ject to an entirely different system of control. These findings suggest that (i) control mechanisms might be relatively stable characters, which tend to be conserved during the evolution of a particular biological group; and (ii) that different control systems governing a given pathway in two biological groups indicate either wide evolutionary divergence or possibly separate evolutionary origins for the pathway in question.

A complex and specialized biochemical pathway might have arisen on several independent occasions in the evolution of a group as ancient as the bacteria. Purely chemical factors severely restrict the play of natural selection in biochemical evolution. For example, if natural selection has to solve the problem of converting the benzene nucleus into aliphatic products that can enter the universal central pathways of cellular metabolism, there are doubtless very few chemically permissible solutions. Accordingly, several different genetic constitutions might each provide favorable evolutionary raw material for achieving the same chemical solution of this physiological problem. If such were the case, the evolutionary end products would appear homologous on the metabolic level, even though the operative enzymes and the corresponding structural genes were nonhomologous.

The development of the relevant set of structural genes is a necessary, but not a sufficient, condition for the evolution of a new biochemical pathway. Physiological integration of the enzymes must follow, and it requires the imposition of a novel pattern of control, with the metabolites of the pathway as effectors. However, not every metabolite needs to be endowed with effector function in order to maintain a well-regulated complement of enzymes (Figs. 2 and 3). A considerable element of choice must occur at this stage in the evolution of a pathway, both with respect to the metabolites which actually acquire effector function, and with respect to the precise manner in which they exercise it. One can readily imagine that the genetic background could influence strongly the choice which is made. Hence, independent evolution of a biochemical pathway in two groups is apt to be mirrored in differences at the level of control systems.

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The Pure-Science Ideal and Democratic Culture

A new scientific ideal in the late 19th century led to continuing conflicts with democratic assumptions.

George H. Daniels

One of the most notable developments within the scientific community in post-Civil War America was a changed image of the scientist and of his role in society. Previously, science had been "sold" to the public in terms of its contributions to important American values—utilitarian, equalitarian, religious—or even as a means of social control, depending upon the speaker's best estimate of his audience. But in the 1870's, for the first time, great numbers of scientific spokesmen began to vocally resent this dependence upon values extraneous to science. The decade, in a word, witnessed the development, as a generally shared ideology, of the notion of science for science's sake. Science was no longer to be pursued as a means of solving some material problem or of illustrating some Biblical text; it was to be pursued simply because the truth—which was what science was thought to be uniquely about—was lovely in itself, and because it was praiseworthy to add what one could to the always developing cathedral of knowledge.

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Historians of art, business, the law, or a great many other subjects will readily recognize this ideal, for in one sense it was the scientific analog of the general fragmentation of life and thought that was occurring in the 1870's. That decade also witnessed, to name only two well-known examples, the origin of "art for art's sake," and of "profit for profit's sake," a notion admirably analyzed by Thomas Mann in his novel *Buddenbrooks*.

In science, fugitive expressions of the sentiment can be found from time to time in the literature of the early 19th century, and it is true that practical values cannot be deduced from the nature of the work American scientists of the first half of the century chose to do. Their labors in science, in terms of fields of interest, were probably indistinguishable from those of the postwar generation (*1*). Yet, the claims of practicality by that earlier generation were no mere rhetoric. Those researches that were not immediately practical no doubt soon would be. Even if they were not useful in a material sense, they would serve a valuable public function by further demonstrating the character and power of God. Scientific education, the earlier generation argued, was necessary because "it would powerfully conduce to benefit the morals"; the government should finance scientific enterprises because of the "pitiful contrast" between American work and the "magnanimous policy of England." An observatory was necessary because one had been established not only in every "petty German principality" but even in the "convict land of Botany Bay"; its chief function would be to establish an American prime meridian, so that the United States could free itself from "degrading and unnecessary dependence on a foreign nation." In other words, a higher practicality underlay the whole of the scientific enterprise, and its justification depended, in some sense or other, upon the manner in which it served the public (*1*).

All of this seems strange, if not downright heretical, to the present generation of scientists. In 1960, the AAAS Committee on Science in the Promotion of Human Welfare deplored the fact that science seemed to be valued more for its uses than for its "fundamental purpose—the free inquiry into nature," and it pointed out that such a misplaced valuation led to pressures which had begun to threaten the integrity of science itself (*2*).

