Physicists and the revolt against science in the 1930's

The Depression provoked an attack upon science, which was aggravated by a highly publicized dispute between Millikan and Compton, exposing difficulties for physics in the United States that remain facts of life today.

Daniel J. Kevles

To most people the main link between the abstrusities of science and the wonders of modern life is technology, seen as tangible machines that spew forth goods or ease the burden of labor. After the crash of 1929 many Americans began to ask whether chemistry produced more than consumers could absorb, whether machines destroyed more jobs than they created. Going beyond earlier humanist critiques, thoughtful citizens wondered whether science was not responsible, at least in part, for the end of the miracle, for the failure of machine civilization. In this context, an emotionally charged dispute between two physicists about the origin of cosmic rays became a focal point in the public reaction against science. The principals in this unprecedented and highly publicized debate were two Nobel laureates, Arthur Holly Compton and Robert A. Millikan. The repercussions of this dispute and the attendant revolt against science exposed difficulties in physics in the US that remain with us to the present day.

Retrenchment

Amid the Depression support for research was plummeting everywhere. In Congress economizers slashed the budgets of all the Federal scientific agencies an average of 12.5 percent. The Bureau of Standards, its estimates reduced in committee, then on the floor, where the senatorial leadership drove through cut after cut with hardly a dissent, emerged with an appropriation almost 26 percent below the 1931 level. Around the country, state monies allocated for research fell sharply at such scientific centers as the Universities of California, Wisconsin and Michigan. In Illinois, where the legislature sliced the University budget considerably, the governor reduced it even further. In the private sector, at Stanford and MIT, campaigns for new capital funds collapsed; a sizable part of the Cal Tech and all the Cornell endowments for research were wiped out, and no private university escaped a decline in its investment income and severe loss in the book value of its portfolio. Washington University in St Louis had so little money to equip its new physics building that the third floor of the structure was turned into a children's skating rink. The annual income of the richly endowed Carnegie Institution of Washington fell by some $1 000 000. At Bell Laboratories, General Electric and numerous other industrial research establishments, retrenchment was the order of the day.

Yet the difficulties of science went beyond the direct economic effects of the crash. The United States had of course suffered previous industrial depressions; none had ever struck after a decade in which science had been as widely hailed for making possible the miracle of modern society.

The prominent economic critic Stuart Chase told the Women's City Club of New York that the advent of "talkies" had thrown ten thousand movie-house musicians out of work. William Green, president of the American Federation of Labor, explained how during the 1920's the castoffs of technology could find jobs in new industries and emphasized that there were no new industries to absorb them now. President Herbert Hoover's own authoritative Committee on Recent Social Trends, publishing its conclusions at the beginning of 1933, ominously warned: "Unless there is a speeding up of social invention or a slowing down of mechanical invention, grave maladjustments are certain to result."

Exactly, cried Howard Scott, who, preaching his own peculiar brand of social invention, made "Technocracy" the new word of 1932. Disheveled beneath his broad-brimmed felt hat and baggy leather engineer's coat, Scott looked like the technological prophet he claimed to be. In Scott's view, endlessly increasing productivity would lead to permanent unemployment and debt, even to the collapse of the capitalist system—unless the system was drastically revised. The economy, Scott argued, had to be run by a central planning mechanism that would match consumption to output. To make the mechanism work, the value of goods would have to be measured not by price but by the quantity of energy required for their production. To keep the system independent of political machinations, it would have to be run by technicians. Under Technocracy, Scott claimed, Americans could have a standard of living ten times as high as in 1929 and on a mere 16-hour workweek.

By fall 1932 Scott had become the sought-after luncheon and dinner guest of industrialists and bankers, editors and future New Dealers; numerous Americans were ready to say with Manchester Boddy, the editor of the *Daily Illustrated News* in Los Angeles: "Out of Technocracy, we believe, will come a movement as important as the great American revolution of 1776." But reporters were unable to verify Scott's biographical details, and his ideas scarcely stood the test of close, dispassionate scrutiny. His energy theory of value ignored the plight of agriculture, omitted the problems of service industries and vastly oversimplified the problem of technological unemployment.

The New York Times jeered at Technocracy's "comic finality," but liberal reformers, anxious over the economic

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Robert Millikan in lively conversation with Madame Curie while Arthur Compton looks on thoughtfully, at a 1931 conference in Rome on nuclear physics. Millikan had aired anew his theories on cosmic rays the year before; Compton was to prove him wrong in a highly publicized dispute.

impact of technology, could still agree with Chase that the question was "not whether Scott has lived in Greenwich Village or Lung Tun Pen, but what his figures show." Whatever the figures actually showed, observers noted that one cause of unemployment cited frequently by Americans was the displacement of men by machines. Dean William E. Wickenden of the Case School of Applied Science caught the meaning in the brief popularity of the Technocratic movement:

"John Doe isn't quite so cock-sure as he used to be that all this science is a good thing. This business of getting more bread with less sweat is all right in a way, but when it begins to destroy jobs, to produce more than folks can buy and to make your wife's relatives dependent on you for a living, it is getting a little too thick."

