# A Call for Unity

**Science: Growth and Change.** HENRY W. MENARD. Harvard University Press, Cambridge, Mass., 1971. xii, 216 pp., illus. $10.

"Science has grown like a bean sprout, faster and ever faster in a simple exponential expansion for 200 years," Henry Menard writes in the first chapter of this book. In the course of this growth, Menard goes on, the character of the scientific establishment has changed: notably, the proportion of administrators has increased, and the control of science has passed from the scientists themselves into the hands of politicians.

The various subdisciplines of science, he finds, display different patterns of growth, analogous to those of the stable, cyclical, and growth securities traded on the stock exchange. Unwary students who opt for stable fields may not be able to find jobs in their specialties, or if they do may have to labor for 20 to 40 years before rising to positions of academic, professional, or economic power. By contrast, students who train for some emerging subfield may achieve influence while still in their 30's and move into policy-making positions with government or industry.

Regardless of the field a young person may select, fame will be hard to achieve. The best bet, in Menard's view, is to acquire visibility through the mass media, as Cousteau and Heyerdahl have done. Prolific publication increases visibility but does not guarantee the highest honors. Comparison of prolific American writers in earth science shows that the all-time champion, vertebrate paleontologist E. D. Cope with 1395 titles, was elected to the National Academy of Sciences, but neither of the runners-up, C. R. Keyes with 1293 titles and J. A. Cushman with 427, was admitted. Not that sobering information of this kind is expected to discourage publication. "Writing scientific papers is like robbing banks. By the time you get good at it, you tend to be cut off from other activities and it is awfully hard to stop." At times when some field of science is in the doldrums, the practitioners can always turn to writing about their own writings and produce bibliographies—"if fiscal winter comes, can bibliographies be far behind?" Also when scientific fields become dormant their literature deteriorates as concern with style of writing grows, jargon flourishes, and citations grow older.

During periods of dormancy sciences tend to splinter. Menard gives as an example American geology, whose dormant period he identifies as running from 1920 to 1955. Long lag-time for publication in the most prestigious journal led contributors to turn to other outlets. The establishment geologists gave little encouragement to budding new fields, refusing to recognize that a man working on a boat might be one of them. Geochemists then refused to be called geologists, preferring to be known as earth scientists.

Old-line scientific agencies tend to grow inefficient in a way that temporary, problem-oriented agencies do not. Menard argues that Parkinson's law has afflicted even the United States Geological Survey, just as Dutton predicted in 1885, long before this celebrated law was clearly formulated. Nonscientists have invaded the Survey in ever-increasing numbers, and the cost of a page of publication increased eightfold between 1920 and 1965. A temporary agency, by contrast, can dissolve after its problem has been solved, and members of the team can disband, go back to school, and train for another mission. "Life," as the author observes at this juncture, "is full of excitement and meaning." If every scientist were given a new education every 15 years, most of us would be more useful. Quoting Tennyson, Menard concludes that the only reason to be a scientist is "to follow knowledge, like a sinking star,/Beyond the utmost bound of human thought." With this motivation science is not work at all, and the hours seem long when one is not doing it.

To improve the present lot and better secure the future of American science and technology, Menard advocates amalgamating appropriate governmental agencies as one establishment at the departmental level. A new Department of Science and Technology could bring together the present regulatory, research, and granting agencies—here characterized respectively as the dodgers, the doers, and the dispensers. The mission would be to advance a broad range of activities from basic research through development to testing. But the author concludes that such a department stands no better chance of being born now than in 1884, when the National Academy of Sciences recommended consolidating many of the already numerous scientific agencies of the government. What was lacking then, as now, was a constituency with enough muscle to turn the political screwdriver. The job at hand, then, is to create this constituency. Menard offers alternative models: a Guild of Scientists, or a General Union of Technologists and Scientists.

The general aims of the Guild would be to bring prosperity and security to scientists and to bring technology under control. More specifically the Guild would seek to limit the labor supply in order to eliminate unemployment, fight for the special benefits required by workers in rapidly changing fields, and threaten joint action if its demands should not be met. Limiting the labor supply would require curbs on importation and domestic production of scientists. On the home front the Guild could threaten to disqualify graduates from departments whose ratios of faculty to students might fall below certain prescribed quotients. Or examinations for admission to the Guild could be set high enough to exclude all but the desirable number of new members. Other series of examinations, whose severity would vary inversely with the number of jobs, would serve to stem the rising tide of skilled and imaginative technicians who aspire to become scientists. Adverse effects of the brain flood from foreign countries would require congressional action spurred by the Guild's lobbyist. Once established, the Guild could require pay for its apprentice boys, enforce job security, demand time for retraining, and require provisions for early retirement—say at age 40 or 50 for university scientists.

A General Union of Technologists and Scientists—a league of engineers, technicians, and scientists with a potential membership upwards of 2 million—could accomplish everything the Guild could, and more. The threat of strike by this conglomerate would inspire awe, especially if the membership should include those who install, operate, and service computers. By virtue of its size the Union would have the political clout needed to influence elections and even to propel its own into high office.

