HAZARD AND SCIENTIFIC ADVANCE

A. G. E. Blake

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In this essay, we look at scientific advance as exemplifying *hazard.* The term "hazard" is used here in a technical sense to designate that which makes progress possible.*

* c.f. Progress and Hazard, J. G. Bennett, *Systematics,* Vol. 5, No. 4.

The elementary structure of hazard has three components: firstly, that the state of affairs has in it "holes" or "gaps" which make indeterminacy a real feature of existence; secondly, that the different lines of action which can arise or be taken have associated with them different degrees of potentiality for significant future events; thirdly, that a decision or commitment has to be made, which is critical for the outcome of the "moment of decision". The notion of hazard sheds new light on the meaning of value as a real component of experience and suggests that values have no concreteness apart from hazard.

Disregarding values, hazard is important for an understanding of anything which can gain or lose coherence in the course of time. There fore science should provide many examples of the role of hazard in progress. Science by its very nature cannot be a static thing: if it repeats or even consolidates the past, it is in decay. Science looks forward and seeks to bring about a future of potentiality — for itself and for the community that sustains it. Because of this, scientists are tempted to take up the mantle of prophets.

Intentionality is allied to the unknown future, since if the outcome of the present scientific activity were predetermined, then we should no longer consider such an activity as science. It must be added that without the element of commitment which accompanies the pattern of intentionality, there can be no initiation of a process which can build up into an episode of scientific advance, **

** The reader should refer to the paper "Resolution of Dilemmas in Physical Science", *Systematics* Vol. 4, No. 2, H. Bortoft, where intentionality in the scientific activity is precisely discussed. "The scientist is not merely one who experiments and observes, calculates and generalizes. He is first and fore most one who decides to engage upon a particular scientific activity. This must fall within the limits of his effectual cognitions and operations. We shall call these limits 'the present moment of the scientist'." (p. 95)

In an earlier essay* we discussed the basic dilemma in science; just what is relevant at any moment to the scientific activity? At any moment it is possible to prescribe and define an area of concepts and operations which strictly belong to the scientific activity. At the same time, it is also the case that many other ideas and operations may turn out to have a great deal of importance for the advancement of science, but are so remote that most people tend to reject them.

* A Critical Essay on the History of Science, A. G. E. Blake, *Systematics,* Vol. 3, No. 1, Section 2 "The Historical Meaning of Science".

When a new intentionality enters into the forms of science, we suggested that this should be termed the "programme for advancement"**— since it is future oriented, and is not a prescription for a sequence of actions. The programme of advancement in science is equivalent to the policy decision in a large commercial organization. This entry of intentionality meets with the forms of thought, description and experiment which represent current practice. In this encounter, no one is in a position to know in advance just how the programme will be realized or what sort of modifications it will have to undergo. It can also be seen that at certain times, the same programme can be made effective, and at other times not.

** A Critical Essay on the History of Science, Section 3 “The Making of Science", p.p. 45-48.

T. S. Kuhn in his classic work "The Structure of Scientific Revolution", describes how for most periods, scientists work according to a normative pattern or *paradigm.* The paradigm is nothing more than a model of the successful way of doing science. Scientists carry on working according to this model—that is, trying to imitate it—until there is some very dramatic failure, at which point a revolution occurs which throws up a new model. The belief in a successful way of doing science is tantamount to a belief that the future is already in some sense known —and thereby in some sense fixed. Such a belief is also tantamount to rejecting any relevance of values to the scientific activity. If there is a definite way of achieving success, then the meaning of success has been fixed. The current scientific method then dictates what is to be termed advance or retrogression, and the scientific activity becomes self- justifying.

It is difficult to distinguish between actions undertaken according to the dictates of a method, and actions undertaken in response to value. In recent times this has led to many dilemmas. One can cite the general problem that scientific method with its associations of a "search for truth" manages through its practitioners to be associated with severe ignorance about the realities of human nature. This has led to the intolerable conflicts which are building up in the spheres of medicine, biology and agriculture. The notion that the search for truth is an absolute, and that a method exists in correspondence with this search, is one of the most archaic in the contemporary world; but whereas in art, philosophy and practical politics a belief in absolute discrete values has disintegrated, it still conditions the general form of scientific research. Science is an activity in a human community. As such, it takes an important significance from how it contributes towards the condition of that community. It is striking how often we can observe a mutual insemination between revolutionary moments in social life and an upsurge in scientific activity. When people wish to break away from the past, then they turn to science in the hope of transforming the present in terms of the future.

In speaking about the advance of science, such commentators as Stephen Toulmin have used the biological model of survival of the fittest. The notion of the survival of the fittest has the following form: there is fortuitously produced a large number and variety of elements, and of these elements only a very limited number are able to maintain their existence in the given environment; these then flourish while the others perish. In other words, the selectivity which any theory of evolution requires is provided by the structure of the environment. Theoreticians such as Marjorie Greene, Alister Hardy and W. H. White have severely criticized the adequacy of this model as a means of accounting for the build up of coherence in the selective process. Significant evolutionary steps are marked by the appearance of a form which embodies a large range of potentialities, which in adaptation to particularities of the environment produces a diversity of populations. The possession of potentialities has no survival value in terms of the present environment. Similar considerations apply to the development of scientific ideas. A fruitful idea is one which survives the tests and applications made of it, but such considerations do not touch upon the problem of the arising of a potent idea in the first place.