The humanist critique

In the 1920's humanists had roundly attacked the ascendancy of science. Now, drawing fresh energy from the economic disaster of the depression, they renewed their critique. Quick with I-told-you-so's, the modernist Protestant and Catholic press reemphasized how mistaken Man had been to put so much faith in the methods of science. As the liberal minister John Haynes Holmes explained: "We can see, without much difficulty, what is the matter. Science is wonderful, but also terrible—and terrible for the reason that it has no values. . . . Science itself is neutral." Religious writers called upon the Nation to turn away from scientism and go back to God, or at least to Christian ethics. Orthodox or modern, secular or religious, various humanists revived the proposal for a moratorium on scientific research first advanced in 1927 by the English bishop of Ripon; in the early years of the depression the scheme won even more attention from the press and the pulpit than it had in the twenties. Arguing for the idea, G.K. Chesterton typically summarized the trouble: "There is nothing wrong with electricity; nothing is wrong except that modern man is not a god who holds the thunderbolts but a savage who is struck by lightning."

Nothing was wrong with scientists either, except that the *New Republic* wanted to know why they should be accepted as "authority on all social questions." Scientists might apply the scientific method with masterful dispassion in their professional work; when it came to emotional issues like economics and war, the noted British popularizer J. W. N. Sullivan told readers of the *Atlantic*, they were "not conspicuous for their detachment and fair-mindedness." By the early 1930's journalists and preachers, not to mention Arthur Compton, soon to become a general chairman of the Layman's Missionary Movement, were celebrating anew the epistemological agnosticism of physics. In the preceding decade commentators had applauded the scientist because he professed to know so little; they now seemed to doubt him because, by his recent public acknowledgment, there was so little he could claim to know.

Academic humanists found their guide in the son of a theologian, the new educational leader Robert M. Hutchins, who had returned from the wartime ambulance service to win a Phi Beta Kappa key at Yale, become secretary of the University at 24, dean of the Law School at 28, and, in 1929, at age 30, president of the University of Chicago. If the Nation was in "despair," he told the university convocation in 1933, it was because "the keys which were to open the gates of heaven have let us into a larger but more oppressive prison house. We think those keys were science and the free intelligence of Man. They have failed us." An enemy of narrow, reductionist specializations, Hutchins decided that students ought to be taught a fundamental core of academic knowledge and moral values.

Doubters wondered how Hutchins was going to persuade the Chicago faculty to agree on just what fundamental principles Chicago students ought to learn. But Hutchins possessed remarkable self-confidence—"Gentlemen," he once responded to a glowing introduction, "I have never heard praise . . . so lavishly, so extravagantly expressed—nor so richly deserved"—and at Yale he had managed to revolutionize the teaching of law, leavening the curriculum with courses in economics and psychology. Now he brashly turned the Chicago curriculum upside down, abolishing grades and examinations, substituting "intellectual" mastery for course requirements and prohibiting specialization until the Junior year.

Christian Century took enthusiastic note of Hutchins's pronouncements and summarily announced that the depression had fused the long-standing discontent of the humanists into what appeared—and what many scientists certainly took—to be a full-scale "revolt against science."

The great cosmic-ray debate

Despite this revolt, Robert A. Millikan remained serene about a few things, in particular his cosmic-ray theories. Millikan was sure that cosmic rays must consist of photons rather than charged particles and that these cosmic photons were clustered in three energy bands. Each band, he argued, could result from the energy radiated when various charged particles united to form the nucleus of an atom. Millikan's three bands happened to contain in turn the characteristic energies for helium, oxygen and silicon, so he theorized that cosmic rays were produced by the creation of photons when four hydrogen atoms fused in interstellar space to form helium, or sixteen to form oxygen, or twenty-eight to form silicon. Because these three elements were among the most abundant in the Universe, it seemed reasonable to Millikan that their formation should account for the bulk of cosmic rays.

To many physicists Millikan's theory seemed a house of dubious hypotheses. His conclusion that the rays were clustered in three energy bands was specious. Most difficult to accept was the idea of the spontaneous fusion of numerous hydrogen atoms in interstellar space. Millikan tried to explain away the kinetic difficulties. He unhesitatingly popularized his hypothesis, calling cosmic rays the "birth-cries" of atoms. The phrase quickly achieved public notoriety. So did Millikan's religiously flavored inference that the creation of the elements was going on continually throughout the Universe, and in the process saving it from the heat death to which, in the view of various interpreters of the second law of thermodynamics, it was doomed. In Millikan's hands, cosmic rays turned into fresh evidence that the Creator was "continually on his job."

In December 1930 Millikan aired his confidence from the presidential podium of the American Association for the Advancement of Science. William L. Laurence, the young science reporter from *The New York Times*, was in the audience. An enterprising and intellectual graduate of Harvard, Laurence had already written in the Sunday supplements how Millikan had done battle with the "dreadful Second Law of Thermodynamics." Now Laurence filed what was evidently an equally enticing story. Back in New York the night editors read of Millikan's theology and wired Laurence for more copy. The next morning Laurence's story was page-one news, and Millikan's address had been published in full, taking up six columns, an amount of space usually reserved only for speeches by the President of the United States. By New Year's Day 1931 newspaper readers across the country could know that, to use Laurence's slightly inaccurate rendition of Millikan's reassuring phrase, the Creator was "still on the job."

