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Finding Sociology in Its Funding? Networks, Relations, and Key Players in Late 20th Century Social Science

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Abstract

In this paper we start from Mark Solovey's *Social Science for What?* to analyze the place and the role of the social sciences in the US National Science Foundation from the mid-1940s to the end of the 1980s. The book highlights the tensions that built up around the epistemic status of the social sciences vis-à-vis the natural sciences and the reputational debates surrounding their role and fate during and after the postwar period. We mostly focus our attention on structures, actors and processes not addressed by Solovey: relationships, networks, and patterns of stratification within and across disciplines; the emergence of novel approaches *outside* the scientistic and positivistic framework sponsored by the NSF; alternative sources of funding, such as the National Endowment for the Humanities; and a set of broader, long-term processes in the macro-field of the social and behavioral sciences. We present some preliminary data suggesting that a wider, theoretically-oriented approach might be fruitful in casting a more complex and dynamic portrayal of the development of American social science.

Keywords: American social science; sociology; Clifford Geertz; anthropology; National Science Foundation; scientific field; fractals; National Endowment for the Humanities.

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In the last twenty years or so, contemporary historian Mark Solovey has become one of the most highly esteemed scholars among those committed to reflecting on the history and the fate of the social and psychological sciences during and beyond the postwar period. In particular, he has been able to combine critical thinking on the Cold War era in general with more focused work on the network of public and private funders of the American social sciences during the Cold War (Kleinman & Solovey, 1995; Solovey, 2001, 2012, 2013). In *Social Science for What?* (2020) he finally concentrates at length on what looks like his first, and crucial, intellectual crush: the National Science Foundation (NSF). The history of this influential institution and its impact on the macro-field of American social science during the second half of the 20th century is traced on the basis of a powerful pair of guiding hypotheses. First, early structural and funding choices favored the adoption of a "scientistic framework" to include, justify, and evaluate social scientific work *within* the NSF. Second, this framework had an impact *outside* the scientific macro-field, as the status and the promise of the social sciences were recursively, and hotly, debated within American political culture and partisan politics (Solovey, 2020, pp. 6–8).

In the following pages, we start by summarizing *Social Science for What?* as a rich account of the place and the role of the social sciences in the NSF from the mid-1940s to the end of the 1980s. In particular, in the first section we highlight the tensions that built up around the epistemic status of the social sciences vis-à-vis the natural sciences and the reputational debates surrounding their role and fate during and after the postwar period. Was social science going to be a valuable instrument for addressing urgent societal problems or the dangerous propagator of corrupting ideologies? In the remainder of the paper our attention turns to structures, actors and processes not addressed by Solovey. In the second section we sketch the intellectual and academic trajectory of the late American anthropologist, Clifford Geertz, encouraged by the fact that Solovey himself presents him as the standard-bearer of that interpretation-oriented social science excluded from NSF funding. We use Geertz's case to emphasise the importance of looking at relationships, networks, and patterns of stratification within and across disciplines that contributed to the emergence of novel approaches *outside* the scientistic and positivistic framework sponsored by the NSF. In the third section we focus on the National Endowment for the Humanities as a relevant example of an alternative source of economic and symbolic capitals for non-positivistic social scientists.[1] In the fourth and last section we indicate how stratification, networks, and institutions might be combined with a number of broader, long-term processes in the macro-field of the social and behavioral sciences. We underpin our argument with some preliminary data suggesting that this approach, although incomplete here, could be fruitful in casting a more complex and dynamic portrayal of the development of American social science.[2]

1 The Social and Behavioral Sciences at the National Science Foundation

As is well known, it was during the postwar period and the early stages of the Cold War that the natural sciences came to occupy a privileged status in the political agenda of the federal government of the United States. The employment of new technologies during World War Two and the subsequent development of nuclear weapons during the so-called "arms race" proved how

1. Another case in point could have been the Wenner-Gren Foundation, a key player for the development of American anthropology (see Lindee & Radin, 2016).

2. We will focus mostly on sociology, even though we know that, as a pluralistic and rather "porous" discipline (Abbott, 2001; Turner, 2014), it might not be representative of the macro-field as such. In this sense, our observations are to be seen more as suggestions for future research than as criticisms of a fine book.

much the natural sciences, and physics in particular, were going to be crucial in determining the relative position of nation states in the new global geopolitical scenario. After five years of debate, in 1950 the National Science Foundation (NSF) was created to provide federal support for research and education in all scientific fields but medical research — which was supported by the National Institutes of Health (Solovey, 2020, p. 50). In the discussions that led to the agency's establishment, the social sciences occupied a peripheral role and were considered by many players as disciplines that should not be included under the NSF umbrella.

