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Kuhn's *The Structure of*

John A. Moore

IN THIS AGE OF SCIENCE, it is vitally important to teach our students something about the nature of science itself. Any course in the sciences must focus primarily on the understanding of the natural world that the particular science provides; but the procedures themselves, their strengths and weaknesses, and their philosophical underpinnings should not escape analysis.

Usually we say, also, that our intent as educators is to produce informed citizens. Despite our rhetoric, few students will leave our classrooms with either sufficient information to make intelligent political decisions about scientific matters or the motivation to pursue such knowledge. We fail here. It is within our power, however, to ensure that our students are given enough understanding of the process of science to evaluate its usefulness in providing answers to important questions.

Bacon and the Scientific Method

It is generally agreed that we do a less-than-distinguished job of teaching "the scientific method." Much of what we do present is warmed-over Francis Bacon (1561-1626). That non-scientist, who failed to appreciate a single major scientific achievement of his time (he rejected Copernicus and seemed ignorant of Kepler, Vesalius, Gilbert, and Harvey) began the long tradition that those who do science and those who write about it are seldom the same. Nevertheless, Bacon did have some important things to say about the procedures we should employ in seeking to understand nature.

Knowledge is power, and, to Bacon, the purpose of scientific knowledge was to give humans control over nature. Knowledge is not sought for the sheer joy of understanding but to enable more efficient exploitation. In this sense, Bacon sounds discouragingly modern. He emphasized that religion should not be confused with science, because the former depends on revelation, the latter on ascertainable facts.

We remember Bacon also for his emphasis on inductive reasoning, in contrast to the deductive mode of other philosophers of his time. He is often associated with the notion that, after all the ascertainable facts pertaining to a question are collected, a satisfactory answer (hypothesis) will emerge almost automatically. Bacon was not quite that sanguine and suggested this metaphor. We should

not rely solely on deduction and, like the spider, spin our knowledge from within. Neither should we rely solely on induction and, like the ant, merely assemble things. Instead, like the bee, we should both assemble things and arrange them in order.

Francis Bacon and the beginnings of modern science were contemporaneous phenomena. He studied and formulated the scientific method—a topic that intrigues philosophers to this day. The enormous success of science in the centuries following Bacon would seem to suggest that some very special methods and procedures must be associated with scientific activity. Science could hardly "just happen." Equally brilliant minds were working on the problems in other fields but solutions were elusive and progress questionable. Today the mystique surrounding science seems more pervasive than ever: alternatively we are in awe of scientific accomplishments or in fear that our new knowledge will be used for evil ends.

Beginning with Bacon, the scientific method was viewed as consisting of several discrete and sequential steps; these were:

1. *Asking the question.* Some phenomenon of the natural world is recognized as an interesting puzzle, and more information and understanding of it is desired.

2. *Preliminary collection of facts.* All information that is thought to be related to the problem is assembled. These preliminary data might suggest an answer. This step, from separate facts to a generalization, is induction. The answer then becomes the,

3. *Provisional hypothesis,* which is nothing more than an intelligent guess as to what the answer might be.

4. *Deductions.* At this point, we start to play a game. We assume that the hypothesis is true, and, if so, a number of consequences (deductions) will follow. Thus, if rivers are thought to be composed of water, samples of them should boil at 100° and become solid at 0°.

5. *Testing the deductions.* Some of the deductions may be testable by observation or experiment. We proceed to do so. The tests of the deductions should tell us whether the hypothesis seems to be true or whether it is false. Thus, if the sample of a river boils at 80°, that river cannot be composed of water.

The more deductions that we can test and confirm, the more probable it is that the hypothesis is true. In general, it is usually far easier to establish the falsity of a hypothesis than the high probability of its correctness. In the actual practice of science, repeated testing of the hypothesis

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Scientific Revolutions Revisited

generally results in its modification. Eventually we will have refined the hypothesis to the point where it can be accepted as the most accurate statement about the initial phenomenon that can be made, given the data and intellectual resources of the moment. Alternatively, we may find the hypothesis false or conclude that we have neither the intelligence nor the methodology to deal with the problem at this time. For example, today we are not successful in answering questions on the human mind.