But serious doubt hung over the key postulate of Millikan's cosmology, that cosmic rays consisted entirely of photons instead of charged particles: If they were charged particles, the magnetic field of the Earth would distribute them unevenly around the globe and the intensity of cosmic rays would vary with latitude. Millikan had never found such a latitude effect; hence his inference that cosmic rays were photons. But other physicists had begun noticing a latitude effect. In 1931, determined to settle the question, Compton launched a worldwide survey to test conclusively whether the intensity of the radiation varied around the Earth.

Compton divided the globe into nine regions and assigned work to some sixty collaborators in half a dozen countries. In 1932 Compton himself traveled about 50 000 miles in search of the latitude effect, visiting five continents, crossing the equator five times, ranging from as far south as Dunedin, New Zealand to as far north as the Arctic, exploring down at sea level and high in the mountains. The sheer scope of the effort compelled public interest. Then in September 1932, fresh from the Arctic, Compton announced that cosmic rays were definitely charged particles and that Millikan was wrong.

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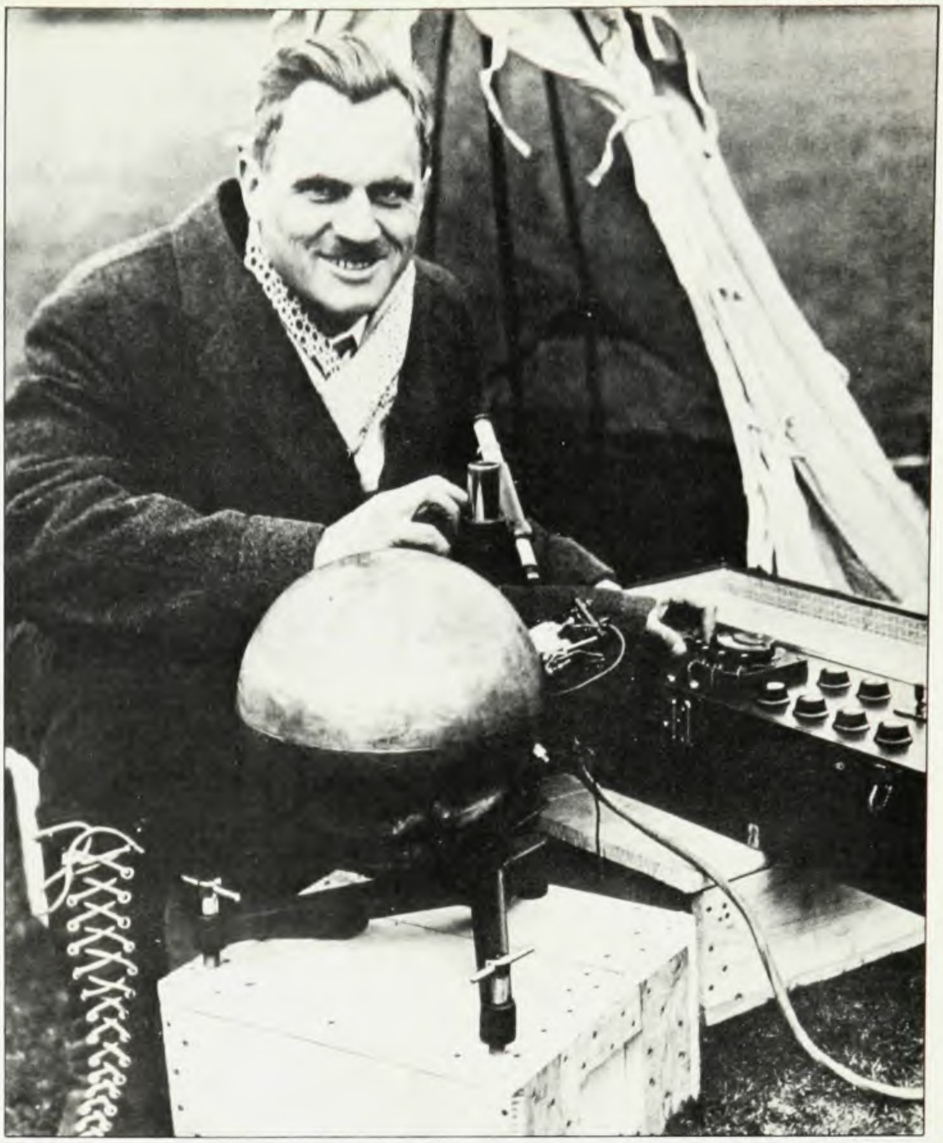

Compton and his electrometer. Millikan's cosmology hinged on the postulate that cosmic rays were photons; these, unaffected by the Earth's magnetic field, would be distributed evenly in latitude. Compton traveled from New Zealand to the Arctic to demonstrate a latitude effect.

Withholding public comment, Millikan went off early in the fall of 1932 on his own expedition to the Arctic Circle. He also sent Henry Victor Neher, a young Cal Tech PhD, on a voyage to South America to look for a variation of intensity in that geographical direction. Late in November Neher cabled from Mollendo, Peru that he had found "no change" with latitude. Neither had Millikan himself, who said so to reporters. By now the press was not so much wondering which of the two scientists was right as gloating that one of the two Nobel Prize winners was wrong. *The New York Times* permitted itself the wisecrack: "It is decidedly a comedown, after being invited to contemplate the wondrous glory . . . of the new physics, to be reminded by the creator himself that of course we cannot say with confidence that we really know what we are talking about."

