

ALTERNATIVE VISIONS:
FRAGMENTATION BEHIND THE SCIENTISTIC FRONT

The main inclination ... was to break away somehow from the prevailing paradigms in the social sciences, poor imitations, mostly, of misunderstood physics, and to adapt those sciences to the immediate peculiarities of their supposed subject matter, the human way of being in the world. The aim was and ... still is, not just to measure, correlate, systematize, and settle, but to formulate, clarify, appraise, and understand.

—Clifford Geertz, anthropologist and long-time leader of the Institute for Advanced Study's School of Social Science, 2001¹

In a 1983 essay, Henry Riecken, former NSF social science division leader, commented on the persistent power of scientistic commitments:

The policies guiding the selection of research to be supported reflect a view of social science that is epistemologically and methodologically congruent with the position of the physical and biological sciences. In this sense, the influence of NSF has nurtured a science that is positivistic, empirical (as social scientists use this term), quantitative, analytic, value-neutral, and fundamental or basic in orientation. ... By and large, for successful NSF grantees, "social science research" means experiments, field studies, or the quantitative analysis of archival data to test an hypothesis about some basic social, economic, political, or other behavioral process.²

Toward the decade's end, yet another NSF-commissioned report from the National Academy of Sciences reaffirmed the dominance of that viewpoint, starting on the very first page: the "proximate goal of the behavioral and social sciences is to discover, describe, and explain behavioral and social phenomena in accord with the canons of scientific logic and methods."³

Many social scientists could be happy, for they embraced that position as their own, or at least found it useful when it came to matters of status,

resources, and influence. Riecken himself observed that the unity-of-science viewpoint, in which the natural sciences were often taken as the gold standard, remained the “dominant form of contemporary, academic social science.”⁴

Yet by the 1980s, opponents of scientism had made substantial headway. From the late 1940s to early 1960s, opponents associated with humanistic, left-liberal, and conservative perspectives engaged in an uphill battle vis-à-vis the scholarly mainstream and its institutional pillars of support, including the federal science establishment and more specific nodes of influence from the NAS to the NSF. But in light of the intertwined political, social, and intellectual upheavals associated most often (although not to say exclusively) with the 1960s, challenges to scientism gained considerable ground in the following decades, within the social sciences, within academia more generally, and within American political culture. Any effort to understand and assess the NSF’s importance needs to consider this larger context and the agency’s positioning with respect to the core issues under debate.

This chapter begins, in the first section, by examining four sources of ferment that nurtured opposition during the 1970s and 1980s: the legacy of the New Left, the resurgence of conservatism, the Kuhnian vision, and the interpretivist tradition. By no means do these four cover all of the developments and ideas relevant to the ongoing debate over scientific identify. However, the challenges they raised were among the most important ones—other major challenges that would merit discussion in a more comprehensive analysis include certain currents in feminist scholarship, critical race theory, postmodernism, and the work of the French philosopher Michel Foucault. The second section discusses the special force of the interpretivist tradition in challenging scientism’s dominant position within federal science policy circles and at the NSF in particular. The third and last section examines how such challenges led to some specific proposals for reform.

SOURCES OF INTELLECTUAL FERMENT

Let’s start with the legacy of the New Left. During the 1960s and continuing into the post-1960s era, some proponents of the New Left and its followers—including many sociologists, political scientists, anthropologists, and scholars in related fields—advanced biting critiques of the scientific outlook. As we have noted before, they raised doubts about the value of supposedly objective and apolitical expertise in the policy arena

and wider society. In doing so, they argued that the scholarly mainstream, including prominent areas of study such as the behavioral sciences, structural-functional analysis, and social systems research, gave short shrift to important value-laden research and studies explicitly critical of the status quo. Informed by the ideals of justice, democracy, and human dignity, the latter work seemed especially useful for identifying, scrutinizing, and unmasking oppressive forces in American society and the world more generally (i.e., racism, militarism, colonialism, capitalism, sexism, etc.).

Remember, too, that in the late 1960s, Senator Harris's legislation to establish a new social science agency supported the call for critical scholarship. This was clear in his suggestion that the proposed agency should fund "controversial" studies that could interrogate the unjust status quo and advance constructive alternatives. Although Harris's initiative failed and the New Left itself lost much of its power and identity by the late 1970s, critical lines of analysis associated with the New Left remained prominent in American society and academia, especially in the humanities and social sciences but also in some new interdisciplinary fields, including women's studies.

