mass, which lies within eighteen miles of the planet.

Eighteen miles is not an impressive distance on land or water. But eighteen miles of atmosphere is a different matter entirely. This misty turbulence is alternately stretched and squashed by the gravitational pulls of the sun and the moon. It is inflated by the sunlight of day in one hemisphere while being shrunken by nighttime cool in the opposite hemisphere. As distance from the earth's surface grows, molecules of the air's constituent gases dissociate into atoms under the blast of solar radiation. Still farther out, the atoms disintegrate as electrons are stripped from their nuclei. The result is a layering in which zones where temperatures fall with altitude are succeeded by zones where temperatures rise with altitude. Thermal, chemical and nuclear processes are at last jumbled in ways that can be only remotely guessed.

Sounding balloons—some carrying instruments that report by radio the conditions they encounter as they rise, and some silent ones tracked optically or by radar from the ground—are now the only systematic contributors (aside from occasional sounding rockets and the relatively near-sighted eyes of severe storm surveillance radars) to knowledge of the third dimension of the atmosphere. The balloon data extend to the eighteen-mile limit of immediate practicality and beyond; but the observation points are restricted in number and consequently widely scattered. Even with extraordinary luck in coordinating the timing of many interdependent components, it will take something more than five years time to tie the present-day balloons and radars into a global net with air- and water-borne buoys, airplanes, rockets, and submarine observation devices reporting through earth satellites.

The master question of the moment therefore is: Where should the change-over begin? While there aren't enough observation posts in operation now to make a workable global system, the computer facilities now available couldn't handle the flood of data that would be forthcoming if a global observation network were ready. This is where the concept of mathematical models of the atmosphere enters. Figurative storms can be driven through computers on clouds of equations in repeated experiments, and the models can be modified gradually until the numerical weather corresponds to actual disturbances in the atmosphere.

Since weather affects the lives of people everywhere, regardless of nationality, color, religious disposition or economic status, it is singularly appropriate to find a concerted effort among science administrators in Washington today toward making the emerging global weather forecast network a genuine reflection of a prudently generous American nation. To put it briefly: The Weather Bureau is being given actual control of weather research for the first time in many years, under conditions conducive of the best scientific values at the least expense to the taxpayer.

The reform began with a White House Budget Bureau *Circular*, issued not long before the assassination of President Kennedy. It amounted to a Presidential order.

Within the office of the Assistant Secretary of Commerce for Science and Technology, Dr. J. Herbert Hollomon, the *Circular* established a Federal Coordinator for Meteorology. To the Coordinator was given responsibility for drawing up and executing a plan covering weather observation and supporting research activities of fifteen different federal agencies. Then Dr. Hollomon named Dr. White as the Coordinator.

Although Dr. White's authority does not extend to basic research in meteorology, he sits on the Interdepartmental Committee for Atmospheric Sciences (ICAS) of the Federal Council for Science and Technology. The Council functions in conjunction with the White House Office of Science and Technology, where scientific policies of President Lyndon B. Johnson are shaped. Since ICAS is responsible for all federally subsidized basic research on weather, including that of the National Science Foundation (banker for the National Center for Atmospheric Research in Colorado), the Atomic Energy Commission, and the National Aeronautics and Space Administration, Dr. White knows everything that goes on in weather research at the national level. On the international plane, he is the United States delegate to the World Meteorological Organization of the United Nations.

Dr. White's first weather plan is due this fall. It could become a model for the great unfilled need of government in science: a reasonable system of research priorities.

—JOHN LEAR,
Science Editor.

SCIENCE IN JOURNALS

SCIENCE IN THE PUBLIC INTEREST

A New Career Pattern

WITHIN one generation, modern science and the complex, sophisticated technology which both springs from it and supports it have suddenly become the primary basis of national wealth and military power and also a primary tool of social and economic revolution. The need to wisely develop and control this tool has produced a crisis in its management—particularly with respect to the public interest.

There is no alternative if we wish to keep the practical fruits and the intellectual adventure of the scientific revolution: We simply must solve the problem of wise administration.