Thus, the procedures of science involve a constant interplay of wondering, learning what others have done, asking specific questions, devising provisional answers, making deductions, testing the deductions in field and laboratory, and hopefully ending with a greater understanding of some interesting phenomenon than was previously available.

This all sounds like a game that, if played by the rules, will allow us to win the prize—knowledge. Yet this scheme is, most assuredly, too simple. I never heard of a scientist who follows those steps in rigid order. Nevertheless, much science is conducted using various elements of “the scientific method” almost simultaneously in a jumbled fashion.

Such a prescription fails to mention two of the most important features of first rate science. In Bertrand Russell’s (1945) opinion, the founders of “modern science had two merits . . . immense patience in observation and great boldness in framing hypotheses.” Thus, a great recipe for doing science may not produce a great dish. Indeed, the gap between asking a question and formulating a truly novel and testable hypothesis is the most difficult procedure in science. It is at this step that one uses words such as “genius,” “luck,” “serendipity,” “elegant,” and so on. And one should note that, at this level of the analysis, a grant is not required.

Deep discussions about “the scientific method” are almost always associated with philosophers and not working scientists. In fact, some outstanding scientists have maintained that there is nothing unique about the methods of science. Thomas Henry Huxley went so far as to say, “science is nothing but trained and organized common sense.”

Well, maybe. This egalitarian view of human knowledge sounds reasonable but one must go on to ask two questions:

1. Why did this sort of trained and organized common sense appear so late in the human scene? And,
2. Why did it originate only in Western Civilization and

flourish only there or in areas directly influenced by Western Civilization?

I believe that science requires more than trained and organized common sense. Possibly “uncommon sense” would be more accurate. Needed also is a nutritious economic, social, political, and philosophical milieu. Trained and organized common sense can be effective only in a sympathetic environment. I wonder how a Huxley would have fared in the Augustinian world where the search for understanding of nature was pointless and impious?

Somehow these various discussions of the scientific method never seem to describe adequately what science is and what scientists do. The Neo-Baconian scheme can be memorized by our students, yet its educational value remains questionable; and, surely, from this perspective, science emerges as one of the dullest of all human intellectual experiences. No student could hope to understand why some science is great and some pedestrian. I cannot see how the conventional description of the procedures of science could convey the excitement of science and make it a viable option for one’s career. Something more is needed.

Kuhn’s Revolution

A decade ago, the second edition of a remarkable book, *The Structure of Scientific Revolutions*, was published. The author, Thomas S. Kuhn, although a philosopher of science with the soundest credentials, was first trained as a theoretical physicist. Thus, we have the unusual combination of a scientist writing about the philosophy of science.

The centerpiece of Kuhn’s analysis is the notion of a paradigm. This he defines as a set of “universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners.” Or, expressed another way, a paradigm is the generally accepted way of explaining a scientific phenomenon of first magnitude. As an example, the paradigm of Darwinian evolution is used to explain the diversity of living creatures.

Kuhn starts by saying that the conventional image of science, presented primarily in the textbooks, has as useful a relation to science as a tourist brochure has to a nation’s culture. The image of science as a steady accumulation of knowledge, where A leads to B and B to

C, is both inaccurate and misleading. Much of previous science is wrong, not simply incomplete. In fact, much of what was science is now regarded as myth. Yet the myths of a few generations ago were formulated by the first-rate scientists of the period who used the same scientific methods that we employ today. A sobering thought.

Kuhn proposes that the image of science can best emerge from a study of what scientists actually do, not from what philosophers think they should do.

