V

THE SCIENTIFIC COMMUNITY

The previous chapter was too abstract. It concerned certain basic operations of scientific method without placing them within the context of the scientific community. In this chapter I will remedy this defect by showing how it is possible for Polanyi to integrate a theory of personal commitment with the social demands within the scientific community for the maintenance of certain common standards. We shall see how conviviality in both belief and knowledge is a central fact around which Polanyi develops his theory of scientific community, tradition and development. I shall explore the relations between belief and knowledge, the scientific novice and the community, and between co-workers in the community. This will enable us to understand the cohesion of the scientific community and provide the context for understanding Polanyi's theories concerning the resolution of scientific disputes and of scientific progress. Polanyi’s strongest justification for his scientific realism will emerge from this discussion.

1) BELIEF AND KNOWLEDGE

Polanyi repeatedly stresses that one must believe in order to know. His emphasis on belief and its acritical nature is one of the reasons he calls his philosophy post-critical. This emphasis reflects the importance of belief in the maintenance and development of the scientific community, an importance that can be glimpsed in the

contention that knowledge presupposes belief. A discussion of belief will not only reveal this importance, but will also provide a convenient link between the significance of individual commitment and group achievement. Though the scientific community is not merely a collection of individuals, the autonomy of the individual thinker must be respected if science is to be possible at all, Just as there is a symbiosis between belief and knowledge, there is a similar relationship between the individual and the scientific community. The bond between the individual and the group is established in a convivial commitment. A discussion of belief will lead us into these topics.

Believing and knowing are both a-critical. As I pointed out in Chapter III, the person must implicitly presuppose the efficacy of cognitional activities if he is to experience any success at all in knowing. If I am critical of my performance all the time, then I will never make a commitment. Knowing is shielded from self-destructive criticism in its first operations, for one must first know before he can subject the knowing process to critical scrutiny. However, once we attempt to justify knowing as objective, we run the risk of undermining our very attempts. In fact, knowing is self-justifying. However, the view of knowing as impersonal can preclude this insight, setting thinkers on the endless task of trying to ground knowing critically in impersonal terms, if they do not give up their native realism, and the whole project, on the way.

A similar analysis holds with belief. In Chapter III I showed how Polanyi's epistemology undercuts the traditional distinction in Anglo-American philosophy between belief and knowledge. I shall now explore another notion of belief. In this

instance we believe when we accept something as true by trusting others4 As Poianyi notes, "in the great majority of cases our trust is placed in the authority of comparatively few people of widely acknowledged standing." For the sake of clarity, I will maintain a strict distinction between belief in this sense and knowledge. Though Polanyi often conflated the two in his use of terminology, he did acknowledge the distinction.

There is a difference between believing and knowing. In believing we need to understand what we believe (of course this understanding can be more or less complete, and it can be less complete than the understanding necessary to know the same fact), but we do not confirm it for ourselves. Instead we trust our source of information. When we know we confirm what we understand ourselves. Trusting our source is extremely efficacious for it prevents us from getting bogged down in needless work. A mathematician need not check the logarithmic or trigonometric tables to verify their ac curacy for he supposes that someone else has done the job correctly. If he did check them he would be wasting his time. Because we do not have to repeat what others have done, knowledge and, hence, culture can progress. We live in a culture by assimilating its wisdom and its norms much more rapidly than our culture first emerged. Presupposing that vision of reality which we acquire predominantly through belief we can contribute to the development of ourselves, and our culture.

Believing as a performance is a-critical. Similar considerations apply to it as those which I discussed with respect to knowing. That belief is a-critical does not mean that it is uncritical. The evaluation of our source of information may be

extremely prolonged and intricate prior to our placing our trust in him or her. However, there is a difference between being critical of a source and being critical of believing. Criticism of belief transposes the issue form questioning the source to questioning belief in general. While I am merely evaluating the source, I a-critically accept the process of believing. That I can be critical of believing in general means that I can question whether believing yields facts. However, by the time we come to the point of questioning the objectivity of believing, we cannot do without it. This is another sense in which believing is a-critical. Our world-view is a mixture of belief and knowledge. We cannot divest our selves of it simply by throwing it into question. The world-view itself makes it possible to question belief, and some beliefs will be a-critically accepted even while the objectivity of believing in general is questioned.

Once we trust the source of what is to be believed, the acceptance of the content as factual is a-critical. If we are learning a skill we follow the guidance of the master. If we do not give ourselves over to his standards of performance, if we do not make them our own before we are assured of their correctness, then progress will be slow if it occurs at all. If the aspiring scientist does not accept most of accepted scientific theory, then he condemns himself to a pre-Socratic mentality, if he is lucky, and is faced with the impossible task of duplicating centuries of scientific development. In this sense, then, we must believe in order to know. Not giving ourselves over to our cultural standards condemns us to a primitive mentality which severely restricts our intellectual possibilities.

However, there is an apparent problem with the claim that you must be live in order to know. So far I have confined myself to Polanyi's notion that belief entails

trusting the testimony of another. Now, that testimony, if true, can be comprised of knowledge which that person arrived at himself, or assertions which he believes. If they are assertions which he believes, then, if true, they have their source ultimately in someone else's knowing. Thus, to have something to believe, there must be someone who knows. However, that person could not know without believing. We find ourselves in an infinite regress. In the general case we must believe in order to know, but at some point there had to be an original thinker who simply knew.

Polanyi circumvents this problem by using belief in three different senses. The first is the one I have outlined above. His second sense is to use belief interchangeably with knowledge. The third usage enables him to bypass the problem. In discussing intellectual passions Polanyi notes that "These emotions express a belief: to be tormented by a problem is to believe that it has a solution…." Again he notes that a belief in the existence of identifiable~ things, to which we can respond by identifiable actions, underlies the process of denotation and that it justifies the kind of induction which underlies the descriptive sciences.'" These two quotations, the first clearly and the second to a lesser extent, indicate that there are natural beliefs" implicit in the cognitional process. Thus, if knowing is in fact oriented to reality, then in this meaning of belief to try to know something is to believe that there is a reality to be known. In this sense, belief would be intrinsic to all knowledge. It is a faith in ourselves and in the intelligibility of reality. However, I am not sure Polanyi accepted as this/a solution to the problem, for I am not sure if he recognized the problem. However, if this is what Polanyi explicitly thought, it would account partially for his failure to confront the problem I have presented, for it may not have existed for him. Even though in the first

sense of belief it is necessary to know in order to believe. for the most part it is necessary to believe in order to know. Without belief learning would not be possible, be it the learning of the student from the teacher or the collaboration of co workers. Neither could those with different, but complementary, horizons mutually rely on one another. For example, the doctor believes the mechanic when he fixes his car, and the mechanic must believe the doctor when he fixes his knee if each is to plan his future.