The really important ideas are always unexpected. One can take as an example the treatment of black body radiation which Planck made in 1900. A few years earlier Rutherford had named what was then known as "the ultra-violet catastrophe"—that is, the failure of classical treatment of radiation energetics—as one of the few small black clouds marring the clear horizon of the perfect system of classical mechanics. Planck's investigation of the problem led to the formulation of the quantum theory of energy, which in its turn became the basis for quantum mechanics and opened the path to wave mechanics. Planck himself recognized something of the radical nature of his discovery, and for that reason restrained himself from publication for many years. Nevertheless, even he could not possibly realize what would flow from his particular step of quantizing the energy of the harmonic oscillators in the walls of the radiation cavity. All of the really important steps in modern science combine the solution of particular problems with a revolution in basic methodology. This should be contrasted with the adaptation of theories to meet experimental criticisms by the addition of *ad hoc* assumptions.

In any period of science, there exists an indefinite number of prob lems in the form of inconsistencies. Any one of these if brought into focus and worked upon might lead to the most revolutionary changes in outlook; but until one is selected and investigated, there is no way of knowing beforehand whether something will come of it. There is also some law of saturation—that is, when a very large number of papers are being produced concerning a particular subject area, then it becomes almost impossible to make any new approach to these problems. A scientist of insight such as Enrico Fermi always knew the time at which it was best to move from a particular set of problems into another domain. In many ways this is equivalent to the brilliant stockbroker who knows when to buy and when to sell, but in this case it involves the creative activity of the man himself to realize the potentialities of the situation.

The steps of scientific advance are the steps of coalescence.* There is far more in what Kuhn calls a "scientific revolution" than a conceptual change. There are adjustments to be made in modes of experiment, and there is a new direction to be brought into practice in the organization of research. There are new forms of expression to be developed, and often the so-called revolution requires the coincidence of various streams of information, and even various types of scientist. How all these things are put together and made to work is not under the control of any individual or group. This renders the direction of research a hazardous thing. Those who are responsible for such direction today are in exactly the same position of members of the Royal Society of London around the time of Isaac Newton. They all had some idea of what was interesting to them, but hardly one of them could realize the situation which Newton had created by the publication of his *Principia.*

* The term "coalescence" designates a mode of togetherness in which order is maintained by the coalescent state itself, e.g. as in an organism.

Every piece of significant scientific work is a communication about what is possible in the future of science. Thus, for example, the theory of relativity was a communication emphasizing the future importance of investigations into the laws of framework and the structure of systems of measurement. This futuristic aspect of scientific achievements has been largely masked by attending to such achievements only in terms of explaining or solving problems. As such, they would be only closing links in a cycle whose content was known.

There is a very important distinction to be made between following an established model of doing science in the belief that this will be the way of attaining success, and trying to "read" the array of significant scientific achievements which are present and effective in science as a pattern of indications of what will be fruitful in the future.

In the next section, we shall attempt to clarify the notion of the hazard of scientific advance, by taking particular moments of uncertainty and potentiality with outcomes of some significance. For this purpose, we consider science as a many-levelled structure and use a scheme of seven strata as a convenient means of dealing with our examples.

THE SEVEN STRATA OF SCIENCE

A structure capable of transformation exhibits the following features:

> It has more than one dynamism (as an organism has independent "systems" with a high degree of regulative independence).
>
> It has a concentration for the totality of the stresses or struggles between its parts—a harmonization that can never be complete

These features alone establish the grounds for choosing a seven- term system—two triads and a central point. But we should then add a third criterion:

> As all terms of a system should be considered as of equal systemic status, so the seven terms should be arranged in an harmonic series within the structure with equivalent intervals between them.

Each succeeding term of the structure should present the context for the relevance of the preceding term. In an organism, for example, the bio-chemistry has its relevance in the cell, and the cell has its relevance in the tissue, the tissue in the organ and so on. We should add that structures capable of transformation are *open systems* and take their coherence primarily from this "nesting of relevance".

The seven terms which can be elaborated for science are as follows:

7. Zeitgeist
6. Creativity
5. Organization
4. Assertion (polemics)
3. Convention
2. Conditions
1. Materials

The materials available to science at any one time includes information and money as well as actual substances. The purity of chemicals, the availability of certain states of matter, access to certain regions of the universe, and so on, constitute the basic existential starting points for any scientific activity. The relevance here of financial resources is also obvious, especially today. Most importantly, it is the available information which constitutes the raw material for the development of any scientific practice.

All this available material is encountered and used under certain conditions of scientific work. What we call techniques, whether these are in experimentation or in calculation, are ways of making meaningful the available material to the theoretician or experimenter. We should also include here the circumstantial framework in which particular scientists or groups of scientists work. It is the conditional factors of places of meeting, of laboratories, of information retrieval systems or libraries, which channel and condition the operations made with materials. In particular, it is the circumstantial framework which conditions the communication network which disseminates the available information.