The following year, René Dubos was warning his colleagues that the continued attempt by scientists to justify science only by its worldly products not only compromised the intellectual honesty of the scientific community but actually helped foster among lay people some contempt for science itself. Scientists, he thought, should utterly drop their claim to an ultimate utilitarianism and popularize their work only in terms of its intellectual value. The fact that, after 100 years of effort, such calls to action, and such ominous forebodings, are still current indicates that what Dubos termed a "schizophrenic attitude" (*3*) is still present within the scientific community; while scientists claim among themselves that their primary interest is in the conceptual aspects of their subject, they continue to publicly justify basic research by asserting that it always leads to "useful" results.

Such an attitude would not have been regarded as "schizophrenic" in pre-Civil War America, for the fact is that, from the time of Francis Bacon to the latter part of the 19th century, it was customary to place science in the framework of "useful knowledge"—witness the statement of aims, or even the names, of the early philosophical societies (for example, The American Philosophical Society Held at Philadelphia, for Promoting Useful Knowledge). This was by no means an exclusively American phenomenon; science and "useful knowledge" were interchangeable terms among followers of Bacon everywhere.

Such resentment as that expressed by the AAAS Committee and by Dubos, however, began to appear in America in the late 1860's, became fairly common in the 1870's and 1880's, and by the early 1890's had become a part of the general ideology of science. In this article I discuss the origin of the idea in America and the conditions in American society which militated against the full realization of the ideal and therefore led to the "schizophrenia" deplored by Dubos and others at the present time. A by-product of the discussion is the suggestion that such schizophrenia is inherent in the system and, perhaps, functional for the scientific community.

Everyone who has investigated the subject is aware that some such change took place in the latter half of the century, and everyone is aware that the avowed model of the new generation of scientists was the German University system. Certainly, the contrast between the situation in America and the life American scholars found in Germany was a dramatic one, and certainly their experiences abroad must have fired the ambitions of returning students to remake the American system. No doubt the contrast with Germany, given the ardent nationalism of the times, had an incalculable propaganda value as well. But I think that, on the whole, rather too much has been made of the German experience and too little attention has been paid to changing conditions within this country that could probably have accounted for the same results. I think it can be argued that the reason so many American scientists studied in Germany during the 1870's was that they wished to obtain the kind of professional education they knew was obtainable only there. If this is so, the simple "copying" explanation for the rise of the new ideal is not tenable. One must ask what conditions in the United States made so many travel to Germany in pursuit of an ideal that they already held. Without trying to resolve the enormously complex question, resembling that of whether the chicken or the egg came first, I deal in this article only with indigenous factors.

Rise of the Professional

The new attitude can, I think, be interpreted as an effort to preserve the purity of science in the face of radically new conditions. The first of these conditions in order of importance was the transition from amateur workers to professionals, a change which J. P. Lesley, looking back over his 50 years in science, characterized as follows (*4*):

> Science was then an early morning stroll with sympathetic friends, uncritical and inexpert, to whom suggestions were as good as gospel truths. Then such a reunion as this tonight was a sort of picnic-party at some picturesque place on the shore of the unknown, hilarious and convivial.
>
> All that has passed away. The sun of science now rides high in heaven, and floods the earth with hot and dusty light. What was once play has turned to serious toil. . . . The few and early risers have become a multitude.

Only a few years earlier it had seemed reasonable for one distinguished researcher to decline payment for his services to the government on the grounds that his time was already devoted to natural history, and for another to protest the necessity of entering into a written agreement with a governmental committee because he did not like the idea of "bargaining like a handyman for intellectual labor" (*5*). Charles Sanders Peirce put the case still more strongly in explaining to his brother why he did not think he would become a scientist. "Now a savan is supposed to be doing that which he most delights to do," Peirce said, "and he does it as the impulse of his rational nature directs and simply in order to satisfy his intellectual desires." Peirce's conclusion was that it would be unreasonable for people to pay a man for simply doing what he liked, and that science was therefore meant for leisure (*6*).