When Millikan and Compton were scheduled to participate in a symposium on cosmic rays at the Christmas 1932 meetings of the American Association for the Advancement of Science in Atlantic City, a knowledgeable young cosmic-ray physicist predicted that it would be "hot . . . at the Compton–Millikan debate, for Millikan has a chip on his shoulder and Compton is . . . [ready] to knock it off." Millikan went at Compton with a vehemence that reminded Laurence of the days when "learned men clashed over the number of angels that could dance on the point of a needle." Compton, who disliked the "exaggerated emphasis" that the newspapers were giving the dispute, presented a dispassionate analysis of why the bulk of cosmic rays must be charged particles. Bristling at the press for emphasizing the controversial, Millikan fueled the debate even further by adducing all the evidence at his command that cosmic rays were photons. At the end of the session Laurence tried to get the two Nobel laureates to shake hands. Millikan testily refused. Relations between the two physicists, Laurence reported, were

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Millikan with his assistant Henry Victor Neher in their Cal Tech laboratory. After Compton announced that cosmic rays were definitely charged particles, Millikan made his own expedition to the Arctic and sent Neher to South America. Neher at first found no change in cosmic-ray intensity with latitude, but later realized that his electroscope had failed to work properly.

marked by "obvious coolness."

Millikan may have been irritated because at Atlantic City he received an unsettling report from Victor Neher. Neher had not detected a latitude effect on his way south because his electroscope had failed to work properly while passing through the area where he would have noticed a dip in intensity. The electroscope had not failed on the way back north. Arriving in Atlantic City, Neher bumped into Richard C. Tolman, professor of physical chemistry at Cal Tech. "Did you find a latitude effect?" Tolman asked in his soft manner.

"Yes," Neher replied.

"Tch, tch, tch," Tolman chirped, shaking his head.

By February 1933 Millikan had admitted the latitude effect; he soon said no more about the birth cries of atoms, and left it up to the clergy to decide whether the Creator was still on the job. When Compton proposed to air a fresh version of the dispute in 1936, Millikan persuaded him against it, not the least because, Millikan warned, "the public ... will simply look upon the whole performance as a dog-fight between two Nobel prize men, and this doesn't help anybody."

When the dispute of 1932 reached its height, some journalists pronounced it distressing. For decades, the comments went, laymen had been accustomed to think of the scientist as hardheaded, factual, cautious, accurate and, as such, worth listening to even when he advanced notions about Nature and the World contrary to common sense. Now here were two Nobel-Prize winners displaying all the passion and fallibility of ordinary men. The Millikan-Compton spectacle may have added another human dimension to the knowledgeable public's idea of scientists, but coincidentally it also reinforced the revolt against science. Paul R. Heyl, a thoughtful staff member at the Bureau of Standards, summarized the reason: The unsettled condition of modern physical theory had "sufficiently penetrated the nonscientific world to produce a mingled state of wonder and bewilderment, suggestive of those earlier days when men began to doubt the authority and infallibility of the Church."

Science and social purpose

Neither doubts nor faith troubled Frederick J. Schlink, a mechanical engineer and former staff member of the Bureau of Standards, who had coauthored a widely read indictment of the food, drug and cosmetic industries, cofounded Consumers' Research Inc and was emerging as one of the country's best known consumer advocates. The public, Schlink knew, tended to believe that "Science and Research ... were uncorruptible and unpurchasable, that they must in the nature of things always and everywhere, serve the common weal." The public, he added, was decidedly "mistaken."

Reformers of the day, whether categorical Marxists, pragmatic liberals or worried humanists, found the trouble in the economic relationships of science, especially its dependence upon private wealth and industry. Schlink himself charged, accurately, that the Bureau of Standards tested numerous products for governmental purchasers, but refused to release this valuable data to the public. It even failed to warn consumers against items it found to be faulty or injurious. Indeed, Schlink asserted, all the Federal scientific agencies were "little more than handy consulting or guidance services to business enterprise." Other critics attacked industrial corporations themselves for treating science as just another weapon in the war for profits. No major industry was exempt from suspicion, and no corporation was more suspect than the giant that controlled some 85 percent of the telephone service in the United States.

