Are We Retrogressing in Science?

Despite superficial evidence to the contrary, science in the United States is in a state of confusion.

M. King Hubbert

Is science really as well off as it appears to be?

It is a truism that we are living in one of the greatest periods of scientific activity in history. Measures of the flourishing state of science are the great increase in the number of scientists, the proliferation of scientific journals, and the vast sums of money that are being spent in support of scientific activity. In the first edition of *American Men of Science*, published in 1906, 4000 scientists were listed; in the 10th edition, published in 1960, there were 115,000. During this period the number has doubled every 10 to 11 years (*1*). The number of scientific journals has been doubling every 15 years and is now approaching 100,000. As for the money that is being spent on science, the yearly expenditures of the National Science Foundation in support of fundamental research increased from $4 million in 1952 to $175 million in 1961, and this year's budget for the space program stands at $5.4 billion.

Yet, certain symptoms that I have been observing over the last quarter century are beginning to fall into a pattern which leads me to conclude that in two of its most fundamental aspects science is not in a healthy state. To explain, let me go back to the work of Galileo Galilei (*2*), the individual above all others to whom physical science owes its origin. Galileo's career was a lifelong fight to establish the proposition that the highest court of appeal, when it comes to natural phenomena, is not a human authority, either ancient or modern, but a valid observation or experiment. From this proposition it follows that the acceptance of any conclusion, valid or otherwise, by an individual who is not familiar with the observational data on which it is based and the logic by which it is derived is a negation of science and a return to authoritarianism. Such a reversion, and the careless retreat from fundamentals that are its corollary, make up the pattern that one sees increasingly manifested today. Consider the following examples.

Anomalous Statements from Recent Treatises

The *American Institute of Physics Handbook* (*3*), published in 1957, is an ambitious work of some 1500 pages written and edited by American physicists under the sponsorship of the American Institute of Physics. Section 2 of this work, comprising 236 pages, deals with "Mechanics"; section 2a, by three professors of physics of a major university, is entitled "Fundamental Concepts of Mechanics. Units and Conversion Factors." On pages 2 to 14 of this section there is a subsection on "Fundamental Units," which is followed (pp. 2–15) by one on "Derived Units."

Before examining these sections, let us recall something of the history of the concepts "fundamental unit" and "derived unit." We all know that the metric system, which is the most widely employed system of measurement in science, had its beginning in 1795, shortly after the French Revolution, when the Standard Meter and the Standard Kilogram were constructed in Paris as the new standards for the measurement of length and of mass, respectively. We also know that this new system was instituted as a means of escape from the chaos of units of measurement in use theretofore (and still in use, regrettably, in the English-speaking countries). It was not until 1822 that a theoretical basis for consistent systems of measurement was established. This occurred when Joseph Fourier, in his epoch-making treatise on heat conduction, *Théorie Analytique de la Chaleur* (*4*), pointed out that in a complete physical equation the separate terms must all have the same "dimensions."

This was followed up 10 years later by Carl Friedrich Gauss (*5*), who, in his development of a method of measuring the moment of a magnet and the intensity of the earth's magnetic field in units that did not depend upon comparisons between quantities of like kinds, pointed out that in any domain of physics there is some minimum number of quantities which are mutually independent and to which arbitrary units of measurement may be assigned. These comprise the *fundamental quantities* of the system. All other quantities of the system are then the *derived quantities* and are definable in terms of the fundamental quantities by means of the dimensional relationships of Fourier. Thus, if, in a given system, the quantities $A, B, C, \ldots N$ are mutually independent and are chosen as the fundamental quantities of the system, then all other quantities are derived quantities and are definable in terms of the fundamental quantities by expressions of the form

$$[Q] = [A^a B^b C^c \ldots N^n]$$

where $a, b, c, \ldots n$, which must be either integers or rational fractions are said to be the dimensions of Q in terms of the respective fundamental quantities. A system of measurement satisfying these conditions was defined by Gauss to be an *absolute system*.

