A Human Enterprise

Science as lived by its practitioners bears but little resemblance to science as described in print.

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In one of his essays, Herbert Dingle (*1*), historian and philosopher of science, makes the following remark: "When we contemplate the ideas of the essential nature of science which are most prevalent and operative today we find a situation fit to make the angels weep." Without doubt this is true. "Science" as it is thought of popularly is a stereotype that bears but little resemblance to science as it is known intimately by those who live it from day to day.

For instance, it is commonly believed that science is a sort of intellectual machine, which, when one turns a crank called "the scientific method," inevitably grinds out ultimate truth in a series of predictably sequential "steps," with complete accuracy and certainty. Its thinking is thought of as exclusively and inerrantly logical in the most formidable sense, and its language, as utterly precise and unambiguous. In view of such magic omnipotencies that presumably make all wrong answers impossible, science is regarded as a monolithic structure of unassailable truth and method on which all scientists must necessarily be agreed.

There are at least two reasons why such unfortunate utopian notions prevail. First, the findings of science are usually presented to students and the public as straightforward, logical developments, rather than in such a way as to reveal how they actually evolved—haltingly, circuitously, with many false starts, and often even illogically. While there are, of course, very good reasons for this, the fact is that, in the absence of further explanation, it leaves the uninitiated with a thoroughly misleading idea of the processes of science. Moreover, most conventional portrayals of science are, strictly speaking, not descriptions at all, but schematic interpretations. They are the product of a process of selective abstraction, by which the interpreter extracts from the complexities of actual science those elements that seem to him most typical and most capable of simple, systematic description or analysis. In this way he creates a simplified, idealized image or model, which the reader innocently accepts as an accurate portrait of science. For both these reasons, then, science as the scientist himself knows it remains essentially unknown.

The purpose of this article is to call attention to a few aspects of the real science, about which little seems to have been said in print.

Science, a Realm of Human Contrasts

A typical aspect of most conventional interpretations is their almost complete silence about the fact that science is a typically human enterprise with the limitations and potentialities, weaknesses and strengths these usually possess. One indication of this is that it is a realm of great contrasts and nonuniformities, a structure that is anything but monolithic.

Consider, for instance, the great difference between the science of the intellectual frontier and that of the interior. These are as different as the laws, politics, or social and economic structures on a national frontier are typically different from those behind the frontier. Frontier science is exploratory and adventurous. Here ideas are tentative and impermanent, coming and going rapidly. More often than not they are audacious guesses or vague hunches that rarely conform to established patterns of thought. Often they are thoroughly unorthodox and what many people would even regard as "unscientific." This is science in the raw—controversial, competitive, inefficient, governed to a considerable extent by the demands and urgencies of the moment, and employing predominantly *ad hoc* methods. It is the science of the restless explorer, always on the trek, never stopping anywhere very long—always looking for new horizons and taking the frontier with him.

The science of the interior, by contrast, is that of the intellectual colonizers who follow the pioneers, consolidate gains, and establish order and respectability. It is characterized by much less fluctuation and change, and therefore by relative permanence; by system, precedence, and closer adherence to established canons of methodology and thought; by logic more than by hunch. Here the crooked pathways are made straight. Here is where the straightforward "proofs" or "logical developments" put in their appearance, and where everything seems to be orderly and logically interdependent.

Now it is the teacher's and interpreter's preoccupation with this systematic, rational science of the hinterland that is largely responsible for the overabstracted, formalized stereotype to which I have been referring. Certainly, such an oversimplication as that of "the scientific method" could not possibly have arisen out of careful contemplation of frontier physics.