In addition, critical modes of inquiry that could inform progressive change in domestic and international affairs gained support from the period's most influential left-leaning think tank, the Institute for Policy Studies (IPS; f. 1963). As James A. Smith noted in an important book on the history of think tanks in America, the IPS's founders and intellectual leaders worked to "recover the radical activist spirit of American pragmatism in which ends are discovered and refined in action." These scholar-activists were thus "suspicious of the claims of a 'value-free' social science that could direct policy."⁵ Marcus Raskin, arguably the most influential thinker in shaping the institute's orientation, was himself a prominent New Left intellectual and activist in the 1960s and later decades. During the middle of the Reagan years, Raskin spearheaded an ambitious project to establish "new ways of knowing" that critiqued scientific and associated technocratic modes of analysis, hoping to replace them with what he called "reconstructive knowledge," about nature, society, and culture.⁶

A second source of anti-scientistic ferment came from the resurgence of conservative thought and political culture. We have seen that during the 1970s and 1980s, the push to develop social research, political and cultural analysis, and policy proposals from an explicitly conservative orientation received support from an influential group of right-wing foundations and



Figure 9.1

Marcus Raskin takes part in 25-Year Reunion of the Boston Five, who in 1968 were indicted for conspiracy to aid resistance to the draft. Left to right, Dr. Benjamin Spock, Rev. William Sloan Coffin, Michael Ferber, Marcus Raskin, Mitchell Goodman. Courtesy of Erika Raskin.

think tanks. In earlier decades, concern about embracing an explicitly partisan or ideological orientation had been widespread in these quarters, for such an orientation seemed to leave researchers, policy intellectuals, and their patrons vulnerable to damaging charges of bias. However, in the post-1960s era, a number of organizations, including the John M. Olin Foundation, the Heritage Foundation, and the American Enterprise Institute, became vigorous advocates of explicitly conservative positions on economics, politics, education, foreign policy, legal issues, and other matters. As explained by Jason Stahl, a U.S. political and intellectual historian, these organizations worked to “theorize and ‘sell’ conservative public policy and ideologies to both lawmakers and the public as large.”⁷

As seen in chapter 6’s discussion of the attack on MACOS for allegedly promulgating such things as moral neutrality and cultural relativism, right-wing foundations, think tanks, and journalists sometimes targeted the scientific project as well. Similar to the charges put forth during the McCarthy era (and at other times) by conservative figures, they found the social sciences guilty of contributing to a host of ills during the 1970s and 1980s, including atheism, moral relativism, liberal social engineering, and socialism. At the same time, conservative funding sources advocated lines of political, economic, and social inquiry supportive of the “right” sort of values. This context also informed discussions during the early 1980s’ fund-

ing crisis, when defenders of the NSF hailed its commitment to putatively objective and nonpartisan research, in contrast to organizations that supported research with an explicitly partisan bent.

The evolving debate over scientific identity also reflected the impact of revisionist accounts of science whose roots were, on the whole, more scholarly and less political in character. Such accounts included a third source of anti-scientific ferment based on new studies in the history, philosophy, and social studies of science. Here, I will only mention the enormously influential work of the physicist-turned-historian-and-philosopher-of-science Thomas Kuhn. In 1962, Kuhn, then a professor at the University of California–Berkeley, although he soon moved to Princeton and later to MIT, published *The Structure of Scientific Revolutions*, containing his famous ideas about normal science, scientific paradigms, paradigm shifts, and scientific revolutions. Based mainly on investigations of the physical sciences, especially astronomy, physics, and chemistry, Kuhn argued that in science, an established paradigm channels the intellectual activity of a given research community in an orderly manner such that it leads to successful puzzle-solving, advances in knowledge, and cumulative scientific progress. However, from time to time, a period of revolutionary change culminates in the replacement of an established paradigm with a new one.⁸

What, if anything, did this account mean for the social sciences and their scientific standing? Kuhn himself did not say much, although he did suggest that they were in a preparadigmatic state of development. This reinforced the notion that they were lagging behind the natural sciences and especially the physical sciences. Furthermore, if to be a science depended on having a paradigm, it might seem that the social sciences were not real sciences, at least not yet.⁹

