The Modes of Science

by George S. Hammond

Scientists face an unprecedented task—finding within a single generation a reorientation that has previously been spread over several generations.

For many of us at Caltech, science really is a way of life. We enjoy the privilege of defining problems in our own way and are stimulated by solving them according to rules that we have largely devised ourselves. It is really delightful, because the game is fun, and the results have often had value that can be shared with the society as a whole. Twenty-two years ago, I began my first faculty appointment at Iowa State College with a good deal of enthusiasm and some trepidation. The intervening years have certainly been the most productive period of scientific learning to have yet been recorded in the history of man, and I am truly grateful for my good fortune in having been a scientist during that time.

It is trite, but true, to say that we live in a troubled society. Although our troubles may not be the greatest faced by our nation, scientists do face problems that I, at least, did not anticipate two decades ago. Financial support of science and of science education built up rapidly, but it has dwindled at an alarming rate during the past three years; the public has turned from overadulation to suspicion of science; and we are particularly vulnerable to the wave of anti-intellectualism that has swept through the western world. I have always been fascinated by change. This probably is the reason that my interest in chemistry has been strongly focused on chemical reactions; but at this time I have an almost obsessive interest in the changes that are occurring in science.

In a sense I believe that we have learned too rapidly for our own comfort. During the 20-year period 1950-1970, we accumulated more scientific knowledge than all mankind acquired during the previous century from 1850 to 1950. I believe that we have passed through an era during my own working lifetime, and this is not an entirely comfortable feeling. There have been no changes in our concepts of what science is all about even remotely comparable to those that occurred between 1850 and 1950. When we ask why, we get a variety of answers—with none being very reassuring.

Some say that science has matured, that its form is fixed, and that we will see only progressive development within the form that is already established. If this is true, the prospect is sobering. We would conclude that scientific discovery will roll on over a relatively smooth path. If the machine has in fact been created in nearly final form, all we will need to do is continue to feed in fuel in the form of new scientists, and oil the works with a reasonable level of financial support. This picture would indicate that the needs of science in human resources are for competence much more than creative genius. During the past three decades we have made a very successful pitch to the young, intended to attract many of the most gifted to science. If the field is really mature, perhaps this approach should be changed. In fact, there is already considerable evidence that some of the most imaginative students are rejecting science because they believe its form is cast in concrete.

Personally, I disagree with this analysis and wish to suggest an alternate point of view. When I look at us and the universe around us, I see much more that I do not understand than I understand. Science is, according to my dictionary, systematic understanding of the physical world. If so, my own observation tells me that science must be far from finished. I further believe that we may have a problem in science at this time because too much of our attention is centered on what we know fairly well and too little on things about which we know very little. This would be a logical consequence of our incredible achievements during the era that has just passed.

My friend Burton Klein, a Caltech economist, maintains that we have a problem because we are still caught up in the scientific philosophy of the 19th century. I believe that our problem arises from the heritage of the first half of the 20th century. Most of the thinking about the structure and goals of science is too heavily dominated by people, such as myself, who were active and knew, or thought we knew, what science was all about in 1950. In a sense, we are in the same position that we would have experienced if Kekulé and Faraday, reigning scientific figures in 1850, had still occupied important scientific thrones in 1950.

If my analysis is even reasonably accurate, scientists of the world now face an entirely unprecedented task. We must find within a single generation a kind of self-renewal and reorientation that has previously been spread over several generations. The prospect is frightening, because we all must share some fear that detailed scrutiny of what is new and what is old might relegate our own finest works to historical museums. Furthermore, the creative young people who enter science must face the challenge of defining the new wave of science for themselves, since those of us who teach are so inextricably involved in what has now become history. Although I have really disqualified myself as a reliable prophet, I still cannot resist throwing my guesses about the future into the mill.

We can find clues of many kinds. If we look at the voluminous current scientific literature, we find depressingly repetitive patterns in the results reported. The work is new and both methods and answers are elegant, but the answers in many cases are not astonishingly different from those published five or ten years ago. The same criticism can be justifiably leveled at much current industrial research and development. For example, synthetic fibers are a tremendously important product of chemical technology. However, dozens of new fibers have been produced during the past decade. Very few have had a major commercial impact because they are not really much better than pre-1960 fibers.