The routine meaning of techniques and their results is established as a convention. Since science is an activity conducted through the cooperation of many people, it depends upon a stable language, both verbal and operational. Without a certain stability of meanings, the scientific activity would become so fragmented that coherent advance would not be possible. For that reason, there is always a big problem in transmitting the acquired insight of one generation to the next. This problem introduces further factors of selection that further determine the conditions under which scientists work.

When we come to uncertain regions then the scientist is forced to make commitments which he is not able immediately to justify. Polanyi has well brought out the significance of this strata of science in his insistence on the "passionate knowledge". The available facts, operations and conventions, will never of themselves enable any discovery to be made. In order to make a discovery, a leap has to be made in the dark. Many scientists are forced to embark on long investigations with no guarantee that any success will accrue to them in the direction they choose. Further, since science is a cooperative activity, scientists are forced to commit themselves publicly to ideas and propositions which may be the subject of severe controversy. Without a commitment that enters into action, assertions about the structure of the world remain mere abstraction. It is the practical commitment associated with assertion that characterizes science.

The realization of any germinal idea or uncertain assertion nearly always requires a combination of talents, and often a critical structure of these talents. In general, it is by the social organization of science that the requisite inter-subjective testing and assessment can go forward which maintains the dynamic struggle which focuses around assertion. However, there are many instances in which it has been only by the existence of a group of scientists, with a certain structure of dispositions, that major steps have been made.

To make a further step through the strata of relevance, we must remember that science is a creative activity. A certain grouping of people is only significant if it brings about the right conditions in which a creative action can take place. Creativity is marked as an action in which there is a discontinuity between past and future. It may be argued that there is always an inevitable continuity in the development of science between past and future, but we should notice the many expres sions of surprise that we can find in works on the history of science over the failure of people at particular periods to make certain steps which in the light of present knowledge seem glaringly obvious. Without creativity, science would degenerate into routine, but it is also a creative mode of expression that enables quite new orientations to be made in science. In other words, we should take account of a creative mode of operation that produces changes in the pattern of intentionality govern ing the orientation to the future of the work of the scientist.

Finally, we should consider the relevance of science in the present moment of human history. In external terms, we can see science playing different roles at different periods in human history, but we can also appreciate that science participates in the mental alignment of an age for which Goethe coined the term Zeitgeist.

PHENOMENA OF HAZARD

For each of these orders of relevance we can easily find examples, as we have said, of moments of significant uncertainty. For the lower orders of relevance, these moments appear in terms of the influence of contingency or chance on the situation; whereas for the higher orders of relevance, we have some of a structuring element which is not under anyone's control. We shall consider examples for each of the seven orders before examining more closely the nature of the hazard involved in scientific advance.

(1) *MATERIALS*

The impossibility of fully detailed prediction is recognized throughout science, but it can also be appreciated that what is inaccessible to fully detailed prediction is what constitutes the proper interest of the scientist. Consequently, what kind of facts turn up, their degree of precision and the time at which they arise, govern the possibility of scientists becoming aware of the limitations of their powers of prediction. In general, when we come to know something, our knowledge arises by means of a nexus of interactions, the detail of which lies deeper than our knowledge. This gives to facts a quality of " bruteness". There is something in them which is quite independent of our determination. They are contingent both in their inherent complexity and in their knowability.

At any stage in the domain of scientific work, there will be available only certain facts, and these will to a large extent determine what can be done. One of the most widely known examples of this in the history of science is in the work of Kepler on the orbit of the planet Mars, Towards the end of the 16th century, Tycho Brahe had brought astronomical observations by means of the naked eye to the limits of their possible accuracy—rarely in the whole history of experimental work has such care been taken. Brahe invited Kepler to work with him just at the turn of the century, and when he died left Kepler a quite unique selection of observational data. Kepler's concern was to find, amongst other things, the exact orbit for the planet Mars. Kepler was one of the few self-professed followers of Copernicus in taking the planets as in movement about the sun, instead of about the earth; but he, like Copernicus, still thought along the traditional Ptolemaic lines of viewing planetary orbits in terms of a composition of circular paths. As he worked, Kepler corrected the Copernican scheme and simplified it to the point where he established a circular orbit for Mars with its centre slightly displaced from the position of the sun. But, in checking his answer, he found, using Tycho Brahe's observational material, that he was in error by eight seconds of arc. Now the accuracy of Tycho Brahe's observations was to within four seconds of arc. It was this alone which set Kepler searching yet further. These latter investigations led him eventually to a realization that the planetary orbits had the form of ellipses, which eventually proved critical for the work of Newton and consequently for the whole development of dynamics and kinetics.

Sometimes the contingency of factual material can have a negative effect. In 1919, for example, the planet Pluto was sought after at the Mount Wilson Observatory, but nothing was found on development of the photographic plates. The reason for this was not discovered until after Pluto had been discovered at the Lovell Observatory in 1930, and old Mount Wilson photographs were re-examined. It was then found that the planet would have been discovered in 1919, but the image of the planet had fallen very precisely on a small flaw in the emulsion.

