanation - in its assimilating of the object, results in the projection of the object beyond the subject if the theory is affirmed as true, just as the clues (e.g. in the use of a probe) are projected into the object. This means that for Polanyi, as for Whewell, facts in science are what are known in correct theories. Reality, then, is a compound of the tangible and the intangible, the sensible and the ideal. The fact that we can dwell in theories and that this indwelling can be so complete that we overlook the facts of our participation in knowing and of the unity of theory and the real is keenly shown by an example of Whewell's. He states:

> Induction has given them a unity which it is so far from costing us an effort to preserve, that it requires an effort to imagine it dissolved.

This refers to the facts that insights are irreversible achievements and that once they have been affirmed as correct they slip into the habitual texture of the mind. In the same vein he notes that

> Men ask Whether Eclipses follow a Cycle: Whether the Planets describe Ellipses; and they imagine that so long as they do not answer such questions rashly, they take nothing for granted. They do not recollect how much they assume in asking the question:--how far the conceptions of Cycles and of Ellipses are beyond the visible surface of the celestial phenomena: how many ages elapsed, how much thought, how much observation, were needed before men's thoughts were fashioned into the words which they now so familiarly use.

This problem will arise again when I discuss the sociology of scientific knowledge and the role of tradition and a-critical acceptance in scientific development.

There are two conclusions to be drawn from the above discussion. First, facts are not identified with the "empirically given" or the content of perception for Polanyi. That content is real, is a component of factual knowledge, but it is not the whole of that knowledge. Second, because facts are known through insight and because insight is an integration, there are no atomic facts. The basic unities in nature for Polanyi are comprehensive entities. The structure of these entities will be examined later. A final note is that some readers will notice that I have not discussed statistical explanation. This it because Polanyi does not have a full-fledged theory of statistical explanation. However, he does make a number of points concerning statistics, and I shall discuss some of them later.

I shall turn now to a discussion of the abstractness of scientific theories in general and of scientific laws in particular. By discussing their abstractness in relation to a remark Polanyi makes concerning the reproducibility of skills in accord with strict criteria I hope to extend Polanyi's analysis by providing additional grounding for his claim that scientific knowing is personal knowing. The abstractness of scientific theories has been discussed by many, but is brought out particularly well by David Bohm and Pierre Duhem. I shall adopt Bohm's meaning of "abstract".

> When one abstracts something, one simplifies it by conceptually taking it out of its full context. Usually, this is done by taking out what is common to a wide variety of similar

things.

Thus, he discusses the fact that experiments are sometimes attempts to isolate certain processes from what we consider to be extraneous factors so that we may determine if the relationship we think holds, in fact holds. As Duhem points out, the fact that a generalization to a natural law is abstract means that comprehension of any concrete situation requires an understanding of a set of laws. As we shall see, Duhem places a different significance on the abstractness of physical laws than Polanyi, but this does not affect the present discussion. Finally, in Personal Knowledge Polanyi discusses the difficulty science has duplicating human skills. I have already mentioned the reasons Polanyi gives for this type of knowledge not being fully specifiable. Naturally, if it is not fully specifiable, it cannot be duplicated by following explicit rules alone. Once we accept that one reason that rules are insufficient is because they are partial, it is only a small step to realize that they are insufficient because they are abstract.

> You obviously cannot adjust the curvature of your bicycle's path in proportion to the ratio of your unbalance over the square of your speed; and if you could you would fall off the machine, for there are a number of other factors to be taken into account in practice which are left out in the formulation of this rule.

In other words, we would need a set of maxims to begin to approximate to a comprehensive knowledge of what we should do. Because these maxims hold in all situations of such and such a kind, they do not respect the particularities of the individual situations. That these particularities be respected by scientific laws would require that we have sets of situations similar in all respects, including relationships to other situations. This does not seem to be the case, Thus, just as personal knowledge is required to mediate between the rules of art and the application of the art to the particular material or situation which faces the artist, so personal knowledge is required on the part of the scientist to mediate between scientific laws and an understanding of a particular situation in accord with those laws.

Moreover, insight is necessary to mediate between observations, or sets of observations, and laws and explanations which account for them, Given the unsystematic nature of most observations, it should not be too surprising that the same data can generally be accounted for by differing explanations. This is especially true if we consider laws which are mathematical in form, for there are an infinite number of equations which can account for any set of data. As Polanyi points out, it may be objected that the choice of equations is subject to a confirmatory check such as predictiveness. However, there is still an infinite number of equations which can account for the new data used as a check. Now, the fact is that equations are often arrived at and they do exhibit remarkable predictive power, Polanyi's claim is that they are discovered by a tacit integration which is not completely ru1e governed; for what rule would we use to select between equations, each of which effectively accounts for the data?