Another symptom is found in the kinds of new challenges now being presented to scientists, and our reactions to them. The demand for new technology to preserve and improve the quality of our environment becomes increasingly loud. Many scientists are eager to respond, partly as a relief from nagging worry as to whether or not their traditional activities have adequate innovative character. I like this move and am pleased that Caltech will take a part in it by establishment of a laboratory for environmental studies. However, the studies we have made in the past year in preparation for setting up this laboratory have shown that we are not particularly well prepared to solve the problems of the environment, which turn out to be terribly complicated. We have to face the fact that technological solutions will be of variable utility depending on what occurs in the socioeconomic area. This somewhat humiliating discovery has real pedagogic value. The fact is that scientists, and even engineers, are ill prepared to deal with the complexity of real systems. I believe that our poor state of preparation for the complex is partly the result of a lopsided value system that has arisen in science during the past half century.

I think there are two complementary modes of science, the analytical and the synthetic. In analytical science, we divide things into smaller and smaller parts and study the small elements in great detail. In synthetic science, we try to construct useful models for thinking about complicated systems containing many elementary parts.

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An extreme example of analytical science is particle physics, and the biologists are surely doing science in the analytical mode when they narrow their focus to the subcellular level and discover molecular biology. On the other hand, astrophysicists seem inevitably constrained to work in the synthetic mode since there is no good way of tearing apart things in remote regions of the universe.

The most important scientific advances during the past 50 years have come from analytical science, and most scientists have worked in this mode or aspired to do so. Many of our most widely useful concepts—for example, quantum mechanics—could only have arisen as a consequence of the analytical approach to the study of matter. Unfortunately, the success of the analytical mode has led many scientists to the view that the reductionist approach *is* science and that no other mode exists. This has led in turn to unfortunate distortion of the scientific value system.

People have for years been raised in the scientific subculture to believe that systems of any significant complexity are dirty and unfit for proper scientific scrutiny. This even carries over to distortions of our language. Obviously a prerequisite for modeling any complicated system must be a description of the system; yet the term "descriptive" has come to be used in a pejorative way. In my own field, it has become a fashionable put-down to refer to a man's work as "descriptive." The term usually conveys subtle implications such as "lacking in true intellectual content" and "having no lasting value." While it is true that descriptive science can easily degenerate to encyclopedic accumulation of uncorrelated observations, I fail to see how we are going to make great progress in understanding the universe unless we take the time to describe it.

Another scientific bad habit is the tendency to apply entirely different criteria to mathematical descriptions and those given in any other language. Mathematics provides a vehicle for two rather different kinds of expression. First, some concepts having far-reaching value can be set down far more conveniently in mathematical form than in natural languages. Second, mathematics provides a precise way of expressing relationships between parts of a system. Each function is valuable in its own way, but we have come to regard almost any equation as automatically involving the best of both. Consequently, we frequently lose the most valuable components of observation by trying to force the description into mathematical form prematurely. This desire can even have a perversive effect on the way in which observations are made since an investigator may eschew any measurement that he cannot fit to someone's mathematical treatment. This acquired characteristic of modern scientists is partly responsible for our disinclination to undertake serious study of the complexities of the real universe.

An insidious mystique has evolved in science—the feeling that ultimately the analytical mode will tell us all about everything. The fanciful folklore about the relationships within science illustrates the point. We blithely chatter about chemistry finding a basis in particle physics and biology finding its roots in chemistry. There is no doubt whatsoever that the more complex sciences have derived invaluable inspiration from the reduced sciences. However, to parlay this into the conclusion that, if we wait long enough, all the elementary components will fall together like the pieces in a jigsaw puzzle is vastly deceptive.

I do not think that we will ever arrive at a total description of a living cell based upon integration of rate equations for the thousands of chemical processes going on within the cell. This conclusion is not based upon mystical notions concerning the physical process that we call life, but arises simply from consideration of the characteristics of complex systems. First, accurate identification and description of all the reactions in a living cell will take a long time and require an accounting system that may even strain the capacity of large computers. Even more important is the fact that in the living system the reactions do not operate independently but are coupled to each other. The rate at which one process occurs is strongly dependent on the rates of many others. In order to describe any such system, we will have to take account of an enormously complicated set of interactions. In the light of these considerations, I am convinced that theoretical models for living cells will always be just that—cell models. They will be incomplete as total descriptions of the chemical systems. However, good models for the cell will surely be strongly influenced by partial knowledge of the chemical activity within the cell.