Although it was made only 10 years later, the statement by Benjamin Apthorp Gould, one of the earliest to entirely adopt the new ideal, is conceptually removed from that of Peirce by a full generation. The scientist, so Gould said (*7*), is compelled "to earn his bread independently of his vocation, that is to say, by work other than scientific research." A more radical opposition of attitudes is difficult to imagine. Where for Torrey, LeConte, and Peirce, research was something *made* for leisure, and where they had even resisted payment for it, for Gould and those who followed him, research was the true "vocation" of the scientist. The necessity of earning one's living by other means, a generally unquestioned *given* of the previous generation, was now regarded as one of the more objectionable features of American society.

The new professional orientation came at a time when the position and the prospects of the American scientist were probably at a high point. Possibilities for employment had increased tremendously in the past few decades. To cite only a few of many possible indices, by 1880 there were approximately 400 colleges and universities employing at least one scientist each, and by 1882 there were 144 observatories in the country, all presumably providing facilities, if not always paid employment (*8*). By the 1880's, science had become of such relevance that the possible establishment of a federal Department of Science was being seriously discussed, and both state and federal governments were spending increasingly large sums on scientific research (*9*).

A number of organizations dedicated to the advancement of science and the interest of scientists had proved to be viable concerns by the 1870's. The AAAS, which since its organization in 1847 had emphasized the *advancement* rather than the *diffusion* of science (*10*), was, thanks to the revolution in transportation, annually drawing hundreds to its meetings and, with this base of support, acted as an effective pressure group to promote governmental science. On another level, the National Academy of Sciences, apparently a permanent fixture on the American scene after its reorganization under the leadership of Joseph Henry between 1867 and 1872, was evolving into an honorary organization for recognizing and furthering "abstract science" (*11*). In terms of jobs, recognition, and the existence of organizations to speak for their interests, American scientists had never had it so good. They were therefore in a position to make demands which would have been utopian 20 years earlier.

Earlier Justifications of Science

It is not likely that such a point of view as that of the pure scientist could have become widely held in America until there was no longer any necessity of making an issue of the utility of science. Science had long since passed so completely beyond the common understanding that there was little chance for meaningful contact with the public on an intellectual level (*12*). In the absence of contact on this level, professionals of the preceding generation had devoted a major part of their efforts to persuading an uncomprehending public that it was in its interest to support the work of the scientific community because the results would be useful. Living in a society with built-in pressures upon the individual to do useful work, scientists had had to greatly exaggerate the utility of their work simply because its utility was questionable at that time. It is natural, given the democratic assumptions, that those who contributed little should claim much, and that they should be especially vocal in their insistence that they, too, conformed to the values of their society. One looks in vain for actual applications of theoretical science, as opposed to products of mechanical ingenuity, before the middle of the 19th century. By the last quarter of the century such applications were so obvious that it was no longer necessary to make a point of them. The contributions of geology to mining technology were common knowledge; the chemical, chemical-process, and electrical industries depended directly upon 19th-century scientific discoveries; scientists seemed to be firmly entrenched in the government and by common consent were recognized as useful public servants. Henry A. Rowland, in 1883, could safely refuse to dignify "telegraphs, electric lights, and such conveniences, by the name of science," because the public well understood that these mysterious conveniences were the products of science, and their morning newspaper, with its news from the far corners of the world, served as a daily reminder of the power of science (*13*). The admission by Andrew Carnegie, master of production and thus an embodiment of the ideal of useful work, that he would prefer to lose his buildings and his plants than the services of his scientists (*14*), symbolizes the new public role of the scientist. The popularizers, engineers, and applied scientists, whom Rowland despised (*13*), had already done their work so well that Rowland and others like him could be spared the necessity of doing it. In short, the claim of utility had to be insistently made in the earlier part of the century simply because it was not even approximately true; the coming of the fact, in a number of areas, in the latter part of the century made formal enunciation unnecessary, and thus for the first time made it possible for a new ideal to be developed.

