BOOK REVIEWS

A Saga of Social Science

Milestones and Millstones. Social Science at the National Science Foundation, 1945–1991. OTTO N. LARSEN. Transaction Publishers, New Brunswick, NJ, 1992. xvi, 285 pp. $32.95.

The famed historian of early modern Europe Fernand Braudel has informed us that "events are the ephemera of history." He noted that "they pass like fireflies, hardly glimpsed before they settle back into darkness and as often as not into oblivion." What is most important in history, Braudel believed, is the cumulative effect of everyday happenings in the lives of ordinary people.

Otto Larsen's detailed history of the social sciences at the National Science Foundation (NSF) is the cumulative record of hundreds of such happenings—the meetings, memoranda, and reports that constitute the raw materials of organizational policy and institutional change. In search of these happenings and their cumulative effect, Larsen, a sociologist who was the senior social scientist on the NSF staff from 1980 to 1986, has combed the NSF files and perhaps other files in Washington as well; he cites most of the relevant periodical literature; and he draws upon his insider's knowledge of recent NSF history. He presents 19 informative tables showing trends in NSF funding over time, and he includes in an appendix a brief summary of each of the 14 national studies of the social and behavioral sciences published during the last 30 years. The name index of the book lists some 500 persons who played some role in these events over a 50-year period. The result is a fascinating story, a superb reference book, and a reader's guide to the NSF files as well; all future research into the postwar history of federal funding for the social sciences will now start with Larsen's book.

Perhaps mindful of Braudel's injunction, Larsen has not searched for major, explanatory events to carry the burden of his story. In this history there are also few heroes (although Harry Alpert, the first senior social science staff member at NSF, comes pretty close). Nor are there real villains; no scandals, fiscal or otherwise; no truly unkind words for anyone. (Numerous "milestones" are described, but the identities of the "millstones" are unclear.)

The social sciences were not given a seat at the table when NSF was created in 1950; it took many years for them to have a seat of any kind; and they had to wait until 1991 for a first-class seat. Larsen provides a memo-by-memo, report-by-report, testimony-by-testimony account of how it happened, but he blames no one in particular for the delay. The news that a Directorate for Social, Behavioral, and Economic Sciences was finally established in October 1991 was added to the text when the book was in page proofs. Seldom in the history of publishing can an author have been as pleased as Larsen must have been by the need for a last-minute addition to his text.

There are two major themes of general interest that repeatedly emerge in this account: the debate and the tension between basic and applied research, and the questioning of the social sciences and their struggle for legitimization in a government agency founded without them in its charter.

The NSF was founded explicitly for the purpose of supporting basic scientific research. Larsen perceptively characterizes President Truman's signing of the enabling legislation for the NSF on 10 May 1950 as follows:

> This was a historic act of faith. The federal government, accustomed to *procurement* models for using science to attain specific goals, was about to commit itself to an *investment* model whereby it would directly support an intellectual activity whose payoffs were long-term and uncertain.

This investment model pleased the natural scientists, of course, and it would have pleased the social scientists as well had they been asked, but it did not necessarily please the Congress, which is occasionally worried about its oversight responsibilities. From time to time programs were established specifically to fund applied research—the 1970s RANN (Research Applied to National Needs) program is perhaps the best-known. Although there were frequent debates about the appropriateness of applied research in a science agency, the social sciences seem not to have played a memorable role in them. Nevertheless, they were occasionally chastised for being either too practical or not practical enough.

This "damned if you do, damned if you don't" reception of the social sciences in the federal government has been summarized by the sociologist Neil J. Smelser in the introduction to the *Handbook of Sociology* (Sage Publications, 1988) as follows:

> There are pockets of hostility toward the behavioral and social sciences on the part of government officials . . . [Hostility takes] the following forms:
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> Research in the behavioral and social sciences produces trivial, obvious, and unimportant results, and expenditures on that kind of research are wasteful of public funds . . . Is of no use to the government, and therefore should not be funded . . . Is basically unscientific—according to the canons of hard science, and is therefore undeserving of study . . . Is basically dangerous ideologically. . . .
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> It is difficult to conceive how the behavioral and social sciences can be simultaneously trivial, useless, unscientific, and threatening.

It is the initial questioning of the legitimacy of the social sciences and the slow acceptance of it that constitute the hard core of Larsen's account. He quotes remarks that were made by Senator H. Alexander Smith of New Jersey during a 1947 floor debate on the creation of the NSF as characteristic of the congressional mood at this time:

> I have conceived of this . . . as a bill for research in pure science, not in applied science. We are trying to subsidize pure science, the discovery of truth. This has nothing to do with the theory of life, it has nothing to do with history, it has nothing to do with law, it has nothing to do with sociology.

