3) That each of those massive undertakings, such as space exploration, the control of thermonuclear energy, and high-energy physics, that requires heavy concentrations of scientific and technical manpower and huge expenditures of the nation's wealth be considered by the government within the total context of possible scientific inquiry and possible economic growth;

4) That the government establish priorities and schedules for those undertakings which require highly trained and creative people who are also needed by the universities and by industry, and that it implement those priorities in a manner that does not drain off an excessive portion of the nation's creative talent and highly trained manpower; and

5) That the total national subsidy for research be broadened to create a more reasonable balance among all areas of inquiry, to foster greater economic growth.

At the same time, the universities have some hard thinking to do. While preserving the integrity of their scholars,

1) They must seriously experiment on methods of training more students to high levels of competence, with fewer teachers. That is, they must find ways to increase the productivity of their teachers without debasing the quality of their product.

2) They must make a greater effort to match up the research interests of their scholars with the research needs of the nation, so that more of our scholars, while pursuing their legitimate research interests, can make a greater contribution to the achievement of national objectives.

3) Finally, they must invent new administrative arrangements which will bring all projects involving both basic and developmental research within the framework of the university or a group of universities. In this way national research efforts can benefit from university experience in research management, and from the consultative services of highly competent research people. At the same time, the universities can benefit from those basic research problems that are consistent with the research interests of their faculties and from the part-time services of those involved in the developmental aspects of the project.

What I have described is not an abstract problem. It is a real and serious one that grows more serious every day and could have extremely grave consequences for our country. Yet it is not insoluble. What is required most of all is a maturity in the American people themselves which permits them to take a calm, long-range view of isolated events rather than to react violently and angrily every time the Communists perform a feat before we do. I am not suggesting complacency, but rather a willingness to understand and accept long-range national goals and to realize that, if we possess a strong military deterrent to aggression, ultimate victory will come to the nations with the institutions and economic strength that provide their people with personal freedom and a good and full life. Within such a climate it would be much easier for the government to assess its activities in terms of legitimate national aspirations than of national hysteria. We must grow up quickly, however, for I sincerely fear that if we continue to pursue our present policy for long we shall reap the whirlwind. I believe that we can grow up, and I know that we must do the hard thinking necessary to serve best the goals of our universities and the goals of our nation.

The Coming Changes in American Science

Science today is experiencing strains which are altering its basic character.

Norman W. Storer

Prediction does not necessarily provide for control, but it can be of vital importance in enabling us to compensate for unavoidable events. The forces now impinging upon American science are producing fundamental changes in the scientific community which, whether we approve of them or not, must be known before we can act intelligently in achieving the best possible adjustment to them.

Alvin M. Weinberg's well-known article "Impact of large-scale science on the United States" (*1*) is a perceptive discussion of some of these changes. It appears to stop short, however, of facing the full consequences of science's new position in society. In discussing the possibility that Big Science will "ruin" science, for instance, Weinberg suggests that by "nurturing small-scale excellence as carefully as we lavish gifts on large-scale spectaculars," we may "prevent the contagion from spreading." Needed now is a better picture of what is happening to all of science and how it is happening. To do something about such symptoms as "journalitis, moneyitis, and administratitis," we must understand the deeper changes that are resulting from science's enhanced ability to command support from society and to exert appreciable influence upon policy decisions at the highest levels.

I would like to argue that Weinberg has examined the top of the iceberg very well but has not seen clearly the greater part which is submerged. This greater part is hidden both in the slowness of time, which disguises important trends, and in the implicit assumption that quantitative change is unrelated to qualitative change. His discussion centers upon the consequences of the in-

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creased support of science. To complement his analysis, I examine here some of the other factors which are at work today and cast the problem in a larger framework.

American Science before 1940

Twenty years ago and more science received relatively little support and approbation from the rest of society. Salaries were low, research was done on shoestring budgets, and the prospects of even modest wealth, social position, or community influence were not promising for a young man entering science. Under these conditions, science tended to attract two largely overlapping groups of people—those who were completely committed to research, and those for whom membership in even this low-ranking sector of society represented a rise in social status.

In her study of eminent scientists, all of them men who had entered science before 1940, Anne Roe notes (*2*) that "[after] the discovery of the possibility of doing research . . . absorption in the vocation was so complete as seriously to limit all other activity. . . ." Knapp and Goodrich found that rural backgrounds were overrepresented among their subjects (*3*). For men from rural areas, science provided a way of moving up in life; perhaps this was due to its connection with academic life, but still it attracted them. It should be remembered, too, that the values of rural life, like those of Protestantism (*4*) seem particularly to fit a person for a scientific career. The scientific community, made up generally of these two types of people, shaped itself to accommodate them.