Cunningham interprets Polanyi as arguing against objectivity in this argument, However, I have shown that Po1anyi is not a relativist and that he affirms the self-transcendence of the knower in the affirmation of an independent reality, Contrary to what Cunningham concludes, Po1anyi is not arguing that because there is an infinity of equations we have no good basis for preferring one over the other and, thus, cannot affirm our choice of one of them as objective, Nor is he disqualifying "predictive power as a rule for objective selection," As we shall see, Po1anyi accepts predictive power as a legitimate criterion in confirmation, What he is claiming is that scientists often do have good bases for preferring one equation over the other, but these are not fully specifiable, He is arguing against the possibility of a fully forma1izab1e method of arriving at results, not against the objectivity of the results we arrive at through personal knowledge. Perhaps this will become clearer if I turn to the topic of confirmation in science,

## 4) CONFIRMATION AND THE ACCEPTANCE OF THEORIES

That a personal appraisal must enter into confirmation can be shown by discussing three major questions in contemporary philosophy of science. First, what is a confirming instance? Second, how do we determine the degree of confirmation of a hypothesis? Finally, when is a theory accepted by science?

To be scientific any theory must have implications which can be subjected to empirical test. The first problem of determining what constitutes a confirming instance is that of determining which of these implications to test. The inadequacy of relying merely on logical considerations is indicated by Carl Hempel's famous analysis of the paradox of the ravens. It seems reasonable that any statement of the form "All A's are B's", such as "All ravens are black" has as a confirming instance the discovery of a black raven. Additionally, it is sound to suppose that whatever confirms one statement also confirms any logically equivalent statement. Thus, "All non-black things are not ravens" (All non-B's are non-A's) has as a confirming instance the existence of any non-black thing. This would also be a confirming instance of the hypothesis "All ravens are black.” The scope of the problem can be expanded by considering other forms of statements logically equivalent to "All ravens are black." The essential problem is, however, that on these criteria almost anything can be seen as a confirming instance of a universal hypothesis. (The exceptions being B's Which are non-A's)

Most writers on be subject follow Hempel in attempting to formulate a set of rules limiting what counts as a confirming instance. Polanyi has no quarrel with this approach per ~ as long as one does not think that a set of rules is sufficient for determining a confirming instance. Regarding confirmation in general, Polanyi has stated

> Nor am I saying that there are no rules to guide verification, but only that there are none which can be relied on in the last resort.

He mentions three "powerful criteria" "reproducibility of results; agreement between determinations made by different and independent methods; fulfillment of predictions." Yet, any set of rules can have only a subsidiary function in judging what counts as a confirming instance. Additionally, the rules must be discovered and judged as appropriate. Now, there are no rules for applying rules. ('Thus, the acceptance of the general rules as well as their application in particular instances is in turn based on a personal appraisal or judgment. However, the judgment is not any judgment. It is a scientific judgment. In making it the scientist is guided by scientific theory. Thus, the scientist will determine what in terms of his theory functions as a significant test of his theory. Conversely, he will exclude what he considers to be insignificant. In either case he will rely on his own standards of significance and insignificance. Just as scientists select hypotheses which they think have a high probability of being true, they select tests l1hich have a high probability of confirming or disconfirming their hypotheses. This can only be determined in the light of a personal judgment guided by theory and heuristic anticipation. As Polanyi points out, "Things are not labeled 'evidence' in nature, but are evidence only to the extent to which they are accepted as such by us as observers."

Besides the problem of determining which implications of the theory to test, there is the problem of determining whether or not the test confirms the hypothesis. A commonly accepted view is that scientists test their theories by determining whether the predicted results are the acquired results. As previously mentioned, while the conditions for accepting a theory as true may be precisely specified from an abstract or theoretical stance, in actual practice there is some imprecision involved. The scientist must be skillful in observing. The imprecision of observation may be relatively obvious in the biological and human sciences, but even in the "exact." sciences there is some indeterminacy involved which can only be resolved on the basis of the scientist's personal judgment. For example, is a reading on the instrument .752 or .751? Is the color band in the spectrum blue or blue-green? These questions call for a personal appraisal. The indeterminacy involved in any observational science is compounded by the fact that tests are many. Thus, a range of values is attained for any measurable quantity. Which values are to be accepted and which are to be dismissed as due to observational error or other causes? Which differences are significant and which are not? A result which apparently contradicts a theory and thus falsifies or disconfirms it may be dismissed as an anomaly. In addition, Polanyi notes that

> We often refuse to accept an alleged scientific proof largely because on general grounds we are reluctant to believe what it tries to prove. It was the presumption of Wohler and Lie big against the idea that fermentat10n was due to living cells which made them disregard the evidence in its favor.

We may conclude, then, that in determining which instances confirm a non- statistical scientific statement personal judgment enters in at least four ways. The scientist selects the tests. He must be skillful in reading his instruments, observing with his microscope and so on. He must determine which results are significant and which are not. Finally, his general background will condition his acceptance of the theory in the first place, and consequently his acceptance of tests of the theory as confirming.