Another inhomogeneity of science appears in the tremendous contrast between the science of the great masters and that of the ordinary, common man of science. The conventional images of historical, as well as contemporary, science are the result of using glasses that bring into focus mostly the great towering figures of our science, leaving much of the picture unseen. To know America only in terms of its great heroes—George Washington and Abraham Lincoln, Ralph Waldo Emerson and William James—is to know it only partially. True understanding requires that it be known also in terms of the common man and his way of life. The same is true of science. To see it only as the creation of its towering geniuses—Galileo, Newton, Harvey, Pasteur, Einstein, Bohr, and Heisenberg—is to have a foreshortened view of it. There is also the science of the ordinary, garden variety of scientists. To see it in true perspective means to be aware of and understand science as the work and way of life of these, its lesser devotees—who are almost unknown except in their own localities or to fellow workers in their own particular narrow subfields

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of science, who are not at the very forefront of modern research, and who in a whole lifetime publish only a few papers of restricted significance, but who nevertheless are real scientists.

Now to depict the common-man aspect of science clearly, and to demonstrate what many of us feel—namely, that it is significantly different from the better-known science of the masters—would require much more factual knowledge than is now available. I am rather sure, however, that if this aspect of science were studied carefully, we would be forced to revise rather extensively many of the conventional descriptions and "models" of science—for there can be no doubt that most current conceptions of the operations and modes of thought of science and scientists have resulted from a disproportionate preoccupation with, and abstraction from, the science of the great masters. I suspect that such studies would make it abundantly clear how completely unsatisfactory any statement must be which began, say, with "the scientist does it this way, but not that way," or "the scientist believes this, not that." It would be evident that there is no such thing as "the scientist," that this term must necessarily stand for many kinds of scientists, whose ways of thinking and habitual modes of experimentation and research differ widely, and among whom there are many degrees of sophistication with regard to the purposes, goals, and methodology of science and many fundamental disagreements about both the content and meaning of its principles, concepts, and generalizations. And especially, I suspect, such study would reveal pronounced dissimilarity between the patterns of intellectual strategy and tactics prevailing in the common-man science and those of the great-man science.

Historical analysis would probably reveal that much of the growth of physics is the aggregate effect of the interests, attitudes, professional habits, and contributions of the lesser men of physics. In all probability the meandering onward flow of science is determined helpfully and positively—and to a large extent—by the rank and file, who, because of their persistent interests and preoccupations, carry exploration and exploitation in particular fields to their logical conclusions long after the geniuses have lost interest and turned to other more enticing problems. It may show also that progress is aided greatly by the damping and filtering effects of the intellectual inertia and skepticism of the ordinary man of physics upon many of the exuberant, freewheeling, and less useful ideas of the great or near great. Finally, the sum total of the relatively less important research endeavors of the very large number of individual mediocre scientists is tremendous and probably accounts for most of science as measured by both input of energy and output of results. It is amazing how little of this is realized by most people outside of the sciences.

Science as a Social Enterprise

Science is not only a human, but more particularly a social, enterprise—that is, one of sharing, cooperation, and interaction of people. Among my students the following question has always generated considerable interest and discussion: Is a one-man physics possible? Could a man completely isolated from other people, but possessing great intelligence and ingenuity and unlimited material resources and time, eventually develop a physics like the one we know, which has developed by social action? Almost invariably the student replies that it would certainly be possible. The lone scientist could, it is argued, make observations and generalize from these. Then he would discover the need for experiment and measurement, and for instruments. Learning that the use of these enabled him both to reduce personal errors and to increase his output, he would then use microphones as much as possible instead of his ears, photoelectric cells instead of eyes, analyzers and computers to augment his brain. Finally, he would build laboratories with complete automation and, in time, obtain all the data and curves required to establish all the functional relationships and generalizations of physics.

Now this kind of answer comes out of the prevalent notion that physics is a dehumanized science that owes its success almost exclusively to instrumentation and measurement. It is the kind of answer one gets from people who have had little direct contact with physics. Experienced physicists, however, give a different answer. They point out that unless our lone physicist were a very different kind of human being, he would be in need, whether he realized it or not, of checking and confirming his data by those of other physicists; otherwise he might go on blissfully unaware of serious shortcomings and systematic errors affecting his instrumental readings and settings and therefore his results—even if there were automic controls. In other words, physics as we know it requires checks and balances, and mutual validation and verification.