As one of the major earlier forms of justification had become unnecessary, the other had become virtually impossible. After the brief flurry caused by the evolution controversy, the old conflict between science and religion had, at least on the higher intellectual level, been settled. From the 1870's on, the things that were science's were generally rendered unto science, and the same was true of religion. Theologians had generally found it advisable to surrender all of nature to science, reserving for themselves a domain of purposes and values outside of nature (*15*). A statement by Henry Drummond (*16*), prominent British clergyman who adhered to both evolution and Christianity, is illustrative of what came to replace the older view of natural theology that science and religion, if each were "true," must agree on all points:

> Nature in Genesis has no link with geology, seeks none and needs none: man has no link with biology, and misses none. What he really needs and really misses—for he can get it nowhere else—Genesis gives him; it links Nature and man with their Maker. And this is the one high sense in which Genesis can be said to be scientific.

Speaking for the scientists, J. Lawrence Smith told his fellow members of the American Association for the Advancement of Science (*17*) that the old task of "reconciling" science and religion, which had been a major preoccupation of the preceding generation, was a "mischievous work," and that in his opinion there was "less connection between science and religion than there is between jurisprudence and astronomy." The sooner this was understood, the better Smith thought it would be for both. Although Smith's statement *does* seem to be a declaration of independence on the part of science, such statements during that period should always be taken in context. It is clear, in the remainder of Smith's address, that he was fully as concerned for religion, if the old association were continued, as he was for science. Change was in the very nature of science, he said; that which was accepted as scientific truth by one generation was often rejected by another. But religion should not change; it should be concerned with eternal verities like faith, hope, and love. The best way to preserve them, as many were coming to recognize, was not to tie them to *any* scientific doctrine.

If the Copernican hypothesis, Newtonian mechanics, and uniformitarian geology had not been enough, the coming of evolution had finally demonstrated that no amount of "reinterpretation" could any longer hide the fact that the Bible could not be read as a scientific document. One important result of the evolution controversy was to free religion from a dependence on science—a dependence which, since the 17th century, had resulted in one retreat after another—and therefore to free scientists, qua scientists, of any religious obligations.

Although a great many exceptions remained, and some still do, the older natural theology was virtually dead by the mid-1870's—and it died by mutual agreement on the part of scientists and theologians. A source of frequent conflict had thus disappeared, but so had the most common justification for abstract science. If "reconciliation" was no longer desirable, then neither was a "scientific demonstration" of the power and character of God. There was therefore no longer any serious possibility of an appeal to the religious element in American culture.

Even though the position of the scientist had improved by the 1870's, one should not conclude that the new professional orientation was accepted without question. Quite the contrary, for the pure-science ideal carried with it many implications which were bound to precipitate a clash with the public or with public representatives. The most important were those relating to the two institutions where the old scientific ideal had enjoyed its greatest success—education and the government.

Education for Pure Science

The proliferation of colleges, technical schools, and other institutions during the middle of the 19th century had at first been hailed with joy by the scientific community. Scientists had been, in fact, in the forefront of such expansion, and for obvious reasons. Virtually all of the colleges provided employment for at least one professor of science, and they were generally successful in expanding the scientific content of the curriculum. The multiplication of such schools had been one of the major factors in the rising status of the scientific community. But the professor of science in the college was regarded primarily as a *teacher*, and whatever amount of scientific investigation he undertook remained in the tradition of the amateur—an avocation which one took up after working hours, which was in no way thought to be related to his main business. As David Starr Jordan said, about 1870, he had begun to prepare himself for *two* unrelated professions—naturalist and college professor (*18*). When scientists of Jordan's generation began to develop a new ideal of the professor they found that the very success of the educational movement had now become the greatest stumbling block. The new ideal can be expressed in the words of Charles S. Minot, president of the ultraprofessional American Society of Naturalists. The qualifications for a university professor, he observed (*19*), were two in number: (i) the ability to carry on original researches himself, and (ii) the ability to train others to carry out original work. A Texas scientist (*20*) stated the ideal in an even more extreme form in the very title of his address before the Texas Academy of Science: "Original Research and Creative Authorship the Essence of University Teaching."

Wherever one turns in the scientific literature of the last quarter of the century, he finds such statements. Gone, apparently forever, was the balance of teaching, research, and application that had been the highest ideal of early professors like Joseph Henry. It had been replaced now by purely professional considerations, such as contribution to science and preparation for the reproduction of a self-sustaining scientific community. But the college system of the period was totally unable to accommodate such an ideal. In 1876, to take one example, there were over 500 institutions claiming the name of college or university, most of which had sprung up overnight, responding to no particular need other than the divisiveness of an expansive democracy and a proliferation of religious sects. They were, therefore, for the most part, small, impoverished, and certainly unable to provide adequate research facilities for their professor of all the sciences. Neither was the curriculum considered at all adequate by the new generation. Of nearly 400 such institutions which T. C. Mendenhall (*21*) surveyed in 1882, almost all offered some instruction in physics. Of these, only six met his standards of adequate preparation for graduate study, and less than 30 met what he termed *minimal* standards, meaning only that some laboratory instruction was offered in addition to lectures.