So, what does happen? In the beginning of a study of any large scientific question, e.g., "Why is there so much organic diversity?", there are many competing explanatory hypotheses—Lamarckian evolution, Chain of Being, Darwinian evolution. Some are mutually exclusive. All of these hypotheses would have been reached by following proper scientific methods. Gradually the one that appears to explain the data most adequately and that is more compatible with antecedent scientific beliefs, wins. The winner becomes the paradigm of the moment, but, with continued observation and experiment, data are obtained that cannot be accounted for by the paradigm. There may be a tendency to suppress these in order to maintain the generally accepted paradigm. This need not be an example of scientific wickedness. Not infrequently, data that seem to be at variance with an established paradigm prove to be false (supposed remains of human beings in very ancient rocks, etc.)

Gradually it becomes clear that some facts cannot be accounted for, and these exceptions become so annoying that "then begin the extraordinary investigations that lead the profession at last to a new" paradigm. Examples from biology are Darwin's *On the Origin of Species* (1859), Mendel's *Experiments on Plant Hybrids* (1866), Sutton's *The Chromosomes in Heredity* (1903), and Watson and Crick's *Molecular Structure of Nucleic Acid* (1953). This shift from one paradigm to another is Kuhn's "Scientific Revolution"—the "tradition-shattering complements to the tradition-bound activity of normal science."

Thus a Scientific Revolution replaced the Ptolemaic paradigm with the Copernican paradigm for explaining the relative movements of the earth and the heavenly bodies. With Copernicus came not only a new way of looking at the world, but also new problems and new standards of what statements are admissible. The Copernican paradigm was much more than the slow accumulation of facts to be added to the Ptolemaic paradigm. There was a complete reconstruction of how one viewed the universe—and this took time and the effort of many, not merely Copernicus. In fact, victory came long after Copernicus died. The way in which the old paradigm was replaced by the new bears little relation to what is generally regarded as the conventional methods and results of science. The scientific community had accepted and worked within the Ptolemaic paradigm. It provided intellectual satisfaction, set the limits to the type of questions that would be asked, and, to some degree, limited

the answers themselves. But it became increasingly more difficult to adjust the paradigm to new facts. Copernicus presented a new point of view that had virtue in its novelty but lacked the data to make it compelling. Then ensued the squabbles of a generation until it became obvious that the old paradigm could not be modified to form the new—it was replaced by the Copernican paradigm. One should add that opposition by the church was a powerful force in maintaining the Ptolemaic paradigm and resisting the deflating view of humans and their world suggested by Copernicus.

New paradigms have two important features:

1. They are so novel and so useful that they attract a critical mass of scientists from the old paradigm. This occurs at the time of scientific revolution.

2. They are open-ended. The new paradigm is a fresh way of thinking and suggests new experiments and observations to be made. Thus it gives the adherents something to do. The old data and the new data are made to conform. Most scientific activity, which Kuhn calls "normal science," consists of using the paradigm to explain the micro-problems of the day.

Revolutions are rare; the intervals between them long. Formulators of the revolutions are also rare and most scientists do their "normal science" in the inter-revolutionary periods.

Thus, Kuhn adds a human dimension to science. What is done reflects the abilities, intellectual framework, jealousies, conservatism, community spirit, commitment, and effectiveness of human beings who do science. The shared paradigm is the fundamental unit for the student of the sociology of science.

The existence of a paradigm implies maturity in a science—there is a generally accepted way of looking at the field. There was no paradigm to explain organic diversity before *On the Origin of Species*. Many competing views existed, some close to what was to become Darwinian evolution, but none that satisfied a majority of scientists. Similarly, no paradigm existed for inheritance before Mendelism went public in 1900. None of the competing notions about gemmules, idioplasm, pangenes, blood, and the like, found general acceptance among biologists.

In Kuhn's view, astronomy, physics, chemistry, and most parts of biology have paradigms but "it remains an open question what parts of social science have yet acquired such paradigms at all." In an immature field, one without a paradigm, the making of observations, the collecting of data, and the development of hypotheses are well nigh random events.