If units of measurement are assigned for the quantities selected as fundamental, no other units are required, because the units for the derived quantities are determined in terms of those for

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the fundamental quantities in accordance with the dimensional expression given above. For example, if in a geometrical system the unit of length is chosen as fundamental, no separate unit for area or for volume is required because the unit of area is the square whose sides have unit length, and the unit of volume is the cube whose edges have unit length.

Thus was established the theoretical basis for all subsequent physical mensuration. So great was the importance attached to these principles by James Clerk Maxwell that, in his great work *A Treatise on Electricity and Magnetism* (6), he devoted the first six pages to their detailed exposition. Likewise, A. G. Webster, in *The Dynamics of Particles and of Rigid, Elastic, and Fluid Bodies* (7), devoted pages 26 to 33 to the same subject.

In mechanics it has long been found convenient to choose the quantities length, mass, and time as fundamental, with either the centimeter, the gram, and the mean solar second as fundamental units (the centimeter-gram-second system) or the more recently preferred meter, kilogram, and mean solar second as fundamental units (the meter-kilogram-second system). Other systems based on length, force, and time also are used in engineering, and, of course, systems which employ the English units of length and of mass or force are used in engineering in English-speaking countries.

In view of this history one would expect to find in the section on mechanics of any handbook of physics a unit of length, a unit of mass, and a unit of time specified as fundamental units. More specifically, one would expect to find the centimeter or the meter specified as a fundamental unit of length, the gram or the kilogram as a fundamental unit of mass, and the mean solar second as a fundamental unit of time, in virtue of the fact that these have been in almost universal use in science for the last century.

As derived units, one would then expect to find units defined according to the dimensions of the derived quantities by the appropriate combinations of the fundamental units.

In the *American Institute of Physics Handbook,* however, this is not what one finds. Under "Fundamental Units," the first unit listed is the circular mil, defined as, "area of a circle whose diameter is 0.001 in." Instead of being a fundamental unit, the circular mil is, by the authors' own definition, a derived unit of area, defined in terms of the fundamental unit of length, the inch, with the inch nowhere defined.

The remaining "fundamental units" are equally astonishing. They include the day (defined as the period of rotation of the earth), the hour (1/24 of a day), the minute (1/60 of an hour), the second (1/60 of a minute), the sidereal year, the tropical year, the light-year, the degree (of arc), the radian, and the steradian.

Among these "fundamental" units there is no unit of mass and no unit of length (except the light-year, which is defined as a derived unit in terms of the mile, the second, and the year, with the mile nowhere defined), but there are six units of time. Of the time units, three—the day, the sidereal year, and the tropical year—are independently defined, while the other three—the hour, the minute, and the second—are derived units by definition. Hence, of this entire system the only unit which does represent one of the fundamental units in standard usage in physical mensuration is the second, but even this is in error by the factor 365/366 with respect to the second used as a physical standard because the latter is the mean solar second, whereas the one defined above is the sidereal second.

The section on derived units is equally incredible. It contains a hodgepodge: the atmosphere, the centimeter of mercury at 0°C, the foot of water at 4°C as units of pressure; the dyne, the newton, the poundal as units of force; the British thermal unit (mean), the calorie (mean), the erg, and the kilowatt hour as units of energy or work; and, finally, the watt as a unit of power.

These are defined in terms of such fundamental units as the foot, the centimeter, the meter, the degree centigrade, the degree Fahrenheit, the gram, the kilogram, the pound mass, and the second; none of these, except for the second, was previously listed among the fundamental units, and the second, as we have observed, was listed erroneously. Besides, in giving the foot of water and the centimeter of mercury as units of pressure, no mention is made of the acceleration of gravity.

The question of present interest is this: In view of the history and theory of physical mensuration just reviewed, and the almost universal practice in physics, at the present time, of using either the centimeter-gram-second or the meter-kilogram-second system, how was it possible for three professors of physics to have compiled, as fundamental and derived units in mechanics, such a conglomeration of units as those listed in the *American Institute of Physics Handbook*? Granted that such a list was compiled and submitted to the editors in manuscript form, how was it possible that it was not rejected by the referees and editors?