It had seemed natural to applaud the growth in numbers of the colleges, astronomical observatories, and other institutions in the days when the lone researcher, working with simple equipment and few expenses, was able to contribute significantly to scientific knowledge. But even by the 1870's that day was plainly on its way out. Frank W. Clarke in an address to the AAAS in 1878 (*22*) spoke of the urgent need for a large endowed laboratory for research, a place where the fundamental data of chemistry and physics could be accurately established "without more than casual reference to particular industrial questions or to theories." The necessary apparatus for determining physical constants was too expensive for individuals to own, he said, and the work could be successfully done only by a group of trained specialists together with assistants, computers, and other staff members.

Five years later, Rowland (*13*), was deploring the "folly" of filling the country with telescopes and calling them observatories; a few "first class institutions" would be preferable to the multitude of inferior ones then existing. The same was true of the colleges and universities; the total wealth of the hundreds of such institutions in the country, he calculated, would be sufficient for one great university, four smaller ones, and perhaps 26 colleges. Some work could still be done on a shoestring, he concluded, "but not the highest kind." The situation was so bad that Clarke (*23*) had concluded by 1876 that the college system was itself as great a drawback to American scientific growth as any other factor. Clarke, later a distinguished government chemist, could only suggest that perhaps the best way to deal with the colleges was to tax most of them out of existence.

Pure science did not fare much better in terms of the curricula of the colleges. Although scientists in the earlier part of the century had been in the forefront of the drive for "practical education"—including a de-emphasis of the classics, a re-shaping of the colleges to better prepare students for "life," and establishment of the practically oriented technical schools—they now found that the fruit of their labor was actually destructive of the new professional ideal. For the same argument that threatened to cast the classics out of education could be used to attack the equally "useless" abstract science. As C. Hart Merriam bluntly put the matter (*24*), pure science, which serves only the specialist, was not suited to the college curriculum. The tendency of the times, he said, "is to render undergraduate courses more practical, so that the knowledge acquired may be useful later in life." Not 10 percent of the biological instruction favored by the professionals could possibly be of any value in later life to anyone not destined to become a specialist, so Merriam said, and certainly no more than 1 percent of those taught biology became specialists. According to a quick calculation, this meant that 90 percent of the professional-type training was useless to 99 percent of the people subjected to it. That Merriam's point of view is not extinct is illustrated by the following statement made in October of 1966 by a reader of *Science* (*25*): "And for the future housewife, or engineer, or shoe clerk, which is apt to be more useful: an understanding of the Krebs cycle or the knowledge that the maggots in the garbage can will turn into flies?"

A story told by Ira Remsen (*26*) of his early days at Johns Hopkins is as illustrative as any of the new orientation of professionals toward their subject. A young man had come to consult with Remsen about the possibility of studying with him:

> He regarded me with some curiosity, and after a time he ventured to say: "Professor, I should like to enter the Johns Hopkins University, but your work doesn't seem to be practical, and others are saying the same thing." I acknowledged the truth of the observation, and added that I feared it was an incurable case, that there was, in fact, no prospect of my work ever becoming practical in the sense in which I supposed he used that much abused word.

B. A. Gould had warned his colleagues as early as 1869 that their earlier enthusiasm for curriculum changes had been misplaced and had now become a positive danger. The crusade against classical culture "bode no good to science," he observed (*7*). "The champions in this crusade occupy simply the utilitarian ground, and their alleged advocacy of science is in fact scarcely more than an advocacy of the useful arts. . . . The crusade is not in behalf of this or that form of intellectual progress; it is against such intellectual culture as has not some tangible end, capable of being represented in dollars, or finding expression in some form of physical well-being."