There is really nothing new in this view. The interactions among the fields of science have always been a kind of bootstrap operation. If there is any legitimate ground for delineating the various fields of science and engineering, it is to be found in certain intellectual units useful in the various fields. In high energy physics the unit is a particle; in chemistry, the molecule; in biology, the cell; in psychology, the individual; and in sociology, the population. Disciplinary description in these terms is rather shallow, but may be helpful in understanding relationships and distinguishing between synthetic and analytical science. For example, the branch of theoretical chemistry devoted to molecular quantum mechanics is really an example of science operating in the synthetic mode. The best practitioners are developing valuable new models for molecules. They use many ideas and techniques, including concepts borrowed from particle physics. However, the notion that they are "analyzing" molecules in terms of elementary particles is quite deceptive. Yet many people in the field are so imbued with the value system of analytical science that they

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pretend they are doing analysis rather than synthesis. In short, they claim an objective that would be rather silly and fruitless, thereby hiding the real genius of their work.

The models for complex systems put together by synthetic methods will never be permanently fixed. To work effectively with models without jeopardizing our future, we must continuously work to distinguish between our conceptual models and reality. The models we can describe and examine in infinite detail whereas total physical reality will never be described by the mind of man. This seems to be one of the most solid theoretical conclusions that one can reach, simply because the number of elementary components in the brain is far less than the number of components in the universe. The necessary incompleteness and changeability of the models in synthetic science conflict with more than current values of science. They seem in conflict with that precept of our culture which drives us to seek definitive and final answers to everything. The notion that we can find *the* solution to any problem has probably been a powerful stimulus for development of analytical science but now stands in the way of full exploitation of our analytical success in building our synthetic capabilities.

People, including scientists, are funny. The challenge of really very complicated problems, such as preservation of the environment, has considerable appeal, and many scientists will surely be working in these areas in the future. I admire their enthusiasm and dedication and believe that they will make valuable contributions. However, it is always interesting to see people who are afraid to walk—but eager to run. Chemists who have been haughty in their attitude toward systems of moderate complexity in chemistry now rush to try their hands at the study of some of the most complicated systems available. Included are those who have long expressed utter contempt for the shallowness of social studies. I don't know how it will work out. Certainly some of us will learn appropriate humility, and I also expect that our real accomplishments in fields such as environmental studies will be significant.

Along with the big leap, we will probably undertake less glamorous but highly instructive forays into synthetic science. We should be able to learn a great deal about scientific systems analysis by moving out from areas where we have learned most from analytical study. A modest example from the work of my own research group is our attempt to use our knowledge of photochemistry as a tool in modeling the much more complicated chemical changes induced by high energy radiation such as gamma rays. As I indicated earlier, I believe that a tremendous opportunity exists to create useful models for living cells based upon the concept that a cell is a complex chemical machine. In recent years, there has been a good deal of interesting work in the field of properties of materials, their strength and hardness, how they fracture, and so on. Attempts have been made to relate these macroscopic properties to chemical structure. Although the field is in its infancy, I think it will develop rapidly in the near future. Surely, if the minds of men can construct imaginative and believable models for the history and current development of the universe, we can also formulate workable theories about the relationship between behavior of materials and the molecules in them.

If we are led to initiate a new era characterized by reemphasis of the synthetic mode of science, we have much to learn from a group of engineers who are trying to develop the field of systems analysis. For example, I anticipate that within a few years there will appear a group of people doing chemical science and calling themselves "systems chemists." Some of the classicists from the bygone era of 1950 to 1970 will undoubtedly attempt to denigrate the new activity by calling it "only engineering." Nor will even this kind of patrician conservatism be new; I can still recall a few people who bewailed the demise of real scholarship when the study of Greek was all but abandoned in the public schools.

It is no accident that my own examples are taken from the interfaces of chemical science with biology and engineering. When one reaches out, he reaches from wherever he happens to be, and I am in chemistry. I also want to say that Caltech is a remarkably good place for such speculative excursions. We are not immune from the kind of insularity that is characteristic of established disciplines, but we are small enough that a chemist can at least find the people working with complex systems if he hunts.

I have shared with you some of my own views as to the current problems within science. In some ways this seems risky because my doubts may be thrown back at me by those whose disenchantment with science takes a destructive turn; and there are many people who want to destroy science, or at least punish the scientists for their arrogance without concern for the consequences. I believe that science is still a baby, with great potential for further growth. I am disturbed to look at the baby and find it somewhat dirty. However, as the father of five, this is not an entirely new experience to me. Obviously, the baby needs washing. I fervently hope that we will not end up throwing the baby out with the bath water.