In the passage from the pre-paradigm to the paradigm stage of development, or from one paradigm to another, the most extraordinary insight is required. The Scientific Revolutions that mark these passages are the key events in the development of science but the events that occur in the human mind that make them possible remain to be discovered. No prescription among the scientific methods

suggests how these revolutions are to be fomented. Creativity cannot be planned even though there are devices that increase the possibility of its occurring (stimulating colleagues in person and in print, uninterrupted time for thinking, absence of competing distractions, perseverance, and motivation.) It would appear that what we teach as the scientific methods have much to do with the elaboration of detail, once the scientific revolution has occurred, and have nothing to do with the revolution itself.

No new paradigm explains everything. Indeed, in the early stages of development, the new paradigm may be only marginally superior to the older one. However, it suggests new routes for experiment and discovery better than its competitors. The paradigm is finally established by the "mopping-up operations that engage most scientists throughout their careers." This mopping-up stage in Kuhn's terminology is "normal science."

The Life Cycle of a Paradigm

In the Kuhnian world, then, the sequence in science would be:

1. A *pre-paradigm period* characterized by random activities among the independent scientists who make their observations and conduct their experiments. No "main problems" and no dominant "schools of thought" are identifiable. The facts are derived by scientific methods but they are, to a large degree, unrelated to one another.

2. *The birth of a paradigm.* Some isolated observation, some chance remark, some new experiment, or some flash of genius suggests a new way of looking at a grand phenomenon of nature, some way of bringing the existing mass of unrelated facts into a coherent system. The field now has its first paradigm and its first hero.

3. *Expanding the paradigm.* The new paradigm appears so promising, so intriguing, and so useful that scientists employ it in planning their experiments and guiding their observations. It will even color their conclusions. There is now a "school" of adherence to the new way of thinking. Masses of data are accumulated, careers run their courses, and the paradigm is expanded, refined, and taught. Non-scientists accept it as "truth." (So do many scientists but they should know better.) The aim is to make nature conform to the dictates of the paradigm with the consequence that radically new ideas at variance with the paradigm may be resented or rejected. Adherence by a community of scientists to a paradigm means that their work is interrelated and they receive mutual stimulation. The result is that this branch of science progresses quite rapidly. The bulk of scientific work—the mopping-up of the consequences of the new paradigm—may seem uninteresting but, clearly, there are first-rate minds that engage in this activity. What challenges most scientists is the conviction that if they are skillful

enough, they will succeed in solving a puzzle that no one else has solved or solved so well. As Kuhn observes:

Many of the greatest scientific minds have devoted all of their professional attention to demanding puzzles of this sort. On most occasions any particular field of specialization offers nothing else to do, a fact that makes it no less fascinating to the proper sort of addict.

Adherence to a paradigm by a group of scientists emphasizes the social nature of science. Few scientists conform to the pattern of the lonely investigator working on some unique and isolated phenomenon of nature. Of course, a selection process eliminates such individuals; the paradigm-prone editors would not accept their papers.

4. *Seeds of destruction.* The paradigm is a powerful force for its own demise. Because it tells the scientist what to expect, deviations from expectation are identifiable. Accommodating the deviation with a slight modification of the paradigm might be possible, but numerous and severe deviations signal to everyone that the days of the paradigm are over and a new one is needed in its stead. So, the cycle returns to step 2.

Will these cycles of hope and disappointment be the hallmark of science for all time? If past portends the future, the answer is yes. But recently it has been suggested that this will-o'-the-wisp scenario need not be forever. Each successive paradigm in a field explains more data, predicts with ever greater certainty, and lasts for longer periods. It is not unreasonable to suggest that a paradigm might reach such a state of reliability and universality that it can be accepted as an elegant and emotionally satisfying way of looking at a phenomenon. For all intents and purposes, except the philosophical, it can be accepted as "truth." Is there anything else, for example, that we wish the chromosome theory of heredity to do? As a general statement, isn't it sufficient? Of course, a huge amount of work still can be done—the normal science aspect of inheritance. Unless some grand cosmic joke has been played upon us, there is no conceivable way that the chromosome theory of inheritance could be voided. It has reached such a mature state that other forms of inheritance, such as via plastids, can be understood and accepted as minor alternate pathways.