All four of these types of personal judgment also enter into the confirming of a statistical hypothesis, In fact, the determining of the significance of a range of values will often be done in terms of statistic~. However, statements of probability differ from natural laws. A statement of a correlation can, at least ideally, be disconfirmed by the failure of predicted results to materialize, However', a probability statement allows for a variety of events, They can either occur or not occur, It simply assigns a probability for their occurrence, Therefore, they cannot "be strictly contradicted by experience," If the sample population fails to yield statistics in accord with the probabilities, this is not taken as disproving the probabilities, but may even confirm them. For example, the probability of a six coming up on an unbiased die is 1/6. Likewise, with the rest of the numbers on the die. But in any finite number of throws we do not expect each number to come up exactly 1/6 of the time. Instead we expect some nonsystematic divergence from 1/6.

There is, however, a way of testing statistical hypotheses outlined by Sir Ronald Fisher in his Design of Experiments. As Polanyi points out, it too depends ultimately on personal judgment. Let us suppose that we wish to determine if a die is biased in favor of rolling 6's 1/3 of the time. This will be our null hypothesis. Our other hypothesis will be that it is not biased. We can roll the die a number of times, It is possible to determine the probability of the results of this test given that the die is biased and given that it is not biased, According to Fisher's theory, if the probability of getting results 'which indicate that the die is biased is less than 5 (the percentage differs for different kinds of studies) if these results were due strictly to chance, then if these results are obtained, the null hypothesis should be accepted. This figure of 5 is accepted because it seems reasonable (5 of the time we would be in error). It rests, therefore, on a personal appraisal.

The involvement of personal judgment in the confirmation of statistical hypotheses is magnified if we take our background assumptions into account. Thus, in Rhine's parapsychological experiments ••••• the probabilities of the observed results as evaluated on the basis of the null hypothesis had to fall very far below 5 per cent in order to shake one's belief in it." In general, then,

> The contradiction must be established by a personal act of appraisal which rejects certain possibilities as being too improbable to be entertained as true.

The second question I wish to consider concerns determining the degree of confirmation of a theory. For Polanyi there are two meanings of probability. The first meaning is the statistical meaning; which I assume he accepts as referring to the relative frequency of events. The second concerns the probability of a statement, law or theory being true. It is the second kind of probability that some philosophers of science try to determine when they discuss degrees of confirmation. Many philosophers have attempted to provide some numerical measure of scientific hypotheses. Polanyi agrees that there is some quantitative evaluation of the probability of hypotheses. We can be more or less sure of hypotheses. We may be surprised at the occurrence of a statistically rare event. The degree of our surprise may be roughly commensurate with the low statistical probability of the event. However, Polanyi categorically rejects any attempt to measure the probability of a hypothesis being true.

Any assertion is made with some degree of commitment by the person making it, It follows, then, that a statement simply printed on a page is an incomplete symbol. For this reason Polanyi advocates placing an assertion sign ( ) in front of statements to denote that they are asserted by the particular person asserting them. 1tI believe" functions in the same manner as the assertion sign. By itself it is incomplete, just as an unasserted statement is. However, if the statement and ."I believe" are conjoined, then the personal endorsement of the statement is symbolized. Now, probability statements by themselves are impersonal. To be completed, they must be conjoined with a symbolization of my personal endorsement. However, if the statement is symbolized in such a manner that it could be expressed by anyone, then the personal coefficient is overlooked. This is the case with a bare probability statement. Thus, the degree of personal commitment cannot be expressed by a probability statement.

The determination of the probable truth of a hypothesis is a personal judgment. Hypotheses and theories are accepted in science as the result of such appraisals. If these appraisals cannot be quantified, and if we can not conclusively confirm many scientific theories, even some which we accept, on what basis do we determine that one scientific theory has a greater probability of being true than another? Confirmation would be a factor, but for Polanyi it is not the determining factor. We have seen that what counts as confirmation is determined by the theory and the scientist's personal judgment in the light of that theory. If the choice is between two theories, each of which has a series of confirming instances to its credit, then the fact of confirmation is not decisive. It follows, then, that determining the degree of confirmation is not the same as determining the probability of a theory being true, as some philosophers would have it. There must be some anticipation of reality which extends beyond what we presently know and surmise in order that we may evaluate the probable truth of what we hold. Polanyi finds intellectual passions intrinsic to this anticipation. Yet before we can discuss the anticipation which is used as a guide in determining if a theory is probably true, we must show how passions can guide us to the acceptance of a theory as valuable for science. We must jump ahead to a third question, '~hat determines a theory's being accepted by science?" A more complete answer to this question will be given in the next chapter in the discussion of scientific community.

There are at least three factors to which intellectual passions respond. They are intellectual beauty, truth and value. When we discover something we do not merely have an idea, but we have an emotional appreciation of the idea also. Usually, the better the idea, the finer the appreciation. Such is the appreciation of intellectual beauty. Ideas can be true or false. For Polanyi "to attribute reality to something is to express the belief that its presence will yet show up in an indefinite number of ways." Reality is what has been discovered and accepted as so and what remains to be discovered. We have, then, a vision of reality to which we are passionately attuned which can be used in science as a guide for determining what is of scientific value.