In view of the perceived inadequacy of the technical curriculum, with its emphasis upon the "practical," it comes as no surprise occasionally to find scientists lined up with classicists at the more practically oriented schools in defense of a traditional curriculum. The dissension became so great in at least one case, at the Kansas State Agricultural College, that Benjamin F. Mudge, an authority on Kansas paleontology, was fired, along with two classicists, for opposing the introduction of what the regents termed "science" into the curriculum. The regents accused Mudge of teaching "the abstract sciences in a loose and unprofitable way," and wished scientific instruction to concentrate on "practical agriculture and the mechanic arts" (*27*).

The younger generation of professionals in the post-Civil War decades had moved completely away from both the concept of practical education and the old liberal-arts notion of giving the masses the smattering of scientific knowledge necessary for them to "appreciate" nature; instead, they had come to concentrate on the problem of preparing entrants for professional work. Those who discussed teaching problems in public quite generally chose to consider their subject "more especially from the standpoint of the preparation for professional occupation." That is to say, the main function of the college was now seen to be that of providing facilities to enable the profession to reproduce itself. The ideal could be realized only at Johns Hopkins and a very few other places, but, nevertheless, it was a generally accepted standard by which one's success as a teacher was to be measured. Only a few natural scientists, usually those who still had one foot in the social sciences, continued to think in terms of the older ideal of science in a liberal education. The astronomer Simon Newcomb, who wrote an economics text on the side, is one such exception, and his point of view—"a wide and liberal training in the scientific spirit and the scientific method"—was that generally held by social scientists about the turn of the century (*28*).

Science and Democratic Politics

In the same way that professional aspirations foundered on the democratic educational assumptions of their time, they ran into difficulties with the other great patron of research, the federal government. Once again, the problem was compounded by the fact that an earlier argument had been, by any ordinary standards, astonishingly successful. By the end of the 19th century the American federal government had become possibly the world's greatest supporter of scientific research. But in keeping with the egalitarian democratic context, every single advance in government support had been made on the basis of some presumed public purpose of a practical character that the research would serve. That a democratic government should become a "patron" of a privileged group of pure scientists was unthinkable. This orientation, however, came increasingly under fire with the rise of the pure-science ideal. Alexander Winchell, in 1886, could see no reason for praising government science, for he could not think of a single case where any public provision had been made for pure research. Furthermore, he pointed with alarm to the fact that popular and legislative prejudice against pure research, and the corresponding effort to limit the activities of governmental science, was apparently increasing (*29*). There is no doubt that Winchell was correct in his assessment of the situation. The establishment of the Allison Commission (a congressional committee appointed to study the administration and organization of scientific agencies), the cuts in appropriations for scientific bureaus in the 1880's, in some cases the actual firing of research-oriented scientists—all testify to a growing public suspicion of government science. However, I do not think he was correct in ascribing the situation to a growing anti-intellectualism on the part of the American people—or, for that matter, to any change on the part of people in general.

Scientists had successfully gained support for a great many enterprises on the grounds that they would provide material returns for the money expended. Despite Winchell's disparaging remarks, the Coast Survey *had* originally been defended by scientists in terms of its benefits to "harbors, commerce and national defense," and the Geological Survey, like the numerous state surveys, *had* been advanced as a means of discovering the material wealth of the land. What had happened had been entirely predictable. State legislators, for example, who had been prevailed upon to finance geological surveys in the expectation of immediate returns in mineral wealth, had become disturbed when such results were not forthcoming—as was usually the case in the early days. Time after time, legislators refused to publish the scientific results of a survey, insisting upon publishing only the "economic geology." If government work was being done in the public interest, as the scientists had claimed it was, then that work should be restricted to enterprises clearly in the public interest, went the not entirely unreasonable argument. Scientists found, more and more, that the work that really needed to be done, from their point of view, did not meet the requirements of being clearly in the public interest. The paleontological work of the Geological Survey and the Survey's efforts to reconstruct the geological history of the continent were under continual attack, as was all but the most obviously practical astronomical work of the Naval Observatory; the agricultural experiment stations were expected to confine their efforts to reaching "an empirical solution of one problem after another" to the neglect of a rational, scientific approach (*30*, p. 693; *31*). As one critic put it (*32*), the scientific bureaus were established by the United States in order to make surveys of the country, and the government's only authorized scientific investigation was in connection with making surveys possible. "It is a step in a radically new direction to introduce the prosecution of investigations *per se*."