The insularity of their interests was reflected in their condemnation of applied research, the idealization of curiosity, and a single-minded commitment to the development of science. Such values not only expressed these men's personal feelings but served as well to protect science by reinforcing its members' distinction from the rest of society. Strong barriers were erected to keep scientists from succumbing to temptations that would make them desert the community or compromise its integrity.

The picture here is of course exaggerated, but in the main these seem to have been the conditions that existed. When the members of this group were devoted to their work, and when no other rewards for it were available, the primary reward which was available and could be used within the group was professional recognition. This reward, the affirmation by one's colleagues that one has made a contribution to a body of knowledge, was entirely appropriate to science (*5*).

In other fields—law, medicine, engineering—where the practice of one's profession requires interaction with laymen and yields tangible results, professional recognition is important primarily as it signifies ability; it forms the basis of one's reputation and proclaims one's potential for excellent performance in the future. But in science, without regular relationships with nonscientists and often without tangible results, it signifies achievement. Despite increasing tendencies to make recognition important to the scientist as a basis for obtaining further research support, the original importance of recognition was in validating the scientist's own creativity. Thus, particularly in science before World War II, professional recognition was looked upon as an end rather than a means. The importance of priority, especially as commemorated in the practice of eponymy, testifies to this.

In a social setting where science received little support, it became, to a high degree, a closed system, since both research output and its rewards were contained in the same community. Basic research flourishes under such conditions, which ensure progress along all fronts and provide maximum opportunity for heuristic cross-fertilization. With practically no other market for one's work besides one's colleagues and with little outside support for any research, there was no more reason to work in one area than in another except for those reasons inherent in the development of science itself, plus personal predilection. The success of science in a democratic society may be due as much to its being relatively ignored as to the felicitous agreement between the basic values of science and democracy.

That such a community was able to survive in this relatively closed form depended upon its having a relatively small number of members and stringent criteria for membership. Many older scientists today remember nostalgically their professional meetings of the 1930's, when they could identify almost everyone in attendance and knew something of each man's work. The difficulties of going through graduate school then, and of finding employment afterward, meant that these men were automatically selected on the basis of deep devotion to their work. The sharing of such common hardships forged another link in the bonds uniting scientists.

This intimacy within science meant that professional recognition was reasonably easy to gain when one had done good work. It was possible for everyone interested in a given field to follow and evaluate nearly all new research in it—and without such feedback, a scientist would have had difficulty in satisfying himself that he had accomplished what he had set out to do. The moral obligation to provide this feedback, to "keep them honest"—the obligation called "organized skepticism" by Merton (*6*)—was supported by the nature of the reward which perforce had to be important in science; the other side of the coin of professional recognition is professional criticism.

Lack of outside support, then, and relative smallness were the major factors which shaped American science in the decades before World War II. Employing the currency of professional recognition, the community was able to encourage its members to carry out basic research, to report it, and to maintain the system of double-checking its validity. These three elements of science are necessary for the production of new empirical knowledge, but they do not define the character of what is produced.

I have argued elsewhere (*7*) that the traditional values of science are actually rooted in the requirements for continued, adequate allocation of professional recognition rather than in the utilitarian relationship of these values to the goal of science. It follows that as the basic currency changes, these values will be weakened.

Consequences of Increased Support and Growth

I suggest that in American science today this older currency is being gradually replaced by coins minted in the larger society: money, prestige, and power. The new coins will "buy" the three necessary activities (production, dissemination, and evaluation) just as the old ones did, but in themselves these activities do not make up genuine science as we have known it. My argument is that the new coins coming into greater use within science today are having consequences of the utmost seriousness for the fundamental character of science itself.

The new coins are a product of the

larger society; society's increased support of science has made them available for use within science. Scientists are members of this larger society as well as of their professional community, and they are accustomed to the use of these currencies. Because we have Big Science, graduate fellowships in abundance, larger research grants, and more interest in learning scientists' opinions on the great political issues of the day, there is more opportunity to spend these currencies within science. The need for more scientists to administer Big Science and to act as ambassadors to the public in order to maintain the flow of support has been recognized. The need to control the allocation of research funds, resulting in the creation of more and more study sections, has been noted. But one further consequence of the increase in public support must be recognized as well. This is the atmosphere in which new scientists are being trained.

With more money available, it is a rare thing now for a competent young man to have to put himself through graduate school by his own earnings (other than those earned through teaching or research assistantships). He *expects* to be supported, and he may even choose a graduate school on the basis of the financial aid it offers rather than by the quality of its professors. From the beginning, he is entering science with the expectation that it will yield a respectable living. This is a major factor in the gradual replacement of professional recognition by rewards native to the rest of society.