In a different part of the same section of the *Handbook* there occurs (pp. 2-179 to 2-181) a short article by another professor of physics on fluid-flow properties of porous media and viscosity of suspensions. Here, in discussing the flow of fluids through porous solids, the author cites "the empirical relation known as Darcy's law" given by the equation

$$v = k' \frac{dp}{dx}$$

in which $k' = k/\eta$ (k is the permeability of the solid and η is the viscosity of the fluid), v is the volume of fluid crossing unit area in unit time, and dp/dx is the x-component of the pressure gradient in the fluid.

In order to test the validity of this equation it is only necessary to apply it to a fluid at rest, which is a special case of a fluid in motion. In this case we obtain

$$k' \frac{dp}{dx} = 0$$

and since x is a linear coordinate in any arbitrary direction, it follows that p must have a constant value in three-dimensional space. Since this is not true for any actual fluid anywhere on earth, one wonders who performed the empirical experiment, and where.

This might be considered almost a trivial example were it not for the fact that the equation cited was for 25 years the most widely used equation in the petroleum industry. It was used as the basis for nearly all reservoir engineering, for several major physical treatises, and for most of the journal literature in petroleum engineering during that period, and it was accepted, with rarely a dissenting voice, by a technical personnel which was a representative cross section of the output of all the institutions of higher learning in the United States, before it was ruefully discovered that the equation in question was neither physically correct nor a valid statement of a result established a century earlier by a Frenchman named Henry Darcy (*8*).

Darcy's result was of the form

$$v_z = -K(dh/dz)$$

where

$$h = z + (p/\rho g)$$

Here g is the acceleration of gravity, ρ is the density of the flowing fluid (water, in Darcy's experiments), and h is the height above a reference elevation $z = 0$ to which the liquid will rise in a tube terminated at a point in the flow system of elevation z and fluid pressure p. Darcy's experiments were restricted to vertical flow, but his equation is equally valid for flow in any other direction.

Again, the question of present concern is not how the mistake of writing an erroneous equation was made in the first place—we all make mistakes—but how such an equation of elementary mechanics, which can be seen, on simple inspection, to be erroneous and can be violated experimentally in any desired manner, could have been accepted as valid by a very large cross section of American scientists, both inside and outside of universities, for nearly three decades.

In an attempt to find an answer to questions such as this, I was prompted, about 5 years ago, to examine some of the college textbooks of physics which were in wide use at that time. I examined and evaluated five such books on the basis of the two criteria: (i) Were the propositions stated in the book correct? (ii) Were they given valid derivations, or were they to be taken as true "because the book said so"?

On the basis of these two criteria the best of the five books could hardly be given a rating of validity higher than 85 to 90 percent, whereas the rating for the poorest would be closer to 50 percent.

For example, in about the second or third chapter, each of the books gave a statement of Newton's law of gravitation, and, on the basis of this, each computed the mass of the earth. Notwithstanding the fact that Newton's derivation of the law of gravitation is one of the greatest achievements in the history of science, only one of the five books so much as mentioned that it was derived from the Keplerian laws of planetary motion. According to the other texts the student could only infer that the law was arrived at by some mysterious sort of inspiration.

Of the five statements of the law of gravitation, four were erroneous. The erroneous statements were all of the form, "Any two *bodies* attract each other with a force which is proportional to the product of their masses and inversely to the square of the distance between them." That this statement is erroneous may be seen by the fact, proved by Newton himself in his *Philosophiae Naturalis Principia Mathematica* (*9*), that if one of the two bodies is a spherical shell of uniform density and the other is a mass of arbitrary shape placed anywhere inside the shell, the attraction between the two bodies is precisely zero; or, if the two bodies are coaxial parallel discs whose separation is small in comparison with their diameters, the force of attraction, to a close approximation, is constant and independent of the distance between them.

All five textbooks gave a computation for the mass of the earth, and all five were in error in that the method used could not be regarded as valid without the proof of an essential intermediate proposition which Newton himself had to prove originally. The best of the five books admitted that the computation without the proof of this missing proposition was not valid, but it justified the omission by the plea of lack of space.