> Our vision of reality, to which our sense of scientific beauty responds, must suggest to us the kind of questions that it should be reasonable and interesting to explore. It should recommend the kind of conceptions and empirical relations that are intrinsically plausible and which should therefore be up held, even when some evidence seems to contradict them, and tell us also … what empirical evidence to reject as specious, even though there is evidence for them….

Polanyi is here developing Henri Poincare's theory of intellectual beauty. In the1light of this vision, any scientific statement or theory will be evaluated in terms of three criteria: certainty or accuracy, systematic relevance, and intrinsic interest. The degree to which these three criteria are met will vary with different theories. Yet a failure to meet one of the criteria can be overcome by meeting the others to a greater degree. A statement in physics may have great accuracy, but little intrinsic interest or systematic relevance. A psychological theory may have little accuracy but much intrinsic interest and be a profound systematic achievement. In any case, the acceptance of the statement or theory will be based on a personal appraisal in terms of a passionately adhered-to intellectual vision.

As the above quotation suggests, just as passions can help guide us in selecting which theories and statements are to be accepted as scientific, so they guide us to the truth. Such is their heuristic function. They keep us on the road to discovery and provide us with intimations of achievement. Also, they provide us with intimations of the fruitfulness of our discoveries, intimations of their confirmation in presently unspecifiable ways. By embodying our heuristic anticipation of reality they provide a criterion for determining whether a theory is probably true or probably false. It would appear that the selective and the heuristic functions of intellectual passions combine in the appraisal which accepts a theory as true.

Several criteria are put forward for the acceptance of theories other than the fact that in theoretical understanding we make contact with reality and that we appraise this contact in terms of a passionately informed heuristic vision, or intellectual beauty. Beside degree of confirmation, some of the criteria are: explanatory power, simplicity, economy, coherence, and fruitfulness. It can be shown that appreciation of each of these is an appreciation of intellectual beauty. This is clearly so with explanatory power. A theory is beautiful precisely because it explains, and it is more beautiful if it is true. Simplicity and economy have been used interchangeably (i.e., Quine calls simplicity what Mach calls economy), but they can also be distinguished. An explanation which hits the mark is simple. There is a lack of superfluities. Why do scientists pick the simplest curve when they can choose from an infinity of curves? Simply because there is no reason to choose a more complex one. One would have to understand more to understand the theory, but this greater understanding would not be matched by a greater understanding of the data. Hence, it would be superfluous. According to Mach, the economical function of theories is two-fold. First, one law can represent many experiences. Second, this permits economy of thinking, for to think of the one law is to think of all the experiences simultaneously; that is, without laboriously reconstructing them in detail. The economy of a theory can be appraised in terms of beauty. Henri Poincare notes that in mathematics "'the useful combinations are precisely the most beautiful--the best able to charm this special sensibility that all mathematicians know." Coherence is the product of understanding, that which is appreciated by our intellectual passions. The fruitfulness of a theory is its predictive and heuristic power. Now, fruitfulness by itself cannot be a standard for accepting a theory. The fruitfulness of a theory can only be decisively affirmed after its implications have been confirmed (both its logical implications and its heuristic implications, the discoveries which result from using the theory as a clue. However, the "intimation of its fruitfulness" can be a standard. But this is simply the recognition that the theory will manifest itself in the future in currently unspecifiable ways. This intimation is simply the passionate appraisal Polanyi discusses,

Finally, for Polanyi it is possible to: use these criteria as pseudo-substitutes for truth. It seems that the simplest, most economical theory would be the true one. Likewise, it should have the greatest explanatory power, coherence, and fruitfulness. However, as pseudo-substitutes these criteria are used in assertions to proclaim that a theory is true while the fact that this is being done is denied. It may be claimed that one theory is not closer to the truth than another, but is simpler or more coherent. However, these criteria are used to reject some theories and to accept others. It would be absurd for a scientist to accept a theory which would lead to no new results or would not "manifest itself in unpredictable ways." If scientists do choose theories which they think will prove fruitful, then, for Polanyi, they are choosing theories which for them are probably true. The insistence of philosophers of science on criteria of simplicity, coherence, explanatory power and so on is an insistence on qualities of theories which can only be evaluated in the light of an intrinsically passionate heuristic vision which is a guide for judging with universal intent; that is, for Polanyi, theories can only be evaluated within the light of personal knowledge.

Though the intellectual beauty of a theory is a mark of its contact with reality, merely formal elegance is also beautiful. Polanyi points out that there is a problem of distinguishing intellectual beauty which achieves a contact with reality from merely formal attractiveness which does not. This problem is most acute for Polanyi because he often writes of intellectual passions as if they were the final arbiter in resolving the problem. Thus, I can distinguish standards which are passionately adhered to and standards which are passionately determined: that is, where the standards are set by the passions. The latter is Polanyi's stand. He notes

> There is present a personal component, inarticulate and passionate, which declares our standards of values, drives us to fulfill them and judges our performance by these sel£-set

standards.