According to Solovey (2020), in order to carve out a space for themselves in the newly-founded federal agency, American social scientists depicted their work as part of a unified scientific enterprise, striving to depict it as rigorous and objective from the beginning. This had profound implications for both their positioning with respect to the natural sciences, and the way in which politicians understood (and interacted with) them. The push-and-pull effects of these two cleavages are constantly highlighted throughout the book's ten chapters, which are organized chronologically to underscore the consequential impact that the foundational moments of the NSF had over the development of the social sciences. In the first chapter, Solovey (2020) portrays the problematic processes of inclusion of various disciplines under the NSF aegis. As natural sciences such as physics acquired a prominent and representative role during the nuclear age — i.e., physics as the paradigm of what "science" looks like — heated debates on the reputation of the social sciences as real sciences centered on their "shaky scientific status" and their "scientific identity," undermined by their "engagement with politics and other matters involving value judgments and perhaps ideology" (p. 30). Solovey sheds light on how different actors — including the Social Science Research Council (SSRC) — strived to put the disciplines they represented under the umbrella of "the natural sciences," and to make them eligible for NSF funding by avoiding approaches that explicitly addressed social problems or engaged directly with progressive or leftist ideologies.

The reader is then led through the 1950s (Chapter Two), when the first research program in the social sciences made its debut under Harry Alpert, the "first social science policy architect." According to Solovey, Alpert "recommended to his superiors that the NSF should develop a program based on 'an effective integration and partnership between the natural and social sciences' " (2020, pp. 51, 55) — thus leaving the task of funding applied research on difficult or problematic issues to private, philanthropic foundations. This phase consolidated both the ideology of basic research and the idea of scientific merit — strengthened by the creation of the peer review system — which in turn legitimated the social sciences at the price of drawing them closer to the natural ones. Still, funds for these disciplines remained low (in 1958, only US$ 600,000 out of a US$ 16.3 million research budget were assigned to the social sciences), and grants were generally smaller and shorter than those awarded to natural sciences (p. 69). Backstage, Alpert was in favor of applied (as opposed to basic) research and interpretive (as opposed to positivistic) approaches, but the early projects funded by the NSF still embraced scientism because of the under-recognized role of the social sciences in the Foundation's charter, combined with a hostile political climate, the unwelcoming positions of hard scientists, and the existence of other funders.

Chapters Three and Four describe the 1960s as a supportive period for NSF social science, thanks to a dramatic increase in the agency's total funding — from US$ 40 million in 1958 to US$ 500 million in 1968 — and the favorable political climate epitomized by the Kennedy and Johnson presidencies (Solovey, 2020, p. 80). During this era, social sciences were granted their own division, for the first time matching the natural sciences' status in the organization. Moreover, their budget increased — from US$ 2.2 million in 1960 to US$ 15.3 million in 1969 (p. 85)

— and funding was extended to applied projects. Nonetheless, the natural sciences continued to receive the lion's share of the budget, and the NSF approach to social science "continued to be defined by a unity-of-science viewpoint and hard-core emphasis" (p. 89). Solovey presents two stories that, together, prismatically reflect the period's constant political tension and repeated attacks. Following the trajectory of political science and two legislative initiatives that, while originating in the Democratic camp, framed the position of social science in the federal funding system in dramatically different ways, Solovey highlights the NSF leaders' continuing worries about the politicization of the social sciences and its effects on the reputation of the agency, on the one hand, and the constant attempts of politicians to use the social sciences to fuel their strategic talking points, on the other.

The period from the late 1960s to the late 1970s saw a dramatic decrease in federal funding, a widespread distrust of the social sciences and their practical relevance — probed by the Interdisciplinary Research Relevant to Problems of Our Society (IRRPOS) program. In addition, the Vietnam War, the Watergate scandal, the 1973 and 1979 oil crises, and the Iran hostage showdown led to a general skepticism about government and expert knowledge. The convergence of "[b]udgetary pressures, growing skepticism about the social sciences and their policy relevance, a conservative resurgence [...] and heightened demands for political responsiveness" posed serious problems to the social sciences at the NSF (Solovey, 2020, p. 137). In the fifth chapter Solovey thus focuses on three cases embodying this convergence: the debate over the Research Applied to National Needs (RANN) program, which sought to produce policy-relevant research; the Golden Fleece Award instituted by Senator William Proxmire (D-WI) to draw the attention of the media and the general public to research projects he considered to be a waste of taxpayers' money; and conservative attacks on "Man: A Course of Study" (a.k.a. MACOS), an evolutionary-oriented curriculum program devised to disseminate social scientific knowledge and sensitivity among grade school students.