By the time the subject of electricity and magnetism was reached, any pretense of logical consistency was abandoned in most of the books. Several of them stated that it would be possible to develop the subject by old-fashioned methods involving the attractions and repulsions between charged pith balls, but that it was much simpler to develop it in another manner. Then would follow the statements that matter is composed of atoms; that an atom consists of a nucleus with planetary electrons; that the electric charge on an electron is a specified number of electrostatic units (electrostatic unit not defined); and that the number of molecules in a gram-molecular weight is a specified number, Avogadro's number. Then, with these building blocks (given without supporting evidence or derivations), the book would proceed to develop the subjects of electricity and magnetism and, eventually, nuclear physics (*10*).

As stated earlier, the almost universal acceptance and use of physical statements which may be shown by the simplest tests not to be valid are, when seen as isolated occurrences, indeed puzzling. However, after the examination of the textbooks of physics which I have just recounted, such occurrences are not only no longer puzzling but, in fact, rather to be expected. For, from the evidence reviewed, it appears that in physics, at least, there has occurred during recent decades a serious reversion to pure authoritarianism, whereby statements, if made by proper "authorities," are to be accepted as valid, independently of any supporting evidence. There is a corollary: If a contrary statement is made, even with ample supporting evidence, by one who is not an "authority" on the subject in question, little credence can be given it.

Abandonment of Classical Physics

What factors may have contributed to such a retrogression can only be conjectured. Certain it is, however, that about 35 years ago the preoccupation of physicists with nuclear phenomena became so great that there occurred a wholesale abandonment of interest in classical physics, and that with this abandonment the intellectual foundations of the whole structure of physical theory were lost sight of. As a result of this abandonment, it is possible that students within recent decades have not learned classical physics, and that now those same students have become authors of textbooks.

Transition from Educational to Research Institutions

Another major contributing factor—possibly the greatest—has been the confusion which has arisen during and since World War II concerning the functions of our universities in the evolution of science. Traditionally, a university is a community of scholars whose dual functions are (i) to inquire into the foundations and to extend the borders of knowledge, and (ii) to impart this knowledge and the mental attitudes of inquiry and scholarship to their students.

During World War II our universities were seriously disrupted. A large proportion of their potential students were in the armed forces, and many of the scientific members of their faculties became engaged in work related directly to the prosecution of the war. Under government contracts many universities became essentially war-research laboratories employing large staffs of nonacademic personnel. Since the war this pattern has tended to become perpetuated, and as recently as 1958–59 most of the endowed universities were supported, to the extent of 25 to 88 percent of their total budgets, by funds obtained from one kind or another of government contract (Table 1). The outstanding exception is Yale, with no contract support.

Table 1. Federal contract support of representative endowed universities, 1958–59. [From *American Universities and Colleges* (*12*)]

University	Total income ($)	Income from contracts ($)	Contracts (%)
California Institute of Technology	60,675,342	53,600,442	88
Massachusetts Institute of Technology	101,386,000	67,276,000	66
University of Chicago	103,771,777	61,531,262	59
Princeton	31,563,000	17,723,000	56
Harvard	67,292,489	16,307,946	24
Stanford	34,663,961	8,312,208	24
Rice	6,366,700	633,500	10
Yale	36,985,998	0	0

In addition, universities have entered the field of big business by becoming the operators, under government contracts, of several very large industrial-research laboratories. Among these laboratories, whose aggregate budgets total hundreds of millions of dollars per year, are the following installations of the Atomic Energy Commission: the Lawrence Radiation Laboratory at Livermore, California, and the Los Alamos Scientific Laboratory at Los Alamos, New Mexico, operated by the University of California; the Argonne Laboratory near Chicago, operated by the University of Chicago; and the Brookhaven National Laboratory on Long Island, operated under contract by the Associated Universities, Inc., of which Columbia, Cornell, Harvard, Johns Hopkins, the Massachusetts Institute of Technology, Princeton, the University of Pennsylvania, the University of Rochester, and Yale are the member universities.

At present there is academic jockeying for the capture of large fractions of the largess represented by the billions of dollars per year which are to be expended on the government-financed space program.