We can consider another example from the realm of materials as such. Hopkins, one of the pioneers of biochemistry, set one of his practical classes to make a routine test for protein using the then standard reagent of acetic acid. Not one of the class could obtain a positive result with their protein samples. On investigation, Hopkins discovered that the sample of acetic acid used was exceptionally pure, and it was missing the presence of glyoxylic acid, which he then determined to be the natural agent necessary for the protein test. This then led on to his discovery of the group in the protein agent which reacted with the acid, an important step in eliciting the structure of protein.

As a further example of the significance which can arise from a contingency in the realm of materials, we can cite a story from the work of the Curies. It was by means of a gift of ten kilogrammes of pitchblende from the Austrian Government, that they were able to begin their labour of isolating a sample of radium. The government had considered the pitchblende to be quite useless, since they had extracted the uranium salt from it for use in the manufacture of glass.

(2) *CONDITIONS*

The techniques operative within science are so pervasive that it is often difficult to disentangle their particular operations and modes of influence on what can be done. It is perhaps easiest to discern the influence of techniques on what can be done in terms of their conditioning effect. For example, there is little doubt that the introduction of the tensor calculus through the theory of relativity has had an inhibiting effect on the development of other schemes of mathematical representation of much greater relevance to problems which arose in quantum mechanics and field physics after the theory of relativity. The tensor calculus happened to be a mathematical technique which was convenient to Einstein for the presentation of his ideas. It had no special merit of its own. However, in assimilating the implications of the theory of relativity, the scientist assimilated the representational system of its mathematics at the same time.

One need not regard techniques purely from this aspect of conditioning. Again, in respect to relativity, it was the prior existence of the non-Euclidean geometries developed by Riemann and Lobatschewski that gave a precedent and a framework for Einstein's analytical investigations. The inter-connections between mathematics and physics offer many such examples of the seemingly chance interplay of mathematical techniques developed for internal theoretical reasons and critical developments in theoretical physics.

(3) *CONVENTION*

The arising of forms of thought and forms of operation appears at first sight in the history of science as something almost random. We can begin by taking an example of a development that was formally possible but which did not in fact occur. Neugebauer* has drawn attention to an interesting possibility open to the Greek mathematicians of the tune of Aristotle and the later Stoics: their syllogistic logic only needed the introduction of variables represented by letters for the formation of the basis of an algebra. He writes: "Such a development would have been still more natural, since the axiomatic approach to mathematics originated in the same time and among the same circle of men . . . this is a good illustration for the futility of any attempt to reconstruct 'reasons' for the incidence of historical events ... all that we may ever hope to establish in historical research is facts and conditions, but never causes."

* *The Exact Sciences in Antiquity.*

The point about convention is that it is rarely established deliberately by any individual or group. This means that scientists at any particular period have to take for granted the existence of stable mean ings for operations and terminology. This in its turn means that they have to take over the ambiguities and uncertainties inevitable hi the conventional stratum. All words carry a welter of associations and historical connections quite incidental to any precise use that may be wanted of them. Even neologisms become hi tune grouped around various modes of applications which sooner or later are found to have embodied meanings in usage quite different from the established ones which gave birth to the new words. All this makes the establishment of a new way of talking an extremely difficult and complex undertaking. The usual component is that of haphazard compromise between the specific intention of a scientist and the unfocussed associations of his words.

The inevitable inertia and ambiguity in the meanings which can be associated with words results in the situation that new ideas have to wait upon a suitable growth of language. This helps us to see why there should be very great difficulties in establishing radically new ideas hi science. The effective formation of new meanings has to combine directed experience together with the use of terminology. Before Galileo could communicate meaningfully about the Copernican view of the solar system, he had to persuade people to look through the telescope or come to some practical understanding of what the observations he published in the "Starry Message" concerning sun spots, the phases of Venus and so on, meant. The connection of words with experience is at the foundation of great synthetic moves in science such as that made by Galileo himself in the sphere of mathematics. His work required the gradual formation of meanings for such terms as "acceleration", "motion", "impetus" ("impeto"—akin to our modern "momentum") and others.

In his early writings on mechanics, the evidence seems to indicate that he was not clearly discriminating between an idea of acceleration as velocity increasing with respect to time, or velocity increasing with respect to distance traversed. Indeed, for a long time after this work of Galileo, scientists were still confusing the two definitions. Both the correct and the incorrect relation—the acceleration discussed was that of gravitation—had been transmitted from mediaeval times, so that the term "acceleration" carried a complex of associations which made it difficult to establish a specific use for it as Galileo came to do.

What is difficult to see in this is that it is this very ambiguity that makes possible any significant advance in science. If all ambient forms whether in language or in operations, were strictly definite in their meaning and application, then it would not be possible to use them for the expression and working out of any really original idea or insight It is also just this that makes the transmission of scientific ideas subject to hazard. This we shall see more clearly when we come to examine the stratum of assertion in which we encounter the polemical aspect of science.

(4) *ASSERTION*

Aquinas made a clear division between true knowledge (which is of Being), and opinion, or *doxa* as it was called by Plato. What is apprehended by reason cannot fail to be assented to by the mind, but some things are apprehended by the mind in such a way that we are free to assent or dissent concerning propositions about them. In the latter case, there is a judgment on our part, associated with an act of will. Pascal wrote: ". . . the more usual (way of coming to think) is that of the will, for all men are nearly always led to believe, not by proof, but by inclination." Opinion is necessary for the advancement of science. It is not feasible that a commitment to an idea should wait upon a full and complete support from extensive tests—indeed, appropriate tests can only be provided by the development of the idea which depends upon a prior commitment. If this were not the case, then science would indeed be purely empirical and could be regulated there fore by some kind of mechanism.