The book consists of about 184 pages of analysis and 23 of recommendations. The analytical section contains many statistical tables and graphs showing, as examples, growth in the number of scientists, scientific writings, professors and students, earned doctorates, and federal dollars for research. Other graphs analyze the visibility of selected scientific writings as measured by the number and temporal distribution of the citations made to them. The illustrations, and the spirited text in which they are set, tell much about those aspects of science that can be reduced to numbers. The one aspect that is neglected is the attribute of quality, whether of the lives of scientists or of their works. This is not to say that the book is without value judgments. Among other matters Menard identifies a dormant period in geology, deplores paper pollution, and praises the development of the scientific method and the construction of the edifice of science as the greatest group achievement of mankind. But assessments of quality come off second best.

What of the author's judgment that the years from 1920 to 1955 constitute a dormant period in American geology? During these years experimental petrology flourished at the Geophysical Laboratory of the Carnegie Institution, and N. L. Bowen's *Evolution of the Igneous Rocks* appeared in 1928. P. W. Bridgman's experiments with the deformation of rocks under elevated temperatures and pressures continued at Harvard. The worldwide search for petroleum, and all the drilling that went with it, added the dimension of depth to geological maps. Requirements for correlation of strata between boreholes accelerated studies of micropaleontology. Exploration geophysics flourished after the invention of the reflection seismograph in 1921. The monument at Oklahoma City which commemorates the invention of this instrument bears an inscription boasting that the billions of barrels of oil found with the aid of seismic methods have enabled "our nation to pass from a horse and coal economy to an industrial petroleum economy." To sample the quality of publications during this period one might turn to the Professional Paper series of the U.S. Geological Survey. The list includes geochemical investigations of F. W. Clarke and H. S. Washington on the evolution and disintegration of matter and the composition of the earth's crust; studies of paleontology by T. W. Vaughan, E. W. Berry, F. H. Knowlton, and J. B. Reeside; W. H. Bradley's writings on the varves and climates of the Green River Epoch; and works on structural and mining geology by S. F. Emmons, Adolph Knopf, D. F. Hewett, James Gilluly, T. S. Lovering, and T. B. Nolan. Regional studies now regarded as classics of their kind include P. B. King's "Geology of the Marathon Basin" and F. E. Matthes's "Geological History of the Yosemite Valley." Toward the end of the period came the first of a large number of papers on the oceanography, geology, ecology, paleontology, and geophysics of the Marshall Islands. It would be easy to document the proposition that authors of Professional Papers written during the allegedly depressed period of geology won more than their share of awards and election to positions of importance in their professional societies.

The name of micropaleontologist J. A. Cushman appears alongside no less than 15 of the titles in the list mentioned above. His Professional Papers range in vintage from 1920 to 1954. This is interesting in view of the fact that Menard cites Cushman and C. R. Keyes as examples of prolific publishers who didn't make the Academy, presumably because each owned his private printing press. Here again quantities are confused with qualities. Keyes and Cushman were alike only in that each published upwards of 400 papers. Comparing the quality of their publications is like comparing the poetry of Eddie Guest with that of Robert Frost. True, Cushman controlled a private press, but over a long span of years it would appear that he also owned a piece of the Government Printing Office.

Sagging productivity of the Geological Survey during the long drought that began in 1920 is attributed by Menard in large measure to overzealous concern with style and format of writing imposed by the issuance, first in 1909, of the Survey's style manual, *Suggestions to Authors.* Menard hopes that the fourth edition of the *Suggestions* (1935) will be the last. (But my desk copy is labeled fifth edition and dated 1958.) Both the purpose and the effect of the manual are misunderstood. The purpose was to encourage authors to translate their manuscripts into English, which, as H. L. Mencken observed, is the most difficult of foreign languages for Americans. The effect was not to slow the rate of processing manuscripts; if so the brake would have been applied in 1909. Moving a manuscript through the Survey's mill has with some justification been compared to moving a cemetery, but the causes are mental and not manual.

Passing over the debatable propositions that bibliographies are symptoms of decadent science, that computers make bibliographic work easier, that universities lose money on students but make money on research, that it is simple to identify superb students by conventional tests, that graduates in emerging subdisciplines are showered with offers of jobs at prestigious institutions, and that students should avoid fields with lengthy bibliographies, we come to the question of the Union.

The premise on which the call for the Union is based is that scientists and technologists are exploited workers rather than members of an elite. Does this view reflect the self-image of scientists and technologists? One cannot know until the poll is taken, though at least one respondent would suggest that "exploited worker" and "elitist" do not exhaust the taxonomic possibilities. Or, does the premise jibe with the image of scientists in the eyes of the general public? The editors of the Marquis Company thought not when they issued *World Who's Who in Science* in 1968. In his preface to that work, Allen G. Debus, historian of science, noted that prior to the second World War "John Q. Public pictured the scientist as a distant creature who lived in his ivory tower and had no interest in or concern for the needs of mankind." Radio comedians were assured of a laugh by the mere mention of Einstein and the theory of relativity. All this changed after the bombing of Hiroshima. Studies sponsored by the University of Chicago showed that scientists rose in the public esteem from eighth place in 1947 to third place in 1963. Menard rightly deplores what he calls the "warlock" image of science which was evidently involved in this upward leap in public acclaim. But the fact remains that scientists are not publicly regarded as exploited workers, or as exploited warlocks for that matter.