Beyond Kuhn's limited comments, a cottage industry of scholarly studies revealed divergent viewpoints regarding implications for the social sciences. Not surprisingly, one common response from social scientists focused on strengthening the scientific status of their work by claiming that specific disciplines or particular research communities operated in a manner similar to Kuhn's physical science communities. As the philosopher of science Steve Fuller has observed, a good number of sociologists, political scientists, and the like found Kuhn's account attractive because "it seemed to provide a blueprint for how a community of inquirers can constitute themselves as a science, regardless of subject matter."¹⁰

But another and quite different response held that Kuhn's work showed why the social sciences should not take physics (or any other science) as a model. Accounts in this vein pointed out that in Kuhn's view, the establishment of a paradigm in a particular field depended not on following general or abstract rules about the process of scientific inquiry, but on developing a community-wide consensus about the field's foundations. From this viewpoint, it seemed that social scientists should proceed not by embracing some set of abstract notions about the proper conduct of scientific inquiry associated with physics or chemistry, but by deciding for themselves what sort of inquiry, what type of investigative framework, and what set of methods would be most appropriate in light of their field's specific character, needs, and opportunities. In a 1980 survey of the lively discussion about Kuhn's influence, Gary Gutting, another philosopher of science, captured this position nicely with the following suggestion: if the social sciences were "lacking the consensus on a single paradigm," then perhaps the most valuable take-away message for the practicing scholar was not to insist on one or another paradigm but, rather, to "cultivate" one's "own garden."¹¹ As we saw in chapter 4, leftist scholars wanting to reform the discipline of political science also invoked Kuhn's ideas as justification for a paradigm shift.

Growing opposition to scientism also gained support from a resurgence of the interpretivist tradition, whose roots lie deep in the history and philosophy of modern social science. In broad brushstrokes, proponents of this tradition held that knowledge of social phenomena depends on understanding the meaning of social actions, and understanding requires interpretation on the part of the investigator. Consequently, the social sciences differ in fundamental respects from the natural sciences, which don't have to deal with meaning, understanding, and interpretation in the specific ways needed in the social sciences. We have encountered key tenets of this fourth source of intellectual ferment before, including in Harry Alpert's view that the study of human actions in social context depends on *verstehen* analysis, which produces rich qualitative understanding of human meanings, intentions, and purposes. Arguably, such research was quite different from work in the physical sciences at the levels of ontology, methodology, epistemology, and practical value. During the 1970s and 1980s, interpretivism acquired renewed energy as it was embraced by leading scholars in various disciplines.¹²

The interpretivists found a powerful advocate in the figure of Clifford Geertz. An anthropologist trained in the Boasian tradition of fieldwork and

cultural interpretation, which emphasized the need to understand the uniqueness of each culture, its particular constellation of values and standards, and its singular historical trajectory, Geertz received his PhD from Harvard's Department of Social Relations in 1956, then worked as a professor at the University of Chicago. In 1970, he moved to the Institute for Advanced Study in Princeton, where he helped establish the School of Social Science. As the school's founding figure and long-time leader, Geertz nurtured an interdisciplinary milieu conducive to interpretivist study. This meant welcoming contributions from scholars in the humanities, such as history and literature, and from humanistically oriented social scientists.¹³

The main goal, as noted in this chapter's opening quote from Geertz, was to push beyond "the prevailing paradigms in the social sciences," which seemed to be "poor imitations, mostly, of misunderstood physics," with the aim of adapting these sciences "to the immediate peculiarities of their supposed subject matter, the human way of being in the world." Scholars at the



Figure 9.2
Clifford Geertz, participating in a political theory panel, celebrating twenty-five years of the IAS School of Social Science. Left to right, William Sewell from the University of Chicago, Clifford Geertz, and Laura Englestein from Princeton University. May 1997. Photo by Randall Hagadorn. From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA.

School of Social Science were thus encouraged to free themselves from “the narrowed confines of a fixed and schematized scientific method.” Liberation would enable them to address “moral, political, and spiritual concerns,” which, in turn, meant reaching out to “the humanities—to philosophy, literature, history, art, religion.”¹⁴

In the context of this multipronged attack on scientism described above, what, if anything, did critics say about federal science policy and the NSF?