There is a nexus of conflicting opinions throughout the history of science which should serve constantly to remind us of the incompleteness of scientific insight. At the same time, if there were none to stand by ideas which could not be demonstrated as important to the satisfaction of the majority, then there could arise no force sufficient to implement research contributing towards really significant advances. The full event of a piece of scientific work requires the incorporation of the nexus of uncertain ideas into its structure.

In writing about this, Polanyi says: ". . . we have seen that the progress of scientific discovery depends upon heuristic commitments which establish contacts with reality, and that the hazards involved in entering on such a commitment are twofold: namely, (1) that it may be mistaken, and (2) that even if it is right, its future scope and significance is largely indeterminate." Following Polanyi, we can speak of the contingency present in assertion as that of hazard. It is in this sense in ordinary speech that we use the phrase "to hazard a guess".

A very interesting case to consider is that of the Berthollet-Proust controversy. Berthollet had grasped the importance of relative masses, temperature and other conditions to chemical change—factors which were largely ignored by his contemporaries in their search for simple tables of "affinities". In following up his ideas—-on what today we would call "chemical equilibrium"—he concluded that the composition of certain substances could vary continuously between certain limits. Such a view directly contradicted the notion of a law of constant pro portions and such a law had been clearly developed by Proust. There ensued an involved controversy lasting from 1801 to 1808. The key to the arguments was that there did not exist a clear distinction between compounds and mixtures (remember our comments on the relevance of convention). Berthollet for his arguments looked at mixtures and Proust looked at compounds. Eventually, Proust was able to demonstrate to nearly everyone besides

Berthollet the validity of the notion of constant proportions. The importance of Berthollet's ideas was even more obscured than at the publication of his *Essai de statique chemique,* which in its concern for the complexity of chemical phenomena, had proved much too difficult for his contemporaries.

The full potentiality of the situation failed to be realized; neither side of the controversy—though they argued with great courtesy—was prepared to make the smallest sacrifice of their own convictions. This example also demonstrates the inadequacy of treating scientific advance as a series of linear steps.

All of this is very important when we come to look at the outcome of the work of someone of the quality of Isaac Newton, who, reacting against the Cartesian dogmatism of his tune, firmly pronounced that he did not "make hypotheses"—in other words, that he did not assert beyond the "self-evident". An examination of his actual work will serve to show that his significance lay in his very assertions, because these implied something beyond mere convention—they were the assertion of new conventions. The way in which he spoke of gravitation was the assertion of a new way of linking together facts and systems of order. He did not say that there was some "force" pulling bodies towards each other, but that there was an order in the acceleration of bodies that necessitated the recognition of a connection of acceleration with the "amount of matter" in things. But this assertion carried with it many implications and possibilities, and, in making it, Newton contributed to unforeseen results.

Gravitation opened people's eyes to the possibility of "powers" residing in matter previously dismissed as "occult" nonsense on a level with alchemical obscurantism. After Newton, it was possible, "scientifically" to speculate on matter being more than just passive, inert, lifeless, solid "stuff', which had been the Cartesian and Galilean picture. One of the developments of this is exemplified in the work of the Frenchman Maupertuis, who was foremost in his country in realizing the worth of Newton's work, not only in the *Principia* but in the speculations of the *Opticks,* where Newton concerns himself in the "Queries" with the possible forces responsible for the cohesion of bodies and their chemical properties. Maupertuis fell to seeing that if "particles of matter" were possessed of sensitivity, and even of memory; this would account for the phenomena of hereditary, such as the order inherent in the sequence of similar organisms arising through a chain of births. He had the notion that from the parents came various "particles" corresponding to each organ which were characterized by a "memory" of the organ. Nothing could have been further from the mind of Newton himself, but the precise nature of his own assertion was not only obscure in the middle of the 18th century, when Maupertuis did his work, but was already obscure in the 17th century. Around Newton, amongst his most ardent admirers, there seems to have been no one who really saw what Newton was asserting in the notion of gravitation, and Newton himself was not free of uncertainty on the matter. His followers, for example, Samuel Clarke, engaged in polemical battles on behalf of his ideas, and thus threw into the field their own assertions. Assertion has this nature of being open to being caught up in whole cycles of assertions *continually in action.* This is why *assertion is essentially hazardous.*

It is important to consider the example we have just discussed in relation to the lower three orders of science. In our descriptions of these lower orders, we should have established a sense for the complexity and pervasiveness of factors which have to be taken as given by a scientist at any given moment. If there were nothing but these lower three orders, then science in its changes would appear simply as the result of chance combinations of its components within a given social and historical environment. The situation would then parallel that pictured in traditional Darwinian models of the evolution of life.