Now, except for a few penetrating observers, this new demand has sort of sneaked up on everybody. The businessmen (and the business schools) are not ready for it. The generals are not ready for it. The public is not ready for it. The Congressmen are not ready for it. But, most important of all, the scientists and engineers are not ready for it, either, and that is the main point of my remarks. We, as a professional group, have been suddenly exposed to a role for which we are not prepared.

We are prepared to practice science and engineering as the private attorney practices law, seeking only a successful end to a given technical task. What we *are not* prepared to do is to devote our specialized technical knowledge in a professional way to the solutions of the many complex problems of public concern where scientific knowledge and experience are critical.

We do not, for example, as yet have either a tradition or an established career pattern corresponding to that of the judges in our law courts, who start their careers with specialized training, then obtain extensive experience, and later turn their energies to representing the public interest. Indeed, lawyers have three career patterns, all honorable: attorney, judge, and lawmaker. This is a natural consequence of a highly technical discipline whose wise public administration has been essential to civilized society for many centuries. I perhaps betray my parochialism when I suggest that in this regard science and technology as applied to many government problems are becoming as important and as intimately concerned with substantive technical content as is law.

WHAT is needed for this new career pattern to develop?

First, the scientific profession (by which I mean both scientists and engineers) must show its willingness, maturity, and ability to pick up its public responsibility wherever this is appropriate. It must get over its bad case of "advisoritis," whereby many members feel that their duty ends when they have given some advice to a Congressman or to a "general-purpose" government manager.

Incidentally, I define a "general-purpose" manager as one whose motto is: "You name it, I'll manage it." This type of person always has been and always will be essential to many parts of business and government. He is not, however, a suitable person either to operate the law courts or to direct modern technological enterprises (although he did quite well at managing the relatively simple technical operations of the industrial revolution, particularly operations based on mechanical devices). A lawyer or a businessman can master in a few weeks the essential technical aspects of the operation of a railroad or a bus line. But just let him try to rationally guide the development of an inertial guidance system, a laser communications system, or a radical new computer!

The kind of technical knowledge needed in these matters, along with the subtle skills of management which bring out the creative efforts of scientists and engineers, simply cannot be picked up after hours or as part of a job. It can only be obtained one way: by a systematic, thorough technical education plus extensive professional experience in research and engineering. Unfortunately, the lingo of science is easy to pick up. But, as any experienced technical person well knows, there is nothing so depressing as to listen to a "general-purpose" manager using all the right words without real comprehension.

Returning to comparison with the legal profession, we note that judges are made neither from amateurs at law nor even from those merely educated in law. As a group, they are drawn from successful *practitioners* of law.

Thus, the second requirement for the new career pattern is that the scientific community must deliberately set about to build respected career patterns in public service. It must invent a system of nonmaterial rewards corresponding to those which make a judicial career desirable. Objectivity, impartiality, and broad technical competence must be recognized as desirable, in addition to the traditional factors of individual creativity in the practice of science, pure or applied.

The universities can help in this process of career preparation. They can insist, for example, on more social sciences and humanities, as well as on more basic science and mathematics. Both categories of studies are needed, particularly for engineering students, to provide the breadth which new careers will demand. However, in the last analysis, this new function cannot explicitly be taught in school, any more than one can take formal courses in law school on how to be a judge. This particular teaching must be done rather by tradition and by example.

The third requirement for the new career pattern is to find methods of maintaining technical competence in management functions.

Judges automatically maintain their technical competence by continuous and spirited technical legal discussions with practicing lawyers. They retire to their chambers and look up cases, and come back and challenge the lawyers. In short, in their judgeship function they do not escape from or ignore the details of law.

Now the Office of the Director of Defense Research and Engineering has been criticized for an excessive concern for the minute technical details of weapons systems. But, just as in judicial cases, it is often the technical details upon which the whole matter hinges. Indeed, one can define the proper arena of the new-style scientific manager as that in which the technical details *are* critical.

Where technical details are the dominating factors, one simply cannot divorce authority and knowledge.