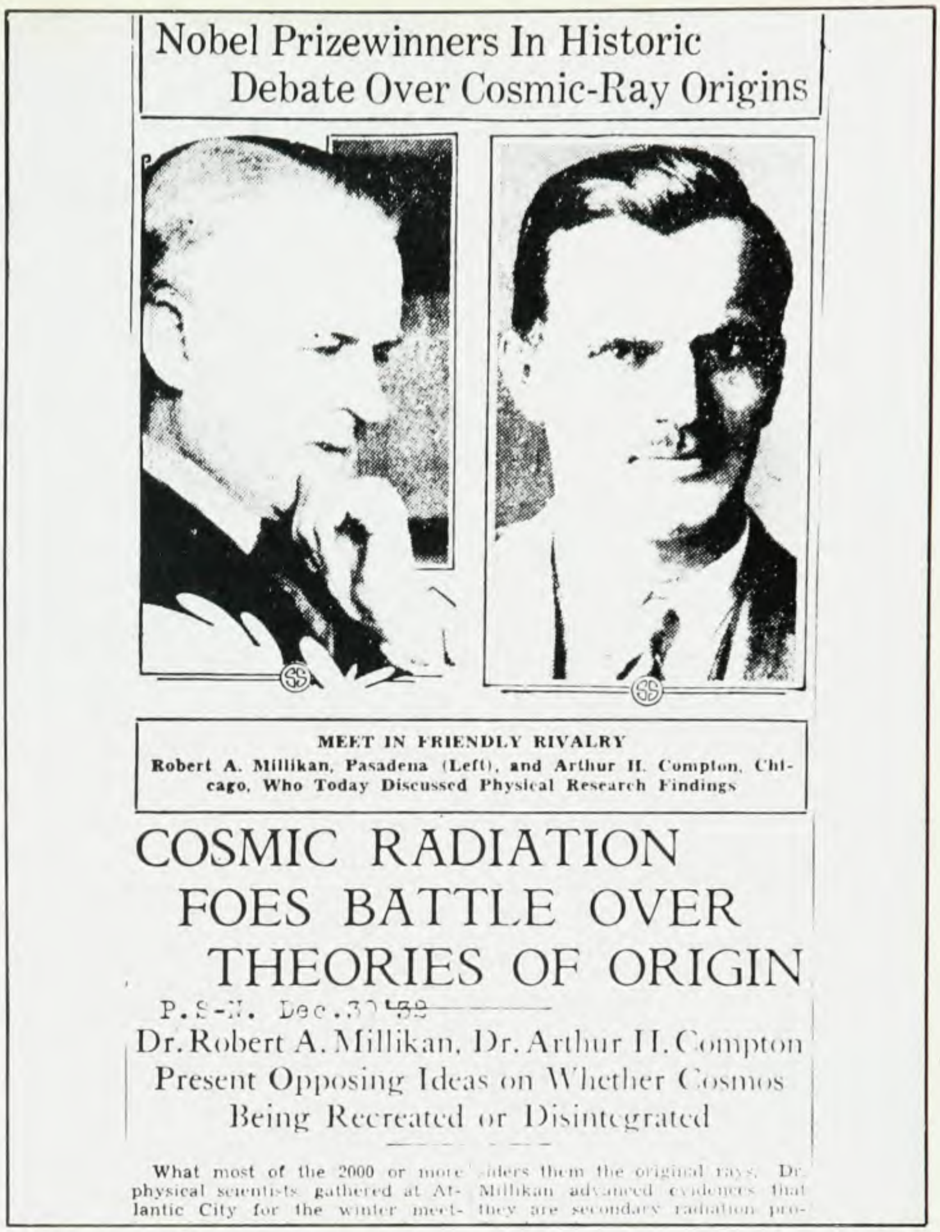
By the late 1920's the AT&T applied-research program had given the public the convenience of dial telephones. But the conversion to the new system, which the company accelerated in the early years of the Depression, was said to be costing thousands of central-station operators their jobs. Moreover, despite the gain in efficiency as well as the general decline in prices, AT&T neither reduced its rates nor failed to pay its regular $9 dividend. Telephone charges, Congressman Joseph Patrick Monaghan of Butte, Montana declared on the floor of the House, were in many instances "excessive and extortionate." AT&T was also said to be writing off against the telephone business such Bell Laboratory costs as the development of movie sound equipment, which had nothing to do with telephones. Early in 1935 the Congress, led by Chairman Sam Rayburn of the Committee on Interstate and Foreign Commerce, authorized the new Federal Communications Commission to launch a full-scale investigation of the telephone company, including its overall policies toward research and development.

According to the Commission's report, AT&T had patronized research in part to protect its telephone operations against intrusion from any technologically innovative competitor; for this reason it had aimed especially to gain control of key patents in radio. The company had also charged telephone subscribers for the cost of developing nontelephonic equipment, and it did not pay them any return on their forced investment, in the form of reduced rates, out of the profits on the new items. The commissioners applauded the Bell System for developing so many marvels of modern communications. But whatever AT&T's commitment to better service, Noobar R. Danielian, an economist and FCC staff member, flatly concluded in a widely praised book: "The most significant consideration in research, acquisition of patents, and pooling arrangements, has been the attainment and preservation of monopoly control in the principal fields of operation."

Even before Congress voted the investigation, John B. Matthews, a vice-president of Consumers' Research, director of the League for Industrial Democracy and a devout Marxist, declared AT&T guilty of having bought Millikan's loyalty with a $3 000 000 gift to Cal Tech. In point of fact, AT&T had given no such money to Cal Tech, and though Millikan himself had been on a $500 annual consultant's retainer since 1913, he had voluntarily terminated the arrangement in 1931. The liberal press generally dismissed Matthews's indictment for the shrill piece of demagogy it was. All the same, the Federal Trade Commission itself had independently concluded that public utility corporations, by subsidizing university departments of engineering with money for fellowships and research, aimed for more than a technological corporate benefit. If the public utilities were to win college graduates over to their economic point of view, Mervin H. Aylesworth, the director of the National Electric Light Association, had told a utilities group, they had first to win over the college professor. "Once in a while it will pay you to take such [a man], getting five or six hundred or a thousand dollars a year, and give him a retainer of one or two hundred dollars per year for the privilege of letting you study and consult with him."