That he is referring to intellectual values and affirmations of reality is shown by this affirmation from the same discussion.

> The standards of scientific value and of inventive ingenuity must still be satisfied, and these standards are set by the scientist's and the engineer's own intellectual passions.

There are conditions on the side of the person which must be satisfied before the real is affirmed as the real. But these are not the only conditions which must be satisfied, nor are the passions the final arbiter of the truth. There is a problem in the first place because it must be determined if the intellectual beauty is of the merely formal or if it is of the real. Polanyi gives examples from twentieth-century physics where this question had to be resolved, including de Broglie's initial contributions to wave mechanics. His examiners were not sure of the merit of his doctoral work and sought Einstein's opinion, which was favorable to de Broglie. Simpler examples are found in the evolution of the atomic theory. For example, the law of definite proportions proposed around the turn of the eighteenth century stated that "compounds are formed by the combination of fixed numbers of different atoms. This in turn suggested the law of equivalent proportions. If for any two substances, there are certain weights that are equivalent in their capacity for reaction with some third substance, the ratio of such weights is the same regardless of what the third substance may be. Additionally, the ratios were expressed in small whole numbers. The formal beauty of these hypotheses is evident in their mathematical simplicity and the promise they held for directing further chemical research. However, there was still the question of whether the formal beauty was more than formal, for simple ratios were not attained, though they were approximated. As these examples indicate, the problem is not resolved by the intimation of intellectual beauty alone. The final arbiter is the judgment by which we determine if our standards have been met. In judging we distinguish the real object from the mere object of thought, the formal object. Again, it cannot be done by the intimation of beauty alone, for the beauty cannot be distinguished until the judgment occurs. Po1anyi notes that the passions are not the only conditions when he discusses the necessity of using observation as a guide in science. His main point is that objectivity is not merely consistent with, but is also constituted by a passionate appreciation of the intelligible and of the real.

5) SCIENTIFIC REALISM

Polanyi's scientific realism presents many problems, one of which is especially important. How can we substantiate the reality of theoretical entities? Their reality is essential to his metaphysical view. Polanyi never addressed himself to this problem in detail. I shall show that the reality of theoretical entities can be conclusively established in a manner consistent with his thought. Scientific method is a specialization of tacit knowing. The argument for its objectivity is the same as that for knowing in general, unless there is something peculiar about the objects of scientific knowing. One trend of empiricism claims that there is, that some of the objects of science are unobservable and, since the real is what can be experienced, these postulated objects of science are merely theoretical entities. They derive their "existence" from thought alone and are countenanced in science only because of the role they play in theories. In contrast, reality for Polanyi is known both by experiencing and by thought. ••• discovery of objective truth in science consists in the apprehension of a rationality which commands our respect and arouses our contemplative admiration; ••• such discovery, while using the experience of our senses as clues, transcends this experience by embracing the vision of a reality beyond the impressions of our senses, a vision which speaks for itself in guiding us to an even deeper understanding of reality … Thus, Po1anyi is not an empiricist in the sense that reality is confined to the sensible, the observable and so on. But his view of knowing is empirical because for him we are trying to understand our experience, and our experience is both a clue to discovery of reality and, within the more critical attitude of verification, a check on our conclusions. His insistence on a meaningful element in reality does not make him an idealist. The meaning which is discovered is posited as independent of the person. Likewise, it is not created by the subject, but discovered by him. Knowing is self-transcendence. The effort of knowing is thus guided by a sense of obligation towards the truth: by an effort to submit to reality. Knowing reality is not only a commitment to something beyond us, but also a commitment to something beyond the scope of our achieved knowledge. We are committed to a "vision of reality" which extends beyond our knowledge because knowing is claiming to have established contact with the real, and because we have not grasped all of the real. Po1anyi asks “Why do we entrust the life and guidance of our thoughts to our conceptions?” Because we believe that their manifest rationality is due to their being in contact with domains of reality, of which they have grasped one aspect. And it is an aspect which we believe will "manifest itself in unexpected ways in the future." This is the strongest sense in which something is real for us, including the objects of scientific theories. The evidence for this position is found in the person's experience of himself. C. P. Snow, in discussing differences he had with Polanyi, said he found himself at right angles to his position. It is this insistence on self-experience, the challenge to indwell his philosophy, which, I think, places Polanyi at right angles to the positivistic and analytic positions in the philosophy of science. A complete dialogue with these positions is beyond the scope of this work, but the fact of their existence raises a problem. Many philosophers of science, including some who are scientists themselves, do not claim to have made a commitment to reality in the sense that Polanyi understands it. This has led some to the conclusion that some of the objects postulated by theories do not exist, because they are merely 'theoretical entities' I will show how they are wrong in a manner consistent with Polanyi's thoughts about reality. Because of the variety and complexity of non-realist positions I must confine myself to general arguments and equally general characterizations of positions. I hope that lack of detail is not also a lack of precision and understanding. It has virtually become a methodological precept to regard nothing as certain in science. This is a lesson learned from the fact of many revolutions in science, particularly the displacement of classical mechanics by relativity. However, there are many scientific facts of which we can be sure beyond a reasonable doubt. Eclipses do occur. At least from one reference frame we can say that the earth travels around the sun. There are inherited characteristics and they have something to do with DNA. And so on. But it is also the case that there are many outstanding questions in science. Some of these have answers, but they are accepted as incomplete or simply as provisional. Given the history of science it seems reasonable to conclude that many present theories will be replaced in the future. Thus, many scientific judgments are considered as merely probable. Scientists withhold their unconditional commitment to their truth. As Polanyi points out, this is a commitment on the part of the scientist. He is committed to the view that his conclusions in a certain area are merely probable. Because they are merely probable, the question of their relation to reality arises. It is consistent with Polanyi's view to consider science as realistic in intent, even if contact with reality is not established. Insofar as theories are merely theoretical they are attempts to understand reality which may or may not be true. But we develop and pursue them in the hopes that they will prove to be true, or at the very least lead us to a new theoretical vision which is true. Their relation to reality, insofar as they are merely probable, is heuristic. However, because there seems to be so little certainty in science it is easy to consider science as merely theoretical. Theories have a function other than getting us in touch with reality, or they get us in touch with reality, but reality is not "represented" by the theories. It will be useful if I present a well-known model of scientific theories and discuss this problem in its context. This model is presented by Ernest Nagel in The Structure of Science. Just as in the deductive-nomological view of explanation particular instances are explained by being deduced from a set of laws and a set of propositions concerning other particular instances, so laws are deducible from the theories which contain them. As in any deductive system some laws must be taken as axioms, and it is from these that the others are deduced. In some well-developed theories such as the kinetic theory of gases, the laws which serve as axioms relate unobservables to one another. These are termed theoretical laws. From them laws may be deduced which relate terms which refer to observables to one another. These laws are experiential laws. The laws are linked by correspondence rules which relate theoretical terms to observational terms. Such a correspondence rule is "The temperature of a gas is the mean kinetic energy of its molecules." Molecules are unobservable. Hence they are theoretical entities. Temperature can be an observational term for