It is just in this order of relevance which we associate with assertion that we can discern the need to take into account holes or gaps in the nexus of conditioning factors which enable a significant step to be made. In the case of the Newtonian advance, it is striking how blind the Cartesian school were to the limitations of their physical method. It was just in the sphere of rationalizing the planetary motions and relating them to the emergent science of mechanics that it was possible to demonstrate the limitations of the Cartesian approach. Without that demonstration, the future power of strict mathematical methods in physics used separately from qualitative models would not have been apparent for perhaps many decades. More is involved in such a step than the solution of particular technical problems. There was of course the importance of solving the problem of the mathematical description of a particle moving under a centrally directed force; but this was eventually used by Newton only as a stepping stone to realizing a new programme in physics.

The Newtonian step established a means for future scientists to connect with a way of doing physics and even chemistry which was different from that which went before, and which eventually had results far beyond Newton's anticipation. This exemplifies an important feature of all moments of scientific advance: what is released is a means of access to potentialities for future science which enable it to take a different direction from that which came before. Such moments reflect into moments of uncertain assertion manifested by individual scientists. We should add that in this respect we should distinguish between the assertion which is not connected with practical scientific work and the assertion which is. In the former case we have simply a programme for advancement which may or may not be brought into the sphere of realization. In the latter case, we have the truly significant moment in which a door is opened to the then unknown world.

(5) *ORGANIZATION*

We can look at the "present moment of science" as a structure of *roles.* There are technicians, teachers, applied scientists, philosophers, historians and creative researchers. A weakness in any of these roles is a weakness in the coherence of the scientific present moment. The roles can be correlated with the six "dimensions of the present moment"* in this way:

* c.f. *Dramatic Universe,* Vol. Ill, J. G. Bennett, Chapter 42.

1. Technicians: deal with the material of the past as it affects current practice.

2. Teachers: deal with the values and patterns important to the right advance of science.

3. Applied scientists: deal with the anticipated future by using the content of present science.

4. Philosophers: deal with the forms of thought, practice and language on which science depends for its activities.

5. Historians: deal with eliciting the significance of critical events in the history of science.

6. Creative researchers: deal with the unknown future of science.

Something of this structure is always to be found. We can, however, also discuss the order of relevance of organization in terms of the variety within any of the components. Organization arises from the need to order together all the various different contributions that can be made to some task. Often the organization is something that has no obvious external form, but is instead something that arises spontaneously within some group of scientists. What usually happens in scientific groupings is the dominance of one group or personality in terms of authority. It is very rare that a state of coherence can be achieved in which the different types are brought into cooperative balance. In particular, a scientist carries with him an approach to science which is so integral to his thinking that it is very difficult for him to objectify it, and hence to appreciate the ways of others. The right encounter between scientists, the proper balance of organization, specific contributions coming at the right moment in some undertaking, are all major factors which are crucial in scientific advance. It is very rare that such coherence is intentionally established; indeed, all that we can observe are moments of spontaneous organization which constitute a powerful means for the concentration of creative work.

It is difficult to over-estimate for example, the significance of the concentration of creative activity of the University of Gottingen during the 1920's. There were gathered Bohr, Heisenberg, Dirac, Oppenheimer and many others who became the pioneers of new methods in physics. During the really effective period, no authoritarian distinction existed between professors and students, and the excitement of the work spread throughout the whole town. Here were some of the most brilliant and diverse minds of the age gathered together to confront the disintegration of classical ideas in physics. Within seven or eight years the new seeds of modern physics had been formed, and after that there came the inevitable decay and crystallization as new authorities and conventions became established. Later on still, the free cooperative spirit of Got tingen gave way to the mutual incomprehension and suspicion which spread with the political difficulties that led to the second World War and the explosion of the first atom bomb.

Without actual experience of the way in which diverse types can cooperate in the moment of significant advance, it is difficult to appre ciate how important this stratum of organization is. At the same time, only experience can show that individuals responsible for the constitution of groups and research teams are not in a position to find and select those whom they take to be appropriate types. It seems rather that moments of significant advance attract the necessary people to enable something to be made out of the possibilities.

(6) *CREATIVITY*

In this stratum we are concerned with a patterning that is certainly not under anyone's control. No one can seriously doubt that at all times in human history there have existed men of creative power— nevertheless there have also existed radical differences in the concentration of creativity in the realm of science. We may think of the creative period of Egyptian science around 2000 BC or the flourishing of Hellenic science between the 4th and the 3rd centuries BC; again we may think of the resurgence of science in Islam, between the 9th and 12th centuries and so on. It is of course quite feasible to correlate these fluctuations of creative intensity in science with general social and historical and even economic factors, but such correlations cannot explain the differences between different moments in human history which are strictly comparable in terms of having men of high intelligence with sufficient leisure and resources to develop scientific insight. There remains, above all, the problem of why it should have been in Europe that the explosion of science took place in recent times.

There is another important aspect of this stratum which concerns the intrinsic differences between the patterns of intentionality working hi science at different periods. There is more in this than trying to bring into the compass of our discussion the prevalence of certain ways of thinking which might in themselves be important, but which had no practical effect on the mind of a community in the way that modern science has—such as Taoism in China, for example.* Unfortunately, most people do not discriminate between making a distinction between classical and modern science in terms of the content of the correspond ing theories and making a distinction in terms of the pattern of intention. In most spheres of human activity today, it is still taken for granted that to be scientific means to follow the analytic and atomic modes of analysis and action which flourished at the beginning of the 19th century. In modern research, however, what is happening is the achievement of a quite new kind of coalescence between theory and experiment that is destroying the now artificial separation of model and phenomena which has previously obscured their mutual inter-penetration in practice. There is also a concern, not so much with explanation, as with a kind of structural description that reflects our modes of observation and experimentation, as well as the characteristics of the natural structures dealt with. The conventions of language and operation have not yet caught up with the essential content of what is important in modern science.