Whatever industry's seemingly venal designs upon individual academics, reform-minded critics worried still more about what the Marxist Benjamin Stolberg, a frequent contributor to *The Nation*, called capitalism's general "degradation of learning." Liberals might not agree with Stolberg that every arm of American culture was controlled by an "impersonal, institutionalized, monopolistic plutocracy, cheap, vulgar, and empty"; like dissidents at the turn of the century, they did fear for the intellectual independence of the universities, especially private universities dependent upon wealthy donors. More important, some recognized that the issue had taken on a new and more subtle dimension since the days when the wife of railroad magnate Leland Stanford could dictatorially, and successfully, demand the ouster of the reformer Edward A. Ross. Now that the philanthropic foundations had assumed a major financial role in American higher education, just a few fabulously wealthy institutions could shape the objectives of a sizable fraction of American scholarship.

A statistical assessment of that point came from Eduard C. Lindemann, a contributing editor of the *New Republic*, staff member of the New York School of Social Work, and the director of the WPA Department of Community Organization for Leisure. Lindemann had studied a hundred foundations—another two hundred refused to cooperate—distilled his findings into a brief, densely written compendium, *Wealth and Culture*, and interpreted his results at the invitation of the *New Republic* editors. "While some critics believe . . . American foundations represent a gigantic conspiracy on the side of reaction," he had found "no evidence" to that effect. But Lindemann did stress



Nobel Prizewinners In Historic Debate Over Cosmic-Ray Origins

MEET IN FRIENDLY RIVALRY
Robert A. Millikan, Pasadena (Left), and Arthur H. Compton, Chicago, Who Today Discussed Physical Research Findings

COSMIC RADIATION FOES BATTLE OVER THEORIES OF ORIGIN

P.S-N. Dec.30 '32

Dr. Robert A. Millikan, Dr. Arthur H. Compton Present Opposing Ideas on Whether Cosmos Being Recreated or Disintegrated

What most of the 2000 or more physical scientists gathered at Atlantic City for the winter meet- ...siders them the original rays, Dr. Millikan advanced evidences that they are secondary radiation pro-

The Pasadena Star–News for 30 December 1932 emphasized the cosmological implications of the "battle" between Millikan and Compton. From the Archives, California Institute of Technology.

that the legal control of the major foundations rested almost entirely with financially successful citizens of six eastern commercial states. "These accumulations of vested wealth," Lindemann went so far as to say, "cannot exert anything save a conservative influence in relation to American life."

The historian Charles Beard, in a review of Lindemann's book, extended the argument. The advance of science and technology, he noted, explaining an idea then widely current among reform-minded thinkers, produced a "cultural lag," a mismatch between the reality of material circumstances and the web of prevailing social practices and beliefs. The question was whether the philanthropic giants would—or could—help America catch up with herself. Beard, the veteran reformer, doubted it: "If a foundation throws its weight on the side of public or collective interests . . ., its trustees will be brought under the fire of [the] private interests adversely affected." Beard, a sensitive humanist, was no less dubious: The crisis of the day required "the assertion of moral values," and the foundations were inclined to support only those noncontroversial subjects that could be treated with "scientific assurance."

Frederick P. Keppel, the head of the Carnegie Corporation of New York, attested that foundations had improperly forced the techniques of the natural sciences into the social sciences and the humanities; he also declared that the foundations had "over-stimulated certain fields" and "spoiled certain individuals." True enough, in the 1920's the Rockefeller Foundation had handsomely sustained the National Research Council postdoctoral fellowships. And the Rockefeller General Education Board had single-handedly enlarged the funding of academic science by some $19 000 000, about three times what it had awarded the humanities and some six times what the total endowment for science in the United States had been at the turn of the century. Almost two thirds of the Board's total grants for science went to just eight institutions—Cal Tech, Princeton, Cornell, Vanderbilt, Harvard, Stanford, Rochester and Chicago.

Yet if the foundations had stimulated such natural sciences as physics, the physicists on their own part had been influential and prestigious supplicants for support; they had helped shape the programs of the foundations. And whatever their conservatism, their eagerness to fund the exploration of the atom had derived in no small measure from purely professional considerations. The field was intellectually exciting and the high-status object of interest among physicists around the world. The reformist critics failed to recognize that, at least in science, if the choice of academic research subjects hinged at all upon the propensity of philanthropic foundations to fund noncontroversial studies, it also depended upon criteria internal to the various professional disciplines.

All the same, while the reformers may have misunderstood the influence of such considerations in setting the balance of academic endeavor, the misunderstanding made their basic indictment no less telling. Even if the logic of the scientific discipline itself determined a professor's choice of research topic, the topic still did not necessarily serve any immediate social purpose.

Examining a cosmic-ray detector at Fort Sam Houston, Texas are Neher, Lieutenant Colonel Walter E. Prosser, General Johnson Hagood, Dozier Lee and Millikan. The photo was taken 4 August 1935.