> … the meaning of "temperature~ is frequently explained in physics in terms of the volume expansions of liquids and gases or in terms of other observable behaviors of bodies; in such cases the explication of "temperature" is given by way of observable primitives.

Though Nagel admits that there is no precise manner of distinguishing experimental and theoretical laws the distinction is asserted. The non-logical terms of the experimental laws are partially defined through their association ·'with at least one overt procedure for predicating the term of some observationally identifiable trait when certain specified circumstances are realized." An experimental law, then, is a relation discovered in observations. An example is Boyle's law which relates the pressure and volume of ideal gases. Clearly, in Nagel's theory a theoretical law cannot be discovered in observation, since its non-logical terms do not refer to the data of observation. This leads to a further distinction between the two. The experimental law has a meaning which is independent of the theoretical laws which explain it, and the theoretical terms do not enjoy this independence from experimental terms. Nagel approaches an empiricist criterion of meaning. Unless the terms of the theoretical laws can be connected with the experimental laws through correspondence rules, they are meaningless. They are terms of mere statement forms which comprise an abstract calculus. Nagel states that "the postulates assert nothing, since they are statement-forms rather than statements." He would seem, then, to adhere to the possibility of a non-realist position on the question of theoretical entities. We find a similar position in the writings of Ernest Mach and Pierre Duhem. Duhem also distinguishes experimental and theoretical laws. Theoretical laws do not relate real things to one another directly, but merely relate symbols. "The sole purpose of physical theory is to provide a re presentation and classification of experimental laws .... But because the representative form is symbolic, there is a gap between the representation and the reality represented. For Mach the real is sensations. The function of science is to reconstruct the real economically. Thus, Mach states that "In nature there is no law of refraction, only different cases of refraction." The law of refraction is a representation which allows us to think economically of many instances of refraction simultaneously. It permits an economical reconstruction of reality. It also has the function of rendering experiences predictable, by going beyond experience.

> … this is exactly what we do when we imagine a moving body which has just disappeared behind a pillar, or a comet at the moment invisible, as continuing its motion and retaining its previously observed properties. We do this that we may not be surprised by its reappearance. We fill out the gaps in experience by the ideas that experience suggests.