Consideration of such hypothetical, artificial, dehumanized situations brings into the open many facts about the real, existential science that rarely are noticed or mentioned. Thus there are, of course, no supermen who can go it all alone, and probably there are none who would want to go it alone if they could. Not only does the scientist always have his limitations, necessitating mutual aid, but he knows it. Not only does he need help in making experiments and interpreting their results, but he is conscious of that need. The recognition of it constitutes part of his professional equipment. Moreover, sooner or later he wants to exchange and share new ideas and findings with fellow scientists. He knows that if there is to be progress he must build on the results of others and must, in turn, make contributions upon which others can build. He realizes that he needs the criticism of his fellows, but also he craves their approval. Few scientists would do research, I believe, if they could not publish their results and get due credit for them, or could not see socially beneficial consequences flowing from their research, or were not motivated socially in other ways that certainly are operative in our present science enterprise. Science is undeniably social.

It is in this social mutuality and interdependence that the so-called "objectivity" of science has its roots. As is well known, the potency of the sciences in their search for knowledge and truth lies to a large extent in their insistence upon empirical evidence and confirmation. It is much less well known that in this connection the word *empirical* refers primarily to social rather than solitary individual experience, observation, and experimentation. A term that is highly suggestive of basic meanings here, and which may be regarded as a synonym of *objectivity,* is *intersubjective testability* (*2*). It calls attention to the fundamental role played by interpersonal exchanges and checks in the testing and confirmation processes of science.

Science Is Communal

Not only is science human and social, as I have just suggested, but it is also definitely communal. Without doubt the term *science community,* heard with increasing frequency, is extremely useful in describing science as it actually is. Certainly it does exist—and it is a community with the usual attributes of human communities. It has it own ideals and characteristic way of life; its own standards, mores, conventions, signs and symbols, language and jargon, professional ethics, sanctions and controls, authority, institutions and organizations, publications; its own creeds and beliefs, orthodoxies and heresies—and effective ways of dealing with the latter.

This community is affected, as are other communities, by the usual vagaries, adequacies, and shortcomings of human beings. It has its politics, its pulling and hauling, its pressure groups; its differing schools of thought, its divisions and schisms; its personal loyalties and animosities, jealousies, hatreds, and rallying cries; its fads and fashions.

The life and operation of this community require an amazing number of different kinds of people and talents. These include at least the following: the experimentalist and theorist, the lone researcher and team researcher, the critic and referee, the philosopher and historian of science, the teaching scientist, the research director, the research manager and business officer, the personnel officer, the report writer, the editor, the translator, the liaison officer, the instrument designer and maker, and—last, but not least—the forsaken "science widow" who keeps the soup warm when her husband is late, or who spends innumerable, endless nights alone while he is pursuing a hot lead in the laboratory. Similarly, the community is supported by an array of personal motivations as varied and extensive as the gamut of talents just referred to. Here, too, one encounters a thoroughly human situation. The underlying drives that lead men to become scientists, or that determine their decisions thereafter, are by no means confined to those commonly associated with ivory-tower conceptions of science or with pure unselfish love of the truth. It is doubtful if science could survive if this were not so.

As I have already said, this community has its own unique way of life and dominating interests that offer their own satisfactions. All this is very hard to describe, and nearly ineffable—though nonetheless real. There is something intimate about it, something shared and deeply felt, though unspoken. It can be understood truly only from within the community. An interesting question arises here: How do physicists, say, recognize each other as belonging to the physics community? I suggest that a physicist identifies a fellow physicist by rather subtle clues. Although he has certain definitions of physics, and therefore of the term *physicist,* none of these is really wholly satisfactory for making decisions in many actual situations. I would assert that basically, in the last analysis, he does not make the judgment that a man is or is not a fellow physicist by Aristotelian logic or careful definition and analysis, but rather on the basis of feelings, and the vague, unspoken, intimate, indescribable, but deeply felt intangibles of unformulated common interest, purpose, and attitude that become compellingly real to those who participate in the shared experience and life of the physics community.