The increasing availability of consulting and advisory work, which carries with it a fair amount of social prestige, is providing scientists with another means of "earning" more of society's coins. Research administration, both formal and informal, as when a man receives a large grant, is increasingly a legitimate career for the scientist. And slowly, the size of a man's research grant is coming to mean more to him and to others than the findings it pays for. These new activities of the scientist —consulting in Washington, becoming an administrator of research, and receiving large grants—are coming to be defined by younger scientists as marks of success in the scientific career. They are easier to come by than a Nobel prize; they are negotiable in the larger society—Gresham's law indeed!

In this way the central importance of professional recognition in the scientific community is being challenged, and the values of science based upon it, which have given shape to American science in the past, are changing as well. Not only is the coinage of science being challenged, it is also becoming more scarce. With the growth of the scientific community, professional recognition is no longer so easily obtainable.

For one thing, the larger number of scientists [the number of doctorates per year in the sciences near quadrupled between 1932 and 1957 (*8*)] has meant a vast increase in the amount of scientific material being published. No one can expect a single article in a given field to be read by anything like the proportion of scientists in that field who formerly read each new article. A senior scientist once remarked to me that the major difference between his day and the present is that young scientists today no longer even hope to master the literature of their fields. And as one's confidence in his command of the literature decreases, he may become more hesitant about proclaiming the fact of another's priority.

Add to this the continuing policy of secrecy in many areas of defense and industrial research and it is obvious that the coin of professional recognition is becoming scarcer. Much of the current concern with information retrieval may be as much related to the problem of getting one's own work read as to that of finding others' work which is pertinent to one's own. The outcry against "publish-or-perish" policies in some parts of academia and elsewhere may be seen in one sense as resistance to making professional recognition a means rather than an end.

Second, the larger number of scientists means that there are relatively fewer opportunities for the old system of controls to operate effectively. Published criticism by a colleague may not be so important to a scientist now, for he can find many other scientists who have not read the criticism or who may not care. With the increased tempo at which science is moving today, such criticism may be lost in the shuffle, or may be thought unnecessary because in the next few months other men will have found the right answer. It may not be forthcoming at all (*9*). While the flow of publication swells, the techniques by which scientific truth was formerly ensured may be more frequently replaced by other means which do not involve the alter ego of professional recognition, professional criticism. The rapidly decreasing lead time between discovery and application may even presage a return to Francis Bacon's criterion: "works themselves . . . as pledges of truth. . . ."

Finally, there are the consequences of a growing tendency to turn to "basic" research to solve specific problems. I know of medical researchers who are now doing "basic research on heart disease" because, as practicing M.D.'s, they were frustrated by the lack of knowledge in this area. Such research is essentially a broadening of the area covered by research that seeks the solution to a practical problem, and it leads to a blurring of the real distinction between basic and applied science. When greater numbers of scientists are concentrating their efforts in the few areas designated "major problems," the audience for research that is genuinely basic in nature—research in the intersitial areas of science which apparently will not have practical consequences—is likely to be diminished. And with less opportunity to gain professional recognition through such work, either as means or as end, there is a natural snowballing of interest in those areas where recognition may be obtained. Thus threatened by a powerful imposter, "basic research *on a problem*," the vital concept of the disinterested quest for knowledge is in danger of being stifled as a legitimate form of inquiry.

In sum, I am suggesting that the new position of science in society has engendered internal conditions which are rapidly altering its entire structure. The two sources of change—increasing support from outside and increasing growth inside—are operating to open wide the older, closed-system scientific community which we may still be assuming, or hoping, will be preserved. I suggest that it will *not* be preserved, and that we must accept this and bend our efforts toward preserving what we can of it in the new situation which is already upon us. An attempt to predict the new shape which American science will take is necessary. After making such a prediction I shall suggest some countermeasures that I think must be undertaken to protect the essential character of our scientific community.

New Structure of American Science

Most subtle, perhaps, but of far-reaching significance, is the decline of the informal atmosphere within science. In a very concrete sense, this change is similar to the sudden growth of a small town during a boom. The older

citizens remember the days when everyone knew each other and when the town's stability was maintained through informal controls. Most of the new people, though, do not know each other personally, nor do they care. The police, welfare department, and city government must take on new and heavier responsibilities. The boundaries between social classes become chasms instead of fences. The pressures of public opinion play a smaller part in the maintenance of an orderly civic life.

The same thing is happening within the scientific community. One of the graduate student's real traumas is his first professional convention, where he first sees just how many people there are in his field. His implicit hope that all these people will someday become friends is shattered. His feelings that he will be a welcome member of a small band are replaced by the fear that he will never be able to make his mark in such a vast company.