However, these ideas are not of realities, but of such things as atoms which are "mental artifices". Again, the logical positivists identified the real with the observable. Many of them insisted that a statement must be verifiable to be meaningful. Thus, they attempted to reduce theoretical language to observational language to explicate its meaning. The pattern which emerges here is that reality is denied to those things which because of their nature can only be objects of thought. That is, they cannot be directly observed or imagined. They cannot be directly observed because we lack the natural capacity to observe them, and they cannot be imagined because we cannot have an experience of them from which to derive a representative, verifiable image. One problem with non-realist theories of science is that they do not explain why science "works". Why should predictions be realized? Why should the postulation of theoretical entities prove so valuable in organizing our experience? J. J. C. Smart points out that to the realist the non-realists require a cosmic coincidence to account for the success of theories. But because this is a coincidence, it is really no explanation at all. For Polanyi, when we accept something as real we expect it to -manifest itself in unexpected ways in the future." If our theories have more than predictive power, if they have heuristic value which cannot be specified at the present time, but only intimated, then there should be some non-coincidental correlation between them and reality. Is this correlation that they are merely impoverished representations of reality, where reality means what is empirically "given" in the wide sense of that term? The answer to this question lies in epistemological analysis. I have shown that for Polanyi reality is both ideal and empirical. Thus, the correlation is one of identity in the sense that what is understood correctly is the real. In his well-known article "The Ontological Status of Theoretical Entities" Grover Maxwell argues against the non-realist position by claiming that there is continuity between observables and unobservables. First, there is & continuity on the side of observing. When do we observe the real thing (or data, or object, and so on)? When is direct observation replaced by indirect observation? When do we switch from observational to theoretical language? He notes that

> … there is, in principle, a continuous series beginning with looking through a vacuum and containing these as members: looking through a windowpane, looking through glasses, looking through binoculars, looking through a low-power microscope, looking through a high-power microscope, etc., in the order given …. (W)e are left without criteria which would enable us to draw a non-arbitrary line between observation and theory.

Where that line is to be drawn is determined by the theoretical context. He notes

> For example, if we are determining the resolving characteristics of a certain microscope, we would certainly draw the line beyond ordinary spectacles, probably beyond simple magnifying glasses, and possibly beyond another microscope with a lower power of resolution.

This continuity on the side of observing is matched by a similar continuity on the side of the object.

> Are we to say that a large protein molecule (e.g., a virus) which can be "seen" only with an electron microscope is a little less real or exists to somewhat less an extent than does a molecule of a polymer which can be seen with an optical microscope? And does a hydrogen molecule partake of only an infinitesimal portion of existence or reality?

He concludes that the continuity from observables to unobservables should not be equated with that from existence to non-existence.

Besides this continuity on the side of observing and what is observable, there is from a personal perspective continuity on the side of the unobservable. Though "temperature" may be an observational term, this does not mean that temperature is solely an object or content of experience. In fact, temperature is an object of thought. A piece of steel and a piece of wood can have the same temperature, but the piece of steel will feel cooler to the touch. Temperature has a meaning fixed independently of sensible experience. Some measure of independence is gained by operationally defining it as the length of a column of mercury, alcohol or some other liquid. A greater degree of independence is had by defining the temperature of a gas, for example, as the mean kinetic energy of its molecules. The same holds for the basic terms of classical mechanics. There are many forces which can be measured, but in the measuring we go beyond experience. "Force" has a meaning beyond "pushing and pulling". Mass is not directly experienced, and there are motions which are not experienced in certain reference frames. Because temperature, force and mass are objects of thought does this mean that they do not exist? This would seem to be the conclusion that the non-realist is forced into if he denies reality to theoretical entities. We may discover that these objects of thought do not exist, but this discovery will itself be theoretical. Intangible meaning extends throughout science. To call any object or content of science unreal simply because it is unobservable is to consign the whole project to the realm of the merely possible. The distinction between observational and theoretical terms does not seem to be as sharp as Nagel thinks. Just as there are experimental procedures for determining temperature, so there are such procedures for drawing conclusions about electrons. For example, we can measure their charge in experimental situations. Undoubtedly part of the meaning of temperature is fixed in terms of "observational primitives", but certainly the whole meaning of temperature is not. For example, the temperature of the human body can be determined by equating it with the expansion of a column of mercury. But the temperature of the human body is a property of the body and not of the column of mercury. Thus, the meaning of the temperature of the human body is not given by understanding the temperature of the column of mercury alone. Temperature is related to a number of other things and events. To understand the meaning of temperature is to ascertain this network of relations.

Turning to the other extreme of the observational-theoretical distinction, we encounter similar problems. Nagel reduces theoretical laws to certain aspects of them. What he describes as a theoretical law is no law at all. It is simply a mathematical equation. As merely mathematical it may bear no relation to reality. However, a theoretical law is at least a hypothetical relation between properties of things, which in many cases is a mathematical relation. Observations, then, are related to one another (integrated) within a network of theoretical notions.

Po1anyi stresses other aspects of the continuity of intangible meaning. He considers "truth and justice and moral and artistic integrity" to be intangible. To disavow the reality of intangibles in science is to threaten the reality of other meanings we may recognize in our daily living.