Believing and knowing exist in a symbiosis, for both yield facts. The more one believes, given that his beliefs are true for the most part, the more he can know. If we believe something it can be a subsidiary element in confirmation, since we accord it the same status as knowledge of our own. Likewise, the more we know, the more we can believe. We can have a better appreciation of our sources and certain things will appear more plausible than others. The more facts we have at our finger tips, the greater the possibility that we will discover significant problems and are able to solve them. In the process of solving them we can draw on our knowledge and beliefs.

Our situation is that we believe much more than we know. We are faced with the fact that we cannot fully substantiate many of the convictions by which we live our lives nor many of the factual claims which we make. I have shown that knowing can substantiate itself, for it can be critically grounded in that its self-transcendence can be empirically verified. It is the nature of belief that, though it can be critical, it cannot be critically grounded to the same extent as knowing, for there is always the necessity of trusting another in believing. However, believing is self-validating. The whole project of Personal Knowledge is an attempt to draw this out. I think it can most easily' be arrived at by considering the difference between validation and verification. Polanyi

states:

> The acceptance of different kinds of articulate systems as mental dwelling places is arrived at by a process of gradual appreciation, and all these acceptances depend to some extent on the content of relevant experiences; but the bearing of natural science on facts of experience is much more specific than that of mathematics, religion or the various arts. It is justifiable, therefore, to speak of the verification of science by experience in a sense which would not apply to other articulate systems. The process by which other systems than science are tested and finally accepted may be called, by contrast, a process of validation.

I am concerned here with the difference between knowing and justifying knowing as objective, and knowing and justifying belief as objective and efficacious. Knowing the processes of knowing and belief involves veri- fication. We accepted the account of knowing presented earlier because that account was faithful to experience. The same applies to belief. However, knowing was seen to be self-justifying because the objectivity of knowing was also verified in experiencing;~; the conscious demands for objectivity and in experiencing the fulfillment of the::s.e,.; demands. To justify knowing it is possible to verify our objectivity.

Belief, too, can be justified through verification if we come to know what we previously merely believed~ However, while we are only believing, such justification has not occurred, and what needs to be justified is the belief of facts and precepts without knowing that they a.re 'true or right. Now, as we mature as persons we gradually move from relatively uncritical believing to a more discerning acceptance or rejection of what others tell us. Like knowing, the process of believing is initially protected from self-destructive scrutiny, for such investigation supposes the operation of what is questioned. Thus, we already dwell in a world constituted partially by belief

and knowledge. Generally, we accept our beliefs to the extent that we find this account of the world satisfactory, and reject them when we see that they are responsible for our uneasiness or dissatisfaction concerning truth and values. Appraisals of comprehensive coherence become important here. Now, we may want to reject the process of believing as a means of attaining facts if we have the ideal of accept ing only what is certain, clear and distinct, beyond doubt. However, such a rejection, if carried out strictly, would seriously curtail the pursuit of knowledge. Accepting belief leads to progress in knowing, for it is the only way in which we can keep abreast of the knowledge of our time, as well as adequately assimilating the results of the past. Thus, even if one accepted the ideal of certain, explicit knowledge, to be true to his desire to progress in knowing, he would have to accept belief. Believing is self-validating for it leads to a self-confirmatory progression. The more we believe, assuming that we are generally right, the more we can know. The more we can know, the more we can believe, and so on. If one's ideal is knowledge, a world which admits the process of belief as efficacious should be more satisfactory than one which does not. Certainly, progress is more attractive than the downward spiral of decline, and a coherent reality is more satisfying than an unintelligible one.

To accept progress in this sense is to accept the personal com itment we make in trusting another's testimony. Polanyi's argument in Personal Knowledge is designed to persuade us to trust ,ourselves in making these commitments so that we may more completely become principles of progress and become more satisfying to ourselves. To deny the validity of the personal dimension is to undermine the human endeavor, including the ideals we have if we construct an objectivist framework, for an

objectivist viewpoint is developed through assimilation of a tradition.

Believing is also an individual commitment which serves to bind the individual more fully to the group. One can exercise his autonomy in believing (and in knowing), but the existence of the autonomous individual need not lead to the conclusion that society is a set of atomic individuals. Indeed, the unity of the scientific community rests on shared beliefs, and for the most part these beliefs are entered into responsibly by the individuals concerned. However, belief alone does not account for the unity of the scientific enterprise. There are also shared methods of investigation. In discussing how the novice comes to acquire these I will be able to illustrate further the role of belief and the role of tacit knowing in scientific education.

2) THE ASSIMILATION OF SCIENTIFIC TRADITION

To learn science the student must submit himself to authority. He must believe. Though scientific results are supposedly open to verification by anyone, to be qualified to challenge most of them requires years of education and extensive funding. While one is acquiring this expertise he cannot assiduously verify everything he accepts. Second, the student learns techniques of experimentation and observation. Before he learns them he must believe in the value of the enterprise. While learning he must follow the example of the teacher. In these instances he is learning a skill, and the analysis of skills in terms of tacit knowing applies to this enterprise. Third, he is learning to solve problems. Much of the theory he learns is to be presupposed in future work, for it is complementary to the work which the student will be doing (it provides a wider context), or it will determine in large measure how he will approach problems.

In his The Structure of Scientific Revolutions Thomas Kuhn acknowledges his debt to Polanyi's theory of tacit knowing in~dis~ cussing one meaning of his controversial notion of paradigms. He notes that in one sense of the term, "paradigm" denotes a set of "concrete problem-solutions that students encounter from the start of the scientific education." The student learns science by "doing science rather than by acquiring rules for doing it.

Doing science includes long hours in the laboratory acquiring observational skills and experimental methods. The novice in biology, for example, needs to develop a kind of connoisseurship in recognizing types of tissues, parts of cells and organs, and so on. He needs to learn certain experimental methods and become skillful in their use. Observation and experimental methods presuppose a theoretical context for further research which he simultaneously assimilates. Thus, the use of certain methods may commit him to some theoretical viewpoint which he has not yet fully assimilated nor brought to the focus of his attention for explicit questioning. This is most clearly the case in psychology and sociology where there exists a tension between those who wish to model these sciences along the lines of the natural sciences and those who wish to develop a qualitatively different model for the human sciences. Often students find themselves in one school of thought before they know there is a difference or what it is. Having been trained in one paradigm of the discipline they find it difficult to understand and sympathize with the concerns of the other. One reason is that the understanding demands an assimilating of a set of particulars which they have yet to ac- quire. This includes the tacit components of the viewpoint which either have not or cannot be made explicit. In any case, they are not made explicit in the practice of the

discipline. In knowing how to do science, then, we know more than we can tell not only in the use of skills, but in our theoretical approach to the subject matter as well.

We acquire mastery of skill through a convivial relationship. We both trust and imitate the master. Thus, the fact that we had a particular teacher can show in our performance. 'There are schools of art which can trace their heritage through characteristics of their work and the fact that they learned their skills from certain individuals. Polanyi claims that the situation is analogous in science with respect to theoretical progress. He points out that scientific communities cannot arise spontaneously in countries without a scientific tradition and expect to rival those which do have a tradition. He states:

> Rarely, if ever, was the final acclimatization of science outside Europe achieved, until the government of a country succeeded in inducing a few scientists from some traditional centre to settle down in their territory and to develop there a new home for scientific life, modeled on their own traditional standards.