This is no mere empty, useless sentimentality. It is utterly realistic and intensely practical. How, by way of illustration, does an editorial board of a physics journal proceed when a paper of high quality is submitted for publication, which they think deals with chemistry rather than physics and should appear in a chemistry journal? Does their judgment result from the conscious, logical application of formal definitions or criteria? By no means. While, to be sure, they may give some thought to more formal considerations, I believe that their decision rests basically on translogical and undefined feelings, on insights, intuitions, and a sense of values they have developed for the most part unconsciously as they have lived and gone about the business of physics as members of the physics community.

It is to this kind of intuitive thinking that many physicists finally resort, after all other reasoning fails, when they are called upon to say what is physics, or what is the difference between physics and chemistry. Many a long, animated, informal discussion—even formal committee deliberation—has yielded the profound conclusion that physics is what physicists do professionally, and that physics is what goes on in the physics building and chemistry, what goes on in the chemistry building of a university. And by means of this truly profound conclusion, many other practical professional questions are answered.

The science community is deeply embedded in the world of affairs and has always been influenced profoundly by other components of society and by human needs and demands (*3*). Contrary to prevalent opinion, science has not risen above its environment by becoming independent of or immune to external influences or pressures. It has interacted with its environment with profound effects, and often its own reactions have been conscious and deliberate.

One of the most profound effects of cultural influences upon science has been, surprisingly, in the area of decision making—with regard to strictly scientific questions. Much historical research has shown that choices within science itself have often been based on considerations that, from present-day points of view, seem much more appropriate to other disciplines and areas of life (*4*). There can be no doubt that the reasons for accepting hypotheses, theories, concepts, or modes of thought within science have often been political, social, economic, philosophical, or theological in nature and origin.

Science is increasingly being regarded as a quest for the rationalization or understanding of certain aspects of human experience—rather than only as an exploration of an external world. From this point of view science is human, not only in its characteristics but also in its most basic concern—namely, the object of its inquiry. We have come to realize, as perhaps no scientists before us ever have, that the human observer or explorer and his experience are integral and determinative parts of whatever world he is studying.

Need for More Adequate "Models"

In conclusion, I should recognize the point of view of many persons who would say that when scientists follow their feelings and hunches rather than formal logic, or accept hypotheses for political rather than strictly "scientific" reasons, they are merely mistakenly letting their humanity intrude into their science, and that "science itself" is something apart from any such intrusions. While this may be regarded as only a question of definition, I submit that it is much more than that—namely, a matter of understanding science adequately in its actualities. A "science-as-such" is, after all, only a mental construct. As ordinarily conceived, it does not correspond sufficiently to the realities of science in the concrete to be adequately descriptive. I would plead that it is much more useful, fruitful, and enlightening from many points of view to conceive of science as I—(and many others) have—namely, as being broadly pervasive and widely inclusive, comprehending within its purview much of life that by other definitions would be regarded as unscientific. I would plead that philosophers, teachers, and other interpreters who construct and employ models of the scientific enterprise so construct them as to represent more adequately and explicitly the great diversities and nonuniformities of science, and many more of its actualities, than most of the conventional current ones do. If the public is to understand and appreciate—as well as intelligently support—science, it must have a more inclusively truthful picture of it than it now possesses. If, in planning for the future, we are to project for science a truly significant function in public affairs, we must base our thinking about how it should operate in the future upon a model that depicts as accurately and inclusively as possible how it does in fact operate now. So far as I am aware, such a model, or image, does not now exist. Our thinking has been dominated altogether too much by a stereotype that is thoroughly inadequate and misleading.

References and Notes

1. H. Dingle, *The Scientific Adventure* (Pitman, London, 1952), p. 4.
2. It is my impression that this term was first proposed by Herbert Feigl. At any rate it appears in his essay "The scientific outlook: naturalism and humanism," *Am. Quart.* 1 (1949). See also Feigl and Brodbeck, *Readings in the Philosophy of Science* (Appleton-Century-Crofts, New York, 1953), p. 11.
3. B. Barber, *Science and the Social Order* (Free Press, Glencoe, Ill., 1952); H. Butterfield, *The Origins of Modern Science* (Anderson, London, 1951).
4. See the following papers and their references: P. G. Frank, *Sci. Monthly* 79, 139 (195[illegible] B. Moore, Jr., *ibid.* 79. 146 (1954); A. A. Koyré, *ibid.* 80, 107 (19[illegible] R. S. Cohen, *ibid.* 80, 111 (1955).