THE SPECIAL FORCE OF THE INTERPRETIVIST CHALLENGE

One cannot fail to be impressed by the persistence of the NSF’s scientific commitment throughout the 1970s and 1980s, as we have seen in agency policies and programs as well as in efforts to defend those by agency leaders and other supporters in the scholarly and political arenas. Yet the growth of anti-scientistic ferment meant that the conception of the social sciences advanced by the NSF was increasingly suspect from a number of different viewpoints. The interpretivist critique received the most attention.

A 1977 congressional background report on NSF programs—mentioned in the previous chapter—pointed out that some social scientists were unhappy with the agency’s hard-core emphasis. They questioned its effort to “impose the criteria of the physical and natural sciences on subject matter which does not lend itself to quantification.”¹⁵ That same year, discussion of this issue appeared in *Science*, in its coverage of a dispute over the criteria used to review NSF proposals in social and cultural anthropology.

The inciting incident occurred in February, when an article by journalist Gina Kolata reported that the NSF anthropology program officer, Nancie Gonzalez, thought that scholars in the social and cultural wing of this discipline received relatively few research grants because they often failed to present their work in rigorous scientific terms. According to Kolata, Gonzalez “was shocked when she first saw some of the ‘mushy’ proposals submitted by social anthropologists.” Thus, to have a better chance of receiving a grant, these scholars would need to satisfy the agency’s scientific criteria better. In a follow-up letter, Gonzalez—who, interestingly, was a social anthropologist herself—added that archeologists were more successful in getting grants because they did a better job in their applications of stating a specific problem and explaining why it was interesting and important.¹⁶

Although Kolata's article said little more about what Gonzalez meant by thinking scientifically, the history of NSF funding for anthropology suggests why her remarks provoked critics of scientism. During the period of convergent research in the mid-1950s, the NSF had privileged work that overlapped with the natural sciences, particularly physical anthropology and archaeology. As early as 1956, it began to support social and cultural anthropology as well, but mainly for work on the hard-core end of the social research continuum. Over the years, this included studies on the functional integration of social systems and on the general processes of cultural evolution. Meanwhile, scholars from the discipline's humanistic wing, who sought to understand the uniqueness of particular cultures in their specific historical contexts, had a tough time getting funding.¹⁷

Against this background, Gonzalez's position did not sit well with anthropologists who believed its implications for scholarship were stifling. In the same issue of *Science* that published Gonzalez's letter, another letter from Richard Adams, a Latin American specialist from the University of Texas, complained that the NSF continued to insist on narrowly construed scientific criteria, as seen in its favoring of research that used "the hypothetical-deductive method." As Adams saw it, that outlook reflected the more general "behavioristic fad" so prevalent in American social science during the last quarter century. Consequently, important types of anthropological inquiry, including "general ethnography" and studies employing an "interpretive approach," would have difficulty acquiring funding. If this situation persisted, the overall impact would, Adams predicted, be "simply disastrous" for the discipline.¹⁸

To underscore the depth of this problem, Adams speculated that if the NSF continued on this path, even such a luminary as Clifford Geertz would be excluded. Although Geertz was "certainly among the most distinguished anthropologists of his generation," an application from this champion of interpretive studies would probably not satisfy "the 'scientific' criteria" used by the agency's anthropology program.¹⁹

Placed immediately below Adams's letter in *Science* was another letter of complaint, this one signed by eight scholars. Many had positions at top universities, including Harvard, Yale, Princeton, and the University of California–Berkeley. And all of them were anthropologists, including Geertz himself. Their letter charged that the anthropology program officer's

approach was misguided because “social science is neither biology nor physics.” In an exasperated tone, they asked,

Are we to assume that NSF has an official policy subscribing to a simplistic and rigid view of social science harkening back to 19th century positivism or, even more disturbing, to an authoritarian insistence that those dispensing funds may dictate to scientists what science is or is not?

Hoping to correct such a view, this group of eight urged the agency to abandon its one-shoe-fits-all outlook, because the “study of society involves factors of value and history radically different from research in those sciences apparently endorsed by Gonzalez as models we must emulate.”²⁰

Gonzalez responded with her own letter to the editor, claiming that those criticisms were unfair. She also claimed that one reason the NSF gave less support to social anthropology and more to archaeology was that “much respectable social anthropological research addresses humanistic, rather than scientific questions.”²¹ Thus, in the end, neither side conceded much ground in the ongoing debate over scientific identity and the agency’s steadfast position.