He also notes that certain teachers have success in turning out accomplished scientists partially because the scientists have worked with them. They could have learned the same explicit knowledge from someone else, but not in the same way. It is the way they learned it and the other tacit knowledge they gained which contributed to their skill as scientists.

Because the student learns a way of going about things he assimilates the values of the community he is entering. However, these values are not merely practical, but also comprise part of the context in which one determines which problems are significant and which not, which solutions are plausible and which absurd, In his primary sense of paradigm Kuhn considers it as "the constellation of group

commitments4t, a distinctively Polanyian View, and perceives these commitments coalescing around shared formulas, models, values, and exemplars. In response to Kuhn, Polanyi has claimed that "A commitment to a paradigm has a function hardly distinguishable from that which I have ascribed to a heuristic vision, to a scientific belief, or a scientific conviction." In becoming a scientist, then, one acquires a set of commitments which constitute one's viewpoint and provide a context for future work. Since these commitments do not all regard fully explicit knowledge, they must be entered through imitation of a master. Such imitation is not only of his skillful performances, but also an attempt to gain some measure of the viewpoint from which he judges particular projects to be significant and valuable and the converse. In other words, it is not enough to know a theory; one must also know how to apply it. Such application requires the cultivation of judgment and this is more easily done under the tutelage of a person who has good judgment than by oneself.

What is most often overlooked is that the assimilation of science is passionate. Besides feelings of trust for one's teachers, the self transformation involved in becoming a scientist requires a passionate appreciation of the values of the group exemplified in the work of its major figures, for it is these values one tries to assimilate. The sharing of a common experience binds the members of the group closer together giving rise to a shared viewpoint not fully accessible to outsiders. Ideally, convivial relationships extend beyond those between student and master to all like-minded group members. The exceptions to the norm demonstrate that passions can divide as well as unify the group.

More importantly, the assimilation of science leads to the cultivation of

feelings. To a large extent the student is taught what to appreciate and what not to, which topics deserve the most concern, and the proper mode of scientific conduct. Indeed, the popular notion of scientists is that they are disinterested and dispassionate investigators. However, those who are, are interested certainly in maintaining that ideal and are passionately committed to it. What disinterest really means is selective interest, while dispassionate in the best sense should mean only selectively passionate.

III. THE UNITY OF THE SCIENTIFIC COMMUNITY

Naturally, there is not only one mentality which all scientists share. Scientific mentality differs between and within the various sciences and between succeeding generations of scientists. How, in the midst of all this diversity, is the scientific enterprise unified throughout its history and at any particular time in its history? As Polanyi points out, there is no central authority governing science. Rather, science is controlled by a general authority which rests on belief and the mutual respect scientists accord one another. The structure of the scientific community makes this possible. As Kuhn notes, different paradigms are used by different scientific groups. One person may belong to more than one scientific community, but no person belongs to all and few belong to many. However, the interests of different communities overlap. The microbiologist knows chemistry, the chemist knows some physics, and the psychiatrist knows neurophysiology and analytic psychology, for example. Also, though one person may be extremely interested in one field of biology, he may still be competent to evaluate the work going on in other fields. There is thus a manifold of overlapping centers of concern which allows for the practitioners being capable of mutually

accrediting one another.

Not only does this make it possible to enforce scientific standards, but Polanyi thinks it can also be exploited for Intelligent planning of scientific research. For example, to compare the sciences with one another to determine their development relative to one another and the relative merit of work going forward or being planned is extremely difficult. No one person masters all of science, so the comparison would have to be done by a committee Now, if the committee were composed of scientists who had overlapping centers of concern, and if taken together their interests spanned the subject matter to be investigated, then they could allocate resources in a manner consonant with scientific merit, for they could mutually accredit one another.

What are the standards of scientific merit? They must be sufficiently general to span the breadth of science without being trivial, and they must characterize the object without overlooking the personal dimension. NOW, what is common to all the sciences is not their subject matter, but the fact that they are products of personal knowledge. Thus, it is not surprising that the standards of scientific merit are personal for Polanyi. Most of them are characterized in intentional terms. That is, the object is characterized at a second remove. We do not so much at tend to the object as the manner ln which the object is known to characterize which knowledge is of scientific merit. If knowledge satisfies certain personal criteria, then it is of scientific merit.

There are three such criteria. "The first criterion that a contribution to science must fulfill in order to be accepted is a sufficient degree of plausibility." There are cases where contributions are offered which have confirming evidence, but which are clearly implausible. Thus, they are rejected, though they cannot be disproven at the

time. One example Polanyi cites is a claim “that the average gestation period of different animals ranging from rabbits to cows was an integer multiple of the number ~ .'" Though evidence was produced for the claim, it was immediately rejected. Thus, in judgments concerning plausibility we meet the same kind of knowing evidenced in one who chooses good problems or hits upon good solutions. The grounds of the appraisal are not fully specifiable, for one is guided by intimations of reality as well as the explicit knowledge at his disposal. It would follow, then, that one criterion of a good scientist is that he can distinguish the plausible from the implausible, though he would not be able to specify his grounds for doing so in all cases.

The second criterion is the scientific value of the contribution. I discussed this earlier. Briefly, there are three aspects of scientific value. The first is accuracy or certainty. The second is systematic relevance, importance, profundity. The final aspect Polanyi calls intrinsic interest. All three of these aspects are present in any contribution, but not to the same degree. Thus, in physics we may encounter a theory of great accuracy, but its subject matter will not have the intrinsic interest of the human mind. In psychology we may meet a profound systematic achievement of great intrinsic interest, such as Freud's, and we will accept it despite the problems we have in determining its accuracy. Mathematical accuracy decreases as one ascends the levels of the sciences, for the subject matter becomes more complete and exhibits more equipotentiality. This means that it becomes more organic and, in the higher levels, more creative. However, intrinsic interest increases, and the potentiality for greater systematic achievements also increases because the complexity of the subject matter must be matched by a complex theory if we are to understand it adequately. As

we saw earlier, scientific value is appraised aesthetically in terms of the theory's intellectual beauty.

The final standard is originality. The degree of originality is ap- praised primarily by the degree of surprise the discovery evokes. Naturally, this can be best appraised by a person who is competent in the field, and who therefore has an appreciation of the logical gap crossed by the dis- covery. The profundity of the discovery will also be an indication of the degree of originality. The scientist must dare to go beyond, and perhaps against, scientific opinion. However, it is the surprise caused by the insight which is the chief criterion. As Polanyi notes,

> There are discoveries of the highest daring and ingenuity, as for example the discovery of Neptune, which have no great systematic importance.