* c.f. *Science and Civilization in China*, J. F. Needham, Vols. I - III

The third important aspect of this stratum of creativity is the patterning evidence in the regions in which creativity is exercised at any particular period. Time and time again important discoveries are almost totally ignored for long periods of time, while others which have no higher merit are made almost immediately fruitful. The ignored dis coveries then stand out as anomalies in the history of science. We can quote here the remarkable observations of Leeuwenhoek who in the early 18th century by using the most primitive microscopic instrument, made actual observations of bacteria 150 years before their existence was acknowledged in biological science. We can also quote the remark able experimental work of Henry Cavendish or the mathematical discoveries of Guass. An extreme example is the famous Format's last theorem—the theorem which Fermat claimed to have proved on a document from which the proof itself is missing. If a proof of that theorem could be found, then it would solve many fundamental prob lems in mathematics today.

The notion of a pattern in creativity is one that runs counter to the prevalent assumption that creativity in science is purely a matter of individual spontaneity and interest. The "think centres" such as Prince ton's Institute for Advanced Studies in America , are set up in the belief that discoveries will be made under free conditions that are both truly creative, that is, significant, and also random. In this way it is hoped that there will become available a pool of creative insights which can be tapped for developments according to social criteria of relevance —but this is to ignore the delicacy of the requisite grouping of people with respect to tasks, and has at its centre a fundamental misconception: if the creative steps to be made are also to be significant, then they cannot be random; if they are not random then they correspond to some kind of pattern—but this pattern cannot be controlled. It is with regard to this that we spoke in the first section of reading the moments of significant advance as an array which can communicate to us the pattern of future possibilities.

(7) *ZEITGEIST*

In this final stratum of relevance we view science in its total context in human history. The notion of a patterning in creative moments naturally finds its place here. If there is indeed such a pattern, then it takes its relevance from what is significant for future human society.

Earlier on we gave the example of the significance of scientific advance for the release of people from external authority. There is probably a corresponding contribution that science can make to the synergic integration needed by future society if it is not to totally disintegrate.

It must be made quite clear, however, that such a view supposes that the control which people can make of the advance of science is limited to the four lower orders. Taken in the wider strata of relevance, we can indeed discern an advance, but this advance then presupposes a higher order of control. It is a control that works through the creativity of individuals and groups involved in science. Just as we think of the scientist as a person who does experiments and learns from these, so we can picture science itself as a total experiment which is making its contribution to the evolution of the human mind.

At the point of contact between the two orders of control—we might also say two orders of intelligence—we have the point of con centration of hazard. In the next section, we will attempt to bring out certain key components of this concentration.

THE COMPONENTS OF HAZARD

The system of seven strata which we have just used in our discussion of the phenomena of hazard in science, has only served to bring to our attention the existence of regions of "objective uncertainty". Between each of the orders of relevance we can speak of an interface which embodies indeterminate states. The examples we gave of moments of significant advance drew attention to various kinds of indeterminism. But, by and large, this indeterminism was looked at from the point of view of what can be known. Only indirectly did we refer to the inherent indeterminancy in the actions of science. It is when we look at science as an activity incorporating various kinds of action that we focus on the importance of *timing* in moments of scientific advance. Taking the structure of an ordering activity to be a four-term system, we can speak of the interplay of the materials, techniques, ideals and research direction. These four factors arise from four partially independent sources, and their coincidence in any region of scientific history introduces a structural element that cannot be looked at in a causal way.

But there is a third aspect besides what can be known and actions: this consists of the patterning of an intentionality to which we have often referred. Although philosophers have been forced to take into account the factor of decision when discussing scientific measurements, little attention has been paid to the significance of acts of will in the totality of science. One of the reasons for this is that in the sphere of measurement it is possible to deal with acts of decision in an atomic manner; but this is not at all possible in more complex fields. There is a great reluctance to speak of a *pattern* in decision, because such a pattern could not be known, and therefore appears an empty notion. But this reluctance highlights the crucial difference between knowledge and understanding: although it is not possible to know a pattern of decision, it is indeed possible to understand it. It is something of this understanding which enables us to really grasp the nature of a scientific discipline so that we ourselves are able to participate in it. It is widely recognized that even the most exhaustive description of any discipline will not of itself serve to communicate the pattern of intentionality that is integral to that discipline.

The phrase "pattern of intentionality" we are using to embrace the notions of decision and commitment. Patterns of intentionality permeate the total data of science, and cannot be localized in any one stratum. Keeping in mind the three aspects of what can be known, actions and the patterning of intentionality, we can re-examine the nature of hazard in science. For this purpose, we can use a five-term system to look at hazard as a focussing of significance.