During the Reagan years, the interpretivist challenge received favorable consideration from a small but well-informed group of scholars who were concerned about the mounting political attacks on the social sciences and associated threats to their funding. Among this group was SSRC president Kenneth Prewitt. Regarding the June 1981 strategy meeting in New York (discussed in chapter 7), Prewitt observed that many social science leaders and supporting organizations wanted to establish better ties with the natural sciences. Elsewhere, however, Prewitt acknowledged the seriousness of the interpretivist challenge to the unity-of-science standpoint associated with that strategy. According to what Prewitt called the “dominant tradition of American social science,” the basic data of inquiry consisted of “systematic observations or measures of individuals or groups, extracted for analytical purposes from their larger context.” Scholars treated these observations as “discrete variables,” “hypothesized causal relationships between them,” and tested the hypotheses “by examining the impact of changes in one variable on changes in the others.” “Modeled on the physical sciences,” this type of investigation emphasized “the use of quantitative data, especially survey data, and to a lesser extent, experimental data.” The underlying expectation was that “an understanding of relationships among significant variables will enable society to develop the means for alterations

and improvements.” Prewitt believed that critics of this tradition deserved serious consideration.²²

Foremost among the critics were the advocates of “interpretive social science.” Their humanistic approach, Prewitt observed, “seeks a detailed understanding of the meaning of actions, customs, events, and institutions to the individuals and groups that perform and participate in them.” To obtain this special type of understanding, interpretivists employed distinctive methods, including the “techniques of literary analysis.” They employed “metaphor, narrative, and scripts as tools for understanding social behavior.” Summing up, the SSRC president emphasized that the interpretivist challenge provoked deep controversy “because it redefines the objectives of the [social science] enterprise, the kind of knowledge desired, and the appropriate ways of obtaining that knowledge.”²³

Although the previously mentioned 1982 NAS report defended the unity-of-science viewpoint (as NAS reports always had done and would continue to do), this report also acknowledged that interpretivism offered an alternative. As presented here, the stakes in terms of understanding the nature of the social sciences and promoting them in a healthy way could hardly have been bigger. Similar to other accounts that presented the matter as a contest between irreconcilable positions, the report identified “two largely competitive visions” of the social sciences. Their existence lent credence to the idea that these sciences comprised an “intermediate” branch of inquiry between the natural sciences and the humanities—an idea suggested three decades earlier by the British cultural commentator C. P. Snow. While the “dominant vision” upheld the unity-of-science position and sought to establish broad generalizations or laws of social behavior, the minority vision, held by “a sizable fraction” of scholars in several disciplines, didn’t aim at “generalization—many would question whether the sort of generalization that characterizes the physical and biological sciences is possible—but interpretation.” These scholars viewed human behaviors and social arrangements as arising from particular concrete historical circumstances best studied by paying careful attention to those circumstances. They emphasized the goal of “understanding what is distinctive rather than what is general,” a difference, as the NAS report also pointed out, that Clifford Geertz had referred to as the “cases-and-interpretations” versus the “laws-and-instances ideal” of explanation.²⁴

NEW CALLS FOR REFORM

In light of the gathering challenges, some scholars called for major reforms in federal social science research policies. It would be a mistake to believe that this push for reform received widespread attention within the federal science establishment or within the NSF. And the agency had no program to support qualitative social research, philosophical social inquiry, or critical scholarship that could have provided a complement to its many disciplinary and nondisciplinary programs that supported research grounded in quantitative methods and putatively objective, value-neutral analysis. I have found no evidence, either in published documents or archival records, that suggests the agency even considered the idea of creating a program dedicated to those alternatives, never mind giving its existing programs responsibility for supporting them. If somebody did suggest as much, the idea certainly did not gain traction. Nevertheless, as we will see below, the reformist agenda that challenged the established NSF framework was advanced by a small group of sociologists, political scientists, and, interestingly, a mathematician who had substantial knowledge about funding policies and their impact on the production and uses of knowledge.