The desire to meet these standards is the principle of scientific growth. As we shall see, this desire is realistic in intent, and it is scientific realism which provides the possibility for progress in science for Polanyi. The first two criteria may be termed conservative, for they induce conformity. However, the scientific community also encourages, at least in principle, the emergence of ideas which run counter to accepted opinion. Thus, the standard of originality is in tension with those of plausibility and value.

Without originality there would be no development in science. Polanyi thinks that knowledge of reality is valuable for its own sake, and on this ground alone he advocates freedom of thought in science. In addition, if scientific thought is to be restricted on grounds other than its own, then these must be political, social, practical, or some combination of these. In general, external control of science would lead to its

decline. The major reason external control is ultimately inadequate is not hard to find. Knowledge of reality generally precedes the recognition of any practical applications of that knowledge as well as the recognition of its social and political implications. To restrict scientific projects to the practical, for example, is exactly backwards and would lead to a contraction of scientific concern and a consequent deceleration of scientific growth. Thus, Polanyi has argued for freedom of thought in science throughout his philosophic career.

Originality, like belief, is an activity of the individual mind. Though more individualistic than belief, for the most part it too is intrinsically social. I have already discussed Polanyi's contention that one must believe in order to know. In science, this means that one must be up on the developments in one's own field if he expects to make an original contribution to the subject. Otherwise he risks duplicating work already done. Having an adequate background is important in another respect too. Given certain achievements, new problems emerge, which, in many cases, could not even be formulated prior to those discoveries. There is a field, then, of problems both implicit and explicit which is evoked by the present state of knowledge. Of course, the recognition of a problem is the first step in its solution. This partially explains why different workers can arrive at the same results at approximately the same time, though they have been working independently of one another. It also accounts for the converse situation discussed by Kuhn of the difficulty of crediting one person with an invention which rests on determining when the object was invented. In many cases there are successive approximations to the object by different people at different times.

In science, then, <fa series of independent initiatives are organized to a joint

achievement." The joint achievement is the body of scientific knowledge which no one person could arrive at on his own. The independent initiatives are the autonomous subjects united by their common beliefs. The advance of science is self-coordinated because each worker is aware of what is going forward in his field and he chooses some problem or set of problems which he thinks he is capable of solving, which he thinks are significant, and where he will not be duplicating someone else's work. Of course, this is the ideal case, but the actual situation is an approximation to it. I have already discussed how this self-coordination works in enforcing the standards of plausibility and scientific values. This same structure of overlapping centers of concern allows workers to learn of work going forward which may be pertinent to their own work. However, the self-coordination of scientific thought does occur within the context of the society of which the scientists are members. In most cases they rely on extra-scientific sources, private and governmental, for the funds for their research. Thus, the planning of scientific research is not controlled by scientists alone. Scientific values should be analyzed in relation to personal and social values. Since science develops best if it is not controlled by a central authority, and the exclusion of non scientific interests in scientific planning would be irresponsible on the part of scientists, Polanyi thinks that science will develop best in a non-totalitarian society where the growth of thought in most other areas relies also on a general authority.

4) SCIENTIFIC PRCGRESS: REVOLUTIONS AND CONVERSIONS

Progress in science is directly related to its social nature, the communal, passionate adherence to scientific standards. Polanyi's theory, then, has an additional

affinity with Thomas Kuhn's, for Kuhn conceives of science advancing partially through the emergence of social control through paradigms. However, Kuhn terms the emergence of paradigms "scientific revolutions· and contrasts the emergence of the paradigm with normal science, or work that goes forward presupposing the methods and theories and problems provided by the paradigm. Polanyi considers the difference between the two to be merely a matter of degree, and not a qualitative difference. No discovery merely conserves science nor does it totally change it. Surprising discoveries can be made by adhering to current scientific theory, just as they can be made by transforming its foundations. However, I am partial to Kuhn's view and would stress the change of methods which accompany some of the changes in science as definitely being revolutionary. This is because the method generally determines the broad outlines of the result. Therefore, a change of method as a change of the principles of inquiry leads to a qualitatively different discipline. Cases in point are Galileo and Lavoisier.

In addition, we may discern three major periods in the attempt to understand nature. The first was the age of the great myths. The second commenced with the natural philosophers of Greece who sought natural causes for events. The third took shape during the time Marie Boas calls the scientific renaissance,1430-1630. The shift away from Greek science was a gradual movement towards an explanatory as opposed to a descriptive viewpoint. The differences between these two viewpoints are still being worked out. In philosophy the problem is that of the thing-for-us and the thing-in-itself, primary and secondary qualities, and the other forms it has taken in the tradition. Thus, I think we can discern at least two revolutions in science in general.

Polanyi acknowledges that a shift occurred from the naturalistic to the scientific viewpoint.

Kuhn's contention that there is a pre- and post-paradigm phase in scientific development also seems sound with respect to some sciences. Thus, for him, as a science matures there is a shift from there being a variety of schools, each with its own paradigm, to one school with one paradigm. This has been borne out in physics and chemistry, though it is problematic whether development in the human sciences will take such a course.

However, the situation in the human sciences seems to support Kuhn's view. The major division within the human sciences is between those who wish to apply the principles of inquiry of the natural sciences to the human sciences and those who do not. It would be a tremendous feat to convince each side that the other was being completely scientific. It would be revolutionary to articulate a paradigm which reconciled the two views. In fact, later we will see that Polanyi has some contributions to make to this task. However, in support of Polanyi, such an innovation would not totally transform psychology, though the change would be more than one of degree.

Both Kuhn and Polanyi envisage the scientific development which occurs through the shifting of a paradigm to be a conversion, i.e., a shift in theoretical viewpoints altering one's world view. They have been strenuously criticized for this. I shall layout Polanyi's reasons for thinking that conversions are necessary in science and then defend him against charges that conversions are irrational. The defense should also help to vindicate Kuhn, whose theory regarding this, though more problematic, is essentially similar to Polanyi's.

Conversions are not always necessary. One can often accept new results with little trouble if their acceptance would not radically alter his view point, it they would fill in gaps, or if they confirm it. However, our situation is that we must be committed to some view, and that commitment is partially constitutive of what we are. It is possible that we may be presented with a view in radical opposition to our own. To accept that view would require a profound change in ourselves, basic enough to be termed a conversion. Conversions would seem to be a matter of degree for Polanyi, just as the difference between normal and revolutionary science is. Though he does not define what a conversion is, the phenomenon should be apparent to most. When one is converted there is a radical shift in thinking and in values. This includes a shift in whom and in what one believes.

Polanyi notes that conversions are necessary when systems of thought are at issue, as opposed to individual facts. This is analogous to Kuhn's view that conversion is necessary to accept a paradigm shift. Lakatos and Schleffler consider conversions to be irrational. However, it can be shown that conversions are part of the natural progression of reason, if in fact we move from viewpoint to viewpoint, That premise should not be difficult to accept if we advert to the history of science or, perhaps, to our own lives,

Insofar as people have different systems of thought it seems appropriate to agree with Kuhn and Polanyi and say that people live in different worlds. We saw that one's theoretical viewpoint can determine the experience which he will have. Second, it determines the significance of that experience. Third, it determines what will count as evidence of itself. Last, it exists in a symbiotic relationship with our valuing. Thus,

if people have different systems of thought, they have different experiences and recognize different meaning, facts and values.