(1) *NOTICING*

The real fruit of a moment of scientific advance is the detection of some anomaly in what is known about the world relevant to science. This applies on many different scales: for example, we can consider from this point of view both the advance made by Hopkins in bio chemistry by noticing the anomaly in the effectiveness of the reagent used by his class, and also such things as the recognition by Bacon and Descartes of the failure of Europe to make any significant advance over the ancients in science. In other words, noticing can apply at any of the interfaces between the different orders of relevance of the strata of science. There is a significance to be noticed, for example, in the alignment of present-day researchers in advanced physics towards psychology and structures.

(2) *QUESTIONING*

To pose a question is almost to make oneself notice; it can also be looked at as an indirect form of action; finally, it can be interpreted as the formation of an open intention. These various aspects of questioning are important, because a question should not only be looked at as the next step from noticing some anomaly. A question has the role of opening up the otherwise interlocked content of science. Without questioning, scientific advance is not possible. Further, when a question is made it should serve as a means of connection with what is unknown. It is here that we can become keenly aware of the hazard in scientific advance, for no one is really in a position to say at any moment what are useful and what are not useful questions.

(3) *DECIDING*

As we said, questioning can be looked at as the formation of an open intention—a pattern enters only with decision. Even a superficial study of decision-making in large organizations is enough to show that even though externally a decision may be between two alternatives already proscribed, the decision itself has to take account of the complex relevance of innumerable factors.

What has not been brought out in these studies is how the decision itself has a structural role in a nexus of decisions. In its most elementary mode, decision in science takes the form of a commitment to this or that line of research or field of investigation. Working scientists are strongly aware of the importance of timing in making such decisions.

We have already referred to the capacity of Enrico Fermi to move on to some new field of investigation just at the moment when that on which he has been engaged had exhausted its most significant potentialities. In the context of human society, the problem of deciding just what lines of research should be supported for the benefit of the community is becoming increasingly difficult and important. Ordinary means of forecasting completely fail to deal with the problem of wanting to know the future of research which has not yet been made.

(4) *COOPERATING*

As we have said, deciding is an element in a structure of deciding. Phenomenologically there can be no doubt that any significant scientific advance draws into itself a complex interplay of ideas, information, experiments and people, the constitution of which is far beyond the bounds of control. Certainly, the coalescence of the six roles we referred to under "Organization" is rarely intentional, since it corresponds to a kind and degree of social coherence that exists nowhere save for brief moments of heightened significance.

If an attempt is made to penetrate into regions which at present only a few have any intimation of, then its effectiveness will almost totally depend upon the spontaneous cooperation of people and circumstances with the project. There are countless examples of men who have seen things far "in advance of their times", and for whom this necessary cooperation did not arise. The ideas of Goethe or Whitehead still wait upon a tune when there is an alignment of the attitudes of mind of scientists to enable them to recognize the significance of what they were after. The capacity of any given community of scientists to take in and appreciate diverse approaches to science is in reality very limited. Whitehead himself in his discussions of history spoke of the “chronoscopic fallacy" whereby people can see the past only in terms of the criteria of the present.

(5) *ACHIEVING*

The history of science is full of examples of where a step has been made by someone with a particular intention, the almost direct outcome of which was in direct contradiction to their aim. We can here think of Descartes who, in his attempt to overcome the scepticism towards religion of his age, laid the foundation for an even deeper scepticism; or of Gustav Fechner, who hoped by his postulate of psycho-physical parallelism to establish the basis for an appreciation of the psychical or inner aspect of all phenomena but, instead, laid the foundations of behaviourism. Such examples are sufficient to show how fantastical must be any hopes scientists have of "improving the lot of mankind" outside the realm of material comforts. If there is an evolution in human history, then it concerns not the external conditions of life, but the human mind itself, and the capacity of the human being to act. It was a very great step when science emerged with the promise of enabling men to know the world otherwise than on the basis of authority; but whether man can come to know himself by scientific means is a critical question. Interconnection between knowledge of the world and knowledge of man then makes it seem likely that future science will have to abandon nearly all the convictions which sustained the emergence of modern science. In this aspect of achieving, therefore, we focus on the significance of science in human history and also the significance of any particular step in the totality of science.

The structure of hazard in scientific advance has a practical relevance for the direction of research. A kind of assessment is needed in deciding research programmes which is not simply a matter of looking at past successes. Today, there are very few people in the scientific community who have any capacity for intentionally guiding research in a fruitful way. Little attention is paid still in education to the development of the capacity to notice and question in those who study science. In research organization, the requirement of a free spontaneous cooperation is usually entirely ignored.

In certain regions of contemporary science the turnover of ideas and information has become so vast that traditional methods of establishing clearly defined theoretical structures independent of experimentation, and then, as a separate activity, dealing with their verification and correction, has become inappropriate. There is coming about a kind of "on-line" interplay between theory and experiment which has all the time to adjust to the way in which research seems to be going amongst all the various groups. Science is beginning to look like a kind of tune machine which is always being driven into the future—the problem is to discern where the significant future lies. Neither a blind faith in progress, nor ordinary means of forecasting are adequate to an intelligent direction of science. We can be sure that extrapolation from the past will continue to be of use in the direction of research, but nothing can replace that positive attitude to the unexpectedness of the future which characterizes an understanding of hazard.