In the early 1970s, the sociologist Harold Orlans reported on the deep divide over Senator Harris's NSSF proposal within the social sciences, as was noted in chapter 4. Orlans himself also advocated a new approach to funding, which he described as the exact "opposite of that which governs scientific research." More specifically, he said federal policies should encourage research where people are "described as people—as children, citizens, unemployed blacks or busy congressmen—not as abstract and timeless 'subjects'"; where institutions are described as "specific organizations with stated characteristics, not as an abstract form which exists only in the academic mind"; and where data are understood to be "the residue of certain procedures employed by designated persons, not as elementary particles of an unchanging universe."²⁵

Other scholars argued that the established funding regime with its scientific commitment had contributed not only to troubling limitations on the pursuit of knowledge but also to widespread disillusionment with applied social research. How the project of advancing the social sciences in the image of the natural sciences could have such a disconcerting effect was explained by the Princeton political scientist Richard Nathan, in his book *Social Science in Government: Uses and Misuses* (1988). After studying a series

of cases involving the public-policy relevance of applied social science, Nathan concluded that the “tendency to emulate the natural sciences” had become a bad habit. As a result, social scientists generally preferred “quantitative research designs and techniques,” while they unfairly dismissed or simply overlooked the value of “qualitative research methods and data.” These were serious mistakes, asserted Nathan, as the quest to make social research scientific through the application of quantitative methods often resulted in a “spurious precision.” Furthermore, although the effort to construct scientific models that made accurate predictions had proven effective in certain natural science fields, in the social sciences this effort tended to produce poor results. Was it possible to “predict human behavior using the ‘objective method’” allegedly found in the natural sciences? The answer, said Nathan, was definitely not, because the data needed to construct such models did not exist or could never be collected. The natural science model of inquiry had turned out to be the real problem, then; work based on this model had failed to produce the promised policy-relevant results, which, in turn, fueled disillusionment with the social sciences more generally.²⁶

As part of this wider rethinking about intellectual foundations and practical value, the impact of the national funding system on the social sciences received critical scrutiny from the Yale mathematician Neil Koblitz. As he saw it, the political economy for scholarly research had unfortunately privileged work of a supposedly rigorous scientific nature at the expense of humanistic studies, including humanistic social research. Not only did “prestige and money go disproportionately to the sciences, especially applied science and engineering,” but “the administrations at most large universities” also tended to favor “those departments that are most successful in attracting outside funding.” Professors “in social and behavioral fields” thus had strong incentives to portray their work “as being closer to the sciences than to the humanities.” On the other hand, “professors in those fields who prefer the honestly subjective approaches of earlier years are often regarded as old-fashioned and out of step.” A fierce critic of the misuses of mathematics and quantitative methods in social research, Koblitz issued a stern warning:

As long as our society continues to undervalue humanistic and cultural pursuits, we can expect to see a proliferation of mathematical jargon and pseudoscience in fields far removed from what one would have thought to be readily quantifiable—fields in which mathematical methods are rarely appropriate and are often misused.²⁷

Against this background, the NSF itself came under scrutiny as well. In a 1969 book, the sociologist Gene Lyons had presented a history of the “uneasy partnership” between the social sciences and the federal government. Two decades later, in a 1986 essay aptly called “The Many Faces of Social Science,” Lyons pointed out that previous efforts to reform the federal science establishment in ways that would have encouraged social scientists to pursue a wider range of work had ended without much change. Not only had Senator Harris’s NSSF proposal failed, but a series of national science policy reports published at the end of the 1960s stood firmly behind the scientific position. With dismay, Lyons added that two decades later, NSF funding still “tended toward a narrow range of work.”²⁸

In Lyons’s view, the basic problem arose from a mistaken unity-of-science notion and the oft-invoked corollary that said the social sciences could and should try to emulate the natural sciences. Yet he emphasized that he was not against calling the social sciences scientific. Similar to the position taken by the economist Carl Kaysen and some others during the NSSF debate, Lyons wanted people to recognize that a great diversity existed among the sciences, even though popular English-language usage of the term science obscured this fact. He proposed that one might reasonably say that the social sciences were scientific, just as one might justifiably assert that they should employ “rigorous methods of research.” However, this did not mean that the social sciences were just like the “other sciences.” Similar to what Senator Harris had suggested, and thus contrary to the stance taken by a Herbert Simon or by any of the NSF directors up through Erich Bloch in the 1980s, Lyons proposed that the social sciences should be encouraged to push forward on multiple and diverse fronts, maintaining “strong lines” to the natural sciences for certain purposes but simultaneously pushing hard to “break out on their own” for other purpose.²⁹