Remarks of this kind are not particularly hostile; they merely reflect a mind-set that is not uncommon in the Congress and is still widespread among natural scientists: the sciences belong in one category of knowledge and all other kinds of intellectual activity in another. C. P. Snow had it right in the title of his 1959 book, *The Two Cultures and the Scientific Revolution*.

Larsen sets out to provide what is called these days an "inside the Beltway" explanation of how the legitimacy of the social sciences was gradually achieved. His data were generated primarily in Washington and so was what seems to be his favorite explanation of ultimate success: "underdogging." This is a term original in this context, I believe, with Henry C. Riecken, one of Larsen's major predecessors at the NSF; it seems to be the bureaucratic equivalent of "defensive driving." Larsen quotes from a retrospective essay by Riecken on his observations of underdogging at the NSF:

> The growth of support for social science at NSF was . . . the result of strong external support for the program on the part of respected advisors, a

rising budget that prevented NSF expansion from being a zero-sum game, a degree of skill at administrative politics within the agency, and the fact that, in the first decade or so, grantees committed no serious gaffes or egregious offenses to the conventional morality or established values of those who controlled authorization and budget.

I don't think that the explanation put forth by Riecken and Larsen is wrong; rather it is ahistorical and thus needs to be placed in a context of change. Neither American society nor the social sciences stood still during these 50 years. The sweeping changes in American society are well known. Let me cite some of the "outside the Beltway" changes in the social sciences that certainly increased their prospects for legitimization:

1) The wartime experiences of hundreds of social scientists—in everything from assessment of the morale of soldiers and civilians to psychological warfare to price control—sent them to the universities as the best-trained generation of social scientists the country has ever had.

2) The enormous postwar expansion of university social science departments created a strong second generation of social scientists, which now dominates most of the disciplines.

3) The postwar flourishing of university-based social, psychological, and economic research institutes both trained students and made research results useful to a broader audience.

4) The postwar development of quantitative research methods and mathematical modeling greatly improved the scope and specificity of research results.

5) The postwar creation or development of disciplines that study the institutions of science—the history of science, the sociology of science, science policy analysis, ethical issues in science—commanded wide respect and created important bridges to the natural sciences.

6) The concepts and language of the social sciences entered popular discourse during this era. Larsen quotes from a 1986 Congressional Research Service report that lists a number of words and phrases that made this transition, including: *acculturation, alienation, charisma, ethnocentrism, fiscal policy, GNP, identity crisis, juvenile delinquency, minority group, quality of life, reference group, self-fulfilling prophecy, sample, socialization, stagflation, standard of living, status, stereotype, the unconscious,* and *youth culture.*

I cannot say whether or not the attitudes of natural scientists changed during this period. A suggestion that Larsen believes they have not is found in his pronouncement that "it would help if social scientists in the major research universities would take a physicist, chemist, biologist, or mathematician to lunch from time to time."

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A Vanishingly Small Case

Cold Fusion. The Scientific Fiasco of the Century. JOHN R. HUIZENGA. University of Rochester Press, Rochester, NY, 1992. xvi, 259 pp., illus. $45.

"One watt input, four watts output!" was the electrifying claim that precipitated a storm of publicity and started a worldwide scientific race to verify the existence of nuclear fusion in a jar. Members of the scientific community and lay readers interested in the history of the cold fusion episode and its broader implications for the scientific process will find much to consider in John Huizenga's thoughtful account of this astonishing chapter in the history of science. Huizenga, professor of chemistry and physics at the University of Rochester, co-chaired the Department of Energy ERAB (Energy Research Advisory Board) panel appointed to investigate the claims made in the memorable 1989 press conference by the two University of Utah electrochemists Martin Fleischmann and Stanley Pons. The most startling aspect of this report was the claim that nuclear reactions could be induced by loading deuterium atoms into a palladium metal lattice and that these reactions released macroscopic quantities of heat detectable by a simple calorimeter. Present nuclear theory predicts vanishingly small D + D reaction rates ($\sim 10^{-64}$ per second) under the reported experimental conditions, but experiment, not theory, is the final authority in science, and it is argued persuasively in this book that the resolution of the cold fusion story came about through careful analysis of the experimental procedures and data.

Huizenga begins his narrative by tracing the history of cold fusion claims and counterclaims through press conferences, scientific meetings, and journal publications. The level of technical detail provided enables the reader to judge the science for himself or herself, and the chronicle of the subject is liberally documented with journal citations and illuminating techni-