In a similar manner, the paradigm of organic evolution is an acceptable way of looking at the diversity of life we see today. The mopping-up has not gone as far as it has for inheritance but, again, it is inconceivable that the paradigm of organic evolution can ever become totally unsatisfactory. We may doubt the mechanism of its occurrence, and the details of phylogeny, but the fact that it has and is occurring is beyond all reasonable doubt.

Gunther Stent (1969) maintains that science, far from being open-ended, may be coming to a close. In fact, there must be limits to what can be known and, so far as the grand generalizations are concerned, these are known for many fields. He gives as an analogy the field of geo-

graphy. The earth is not infinite and some of the geographical questions have been answered. For example, not many new continents have been discovered lately; and, in fact, not many areas within the continents remain unknown to geographers. Thus, in the broad sense, discovery is over—only microgeography remains.

So far as biology is concerned Stent writes,

... there now seem to remain only three deep problems yet to be solved: the origin of life, the mechanism of cellular differentiation, and the functional basis of the higher nervous system.

And to quote further,

Thus the domain of investigation of a bounded scientific discipline may well present a vast and practically inexhaustible number of events for study. But the discipline is bounded all the same because its goal is in view. The awareness of this intellectual horizon embodies in it a yardstick for value, since the greatness of a scientific insight can be measured in terms of the magnitude of the forward leap toward the attainment of that goal that it represents. Hence there is immanent in the evolution of a bounded scientific discipline a point of diminishing returns; after the great insights have been made and brought the discipline close to its goal, further efforts are necessarily of ever-decreasing significance.

So perhaps Kuhn's notion of ever-recurring scientific revolutions that usher in new paradigms may describe better the past than the future.

Kuhn's thesis, itself, was a new paradigm in the history and philosophy of science. It suffered the usual fate of a new paradigm. Some welcomed it as a much improved way of looking at the field; it more effectively accounted for what seems to happen to science. Others found varying deficiencies, some of sufficient weight to warrant its rejection (see, for example, Lakatos and Musgrave, 1970). One argument, especially difficult to accept, was Kuhn's suggestion that the history of science is not characterized by a steady increase in knowledge. Intuitively we do seem to be making progress even though philosophical problems about the nature and existence of "progress" remain.

The Paradigms of Inheritance

But let us not be concerned with whether Kuhn's thesis is philosophically correct or not but whether it is a useful way of telling students something about the nature of science. Let us consider the field of inheritance, which is of extraordinary importance in biology because it deals with the central feature of life: its ability to reproduce itself. Subsidiary reasons for selecting this topic are that (1) it has become the most rigorous of all the subfields of biology; (2) it has had the greatest success in solving its problems; and (3) it remains the most notable example of steady biological progress in our century.

Prior to 1900 the field was in its pre-paradigm stage. That is, there was no generally accepted explanation for inheritance. In 1865, Charles Darwin had proposed a comprehensive Theory of Pangenesis based on the pro-

duction of specific gemmules by each cell and cell part. Gemmules flowed through the blood stream and entered ova and sperm—and so carried hereditary information to the next generation. In 1866, Ernst Haeckel suggested that the nucleus was responsible for the transmission of genetic information. In 1884, Carl Wilhelm von Nägeli imagined that the idioplasm was responsible. He envisioned this as a network extending throughout the body and penetrating cells and the spaces between them. There were other notions.

Later developments showed that Haeckel had been closest to the truth, but he had made a bold guess that was essentially without observational or experimental foundation (we would have said "blind guess" if it had turned out badly). Ideas in science are of little importance unless they can be subjected to critical test. No one knew how to test Haeckel's hypothesis in the 1860s.

Confusion characterized this pre-paradigm stage in which there were innumerable competing hypotheses, and no shared notion of what the specific questions were, or how they best might be investigated. One school sought the secrets of inheritance in a microscopic study of the events that occur in cell division and in the formation of gametes. These investigations laid the foundation for our understanding of what happens to chromosomes, but it is difficult to see how continued work of this sort alone would have led to a notably better understanding of inheritance. Another school, the plant hybridizers and their zoological counterparts, crossed different individuals of the same species or of different species and noted the results. The results were clear—the characteristics of the offspring were usually intermediate between those of their parents. Therefore, what?