All laws are theoretical to some degree. Do things fall into such and such a pattern? Usually the pattern is not given all at once as is the print of a dress. Rather its discovery and the pattern itself often extend over long periods of time. No one has observed all of evolution. In a similar vein, Kepler did not observe the planets describing an ellipse, nor did Copernicus observe them going around the sun. What must be connected are disparate instances in time and space, and there is no special vantage point in the universe from which to watch things unfolding. Even if the connections to be sought are only between elements of experience, the fact that the connections must be sought and that they connect disparate experiences shows that the principle of connection is beyond the tacit integrating of perception. As Polanyi suggests, it is discovered in understanding.

In summary, then, it seems reasonable to suppose that there are real entities which cannot be observed directly. The basic question is “Are we trying to understand them?" If we are, then science is realistic in intent. The real problem in science is not whether theoretical entities exist. The real problem is determining what they are.

## 6) THEORIES

Given the discussion of this chapter, what can we conclude concerning the status of theories for Polanyi? They are objective in the three senses discussed at the beginning of the chapter. However, their objectivity is not such that they are completely independent of knowers. They are discovered, accepted, and implemented by persons. Their abstractness demands a personal mediation by the scientist. The universality, regularity, and systematic nature of theories is not matched by data as not yet understood which, even if considered potentially systematic, often appears as particular, irregular and unsystematic, In addition, the interference of other factors not accounted for by the laws or theories adds a further degree of irregularity as far as the theory is concerned. Experimentation tries to eliminate this. But where experiments are not feasible or where it is a matter of applying laws in concrete situations, the scientist has to mediate between the theory and the situation. A similar situation applies in confirming a theory.

In these instances, theories are at the focus of our attention. However, they can also function subsidiarily. Once we have assimilated them they become latent knowledge. They are not adverted to in themselves, or in their explicit form but are used heuristically to gain more theoretical understanding, or to understand a situation in accord with the theoretical understanding we happen to have achieved. They are also used in solving practical problems in applied science. Just as one's use of mathematics differs from the manner in which one understands how to express it systematically, so the use of scientific knowledge is not a process of explicitly stating all steps in reasoning and logically correlating them with one another in a rigorous manner. I believe it is this difference between expressing scientific theories and using them which underlies Reichenbach’s distinction discussed last Chapter.

Polanyi illustrates the use of latent knowledge by discussing the way in which we use maps. A roadmap, for example, provides us with information which we can use in a variety of ways, depending on our purposes. Just as the laws of Newtonian mechanics permit a set of interesting derivations of laws of motion which can in turn be applied in a variety of circumstances, so a map provides for an infinite number of routes for possible trips. Though we attend to the map in our use of it, we are not concerned with the map per se, but the possible itinerary.   Similarly, once it 1s accepted and assimilated our interest shifts from the theory itself to the concrete situation or the further theoretical problems we are working on while relying on the theory. The map as an articulate framework gives us added intellectual power opening added possibilities-for action. The same is true of theories. They enable the scientist to solve certain kinds of problems and suggest other problems which may be solvable given additional theoretical knowledge.

We have also seen that theories can neither be discovered nor applied without some skill on the part of the person. This is most obvious where observations are concerned and where experiments are performed.

It is less obvious, but no less the case, if one is concerned with discovering and applying laws using mathematics, for mathematical computation and derivation are both skills.

However, what I have not yet discussed is perhaps the most outstanding characteristic of theories. Through them we have a comprehensive view of some part of reality, or at least try to approach such a view. For Polanyi, this means that "Transcendence which renders an empirical theory irrefutable by experience is present in every form of idealization." We have already seen that confirmation is not decisive for the acceptance of one theory over another, for what confirmation is in a particular instance is usually determined in the context of the theory in question. However, in any tacit integration elements are integrated coherently. Polanyi notes that

> An integration established in this summary manner will often override single items of contrary evidence. It can only be damaged by new contradictory facts if these items are absorbed in an alternative integration which disrupts the one previously established.

Thus, we have seen that data which contradicts a theory only becomes evidence against it if it is considered significant. It can be considered significant only if it is conceived as within an actual or potential alternative integration. Otherwise, it may be dismissed as a mere anomaly.

Polanyi's view also implies that while theories can absorb a certain amount of contrary data by leaving its understanding in terms of the theory for the future, even if the evidence is deemed irreconcilable with the theory, the theory cannot be totally rejected unless an alternative theory is developed. What is to guide research until the new theory arises? Indeed, the need for the new theory and the set of problems to be solved by it is conceivable in terms of the old theory. The coherence afforded by the theory and its function as latent knowledge demand its pragmatic acceptance until a new theory is developed.

However, Polanyi contends that mere pragmatic acceptance virtually never occurs. We accept theories because they reveal aspects of reality, and any pragmatic acceptance would be within the context of our attempts to discover more about reality. Thus, scientific controversies concerning the acceptance of rival theories are about the acceptance of alternative views of some set of aspects of reality. Polanyi's understanding of the acceptance of theories in the scientific community is a topic for the next chapter.