The call for reform went one step further in the hands of the sociologists Samuel Klausner and Victor Lidz, who questioned whether one could realistically expect the social sciences to achieve a breakout on their own. In 1986, they published an edited volume of essays called *Nationalization of the Social Sciences*, which included the aforementioned pieces by Lyons and Riecken as well as the first published version of Talcott Parson’s still-born report for the SSRC in the late 1940s. According to Klausner and Lidz, increased federal funding for the social sciences since World War II had proven to be “a mixed blessing.” Early in the postwar NSF debate, Parsons

and a clutch of other SSRC scholars had claimed, perhaps too optimistically, that these sciences could be part of a new national science agency without relinquishing scholarly autonomy. In the ensuing decades, however, the undeniable power of the federal purse together with selective funding policies had given the NSF considerable “leverage ... to impose certain standards” on social science research and training.³⁰

Of particular concern, the social sciences had suffered from “fiduciary overlordship” exercised by the natural sciences. Lidz and Klausner suggested that an entire generation of social scientists had been influenced in questionable ways by NSF policies and programs together with a broader climate of opinion shaped by “figures who are neither social scientists nor profoundly schooled in the nature of social scientific knowledge.” The resulting problems included an unhealthy split between quantitative scholarship and more richly descriptive qualitative scholarship.³¹

Looking for a way forward during a period of lean budgets and conservative displeasure, Klausner and Lidz reasoned that the NSF was not about to reform its ways. An examination of the agency’s track record in combination with political, institutional, and ideological pressures during the Reagan years rendered the prospects for a major reorientation in social science funding unlikely. The two sociologists thus called for reviving the proposal for a separate social science agency. To be sure, the road ahead would not be easy. Common worries about the relationship of social science to social ideology and social reform would surely cause headaches for the agency they envisioned. The possibility that its mandate to promote the social sciences would stir up conservative worries that it was supporting a “form of socialism” could not be dismissed either. For social scientists, letting go of natural scientists’ coattails would also mean losing some supporters, certainly in the short term.³² Nevertheless, if they had an agency of their own, the pressures to mimic the natural sciences would, presumably, be reduced. The influence of natural scientists on funding policies and priorities would decrease dramatically as well. Presumably, the proposed agency would be able to support a much broader range of social research than the established NSF could.

CONCLUSION

Appreciating the significance of NSF social science policies, programs, and practices requires that we consider them in light of evolving challenges to

the scientific project. Two of the main challenges considered here, the legacy of the New Left and the resurgence of conservatism, were inspired by political and intellectual movements associated with left-leaning and right-leaning agendas. The other two challenges were rooted mainly—although not to say exclusively—in academic currents in the history and philosophy of the sciences and were associated with the figures Thomas Kuhn and Clifford Geertz. All four challenges had gained considerable momentum since the late 1960s. Throughout the 1970s and 1980s, they informed the broader discourse concerning the social sciences in American politics, science, and higher education.

Anti-scientific criticism thus produced a palpable degree of fragmentation behind the scientific front. The particular challenge posed by the interpretivists received special attention from a group of figures with substantial knowledge about the politics and funding of social science. SSRC president Kenneth Prewitt and a number of anthropologists underscored the power of this challenge and the central contributions of Geertz, who by this time was the head of the School of Social Science at Princeton's Institute for Advanced Study. In addition, passages from NSF and NAS documents acknowledged the importance of the interpretivist challenge in a respectful tone. Meanwhile, some well-informed social scientists, such as the sociologist Harold Orlans and the political scientist Richard Nathan, argued that the dominance of a natural science model of inquiry had actually done significant harm to the social sciences and their standing in the wider society.

Yet the multiple sources of anti-scientific ferment had little impact at the NSF and within the federal science establishment more generally. During the 1980s, no legislative proposal comparable to Senator Harris's NSSF initiative emerged, even though the sociologists Klausner and Lidz called for a revival of that initiative. As the 1980s and the Reagan era came to an end, any hope that the flourishing of alternative views about scientific identity and social relevance would inspire major reforms at the NSF, never mind the creation of a separate social science agency, seemed nothing more than that—just a hope.