It is not unreasonable to suggest that these two approaches could have continued forever and our understanding of inheritance advanced not at all.

The Mendelian paradigm broke upon the scene in 1900. It required no sophisticated instrumentation, no huge budget and, in fact, no new scientific procedure. Mendel made his breakthrough by using one of the oldest, simplest, and most powerful tools in science. He counted—counted peas. The apparent triviality of what he did is not unique. Surely it was as complex as deciding to see if we could explain things better by considering the sun rather than the earth as the center of our orbiting worlds.

Intense research by many biologists quickly discovered that the Mendelian paradigm would account for most of the data of inheritance but not for all. It could, for example, explain only a few of the phenomena that Darwin had accounted for in his Theory of Pangenesis. Nevertheless, the things that the Mendelian paradigm did explain made more sense than Darwin had.

It took only a few years' work before the Mendelian paradigm began to encounter serious difficulties. Mendel's First Law—Segregation—held almost all of the time. His Second Law—Independent Assortment—was found

to be true only in the early stages of investigations when only a few genes were known. As more was learned about the genetics of a species, instances began to turn up where, in a dihybrid cross, the two different characters tended to stay together instead of assorting independently.

This damaging exception to the Mendelian paradigm set investigators to trying to solve the puzzle. The solution came, not with an extension of the Mendelian paradigm, but with Sutton's suggestion that the units of inheritance, the Mendelian factors, are carried on the chromosomes.

In the new Suttonian paradigm, the units of inheritance ceased to be the statistical dice of destiny and became integral parts of chromosomes. This presented an entirely new way of thinking about them and suggested different sorts of experimentation. Again, a simple notion, but so powerful that it was to dominate the field of inheritance for half a century. The Suttonian paradigm replaced the Mendelian paradigm as the dominant world view of geneticists. Nevertheless all that was Mendelian could be encompassed in Sutton's paradigm.

But it too failed to account for everything. Its triumph in explaining the exceptions to independent assortment, as due to the two genes being linked on the same chromosome, was soon seen as a failure because the genes on the same chromosome were not *always* held together. The solution of this puzzle came with the discovery of chromosomal crossing over. The Suttonian paradigm was not rejected, it was extended and improved. And so it went, puzzle and solution, puzzle and solution—with each solution sharpening the usefulness of the paradigm.

By 1950, it was clear that the Suttonian paradigm was effective in explaining essentially all of inheritance and it was self-confident enough not to be compromised by examples of non-chromosomal inheritance—such as plastid inheritance. One can describe the period of genetics between 1903 (Sutton) and 1950, in Kuhn's terms, as a period of normal science—the mopping-up of the details and the accumulation of a vast amount of information. In fact, understanding had so increased that, by mid-century, the field of classical genetics, or transmission genetics, was not very exciting. As Stent was to remark, no great discoveries were left to be made and mopping-up is not all that attractive to young scientists entering the field. To be sure, I did get a momentary thrill long ago in discovering the first Mendelian gene in frogs but that was hardly an accomplishment of cosmic importance. It would have been exciting if it had turned out that frogs did not do what was expected of them by Mendel and Sutton.

The seeds of the next revolution had already been planted. It began with Griffith's work on mice sick with pneumonia and continued with the work of Avery and others on transformation. DNA was implicated as having something to do with inheritance but one was working with bacteria and whether they had anything to con-

tribute to an understanding of inheritance was questionable.

But when we talk about DNA we have switched to a new field—biochemistry. One can argue whether biochemistry of 1950 had any paradigm of its own. It had spent many decades in applying the rules of chemistry to some very messy systems—living organisms. Much interesting information had been accumulated that helped one to understand some of the events that occur in living organisms and in industrial processes. There were even considerable data on DNA, enough to realize that it was probably a very simple substance—composed of only six sorts of molecules—so it could not have much to do with the very complex field of inheritance.