Although historians do not customarily recognize the fact, it was indeed a change in direction that scientists of that period were arguing for, a change necessitated by the near-exhaustion of the *scientific* possibilities of the old-fashioned survey, the prototype of government science in the early 19th century. Once again, in order to understand the discontent of the scientists, one must refer to the changed orientation on their part, and observe that the new orientation imposed requirements that were in utter conflict with prevailing democratic assumptions about the political process. It was the pure-science ideal that now made the formerly satisfactory situation seem unsatisfactory, and it was the new demands upon government introduced by the ideal that led to the frequent clashes with legislators, administrators, and the public.

The pure-science ideal demands that science be as thoroughly separated from the political as it is from the religious or utilitarian. Democratic politics demands that *no* expenditure of public funds be separated from political control, or, to state it another way, that no power be granted without responsibility, which always includes accountability. With such diametrically opposed assumptions, a conflict is inevitable. As even so staunch a supporter of pure research as the editor of *Science* admitted (*33*), government scientific work was "far removed from that public criticism which is so conducive to efficiency in other branches of the service."

Furthermore, there was broad agreement among representatives of the scientific community that their work *must be* so removed; the scientific establishment, it was often said, must be kept "safe from political spoilsmen" (*34*). In part, the effort to avoid political involvements was a matter of self-defense. For example, in 1888, when John Wesley Powell threatened to encroach upon the political question of distribution of public lands, the Congress, spurred on by Western interests, not only turned against Powell and his irrigation survey but turned upon scientific agencies apparently unrelated to Powell's activities. The irrigation survey was allowed to languish in the Department of Agriculture, and drastic cuts were made in the budgets of the Coast Survey, the Lighthouse Commission, the Smithsonian Institution, and the Naval Observatory (*35*). A few such examples of congressional wrath would surely inculcate caution.

But there are other factors inherent in the notion of pure science which made conflict inevitable. For pure-scientific work to be conducted, two things were held to be absolutely necessary: (i) long-range planning and (ii) flexibility. The first required that scientists be free of the limitations on tenure and the annual appropriations that were customary for others connected with the government. Control of scientific work by the military was especially criticized because it seemed to make impossible the kinds of investigation that require long-range planning (*36*). The second requirement meant that detailed instructions should not come with an appropriation and that a rigid accounting should not be demanded. To state the problem in the manner that most often occurred to critics, the needs of pure science demanded an undemocratic suspension of the rules on behalf of a select group (*37*, p. 151). The ideal appropriation for a scientific bureau would be granted as a lump sum, marked simply "for research," and its expenditure would be entrusted to a professional scientist in charge of the bureau. If cuts in the appropriation must be made, as the editors of *Science* remarked in connection with the Geological Survey, it should be an overall cut, not a paring of some specified operation. In this way, maximum freedom could be retained for moving in any direction that seemed advisable to the director (*38*). Such an administrative head as that desired by the professional scientists would naturally have to be a trained professional himself, not a representative of the public whose job was to keep a watchful eye on the public interest. It is the latter, of course, which is the traditional view of the department head in a democratic framework. The notion, for example, demands that the Secretary of the Navy be a civilian, not too closely identified with Naval interests; it still calls forth outraged protests when a Union leader is named Secretary of Labor; and it was a major reason for public protests against the developing scientific establishment. As one lay correspondent of *Science* put it (*39*) in commending the appointment of a nonscientist, Colonel Coleman, as Commissioner of Agriculture, "Technical experts . . . are, as a rule, those gentlemen who have bees in their bonnets." Unlike the scientific administrator, the reader said, Colonel Coleman was without a pet hobby. The editors, naturally, had a sharp rejoinder (*40*). The worst thing about a nonscientist administrator, they said, was that "any broad, well-planned policy is practically out of the question."