It follows~ then, that if one is to enter into dialogue with another in a mutually comprehensible manner, each of the participants must transcend his own viewpoint and understand the other's. This means that one must have his experience and understanding transformed, though' he need not accept all that the other accepts as real or valuable. However, there must be some mutual acceptances from which to start the dialogue. The most important is that each must consider the dialogue as valuable.

Now, if the systems are logically incompatible, then one cannot accept one of them without rejecting the other. But beside logical incompatibility, there is the wider problem of what Kuhn terms their incommensurability. Thus, though each may be coherent in itself, it is neither coherent nor consistent with the other. The same words may have different meaning, the same things different significances. Each may be well confirmed in itself, but neither may be confirmed in the other's view, for what counts as a confirming instance is determined from within the theory. Additionally, the persons holding the different views will attach a different significance to different problems. Their methods of approaching and solving problems may also vary. Thus, the dialogue involves the difficult process of understanding the other, translating one system into the terms of the other where this is possible, and hitting upon mutually accepted forms of communication in other instances. The efficacy of this approach is limited, however, for the two are separated by a logical gap.

Kuhn thinks that a conversion has occurred when one begins to think in terms of the theory. He compares it to learning a language. Just as we can begin to think in a

foreign language instead of translating from one language to another, so one can begin to perceive the world in terms of the theory. Polanyi emphasizes the role of intellectual demonstration and persuasion in bringing about a conversion. Now, the efficacy of demonstration is limited, for confirming instances are defined in terms of the theories. However, the theories themselves are in the context of the human mind, and one theory can be more satisfying to our intellectual passions and general heuristic vision than another. Thus, we can compare them, for example, in terms of their explanatory power or the intimations of their fruitfulness. However, such comparison supposes that the person has accepted the opposing viewpoint as valuable enough to understand it and has understood it. To arrive at this point it may be necessary that he be persuaded. Demonstration will not work, for it presupposes that one sufficiently understand the views being developed. Even if one did arrive at this point, persuasion may still be necessary to some degree, for we passionately adhere to our viewpoints while often passionately rejecting others. To undergo a shift in our valuing which we do not wish to experience we frequently require persuasion by others.

Often they require little impetus to try to persuade us. Polanyi claims that intellectual passions, after being satisfied by the acceptance of a position, are transformed into desires to convince others of what we have discovered. This is their persuasive function. This attempt to find a resonance in others for our views is the seed of the convivial passionate appreciation of theoretical viewpoints by a group, for the others can be peers, disciples, students, or a future generation of knowers.

Scheffler accuses Kuhn and Polanyi of characterizing the history of science as a series of non-rational conversions. I do not wish to engage in a detailed discussion of

Scheffler's argument with Kuhn and Kuhn's reply. I wish to emphasize, instead, some key points which Scheffler makes and discuss these in the context of Polanyi's philosophy. This will reveal once more some important limitations for the objectification of the development of human thought, limitations which center around the fact that it is personal. As noted, Kuhn distinguishes normal and revolutionary science. Paradigm debates do not occur within normal science, for it is the practice of normal science which is in question. Also, because two incommensurable views of normal science are in question, to conduct the argument only from within the paradigm one accepts is to restrict oneself to a circular argument. For this reason the antagonists must have recourse to what Scheffler terms a second order level, and, if they are to debate success- fully, they must share some standards on this level. Thus, Scheffler stresses that "lack of commensurability … does not imply lack of comparability". Kuhn and Polanyi agree. The problem concerns what the standards are. Scheffler contends that Kuhn and Polanyi believe in the necessity of non-rational conversion rather than deliberation. His major reason for characterizing conversions as non-rational is that the conversion as an experience is invoked as the primary reason for accepting the paradigm shift. He notes that contrary to this

> The very existence of paradigm, debates testifies, indeed, to their belief that independent supporting reasons are available to them, reasons which can sustain themselves in critical discussion of alternatives.

As far as Polanyi is concerned, this is a misinterpretation of his philosophy. Acts of understanding and acts of judgment are not invoked as the reason for accepting viewpoints. It is what is discovered in the insight and the grounds for accepting it in the judgment which are invoked. However, reasons do not "sustain themselves in

critical discussion of alternatives" for they would not be known nor accepted as convincing without the acts. Thus, if we consider conversion in Kuhn's sense, then we must presuppose the ability of both parties to think out of each other's views if the paradigm debate is to be completely rational, if it is not to be open to gross misinterpretations. Thus, conversion as Kuhn interprets it is necessary for fully rational deliberation.

Conversion is not unreasonable or irrational primarily because it is a rational act. In Kuhn's sense, it is an achievement of understanding the other's viewpoint such that I can think from within it, even if I do not accept it. For Polanyi, I not only understand but accept the viewpoint when I undergo a conversion to it. Few human endeavors are completely rational, for man is not a pure intellect. However, Polanyi claims that the succession of conversions is the developing of knowledge, and, to that extent, it is rational. Whether we like it or not, the development of understanding results in the emergence of incompatible viewpoints. If we are to decide between them, we must understand and compare them. If we are unwilling to understand and compare them, then we must be persuaded to do so. If we commit .ourselves to a new viewpoint, then we have modified ourselves in that acceptance, for we have understood more, affirmed different realities, experienced differently, and changed our values. If this change is sufficiently great, we have undergone a conversion. The denial of such changes is an implicit advocating of static thinking. These changes not only concern scientific facts, but also the criteria for accepting a fact as scientific. Conversions are most often necessary when it is a matter of accepting new criteria.

However, to meet Scheffler's charge fully we must confront his insistence on

independent supporting reasons. As I have noted, Polanyi does not think that this concern is completely misplaced~ Rules are extremely helpful. However, their efficacy is limited, for they function as maxims of an art. Reasons for changing a viewpoint function in a similar manner. There are independent supporting reasons, but they must be acknowledged as such in a responsible judgment. That they are valid is a conclusion of an act of personal knowledge. The central problem in paradigm shifts is the acknowledgement of one paradigm as better than the other. If we move to a second order level of deliberation, and if we hope to resolve the dispute quickly, then we should have independent reasons or rules to which we are willing to submit ourselves. Is this the case? It falls within the province of the philosophy of science to reflect on scientific method. We certainly do not find unanimity here, but a variety of conflicting viewpoints. I shall discuss this further below.