A shift in Rockefeller policy

In the early 1930's reformers of different stripes were uniting in a common demand—that the pursuit of science itself had to be reshaped to fit some moral definition of the Nation's pressing social and economic needs. Marxists, sure that science had been tied to the apron strings of industry, insisted that it be used primarily in the interest of socialist reconstruction. Liberals, especially liberal humanists, emphasized simply that it should be governed, as Jacques Barzun said, "by choices arrived at on a humanistic basis." Whatever the ideological persuasion, reformers agreed that science had to be freed from the demands of profit-making and from the whims of private wealth. Some were prone to ask with Grosvenor Atterbury, the well known architect, town planner and pioneer in prefabricated housing: What in science should really interest us most today? Not the verification of Einstein's theory of relativity, Atterbury declaimed. In the present state of affairs, all that seemed "brilliantly useless, especially when you consider the millions who cannot afford decent homes because none of our great minds has ever been focused on the basic everyday problem of human shelter. . . . With a small amount of such brains as are now focused on the speed with which the neutron penetrates the nucleus of the atom . . . the cost of the poor man's housing today could be cut in half."

The need for socially purposeful science often occupied the thoughts of Warren Weaver, who in the early 1930's became director of natural sciences at the Rockefeller Foundation. Weaver had grown up in Madison, Wisconsin, attending church on Sundays and sometimes playing with the sons of Robert LaFollette, the progressive governor of the state. His father, a prosperous but stormy and restless druggist, often traveled; his stepmother was a schoolteacher and a warmly sensitive comfort. At the University of Wisconsin Weaver developed an engaging social presence, prompted in part by his courtship of Mary Hemenway of Junction City, Kansas. In 1920, married, a veteran of the wartime military and of a few years at the nascent California Institute of Technology, Weaver settled back in Madison. Finishing his PhD in mathematics, he became a popular teacher, chairman of the department, and the coauthor of an authoritative textbook on electromagnetic theory with Max Mason. It was Mason, recently appointed president of the Rockefeller Foundation, who brought Weaver to his new post in New York, where the first order of business was to help plan the dispensation of the Rockefeller philanthropic millions over the next decade.

Because of the collapse of the stock market, the Rockefeller headquarters was retrenching, but in choosing the kind of retrenchment to make in the natural-sciences program, Weaver was guided by a group of convictions. The sciences in general had come quite far in the United States, he was sure, far enough no longer to need major capital investment, either in facilities or fellowships, from the Rockefeller Foundation. The Foundation ought now to concentrate its resources on a few selected fields, as opposed to ordinary disciplines, of scientific interest. The choice of these fields, Weaver's argument continued, should be governed by two chief criteria. The first was ripeness for significant intellectual development. The second for Weaver, with his human sensitivity, moral convictions of a religious modernist and loyalty to the progressivism of his friends the La Follettes, was the likelihood that the field would contribute to the "welfare of mankind."

"The welfare of mankind," Weaver told his fellow Rockefeller officials, "depends in a vital way on Man's understanding of himself and his physical environment. Science has made magnificent progress in the analysis and control of inanimate forces, but science has not made equal advances in the more delicate, more difficult and more important problem of the analysis and control of animate forces." Weaver believed that certain fields of biology, especially those likely to exploit physics and chemistry, were ripe for major advance. Combining his sense of scientific opportunity with his reformist convictions, Weaver recommended that the Rockefeller philanthropies give major financial support to selected fields that dealt directly with Man. "The past fifty or one hundred years," he summarized his case in 1933, "have seen a marvelous development of physics and chemistry, but hope for the future of mankind depends in a basic way on the development in the next fifty years of a new biology and a new

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The dispute settled with Millikan's admission of the existence of the latitude effect, relations between the two Nobel Prize winners improved again. They are shaking hands in Millikan's office.

psychology."

Mason, who was notably interested in the physiological basis of mental illness, was all for Weaver's proposal. So were the Rockefeller trustees, especially those who had never been happy with the 1920's emphasis on the sciences. Besides, during the Depression social considerations that complemented Weaver's colored the outlook of many trustees, particularly Raymond B. Fosdick, who was Harry Emerson Fosdick's younger brother, a bookish lawyer, a Princeton classmate of Norman Thomas, a veteran of settlement-house work with Lillian Wald, and a devoted Wilsonian.

In 1919, while in London as Under Secretary General of the League of Nations, Fosdick got to know Frederick Soddy, the British physical chemist who would soon win the Nobel Prize and was already an outspoken social critic. Though his arguments were somewhat muddled, Soddy's essential points were worrisomely clear: Material want made war, and technology made war terrible. If Man did not soon learn how to direct science to the creation of an economically more equitable world, science, especially science that commanded the power in the atom, would soon destroy mankind. Indelibly troubled by Soddy's forecast, Raymond Fosdick kept pondering the social ramifications of science while practicing law in New York. In 1929 he commented in a book of gracefully written essays, *The Old Savage in the New Civilization:* "This divergence between the natural sciences and the social sciences, between machinery and control, between the kingdom of this world and the kingdom of the spirit—this is where the hazard lies. Science has exposed the paleolithic savage, masquerading in modern dress, to a sudden shift of environment which threatens to unbalance his brain."