The effect upon the universities of this type of diversion has been devastating. Instead of remaining primarily educational institutions and centers of fundamental inquiry and scholarship, the universities have become large centers of applied research. In fact, it is the boast of many that their highest-paid professors have no teaching duties at all. Instead of providing an atmosphere of quiet, with a modicum of economic security afforded by the system of academic tenure, where competent scholars may have time to think, the universities have become overstaffed with both first- and second-class employees. Those of the first class, who bear the title "professor" and enjoy academic tenure, have largely become directors of research; those of the second class, whose competence often equals or exceeds that of the first class, are the research-project employees whose tenure extends from one contract to the next.

Since these contract grants are often made only in response to applications from individuals or groups within the university, and since the university itself is largely dependent upon them, it is understandable that a very large premium is thus placed upon the promoter, or the "empire builder," at the expense of the true scientist and the scholar.

The following example is not atypical. Not very long ago the two chairmen of closely related departments in a university applied for a government grant of about a quarter of a million dollars to initiate a program of research in a specified field, with themselves designated as the Principal Investigators. Neither of the Principal Investigators, according to the credentials they themselves supplied, had any particular qualifications for carrying out the work proposed. A closer reading of the proposal, however, showed that they had no intention of doing so. The proposal was to build and equip a laboratory, and to hire a staff to do the work.

Or, as an example of the inequities to which such a system leads, one might consider the following. The faculty of a given department in a certain university consists of 12 men, and the annual budget for 11 of these men is about $635,000; the budget for the 12th man, because of his superiority in the capture of government contracts, is $500,000.

Complementing activities of this sort is the prevailing academic system of preferment based upon the fetish of "research." Faculty members are promoted or discharged on the basis of the quantity of their supposed research, rarely on the basis of their competence as teachers. And the criterion of research is publication. The output per man expected in some institutions, I am informed, is three or four published papers per year. In almost any university one hears the cynical unwritten motto: "Publish or perish." In addition, there is the almost universal practice of paying the traveling expenses of faculty members who are presenting papers at a scientific meeting; the "nonproductive" members can pay their own way or stay home. The effect of such a system on the number and quality of papers with which the program of every scientific meeting is burdened requires no elaboration.

Within the university, it is easily seen, such a system strongly favors the opportunist capable of grinding out scientific trivialities in large numbers, as opposed to the true scholar working on difficult and important problems whose solution may require concentrated efforts extending over years or even decades. It took Kepler, working on Tycho Brahe's astronomical observations—the product of a lifetime—19 years to solve the puzzle of planetary motions, but the result was the now celebrated Keplerian laws of planetary motion. Newton, with few intervening scientific publications, spent some 20 years studying the mechanics of moving bodies before writing his great treatise *Philosophiae Naturalis Principia Mathematica,* in which are derived the Newtonian laws of motion and the law of universal gravitation. Twenty-two years of work, the last 11 essentially free of other writings, preceded the publication, in 1859, of Darwin's *On the Origin of Species by Means of Natural Selection* (*11*). How long could any of these men have survived in an American university of today?

Conception of Evolution of Science

Another factor involved in our reversion to authoritarianism is a prevailing view of the evolution of science. This is that scientific knowledge has become so vast as compared with the limited capabilities of the individual human intellect that one man can only hope to know "authoritatively" a minute fraction of the whole. Hence we are constrained, if we are to avoid being scientific dilettantes, to select some limited domain—our "specialty"—of such small size that we are capable of reading all the pertinent literature and hence of mastering all that is known about it. By this premise, all other scientists must do the same with respect to other domains, so that the only way of knowing anything outside of one's own specialty is to accept the word of an authority or specialist in that field. Hence, according to this view, we are condemned to accept authoritarianism by the very immensity of human knowledge.

In support of this, one need look no further than at the magnitude and rate of increase of scientific literature. As we saw earlier, for the last 200 years the number of scientific journals has been increasing tenfold every 50 years and is now approaching 100,000. In geology alone, no one of us, even by using every system of accelerated reading that could be devised, could read more than a small fraction of the literature that is currently coming off the printing presses, let alone that which has accumulated already. Every working scientist is, accordingly, plagued with the question of how much of his time may be profitably spent in reading; and, more important still, what he should read?