However, let us suppose that there were "independent supporting reasons which were advanced by one group and were correct. How would they achieve agreement with those who do not realize the truth of those reasons? Let us suppose that they come to accept those reasons. By what standards are they guided in coming to acceptance? If the standards are explicit rules independent of the knower, then they would not be in conflict with the "independent supporting reasons", unless they were mistakenly applied. If their explicit standards are in conflict, then they could not be guided by them, in their rejection of them. Neither could they be guided by the explicit "independent supporting reasons" prior to accepting them. But if they come to accept them, and they are true, then there must have been something about their method of coming to acceptance which was objective prior to their acceptance. Thus, agreement

would be reached by appealing to that prior notion of truth of those who do not yet accept the "independent supporting reasons" and which is not set out in other explicit rules. Though Polanyi does accept both an independent reality, that is, one which does not depend for its existence on our knowing, and the existence of standards of performance which are independent in the sense that they are not arbitrary but are the standards anyone should have if he is to rea~ the goal set by the standards, he does not accept standards which are completely independent of any knowers. Now, this may appear trivial, for if there are standards in science, for example, it is fairly obvious that someone must have them. However, the implications of this fact are profound.

If we continue the argument of the preceding paragraph, we are led to affirm the existence of spontaneous cognitional standards of intelligibility and truth, precisely because any independent standards must be recognized as standards by the person. By what standards are we to judge independent standards before we have made any standards explicit? We can only proceed by the light of our own intellects. This means that the ultimate standards for judging paradigm shifts and any other differences in viewpoint are those immanent in the mind. The basic standards are immanent in acts of understanding, responsible judging and valuing. Now, these standards are independent for I must not be arbitrary in meeting them. However, they are personal for they are identical with part of me, my intending of universally valid results, and I can choose to be more or less authentic in submitting to them. It is this power of choice which grounds Polanyi's assertion that they are self-set. I may choose to pursue certain issues until I reach the truth, for example. Again, for Polanyi personal judgment is the pivotal act which constitutes "independent supporting reasons" as both

independent and supporting for the subject. In \his sense, then, these reasons become self-set standards to be met. The next question that naturally arises is "What are the Standards?'. Again, they are not standards which can be set out entirely in terms of explicit rules but are the emotional and intellectual dynamism of our minds. As I noted earlier, in this sense the achievement sets the standards and it is the desire to achieve which leads us to the achievement. If we appeal to successful achievements to formulate rules of method and to justify the use of those rules, there should be no problem with appealing to successful achievements and the satisfaction we experience dwelling in them to justify our acceptance of them. In other words, if we affirm that immanent cognitional norms exist, that they function even if they are not explicitly known, and that their proper functioning results in knowledge of reality, then it would not be unreasonable for someone to uphold his claim to the truth by simply stating that that is the "way he sees things" in the absence of a fully articulated correct epistemology and a set of completely specifiable reasons for coming to the judgment. One could appeal, then, to the conversion experience as justification without being irrational, because the "experience", embodies a normativity which is objective. Scientists rarely resort to such support for their claims, for science has come to some measure of self-objectification. However, Polanyi claims that even complete self-objectification and a full set of rules is not sufficient to decide paradigm debates, for ultimately there are no rules for applying rules. In the end we must rely on our own anticipations of reality and our personal judgment.

The answer to Scheffler may be less 'than satisfying to him. The argument for the rationality of conversions is similar to that for the reality of Polanyi's

epistemology. I can only describe what happens, and if one finds it true to his experience and intellectually satisfying, then he may accept it. I do not deny that this viewpoint may be incommensurable with those of Kuhn's and Polanyi's critics. Thus acceptance of Polanyi's view many entail a conversion for them. Kuhn points to the same possibility in his "Reflections on my Critics" in Criticism and the Growth of Knowledge. He contends that there were some profound misreadings of his work and notes that "Much in this volume testifies to what I described above as the gestalt switch that divides readers of my Scientific Revolutions into two groups." The groups have incommensurable points of view.

Contemporary philosophy of science and philosophy in general, are populated with incommensurable viewpoints. Those in philosophy of science reflect the general divisions in epistemology. There are philosophers concerned with interiority. That is, they discover the basic principles of the philosophy of science in the conscious experience of the person doing science. The concern with performance overflows into a concern for the history of science, for it is in scientific practice that the norms which are and were in fact used may be discerned. Those who do not attach much importance to interiority stress contents of conscious acts as the "scientific products" such as theories and rational reconstructions of scientific thinking. Their generalizations about science stress qualities of these contents. Then, within these two groups there is the possibility of different epistemological positions, primarily rationalism, empiricism, idealism, and realism. Rationalism and idealism are not popular at the present time. Realism and empiricism cannot be strictly separated, for some empiricists are realists. However, Polanyi is neither an empiricist nor an idealist, but he is a realist. Now,

philosophers who are interiorly differentiated relate across a logical gap with those who are not. The same holds for those having different epistemological positions. So we are faced again with the problem of incommensurable viewpoints and the fact that the solution to the problem is simply for one of the disputants to change his view, or for both to develop their views into a common view. The problem arises from a lack of human achievement, and it can only be solved by the emergence of achievement.

5) SCIENTIFIC PROGRESS: GROUNDS AND FORM OF DEVELOPMENT

The conversions within scientific development are normative. A person accepts a new position because he finds it more satisfying intellectually than the old one. The person can be mistaken. Thus, there is the possibility of decline in science. However, Polanyi does not stress this possibility in these terms. He conceives decline as the disintegration of the consensus of scientific beliefs, if the cause of the decline is within science itself, Decline is also a possibility if forces outside science gain control. Then they can destroy scientific values by supplementing them with their extra scientific ones. Naturally, science must exist in some social milieu, and it is not immune to the opinions of those outside it. However, as noted, Polanyi thinks that those opinions should include a respect for the autonomy of science.

The general norms governing conversions are the same as those governing development within some chosen viewpoint. The most general norm is the passionate striving for a correct understanding of the field one is interested in. This striving is realistic in intent. Polanyi considers it the ground of scientific progress and unity.

> We have here the paradigm of all progress in science: discoveries are made by pursuing unsuspected possibilities suggested by existing

> knowledge. And this is how science retains its identity through a sequence of successive revolutions.

Again Polanyi states:

> This view of science merely recognizes something all scientists actually believe. For they must believe that science offers us an aspect of reality and may therefore manifest its truth inexhaustibly and often surprisingly in the future. Only in this belief can the scientist conceive problems, pursue inquiries, claim discoveries …. Ant it is by transmitting this belief to succeeding generations that scientists grant their pupils independent grounds from which to start on their own discoveries and innovations--sometimes in opposition to their own teachers. This belief both justifies the discipline of scientific soundness and safeguards the freedom of scientific originality.

However, the view that scientific progress is the approaching of the truth has gone out of fashion recently. The optimism of the nineteenth century is absent largely because many scientific theories have been replaced by better ones, and the possibility of these being replaced is also envisaged. In the face of the absence of truth, how are we to decide if we are approaching it?