The task that Watson and Crick set out to do was to propose a model for the structure of DNA that would be congruent with what was known of its chemistry and what the genetic data demanded of any candidate for the heredity substance. Thus the bases, sugar, and phosphate had to be assembled in such a manner as to accomplish two major tasks:

1. There would have to be mechanisms that would result in exact copies being made.
2. The substance would have to be able to transmit the heredity information to the rest of the cell.

The solution of this problem is one of the best known stories in biology so it need not be repeated. A highly personal account is found in J. D. Watson's *The Double Helix* (1968). You should read this not for the titillating news that scientists can be human, and even naughty, but the vivid description of how frustrating and lonely it is to face the grand unknown.

The Watson-Crick breakthrough provided a paradigm for probing the structure and function of the hereditary material. It is hard not to conclude that the century old goal has been attained: we know in a satisfying way the molecular structure of the substance of inheritance. What other general sorts of information could we desire—as biologists?

The normal science, the mopping-up that has been done within the Watson-Crick paradigm has been the dominant activity in biology in the past quarter century. One branch of biology has achieved the rigor of the physical sciences and the fruits of this activity are enormously important for our intellectual satisfaction and material progress in the health sciences, agriculture, and industry.

I find Kuhn's way of looking at the world of science exciting and useful. The history of genetics can be made to fit his notion of a paradigm fairly well. The contributions of Mendel, Sutton, and Watson-Crick can be looked upon as scientific revolutions. After each such event, there has been a vigorous and fruitful period of discovery, guided by the dictates of the paradigm. Difficulties in each of the first two paradigms led to their replacement by a new way of thinking and working.

However, the history of genetics does seem to be at

variance with what Kuhn has suggested. By any reasonable definition, there has been progress. One can maintain that there has been a steady accumulation of data and understanding, with the antecedent paradigm having clear relevance for the succeeding paradigm. The Mendelian world view was not wrong, only incomplete. Its discoveries were not made irrelevant by the Suttonian paradigm, they were incorporated within it and came to be understood far better. In a similar way, the relation between the Suttonian and the Watson-Crick paradigm is close and dependent. The Watson-Crick paradigm, although radically different in theory and methodology, had to conform to what the Suttonian paradigm had demanded of any hereditary substance.

Kuhn's scientific revolutions—those enormously fruitful ways of looking at a problem—are the true intellectual triumphs of science. We should not neglect them.

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Visually Impaired

... from p. 294

The biology class for the visually impaired presented in the Fall of 1978 consisted of eight students. Half of the students were partially sighted; the others, totally blind. The problems we encountered in working with congenitally, totally blind students were mostly related to spatial orientation. Concepts of revolving, encircling, layering, and enclosing needed to be carefully demonstrated. The greater than normal range of abilities present was due to the lack of exposure rather than to intellectual capabilities of the students.

Students seemed to interact more easily with one another when they were seated at one large table rather than dispersed in groups of two at separate tables. The small size of the class and the intense involvement with the materials added to the close-knit feeling of the group.

The raised line-drawings illustrating structures discussed in the lectures were not as effective as they could have been primarily because of poor construction and improper presentation. In constructing these drawings, special care should be taken to eliminate all intersecting lines and word labels. Cavities, vacuoles, and other enclosed spaces should be carefully defined. The size of the drawing must not be so large that students lose themselves in the structure. A key at the bottom of the page might be a helpful addition. To be effective, line-drawings should not be duplicates of textbook drawings and diagrams. The way the drawings are presented is very important. Spatial interrelationships and structural content must be carefully delineated for maximum effectiveness.

A standard college course in biology offers much visual material. Making this material understandable and accessible for visually impaired students requires special effort. We have identified many of the problems in planning our course for the visually impaired and modifications based on our assessment of the effectiveness of the materials are in progress. Partially sighted and totally blind students must be given the opportunities and the tools necessary to pursue their interest in all academic areas including the natural sciences.

Symbiosis

... from p. 292

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