To many, the current notion of accountability seemed both unseemly and ridiculous when applied to the highly specialized, technical work being done by government bureaus. How, Alexander Agassiz asked rhetorically (*41*), could "a clerk in the auditor's department" be expected to pass upon the work of heads of bureaus? More than the dignity of bureau chiefs, however, was thought to be at stake. It was generally held (*42*) that "science cannot be carried forward by prescribing too definitely the task of scientific men." If there were to be real progress, the methods of freedom would have to be employed, not those of "petty regulation." As Gould (*43*) explained to Henry W. Bellows in regard to the charge that he had violated the instructions of his superiors on the Sanitary Commission, he had found it impossible to predict in advance the expense of any investigation. "The best computers make mistakes," he said. "One research frequently entails others, & quite as often it conducts to purely negative results, which have nothing tangible to represent them." Powell, in his interrogation before the Allison Commission, spoke to the same point (*30*, p. 645):

> Now, the work of the Survey is of such a character that it cannot be fully specified and planned in advance, from the fact that to a large extent it is research for facts and principles not yet discovered. . . . Only the general object in view and the general line of investigation to be pursued can be designated. . . .

It goes without saying that Powell was technically correct, but it could only appear to critics, like the editors of the *Popular Science Monthly*, who usually took the public point of view, that an effort was being made to establish "scientific pontiffs" in Washington, and that scientists were merely trying to gain some private advantage. The same rules should be applied to the scientific bureau as to any other, the editors thought (*44*):

> An unsupervised and irresponsible scientific department at Washington would be run in the interests of its sharpest managers, would be filled with sinecures, give the least results at the grandest expense, while the results would be aggravated by the sense of exemption from criticism.

The claim that scientific work should be exempt from public criticism—an exemption that had been considered a prime necessity by the scientific community even before the development of the pure-science ideal—was an outrage to the sense of justice of an uncomprehending public that, through both cultural heritage and training, opposed the concept of elites of any kind. The novelist, so one critic said, does not demand that he be read only by novelists. Painters, sculptors, and historians make no such demand. Why, he asked (*37*, p. 139), should only the scientist insist that no one except "his own brethren" be allowed to form an opinion of him and of his work? This was a particularly unacceptable position, so a correspondent of *Science* urged (*45*), when public funds committed to allegedly public purposes were involved. Any public matter, including the Geological Survey, "is a proper subject of criticism, by any citizen."

Conclusion

These early experiences of pure scientists will have an unmistakable ring of familiarity to anyone familiar with the current situation. Charles Sanders Peirce, with characteristic insight, had stated the fundamental dilemma of the pure scientist operating within a democratic framework. How can one ask the public to provide support, much less facilities, for the intellectual gratification of one select group? A part of the answer, of course, is simply that one cannot. As long as a group is dependent upon public support it must seek some means of contact with the values of the enveloping society, and the moment it does this it departs in some measure from the ideal purity. The schizophrenic attitude described by Dubos therefore became a professional necessity as soon as the new ideal was adopted. Since the time of Gould, scientists have been able to tell each other that the man who based science's claim to support on grounds of immediate practical utility was "no loyal follower and true friend of science" and, at the same time, to trust that the popularizers and technicians would convey a different message to the public. On the whole, they have not been disappointed in their expectation, and there has been little need for them to go beyond the standard formula: Utility is not to be a test of scientific work, but all knowledge will ultimately prove useful. Since the continued existence of scientists in this society depends upon the believability of that vague claim, there is little likelihood that the schizophrenia will disappear.

References and Notes

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5. John L. LeConte to Charles Wilkes, 7 Mar. 1847, and John Torrey to John L. LeConte, 9 Apr. 1849 (*LeConte Papers*, American Philosophical Society, Philadelphia).
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26. See F. H. Getman, "The life of Ira Remsen," *J. Chem. Educ.* **1940**, 122 (1940).
27. The quotations are from a letter from J. Copley, Kansas State Regent, to the *Lawrence Journal*, reprinted in the Ellsworth (Kansas) *Reporter*, 19 Feb. 1874. See also the Ellsworth *Reporter*, 12 Feb. 1874. Copies of these publications were supplied to me by Professor Robert R. Dykstra of the University of New Mexico.
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32. L. S., *ibid.* **7**, 12 (1886).
33. "The consolidation of the government scientific work," *ibid.* **5**, 336 (1885).
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36. See, for example, *Science* **2**, 415 (1883) for comments on the deficiencies of the Naval Observatory; *ibid.* **6**, 397 (1885) for comments on the weather service under the Army.
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