I BELIEVE that the scientific community has not realized the magnitude of the problem of keeping the managers technically alive. Should government-supported research contracts, for example, be managed with the part-time services of research people or with the full-time services of specialists? We all know the hazards—the active research people are tempted to be biased, ride hobbies, and even to steal ideas. The full-time specialists, on the other hand, tend to lose the quality of their scientific judgment.

Is job rotation or mid-career education adequate? Can some new form of professional recognition—other than technical-paper publishing—give adequate motivation? I do not know. But I do know this: There is nothing so deadening to an organization as an out-of-date technical man in a position of authority.

The government, on its part, must also do many things in order to face the demands of the scientific revolution honestly. One of them is to correct the inadequate salaries, particularly on the executive levels. Another is to offer technical people suitable environment under the control of professional scientists and engineers. But, frankly, I believe that the limitation today is more in the attitudes and limited horizons of the scientists and engineers themselves.

—CHALMERS W. SHERWIN
in *Naval Research Reviews.*

Dr. Sherwin is Deputy Director, Defense Research and Engineering, U.S. Department of Defense.

McNamara Effect

IN THE DAILY GOSSIP about U.S. Secretary of Defense Robert McNamara that runs through official Washington, mention is rarely heard of one of the most significant results of his tenure in office:

He has changed the nature of scientific deliberations at the White House.

Before Mr. McNamara took charge of the defense establishment, the practice was to dissipate as widely as possible the political heat directed at the Pentagon by committees and members of Congress. One device especially went into operation almost automatically. A controversy in any way related to science—and most military controversies are so related nowadays—would be referred to an advisory panel of scientists. The panels always reported to the President's Special Assistant for Science and Technology either through the President's Science Advisory Committee or through PSAC by way of the National Academy of Sciences.

It was for this reason that first Dr. James R. Killian, Jr., and then Dr. George Kistiakowsky became Assistants to the President. Their competence was in appraisal of weaponry.

Beginning with the late President John F. Kennedy's appointment of Mr. McNamara as Secretary of Defense, the situation gradually changed. Secretary McNamara didn't seek shelter behind scientific advisors. He personally faced the critical fire from Congress.

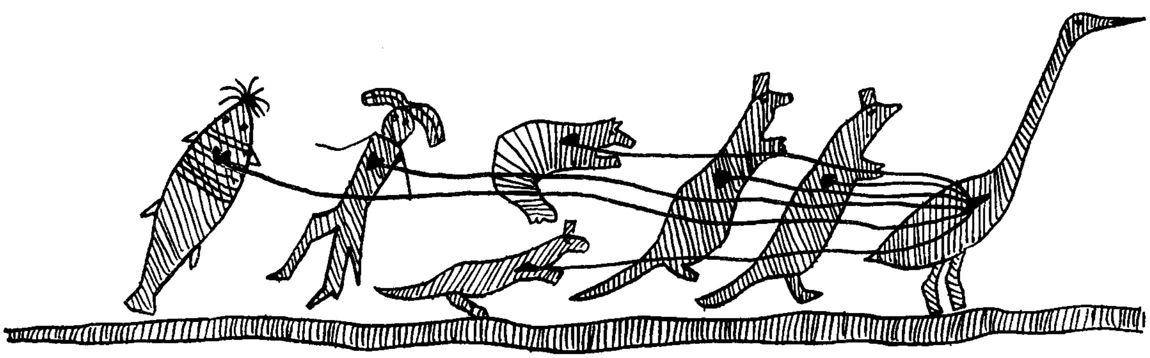
As a consequence, the President's Science Advisory Committee has more leisure today and the President's Office of Science and Technology can concentrate its resources on problems of peace.

It is notable that a McNamara subordinate, Dr. Chalmers Sherwin, should be the one to state the issue of public interest conflict in science administration most succinctly.