Now Fosdick and his fellow trustees endorsed the key elements of Weaver's program, and Mason officially announced that the principal support of the Foundation would go to special areas of the biological and, reflecting Fosdick's influence, the social sciences. Physics and chemistry would be eligible for support only insofar as they related to biological problems. In 1936 Fosdick, the new president of both the Foundation and the General Education Board, summarized the rationale of the departure. "Uneasiness and even alarm are growing as the belief gains ground that the contributions of the physical sciences have outstripped Man's capacity to absorb them... There can be but little question," he added, as though echoing Charles Beard, "that a serious lag has developed between our rapid scientific advance and our stationary ethical development."

The shift in Rockefeller policy substantially enlarged the financial burdens of physical science in the private universities. The Foundation quickly reduced the money it had been making available for the National Research Council fellowships and stipulated that fully half the remaining awards had to go to the biological sciences. At the end of the 1920's the Foundation had pledged the Council \$100 000 a year for the support of academic scientific projects; in 1934 the commitment was terminated with a reduced final payment. Both the Foundation and the General Education Board, cutting their direct subventions to university science, cut those to the physical sciences the most. Not even Millikan could pry funds for cosmic-ray research out of the Rockefeller philanthropies, while at the same time Thomas Hunt Morgan, the Cal Tech Nobel-Prize biologist, had no trouble winning ample help for research in genetics.

The difficulties of physics

Philanthropic and private institutions aside, in 1933 William Wallace Campbell, now president of the National Academy of Sciences, lamented of the state universities: "The attitude of many, perhaps nearly all of the legislatures toward research at public expense may fairly be described as unsympathetic and, in some cases . . ., as severely hostile." In Washington, DC earlier that year, Congressman William Stafford, a lameduck Republican from Milwaukee, Wisconsin who had learned a good deal about the Bureau of Standards during his two terms in the House, argued that it could stand a further reduction in its budget; Stafford saw no reason why the public ought to subsidize work for a few "favored industries." Congress cut the Bureau's appropriation by about another 5 percent and, for good measure, established the requirement that industry would henceforth have to pay for industrial tests.

After the inauguration of Franklin D. Roosevelt, it was rumored that the new President, who had acquired sweeping budgetary powers under the Economy Act of the previous year, intended to lop off even more funds slated for scientific research. A storm of protest from farm groups kept the reductions in the various agricultural agencies down to a minimum. But no dissent could save the Bureau of Standards from the fiscal ax of Budget Director Lewis Douglas. By the end of the summer the new administration had told the Bureau that it could spend no more that some 66 percent of its congressional appropriation. In August a Bureau physicist bitterly reported to an academic friend: "Research work here is badly crippled."

So was the employment situation in science everywhere. By the summer of 1933 the Bureau of Standards had furloughed more than three hundred members—almost half—of its technical staff, and every federal scientist feared for his job. By the same year, General Electric had fired some 50 percent and AT&T almost 40 percent of their laboratory personnel. In the universities salaries were cut, research and teaching assistants let go; if there were no wholesale firings of senior staff members, few faculty without tenure could look forward to a secure professional future. Professors had to scramble to get their students jobs. With the number of National Research Council fellowships reduced by 1933 to 165 and by 1936 to 49, it was next to impossible to gain one of the awards, especially for physicists. As early as 1931 Samuel Goudsmit reported that the spring meeting of The American Physical Society looked "much more like an employment agency than a scientific gathering." By 1935, the job situation for fresh PhD's, Edwin C. Kemble, professor of theoretical physics at Harvard, told a colleague in Europe, resembled a "nightmare."

As the historian Charles Weiner observed in his article, "Physics in the Great Depression" (PHYSICS TODAY, October 1970, page 31), the nightmare did ease. The partial recovery of the economy tended to aid recovery in physics. The demand for physicists increased in business, notably in the electrical and communications fields, metallurgy, mining, textiles, aeronautics and petroleum. Ernest O. Lawrence, a colossal machine builder in a Nation that revered the builders of colossal machines, infused investments in cyclotrons with a certain romantic glow, and nuclear physics found additional appeal in the prospects of harnessing nuclear energy in the future and providing medical benefits through radioactive isotopes in the present. And then came World War II and the Cold War, which catapulted American physics into an era of unprecedented prosperity and prestige.

Yet the 1930's recovery and the postwar prosperity only papered over the difficulties that physics—and indeed the physical sciences generally—suffered in the early years of the depression. The difficulties had originated in the conflict between technical authority and humane values, in the public's ambivalence towards the mixed blessings of urban technological society, in the tension between the scientist's faith in the ultimate value of science done for its own sake and the public's eagerness to see research yield a relatively rapid socioeconomic dividend. The more physics became identified in the twentieth century with industrial—and then military—power, the more did the difficulties become inherent in the pursuit of the discipline in the American democracy. So after a golden quarter century following World War II, they reemerged in the late 1960's and early 1970's to plague the enterprise. And although the virulence of that latter-day revolt against science may have disappeared, the issues raised in the revolt of the 1930's remain facts of life with which American physics will have to contend into the indefinite future.

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Most of this article was extracted from Daniel J. Kevles, The Physicists: The History of a Scientific Community in Modern America, *which was published by Alfred A. Knopf in January. The discussion is based on an examination of published materials, including scientific and popular periodicals, as well as unpublished manuscripts; full references are given in the book.* □