It is my present thesis that this state of confusion is quite unnecessary and that it arises from a fundamental misconception regarding the nature of science and of its evolution. The evolution of science is, in fact, not a progression from the simple to the complex, but quite the opposite. It is a progression from the complex to the simple.

In its initial stage every branch of science is confronted with a chaos of phenomena infinite in detail, about which nothing is understood. The apparent initial simplicity is therefore an illusion based upon the fact that our ignorance at this stage is so complete as to render us impotent. The evolution of science is always in the direction of reducing this initial chaos to a form which is within the scope of human comprehension, and each successive refinement in science results in a still greater simplification.

Consider mathematics, for example. In the initial stages we were mathematically impotent. Then we learned to count and to write numbers, using a variety of systems of numeration ranging from the decimal to the sexagesimal and other complex and mixed systems. And various systems of writing numerals were devised, that of the Greeks differing from that of the Romans, and both differing from that of the Hindus and the Arabs.

With each simplification of arithmetic there was a corresponding increase in the ease and power with which numerical calculations could be carried out. For instance, a story is told (I have forgotten the source) of a German merchant of about the 13th century, when Roman numerals were still in use in western Europe, who asked a learned friend to recommend a university to which his son might be sent to learn mathematics for use in the family business. If he only wanted his son to be able to add and subtract, the merchant was told, this could be learned in a German university; but if he wished him also to be able to multiply and divide, the son would have to be sent to a more advanced institution in Italy. To appreciate the nature of this difficulty one need only try to multiply and divide with Roman numerals.

The whole history of mathematics has been a progression wherein problems which at one stage were solvable with great difficulty, if at all, were reduced during the next stage to almost elemental simplicity.

The same has been true in the observational and experimental sciences. Astronomy in its initial stage dealt with a chaos of stars. Then, systematic observations led to the determination of terrestrial direction, and to accurate predictions of the seasons. Further refinement, in parallel with the development of Greek geometry and trigonometry, led to determination of the shape and size of the earth, to determination of the approximate dimensions of the solar system, and to development of the kinematics of the motions of its members under the Ptolemaic, or geocentric, system of reference.

The next great simplification was achieved by the Polish mathematician Copernicus. This was accomplished by the simple device of shifting the origin of coordinates from the earth to the sun. A still further simplification was achieved by Kepler. He was able to summarize all of Tycho Brahe's precise observations into three simple statements, the laws of planetary motion. Finally, the crowning simplification was accomplished by Isaac Newton, who reduced the entire system of observations of planetary motions to a single statement, the law of universal gravitation.

The phenomena of mechanics, likewise, comprised initially a chaos of infinite variety and detail. Were one to attempt to learn mechanics by mastering these details, or even a significant fraction of them, the task would be hopeless. Fortunately, since the time of Newton this has not been necessary. The mechanical observations required to establish the Newtonian laws of motion are a select few, yet these laws are so comprehensive that familiarity with their derivation and understanding of their use and implications, in conjunction with knowledge of a few properties of matter, renders any particular problem in mechanics susceptible of analysis.

In geology, likewise, the initial observations comprise a tremendous chaos—of landforms, of rock types and configurations, of land and water, of earthquakes and volcanos. It was only after many centuries that the landforms were demonstrated to be the result of a combination of slow deformation and uplift of the rocks of the solid earth and of their sculpturing, principally by running water. Only after centuries was it realized that the shell-like, bone-like, and leaflike forms found commonly in stratified rocks were indeed the shells and the bones of animals and the leaves of plants which had lived upon the earth during earlier geologic times. So great is the simplification that has been achieved by these and other comparable theoretical results that a field geologist need now record in his notebook only a highly selective fraction of his total observations. From these selected observations he is able to make a reasonably accurate determination of the configurations of the rocks and of the principal events of their geological history.