Like relationships among people in a metropolis, relationships among scientists will assume a more businesslike character. They will be restricted more and more to the exchange of specific information and will lack the warmth and the assumption of mutual sympathies which was so typical before World War II. Within a given organization this will not be true, but in the same way that his neighborhood and his place of employment are the places where a person finds friends in the city, so will university departments and research centers or divisions come to mark the boundaries within which a scientist may automatically expect to find warm relationships. He no longer can assume that the holder of a degree in his field will respond naturally to overtures of friendship; before he will feel free to make these overtures, more specific credentials will be required.

With this breakdown in the small-town atmosphere of science will come also a greater amount of stratification in the community. There will be fewer and weaker lines of communication between the bottom ranks and those at the top. The top ranks—the scientific elites—will become more isolated at the same time that they assume greater responsibilities. That the oligarchic "government" of science has not yet been openly criticized may be due to its general responsiveness to science as a whole (*10*); but in coming years we may expect more complaints about "dictatorial powers" possessed by the small number of senior scientists who control the allocation of research funds and the access to desirable positions. The only response that can be made to such complaints is to raise the standards by which men are deemed acceptable for such positions and to make sure that these men keep in touch with all sectors of the scientific metropolis.

At the same time that these changes are going on we may expect an increase in the splintering of science into subdivisions. The appearance of more specialized journals, the need to restrict one's training and reading to narrower areas, and the establishment of research organizations focused upon more tightly defined fields all point in this direction. Just when the continuous subdivision of science will stretch the lines of communication to the breaking point cannot be estimated precisely, although the present feeling in some quarters that medical research is developing an ethos different from that of other areas may be a forerunner of such breaks.

It seems likely that, in the future, science will become a federation instead of a community, or even an aggregate of specialties whose unity lies more in their being classified together by the public than in their own feelings of interdependence and mutual support. How much, after all, do civil engineers today feel that they have in common with electronics engineers?

Some Possible Countermeasures

It is obvious that a science characterized by this type of structure will lack many of the things which have facilitated the growth of knowledge through the encouragement of creativity and of genuinely basic research. In addition to trying to make sure that only the most broadly competent and humane men continue to reach positions within science where they will influence the internal distribution of its support from outside, I think special efforts must be made to provide an enclave within this structure where the conditions that foster basic research are maintained.

There must be a rapid increase in the establishment of "career investigatorships" along the lines being explored now by the National Institutes of Health and by some private foundations, where a man is guaranteed a reasonable income with yearly increments for a period of 10 or more years and is left absolutely free to study whatever he wishes. More unrestricted grants to university departments and laboratories must be made—not so much larger grants to some groups as middle-sized grants to many. We must remember that basic research must have enough support to "get off the ground"; without the expectation that a fair number of other scientists will be interested in his work, a man often seems to lack motivation to work in really unexplored areas. The small-scale and random support of a few men to do such research is not enough.

Finally, there must be found ways and means of encouraging "generalists," men who are admittedly jacks-of-all-trades who have chosen breadth rather than depth and who can thereby strengthen the lines of communication among different disciplines. A promising development along this line already is the increasing importance of the computer programmer, a man who by his detailed acquaintance with several fields and through his continuing connections with them is in an excellent position to engage in this sort of cross-fertilization.

I think we must expect to find that basic research will become the tail rather than the dog in coming years. Unless knowledgeable efforts are made now to protect it, we may find it being wagged right off the dog without our being aware of it. Huey Long's famous dictum, "When fascism comes to America, it will come under the guise of anti-fascism," has its parallel in our own situation: "When applied research comes to dominate science, it will come under the guise of basic research." If we do not recognize the coming changes in American science, we shall be poorly equipped to preserve the creative essence of science—science whose purpose is, in Weinberg's words, "the enriching and broadening of human life."

References and Notes

1. A Weinberg, *Science* **134**, 161 (1961).
2. A. Roe, *Psychol. Monogr.* **67**, 49 (1953).
3. R. Knapp and H. Goodrich, *Origins of American Scientists* (Univ. of Chicago Press, Chicago, 1952), pp. 292–293.
4. R. Merton, *Osiris* **4**, 360 (1938).
5. ———, *Am. Sociol. Rev.* **22**, 635 (1957); ———, in *Recognition of Excellence* (Free Press, Glencoe, Ill., 1960), pp. 297–328; H. Shepard, *Phil. Sci.* **23**, 48 (1956).
6. R. Merton, *Social Theory and Social Structure* (Free Press, Glencoe, Ill., rev. ed., 1957), pp. 560–561.
7. N. Storer, *Am. Behavioral Scientist* **4**, 27 (1962).
8. Naval Research Advisory Committee, *Basic Research in the Navy* (Little, Cambridge, Mass., 1959), vol. 2, pp. 36–39.
9. M. K. Hubbert, *Science* **139**, 884 (1963).
10. B. De Jouvenel, in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi* (Free Press, Glencoe, Ill., 1961), pp. 139–140.