Assuming that all this is irrelevant and that the Union is a reality, could it accomplish the objectives Menard envisions? Could it control the annual input of new labor by imposing sanctions against colleges and universities producing more than their quotas of degree-carrying scientists? Could the Union enforce provisions for early retirement of professors at ages between 40 and 50? Probably not. Blacklisting of institutions by the American Association of University Professors for alleged violations of academic freedom has not been notably effective; at least two institutions exhibited remarkable progress while advertised on the Association's blacklist. American universities will probably not submit to dictations of quotas for graduation by any external agency. And surely the AAUP, whose concern with tenure is exceeded only by its concern for higher salaries, would frown on early retirement. The theme for the March meeting of the Texas Conference of the AAUP was "Collective Bargaining in Texas Colleges and Universities." As the Association passes through the final stages of its evolution from guild to labor union, it will become ever more difficult for more narrowly partisan groups to muscle into its territory.

Even if Menard's Guild or Union were capable of doing all he envisions, would careers in science and engineering become more attractive? Students would be paid to train, and train again. They could expect more gold and fame. Once in the groove they could hope for the bliss that comes of single-minded construction work around the edifice of science. Somehow these lures seem more appropriate to graduates who came off the assembly line in the '30's than to the generation now entering the job market. In any case the expectation of wearing out after one retread and taking very early retirement, which is to say accepting dismissal gracefully, presents a bleak prospect and hardly solves the employment problem for elderly persons in their 50's. At several places in the last chapter the author concedes that some of his prescriptions for maximizing the quality of the profession are not very humanitarian and that comparing the depreciation of persons with the depreciation of equipment is "a terrible thought." These are among the few understatements in an engaging and prophetic exposition of scientism at its operational best.

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## The Quality of Science: Strains and Controls

**Scientific Knowledge and Its Social Problems.** JEROME R. RAVETZ. Oxford University Press, New York, 1971. xii, 450 pp. $16.

Ravetz is concerned with the fundamental question of what motivates scientific work and the persisting conflicts among three classes of goals or objectives: those intrinsic to science and directed toward the advancement of scientific knowledge; the scientist's personal ambitions for fame, advancement, and priority; and the technical and practical goals of the society in which the scientist works. Stated so briefly, this is familiar territory; the nature of scientific knowledge and scientific goals, the nature of the scientist, and the relations of science to the surrounding culture have all been analyzed before by philosophers, historians, sociologists, and practicing scientists. But in the author's judgment the philosophy and the sociology of science are no longer in touch with reality and we therefore need an analysis based on "a new common-sense understanding of science," a point of view which he thinks has been best expressed in W. O. Hagstrom's *The Scientific Community*, C. Wright Mills's *The Sociological Imagination*, and D. S. Greenberg's *The Politics of Pure Science*.

By "science," Ravetz means pure or basic science, not technology or work on practical problems. And he usually means the relatively mature mathematical-experimental fields, not the "immature" social and human fields. Science, in this pure sense, is a "delicate and vulnerable social activity" (p. 72) which involves many fine value judgments and which is guided by the informal controls of scientific leaders and traditions rather than by formal rules and institutions. To protect their work, scientists came to insist that it be separated from social concerns and that its guiding values all be internal to science itself. They recognized that this position was an ideal, for they knew they were not isolated from society and that their work was of long-range and sometimes of fairly immediate benefit to society. However, usefulness was always unpredictable and never to be taken as a guiding value in deciding upon the scientific work to be done. Thus society could be excluded and scientists were freed to work on scientific problems.

To resolve potential conflicts between scientific and personal goals, a very effective means of quality control was developed: the published scientific report whose merit was attested by prior approval of a referee and an editor. The author might be more interested in fame and advancement than in adding to knowledge, but to achieve these personal goals he had to publish meritorious papers in prestigious journals, and that required agreement by referees and editors that his papers contributed significantly to science. Thus editors and referees guarded the intellectual property and integrity of the whole scientific group, while behind them, at the top level, stood honored scientific leaders who set standards, selected editors, and rewarded productive research workers.

This system worked well for science, but not as well for technology, where contributions are to a practical art rather than to knowledge and are often protected by patents or even held secret. If an account is published, it is likely to appear as a staff study not subject to external refereeing. Even further from pure science lie a variety of practical problems which scientists are asked to help solve. The purposes of such work are very different from those of science and are often poorly defined; the problems often lie at least partly in the domain of the immature social sciences; political factors are heavily involved; and quality control is therefore difficult or absent.

It is useful to make these distinctions among scientific, technical, and practical problems, but in real life, as Ravetz points out, the three areas sometimes converge; "science" has become the generic term for all three; neither congressmen nor the public distinguish one from another; and science, in the pure sense, has grown to such a size that its traditional, informal controls are breaking down.

Thus faults and abuses have developed. In the main they are attributed to