Without belaboring the point, let me summarize by stating that the common denominators of all phenomenological sciences are (i) an initial chaos of phenomena, infinite in amount, and (ii) the simplicity and finite capacity of the human intellect. Since it is impossible for human beings to understand chaotic phenomena, it is necessary that these be reduced to a state of simplicity if they are ever to be understood. The entire history of science has been the history of the progressive reduction of one chaos of phenomena after another into a form that is within the powers of comprehension of an average human being. The greatest achievements in science are, accordingly, those master syntheses that have reduced the widest ranges of phenomena into relationships comparatively easy to comprehend. When these general relationships are known and understood, it is no longer necessary for an individual to burden his mind with an infinity of detail concerning the phenomena encompassed; these phenomena emerge as special cases which may be dealt with individually when the need arises.

In the whole field of science these master generalizations number at most but a few tens. They include the three Newtonian laws of motion and the law of universal gravitation, the three laws of thermodynamics and the associated thermodynamics of irreversible processes, the two Maxwellian laws of electromagnetism, the law of conservation of matter, and the concept of the atomic and molecular nature of the chemical elements and their compounds.

Also, although capable of less precise statement, we have in geology the hypothesis that the past history of the earth is in large measure decipherable from present observations of the rocks and their contents, and in biology we have the Darwinian theory of evolution, the Mendelian and gene theory of genetics, and the bacterial and virus theories of disease.

In the field of nuclear physics, despite the fact that there the chaos of phenomena is as yet only partially reduced to an intelligible state, we have several sweeping generalizations by which the chaos has been greatly lessened. These include the Einsteinian equation of the equivalence and interconvertability of matter and energy; the Planck equation of the quantum of radiant energy; the Rutherford picture of the atom as comprising a heavy nucleus with a planetary system of electrons; and the concept of a nucleus which consists of a discrete number of particles, protons and neutrons of nearly equal mass, the number of the former determining the chemical element to which the atom belongs and the combined number determining approximately the atomic mass. Finally, there is the generalization that by changing the number of protons in the nucleus either spontaneously by natural radioactivity or artificially by particle bombardment, the transmutation of the elements can be achieved, with a corresponding emission or absorption of large quantities of energy.

It is precisely because these great generalizations collectively encompass the whole domain of matter and energy —the whole domain of observable phenomena—that a modern scientist cannot afford to be ignorant of them. However, if he does have this type of knowledge, it is no longer necessary for him to burden his mind with the infinity of details in whatever domain of phenomena he may choose to work; neither does he have to read all the literature. Moreover, provided he is willing to take the necessary time to become familiar with the essential phenomena, there is no a priori reason why he may not be qualified to investigate as many different domains as his interests and circumstances may warrant.

On the other hand, to anyone educated in science in accordance with the specialistic view that science has become so vast and so complex that the human individual can only hope to comprehend a minute fraction of the whole, the initial chaos of phenomena must inevitably remain a chaos, simply because such an individual has had no opportunity to become informed of the extent to which this chaos has already been reduced to understandable terms.

According to this specialistic view, any scientific enterprise of broader scope than an individual "specialty" can only be carried out through the cooperation of teams representing the various "specialties" involved. Thus, one hears repeatedly that the future advancement of science is more likely to be the result of such cooperative teamwork than of work done by individuals, and in current literature papers bearing the names of as many as half a dozen coauthors are not uncommon.

I do not mean to imply that in many instances such cooperative enterprises, especially on the technical or development level, may not be the most effective means of doing a piece of work. Moreover, it is obvious that the work of any individual must be based upon what others have done before. It may be well to remind ourselves, however, that thinking is peculiarly an individual enterprise, and that the greatest of all scientific achievements—those of the great synthesizers from Galileo to Einstein—have, almost without exception, been the work of individuals.

State of Geology

It may appear odd that I, a geologist, have illustrated my thesis with examples from other physical sciences. My justification for doing so is that the earth and its biological inhabitants, with which geologists must deal, is a composite of every class of phenomena of primary interest to the other natural sciences. Hence, any retrogression or reversion to authoritarianism in these basic sciences is of fundamental concern to geology, since it is here that erroneous results of the types cited often find their principal application.