I think the possibility of a theory of verisimilitude must be resurrected. I believe this can best be done by first examining a theory which has failed that of Sir Karl Popper. By discussing the reasons for its failure we shall reiterate a key difficulty in the objectivist standpoint which points to the necessity of taking the subject into account in a correct theory of verisimilitude. Second, I will explore Thomas Kuhn's subjectivist objections to such a theory. Lastly, I shall show how such a theory is possible given Polanyi's view, and shall make the minimal cla.im that we are approaching the truth. Popper contends that all scientific theories are false. Although the theories which will replace the present ones will also probably be false, it is possible to consider these theories, if they are in fact better theories, as being closer to

the truth. Central to the notion of verisimilitude is the idea of the truth content of a theory. He borrows Tarski's idea of the content of a statement, or a set of premises in a system, as its consequence class, or "… the class of all statements logically entailed by it." The truth content of a deductive system (or a statement) is the class of all true statements in the system. This class is another deductive system which is a subset of the higher system. The falsity content of a statement or a system is the set of all false statements which follow from the premises.

Popper also thinks that the content of a theory or statement is inversely proportional to its logical probability. Logical probabilities can be either absolute or relative. If they are absolute, then we are merely concerned with the logical content of a single theory. The relative probability of a theory is inversely proportional to the amount of unique content it has relative to another theory or a given body of knowledge. Logical probability should not be confused with the probability of a theory being true. The logical probability of a theory does not increase, for example, as it is corroborated. Thus, a contradiction has the lowest logical probability, zero, and the greatest logical content, since anything can be deduced from it. A tautology has the greatest logical probability, one, but zero content. A conjunction has a lower logical probability and a higher content than either of its conjuncts.

It appears, then, that the logical probability of a theory (TI) should be lower and the content higher relative to another theory (T2) if T1 makes more precise quantitative claims than TZ' and also more de tailed claims than T2• The same relation should hold if T1 explains more facts than T2 and, thus, if it relates things which T2 leaves unrelated. T1 should be more testable than T2z that is, there are more of what

Popper terms basic statements (Which are a type of empirical statement) which are in contradiction with T1 than with T2• If these statements are true, then T1 is falsified. For Popper, all five of these conditions also hold if T1 is closer to the truth than T2 as long as the additional condition is met that T1 has passed tests which T2 has not. Thus, the notions of content and probability can be correlated with that of comprehensive truth in a theory of verisimilitude.

The most basic definition of verisimilitude is that

> T1 is closer to the truth than T2 if and only if either (A) the truth content but not the falsity content of T1 exceeds that of T2.
> (B) the falsity content of T2, but not its truth content exceeds that of T2.

Now, there are many difficulties with the theory, and I refer the reader to some o£ the discussion of them in the literature. However, there is one insight which underlies many of them. For the purposes of this argument only. I shall define "absolute truth" as the correspondence to the facts of a proposition, set of propositions, or a theory. By comprehensive truth I mean the set of all true propositions. Now the ideal of verisimilitude is primarily the ideal of approximating the comprehensive truth. This ideal can be in conflict with the ideal of attaining the absolute truth. Thus, one theory can be closer to the comprehensive truth than another theory (and be a better theory), but be farther from the absolute truth. However, the one which is farther from the absolute truth will be farther from the "truth" given Popper's definition of verisimilitude. For example, let T1 be (p,q,r,s,t) and let each of these propositions be true. Let T2 be (p,q, r,s,t,u,v,w,x,y,z). All of these propositions are true except y and z which are false. Clearly, T2 is closer to the comprehensive truth than is Tl yet we could claim that it is farther from the absolute truth. Indeed, it has less verisimilitude

than T1 If the theory of verisimilitude is to provide some criterion for how closely false theories approach the comprehensive truth, then it has clearly failed here.

Now I think that this failure can be traced ultimately to Popper's methodological presuppositions in doing philosophy of science. He considers himself an objectivist for three basic reasons. The first is that there are two meanings of knowledge, the subjective and the objective, and that a study of "knowledge in the subjective sense is irrelevant to the study of scientific knowledge." Subjective knowledge is for the most part the process of knowing of particular persons. Objective knowledge consists of "problems, theories, and arguments as such." 'The second reason is that he thinks epistemology should concern itself primarily with problems in the philosophy of science. The third thesis is that a study of objective knowledge will shed light on subjective knowledge, but that a study of subjective knowledge will not shed light on objective knowledge.

Now if knowledge is to be completely independent of the subject and we are to deal with problems in the philosophy of science, there must be some "subject-independent" manner of phrasing and solving these problems and there must be some objective manner of examining these solutions. Since' Popper considers an adequate explanation to be a logical deduction and a completed theoretical system to be a deductive system, he attempts to fulfill the first requirement by attending to the logical content and the logical probability of theories, as well as the logical relations between them. Thus, in his Logic of Scientific Discovery and in his later writings he defines explanatory power, simplicity, corroborability (testability) in terms of logical probability and logical content. He attempts to fulfill the second criterion by using

logic as his "organon of criticism”. This is not to suggest that for Popper logic is the complete objectification of the human mind or of the rules of human reasoning. He acknowledges an alogical process of “producing" theories. However, once theories a.re produced they have a life of their own which, as I mentioned above, can be studied independently of any study of the subjective process of knowing.

Though there is truth in Popper's observation that knowledge exhibits an independence of the knower and his particular knowing process, it is false to conclude that knowledge is independent of all knowers to the extent that it is not fruitful for epistemology to study the knowing process. There is nothing in "problems, theories, and arguments as such" which accounts for the emergence of them (which Popper admits) and thus for the succession of problem upon problem, argument upon argument, theory upon theory. But then "problems, theories, and arguments as such" cannot completely account for the emergence of better and better theories, that is, scientific progress. Nor could the ultimate criterion of a better theory be fully objective in Popper's sense if we accept the argument presented earlier concerning in- dependent reasons for accepting theories.

Now, Popper's theory of verisimilitude was refuted by a counterexample. This demonstrates that his theory is not broad enough. The question is whether any theory of verisimilitude could be broad enough given his objectivist assumptions. If the notion of approaching the truth is in fact a subjective intending, then any objectivist formulation is bound to be narrow. Objectivist position could be supplanted by objectivist position, each succeeding one presenting counter-examples against its predecessor. The “best" we could hope for would be a personal position disguised as

an objectivist position, as in the case of Popper's pupil Lakatos. Though he describes himself as an objectivist and condemns both Polanyi and Kuhn for being irrational because they take their stand on personal criteria, he comes curiously close to their positions. He states:

> The hallmark of empirical progress is not trivial verifications: Popper is right that there are millions of them. It is no success for Newtonian theory that stones, when dropped, fall towards the earth, no matter how often this is repeated. But so-called 'refutations' are not the hallmark of empirical failure, as Popper has preached, since all programmes grow in a permanent ocean of anomalies. What really count are dramatic, unexpected, stunning predictions: a few of them are enough to tilt the balance; where theory lags behind the facts, we are dealing with miserable degenerating research programmes. (My emphasis)

Obviously, dramatic, unexpected, stunning predictions" depend not only on the state of the art, but on the state of the person. If all knowledge were purely objective, it would be inconsistent to make the surprise it evokes in the best minds in the field at the particular time a criterion of its value.