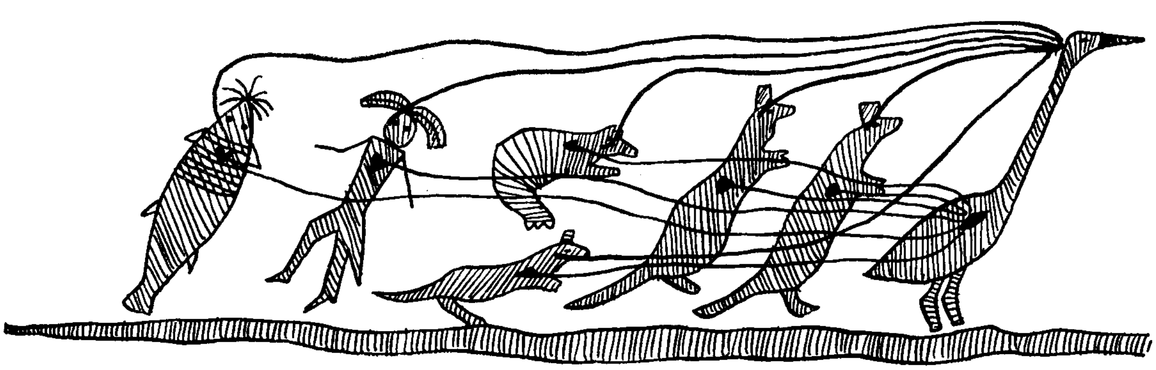
A PETITION TO THE U.S. CONGRESS

WE, THE UNDERSIGNED seven tribes of Indians, hereby declare by the signs of our totems: A crane, three martens, a bear, a manfish and a catfish . . .

. . . that we have agreed in our hearts . . .

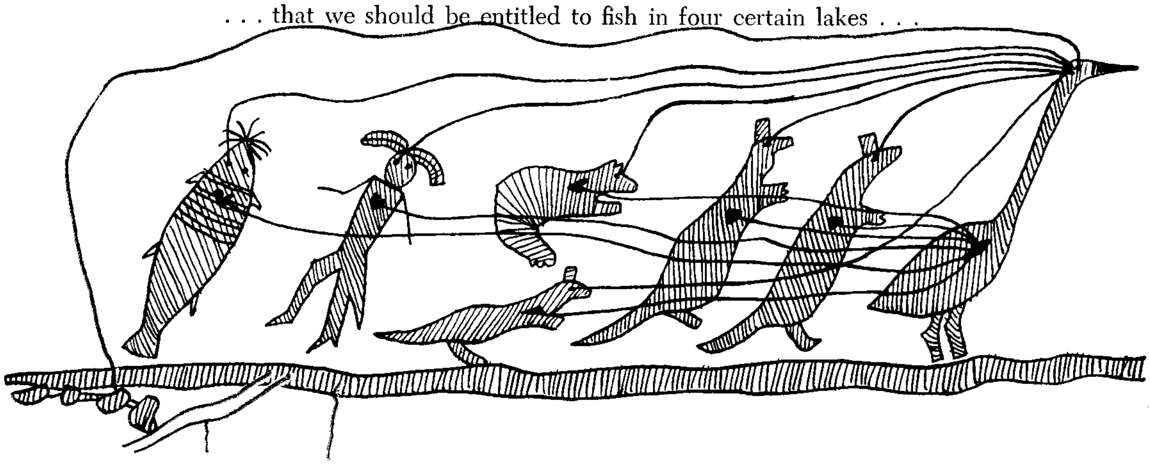


. . . and in our minds . . .