Tatuo Aida, Geneticist

Tatuo Aida, Japanese geneticist well known for his studies on the fresh-water fish *Oryzias* (*Aplocheilus*) *latipes,* died on 16 December 1957 at the age of 86. He was born in Kyoto on 21 November 1871, the only son of Masatoyo and Moto Aida, and was educated in the Third State Junior College in Kyoto and later in the Tokyo Imperial University, where he majored in zoology and graduated in 1896. His main interest at that time was in the pelagic invertebrates of the groups Chaetognatha and Appendicularidae. His Japanese and English papers on the former group, published in 1897, dealt with 12 species, of which four were new, and his English paper on the latter group, published in 1907, included 12 species of which four were reported as new. These papers were the first reports of these groups from the Pacific waters.

After his two postgraduate years in the university, he was appointed professor of biology in the Fifth State Junior College in Kumamoto. In 1904 he was called back to Kyoto by the death of his father and remained there until the end of his life. He taught biology in the Kyoto Higher Technical School as well as in a Buddhist school in the same city.

About 1913 he became interested in the genetic studies of *Oryzias,* varieties of which are commonly kept in Japanese homes, and he kept on breeding this fish experimentally in his home in the city of Kyoto. His garden was traversed by small meandering canals which provided clean water for his nursery. He used concrete tanks and earthenware basins for the pedigree cultures. His time, during the breeding season of the fish, was devoted almost entirely to the experiments.

The results of these seven years of painstaking work were embodied in his first paper, published in 1921 in *Genetics.* The most important finding described in that paper was the presence of a gene for red color, carried in the Y-chromosome, and its occasional transfer into the X-chromosome by crossing-over. This discovery was antagonistic to the then-accepted knowledge of the structure of the Y-chromosome, especially with respect to *Drosophila,* and Aida hesitated considerably to publish it. The discovery was sustained by the result of Schmidt's work on another variable freshwater fish, *Lebistes,* conducted in Denmark and published almost coincidentally with Aida's paper. Aida's finding, as was rightly pointed out by the editor of *Genetics,* E. G. Conklin, went beyond Schmidt's in having demonstrated crossing-over between the Y- and the X-chromosomes.

Aida kept on with experimental breeding of the same fish after the appearance of this classic paper and published two more papers in the same field. The second paper, in 1930, dealt with the findings on the frequency of crossing-over between X and Y and the apparent nondisjunction of the X-chromosome. In 1932 he was awarded the Japan Academy Prize for the excellence of his genetic studies on this fish.

The third paper, published in 1936, was on sex-reversal, which is relatively common in this fish. These two papers, as well as the first one, were the outcome of his laborious, long-continued experiments. His interest in the experiments never waned, even on his deathbed, and whenever he felt better, he got up to perform some experiments. Thus, he left rather extensive breeding results unpublished, and we are hoping that someone will examine his notebooks and publish his further discoveries in an appropriate form.

Aida had a robust physique and enjoyed good health until he contracted, in his 80th year, a fatal asthma. He had the well-controlled temperament of a samurai, and, in spite of his apparent shyness, he was a man of great versatility. For many years, as a consultant to the Shimazu Factory in Kyoto, he practically directed extensive business works in its department of natural history, manufacturing and selling specimens, models, and instruments to schools all over Japan, as well as in China, Korea, and elsewhere. He was interested, as much as in the breeding experiments with fish, in old Japanese swords; he had a great deal of experience in judging the quality, and determining the maker, of such swords and became an authority in this line. He was also a good archer and was ranked among the few champions who were able to shoot a target through the Thirty-three-ken (Sixty-yard) Corridor, in the traditional tournament among the best archers in Japan.

Tatuo Aida disliked publicity, so much so that he never took any doctoral degree, and his death, announced to his friends only some days after the private funeral, was not reported even in the local papers. We have lost in him a geneticist of outstanding ability and originality.

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