However, I fear that Popper's failure has primarily given support to those who do not think that science is approaching the truth, or that it is impossible to justify such a theory. One of the latter is Thomas Kuhn. His case is particularly interesting for he rejects Popper's objectivism and has achieved at least a partial turn to the subject. Though he has a theory of scientific progress where the better theory is more fruitful, accurate, and simple and has a wider scope than its rivals, he does not think that one can construct a true theory of verisimilitude. The major reason is that because there is "no theory-independent way to reconstruct phrases like 'really there' the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle."

Now it is true that if one constructs a theory of truth and reality, then it is not theory independent, but this does not necessarily restrict one to the realm of mere objects of thought. Thus, it is possible to arrive at a correct theory of approaching the truth. Kuhn seems to be saying that because we do not know what the truth is, or what reality is, we cannot formulate a theory of approaching it, for we do not know what we are approaching. This is the problem of the Meno. If we do not know what we are looking for, how are we to find it? And if we know how to find it, then we must know what we are looking for. Polanyi solves this problem by re cognizing that we have a natural heuristic anticipation of reality. Be~ cause it is natural we have a theory-independent notion of approaching the truth, and if we objectify the intentionality of mind, then we have the basis for a theory of verisimilitude. Such a theory would have to describe how problems develop, how solutions coalesce, how a discovery can alter the state of knowledge and disclose new problems and so on. Most importantly, it would have to show that this whole process has led to the discovery of aspects of reality, and that the more of these aspects a theory discloses and promises to disclose, the closer it is to the comprehensive truth. Just as we do not have to have the truth to approach it, so we need not know what the comprehensive truth is to know that we are approaching it. However, in the light of our previous results, a person can only verify for himself that he is approaching the truth. The invitation to self-understanding and verification is an invitation towards the further development of personal knowledge.

Though developed scientific views can be explicit, coherent and consistent, the development of science relies on a movement into the unknown where one relies on intimations which are not explicit, struggles with what is for him not yet coherent, and

risks being inconsistent with achieved scientific results. It is a truism in science that each new discovery releases more questions than it solves. Science, then, is also a progressive uncovering of the unknown as an area to be reckoned with alongside the known. It is paradoxical, then, that science has contributed to the rise of the myth of complete clarity in knowing and of the ideal of a fully knowable universe subject largely to man's control. Knowing is always on the way, unfinished, incomplete. It is a risky movement into the unknown. This too is in contrast to the complacent popular attitude that science, through the influence it has on technology, will resolve such major problems as shortages of resources and ecological breakdown. Though science has been progressive, progress is not automatic, nor will it necessarily come when we most need it. Science is a cultural achievement. The culture, like the individual, can fail to achieve.

The foundation of the progress is the intention of reality by individual scientists within the framework of their commitments to their traditions and contemporary group commitments. However, Polanyi's epistemology allows us to draw more conclusions concerning the structure of scientific development. Is the development of science cumulative or not? One view that it is cumulative implies that each development in a particular area of scientific theory incorporates prior scientific results in such a way that the prior results can be deduced from the new theory. Thus, it is claimed that Galileo's law of falling bodies and Kepler's three laws of planetary motion can be deduced from Newton’s more developed laws of motion. Similarly, Newton's mechanics can be considered as simply a special case within relativity theory. Basically, the question is an historical one. Kuhn and Feyerabend provide powerful

historical reasons for concluding that scientific progress is not cumulative, but more often proceeds by a better theory replacing the earlier theory. Because the question is an historical one, it is not possible to conclude from the structure of knowing to the structure of scientific progress. However, it is an argument in favor of Polanyi's epistemology that it can account for both cumulative and non cumulative growth in science. Cumulative growth approximates the idealized version of the growth of mathematics. Algebra, for example~ goes beyond, but incorporates arithmetic. However, the discovery of algebra, though suggested by absurdities arising from the performance of arithmetic operations, was an intellectual leap which could not be predicted given knowledge of arithmetic alone. Such a leap is accounted for by Polanyi's theory of tacit integrations. Novel insights, then, always exhibit some discontinuity with previous achievements. In non-cumulative growth the discontinuity is greater since it leaves part of the previous achievement behind and transforms the meaning of many of the relationships which are retained. An act which ranges in its scope from the integration of mathematics to those of music, poetry, and skills can also move from one scientific theory to another. Given that these integrations are emergent achievements we should expect some non-cumulative growth of thought.

We have seen how the scientific community retains its unity throughout its history. Later we will see how Polanyi's thought leads to a model of the unity of the sciences. However, the question remains of how scientific thought retains its unity if scientific progress is not cumulative. If the meaning of basic terms and relations are constantly being changed; if mass, for example, means something different for Newton than it does for Einstein~ how do we know that we are dealing with the same aspects

of reality? Indeed, can we even-speak of the same aspects since they are defined differently in different theories? There are two sources of continuity. The first is that "discoveries are made by pursuing unsuspected possibilities" suggested by existing knowledge." We can also add that they are made by pursuing suspected possibilities. Thus, the unity of scientific knowledge in it's development resides in the tacit component of science, the intending of the scientist of the solutions of problems which present knowledge is merely on the way towards solving. Thus, the theory of phlogiston provided a focus for investigation which led to its overthrow by Lav0isier. The continuity between the two theories is not found in an isomorphism between the two or in a demonstration that they both used the same terms to denote the same things, but that they were both attempts to solve the same problem "what is fire?". Though fire is conceived differently by both, and thus, in a. sense, they meant different things, they were intending the same thing.

A second principle of the unity of scientific thought is suggested by Polanyi's statement that "processes of creative renewal always imply an appeal from a tradition as it is to a tradition as it ought to be." This solves the problem of whether certain practices of alchemy, for example, should be considered scientific since they once existed alongside and inspired what we now consider proper scientific practice. The answer is that they were once part of science, but they should not have been. Science is in the process of determining what it is, and this process may entail excluding what was previously included. The same is true of scientific theories. Some which were previously entertained are now rejected as unscientific. Others are recognized as scientific, but are no longer accepted. In other words, current practice is the norm for

determining what is scientific. In the same way current theories determine what was being referred to by other theories. Thus, the theory of oxidation tells us what fire is. At the same time it tells us what proponents of the phlogiston theory were really referring to without knowing it. Likewise, Einstein had a better understanding of what Newton was trying to understand than Newton did. Even though he has different meanings for the same terms used in Newtonian mechanics, he still understood what Newton tried to understand. But in addition, he is telling Newton what that is, so to speak.

Finally, the pursuit of truth requires a "belief·' in its efficacy. Since we cannot prove that all our endeavors will succeed, because proof only emerges with success, this belief is a faith in the long-term results of the scientific enterprise, a faith that we will reveal aspects o£ reality in the future. As we have seen in this chapter, this faith in science can only be upheld by a society of people who share fundamental beliefs. This is the basic relation between science, faith and society. Its verification also requires an appropriation of one's fundamental commitments and recognition of their consequences.