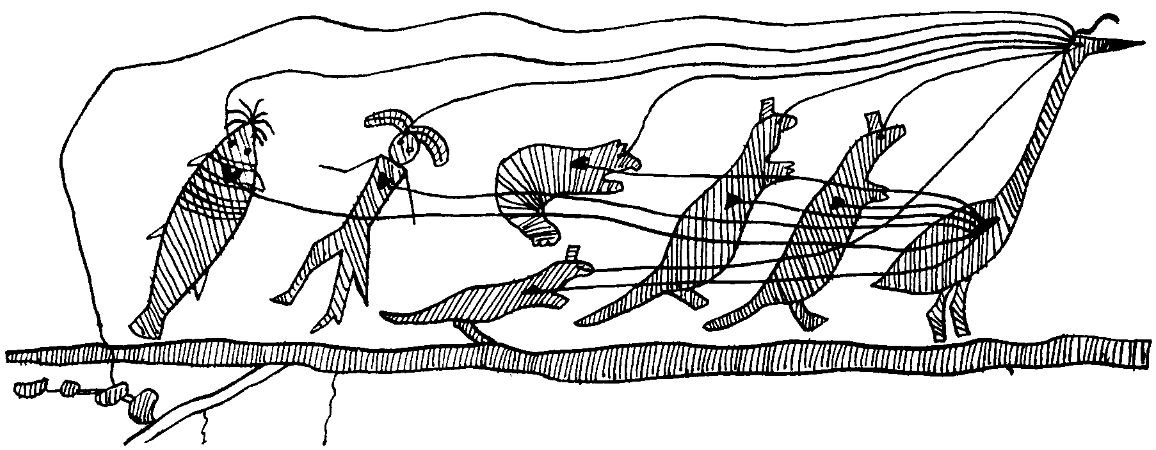


CONCERNING MIDSUMMER SCIENCE

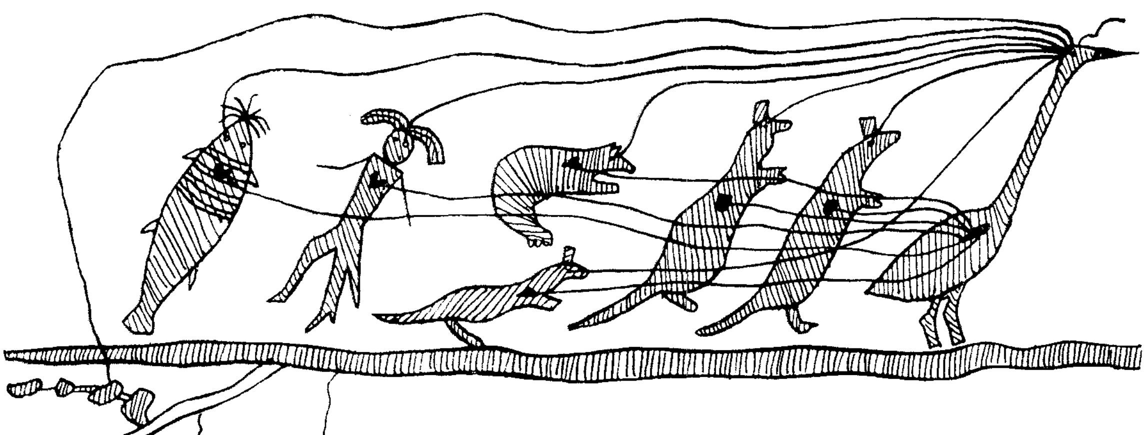
. . . that we should be entitled to fish in four certain lakes . . .



. . . and that we look to you . . .



. . . for gratification of this, our most solemn petition.



—Sketches by Doug Anderson following John Lear's adaptation of a pictograph from "The Story of the Alphabet" (Edward Clodd).

SCIENCE IN BOOKS

IS THE SCIENTIFIC PAPER FRAUDULENT?

Yes; It Misrepresents Scientific Thought

I HAVE chosen for my title a question: Is the scientific paper a fraud?

I ought to explain that a scientific "paper" is a printed communication to a learned journal, and scientists make their work known almost wholly through papers and not through books, so papers are very important in scientific communication. As to what I mean by asking "is the scientific paper a fraud?" —I do not, of course, mean "Does the scientific paper misrepresent facts?" and I do not mean that the interpretations you find in a scientific paper are wrong or deliberately mistaken. I mean the scientific paper may be a fraud because it misrepresents the processes of thought that accompanied or gave rise to the work that is described in the paper.

That is the question, and I will say right away that my answer to it is "yes." The scientific paper in its orthodox form does embody a totally mistaken conception, even a travesty, of the nature of scientific thought.