The task of geology—the science of the earth—is to achieve as complete an understanding of the phenomena of the earth as possible. Since the phenomena of geology are a composite of the phenomena of all the other natural sciences, a usable knowledge of the fundamental principles established in these other sciences is essential if geologists are to reduce the chaos with which they must deal to comprehensible terms.

In view of the complexity of these phenomena, it is not surprising that geologists, too, have tended to become confused, and to adopt the specialistic view. Thus, instead of being students of the earth, geologists have tended to become students of minerals, of rocks, of ore deposits, of coal, of petroleum, of strata, of fossils, of deformational structures, of volcanos, of erosion and landforms, and of the physics and chemistry of the earth.

There is additional confusion between geology as a science and geology as a gainful occupation. This is particularly evident in the field of petroleum geology, which constitutes, scientifically, but a limited fraction of the total field of geology, yet is, by a wide margin, the segment in which the greatest number of geologists are employed. It has been known from the beginning that the world's resources of petroleum are finite, hence that this field cannot continue indefinitely to be a major source of gainful employment. Yet despite this prior knowledge, it is not surprising, as a sociological phenomenon, to see defensive movements of a trade-union nature originate spontaneously, as employment of geologists in the petroleum industry of the United States approaches its culmination and eventual decline. Whether such movements may be detrimental to the progress of geology as a science merits serious consideration.

Offsetting these negative considerations is the fact that during recent decades the view that geology is an integral science, and that an adequate geological education must embrace the fundamentals of the other sciences, has gained wide acceptance. Very good educational programs based on this view are already operating successfully in a number of universities.

Summary and Conclusion

Despite the large amount of superficial evidence to the contrary, the present state of science in the United States is one of considerable confusion. In large measure, we appear to have lost sight of our intellectual foundations and to have reverted to authoritarianism. Contributing to this situation is the state of the universities. Since World War II these have become so deeply engaged in the pursuit of various kinds of applied research that they have seriously neglected their primary duties as institutions of learning and of education. Possibly the greatest source of this disruption is the government contract-grant system upon which the universities are becoming increasingly dependent for continued existence.

At the same time, the problems confronting the human race today are such that a widespread knowledge of science is essential if they are to be dealt with effectively. If noncatastrophic solutions of these problems are to be found, it is urgent that our universities again become institutions of learning, and that we provide for them a more orderly form of support than that which they now receive. It is equally urgent that competent teaching in universities again be accorded the respect that its importance demands, and that the curriculum be revised to make it not only possible but mandatory for students to receive a working knowledge of the fundamental principles of science. It is also urgent that universities abandon their present preoccupation with trivial "research," and its bookkeeping based on the number of papers published per year, and attempt to achieve an atmosphere in which a Gallileo, a Kepler, a Newton, a Darwin, or a J. Willard Gibbs would find it congenial to work.

Should these things be done, a badly needed renaissance in education, in scholarship, and in science, almost certainly could be brought about.

References and Notes

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News and Comment

Space Program: Skepticism Grows But in Context of Cold War It Is Hard for Congress To Say No

Amid reports of growing congressional skepticism, the space agency went to Capitol Hill last week to present an "austere" $5.7 billion budget for the coming fiscal year, an increase of about $2 billion over its present budget.

Congress undoubtedly will fulminate over the grand total, and it can be expected to slice here and there, but it is a safe asssumption that between now and the day of decision the Soviets will come to the rescue with a mighty space spectacular that will drown out the voices of skeptics. Such was the case last year when the critics, who were enjoying a spate of public attention, suddenly found themselves pushed out of view by the excitement—and fear—produced by the simultaneous orbiting of cosmonauts Nikolayev and Popovich. The Russians have not told us what they now have in the works, but it is not likely to be of a petty nature, and when it is carried out, the space agency's budget will ride home free in its wake.

Nevertheless, the ranks of the space critics are growing, and for this the administration itself can take a share of the blame. For, whatever the merits of a massive national space effort may be—and it should be recognized that in the context of the Cold War a big space program is inevitable—the fact is that, in selling it to the public and Congress, the administration has been proffering some glittering but tinny arguments.

It has said that the United States must land a man on the moon and return him safely to earth before the end of the decade, but it has not said why