Just consider for a moment the traditional form of a scientific paper (incidentally, it is a form which editors themselves often insist upon). The structure of a scientific paper in the biological sciences is something like this: First, there is a section called the "introduction" in which you merely describe the general field in which your scientific talents are going to be exercised, followed by a section called "previous work" in which you concede, more or less graciously, that others have dimly groped toward the fundamental truths that you are now about to expound. Then a section on "methods"—that is O.K. Then comes the section called "results."

The section called "results" consists of a stream of factual information in which it is considered extremely bad form to discuss the significance of the results you are getting. You have to pretend that your mind is, so to speak, a virgin receptacle, an empty vessel, for information which floods into it from the external world for no reason which you yourself have revealed. You reserve all appraisal of the scientific evidence until the "discussion" section, and in the discussion you adopt the ludicrous pretense of asking yourself if the information you have collected actually means anything.

Of course, what I am saying is rather an exaggeration, but there is more than a mere element of truth in it.

The conception underlying this style of scientific writing is that scientific discovery is an inductive process. What induction implies in its cruder form is roughly speaking this: Scientific discovery, or the formulation of scientific theory, starts in with the unvarnished and unembroidered evidence of the senses. It starts with simple observation —simple, unbiased, unprejudiced, naïve, or innocent observation—and out of this sensory evidence, embodied in the form of simple propositions or declarations of fact, generalizations will grow up and take shape, almost as if some process of crystalization or condensation were taking place. Out of a disorderly array of facts, an orderly theory, an orderly general statement, will somehow emerge.

THIS conception of scientific discovery was mainly the work of a great and wise, but in this context, I think, very mistaken man—John Stuart Mill.

John Stuart Mill saw, as of course a great many others, including Bacon, had seen before him that deduction in itself is quite powerless as a method of scientific discovery—and for this simple reason: that the process of deduction as such only uncovers, brings out into the open, makes explicit, information that is already present in the axioms or premises from which the process of deduction started. The process of deduction reveals nothing to us except what the infirmity of our own minds has so far concealed from us.

It was Mill's belief that induction was the method of science—"that great mental operation," he called it, "the operation of discovering and proving general propositions." And around this conception there grew up an inductive logic, of which the business was "to provide rules to which, if inductive arguments conform, those arguments are conclusive."

Now, John Stuart Mill's deeper motive in working out what he conceived to be the essential method of science was to apply that method to the solution of sociological problems: He wanted to apply to sociology the methods which the practice of science had shown to be immensely powerful and exact. It is ironical that the application to sociology of the inductive method, more or less in the form in which Mill himself conceived it, should have been an almost entirely fruitless one.

The simplest application of the Millsian process of induction to sociology came in a rather strange movement called Mass Observation. The belief underlying Mass Observation was apparently this: that if one could only record and set down the actual raw facts about what people do and what people say in pubs, in trains, when they make love to each other, when they are playing games, and so on, then somehow, from this wealth of information, a great generalization would inevitably emerge.

Well, in point of fact, nothing important emerged from this approach.

THE theory underlying the inductive method cannot be sustained. Let me give three good reasons why not.

In the first place, the starting point of induction is philosophic fiction. There is no such thing as unprejudiced observation. Every act of observation we make is biased. What we see or otherwise sense is a function of what we have seen or sensed in the past.

The second point is this: Scientific discovery or the formulation of the scientific idea on the one hand, and demonstration or proof on the other hand, are two entirely different notions. Mill confused them. Mill said that induction was the "operation of discovering and proving general propositions," as if one act of mind would do for both.

Now, discovery and proof could depend on the same act of mind, and in deduction they do. When we indulge in the process of deduction—as in deducing a theorem from Euclidian axioms or postulates—the theorem contains the discovery (or, more exactly, the uncovery of something which was there in the axioms and postulates, though it was not actually evident) and the process of deduction itself, if it has been carried out correctly, is also the proof that the "discovery" is valid, is logically correct. So in the process of deduction, discovery and proof can depend on the same process. But in scientific activity they are not the same thing—they are, in fact, totally separate acts of mind.

It simply is not logically possible to arrive with certainty at any generalization containing more information