This discontent in the political arena, science policy circles, and society at large led to a major internal reorganization in NSF social science, as discussed in Chapter Six. Under the guidance of psychologist John Atkinson — the first social scientist to serve as the director of the NSF — the social sciences were refashioned to limit damage and preserve the Foundation's public reputation and its shaky relationship with the political sphere. The social sciences division was closed in 1975, and the disciplines were lumped together with the biological sciences under the latter's organizational branch (Solovey, 2020, pp. 176–178) — reducing their autonomy to protect them, and the agency, from external pressure and criticism (p. 182). *Social Science for What?* then reconstructs how a report commissioned by the National Academy of Sciences, authored by psychologist and economist Herbert Simon, reinforced the idea of a fundamental unity between the natural and the social sciences, underscoring the importance of basic (again, as opposed to applied) research. The late 1970s thus saw a decline in the prestige of the social sciences and the umpteenth downgrading of practice-oriented perspectives in favor of basic research and "big social science" projects, such as longitudinal surveys and national election studies (p. 191).

Chapters Seven and Eight focus on the "dark days" of the 1980s: Ronald Reagan and his administrations massively reduced the budget allocated to the social sciences, to which their practitioners and supporters attempted to respond with a variety of strategies. In 1982, the budget for the social sciences was slashed from US$ 33 million to US$ 10.1 million (Solovey, 2020, p. 211), drastically shrinking the funding for science education programs. By focusing on the actions of Reagan's adviser and Office of Management and Budget director David Stockman (p. 215), Solovey effectively demonstrates that, although budgetary cuts were ostensibly

motivated by cost-cutting imperatives, the administration was in fact driven by worries concerning the ideological underpinning of the social sciences. At the same time, Solovey shows that it became clear, for social scientists, that "mounting an effective case against the cuts (and addressing associated threats to the scientific identity of the social sciences and their standing in the scientific community) would require an effort that was better organized and sustained" than those of the previous decades (p. 221). Solovey thus reconstructs the creation of the Consortium of Social Science Associations (COSSA) and its institutionalization as a nonprofit advocacy organization in 1982. Through public relations, lobbying activities, and educational events, the COSSA successfully defended the NSF's budget by winning support from politicians on both sides of the aisle. This maneuver, however, was not without consequences for the epistemic status of social science: "The funding crisis prompted social scientists and those who represented them at the NSF to focus, once again, on seeking support from natural scientists and establishing their position within a unified scientific enterprise" (p. 229).

As had happened forty years before, late 20th-century social scientists stressed the differences between their work and socially-engaged action research to defend themselves from the "conservative hostility that associated them with a leftist agenda," while framing their disciplines as "part of a unified scientific enterprise led by the natural sciences" (Solovey, 2020, pp. 233–234). Once again, controversies over the funding of social sciences brought the epistemic soundness and political status of these disciplines under the spotlight. These cleavages were successfully mobilized by social scientists themselves to protect their work but determined the shape and content of scholarly work by ruling out theoretical frames and practically-engaged research projects. And yet, even when they conformed to the mainstream scientistic framework, NSF social sciences remained deeply challenged by budgetary cuts, the continuous delegitimization within the federal organizational structure, and "a revival of critical statements from powerful natural science and engineering figures" (p. 250). Moreover, the opponents of NSF social science — and the scientistic approach — were also to be found outside the agency. In Chapter Nine, Solovey brings New Left ideologies, the implications of Thomas Kuhn's philosophical work, and the rise of the interpretive tradition captained by the cultural anthropologist Clifford Geertz into the picture. In doing so, the author tries to highlight how these ideas sparked a rethinking of what the social sciences were, and how and why they were to be practiced — thus leading to reform proposals that successfully reshaped their epistemic and political identity. Lastly, Chapter Ten provides the reader with a quick overview of major debates and developments from the late 1980s up to the present day, together with Solovey's reflections on the possible scenarios of the relationship between public funding and the American social sciences.

Social Science For What? is, in our opinion, a major step past *Shaky Foundations* (Bortolini, 2014a). Here Solovey is more careful to define basic terms, like "scientism," which might convey an ambiguous meaning, and he is more willing to outline the positions of individual social scientists — a strategy which not only allows him to depict a plural and complex scientific field as opposed to providing a doxastic or simplistic representation, but also shows the cleavages running *through* the individual scientists (and politicians and policy-makers as well) who had to play a part in the game (see also Bortolini, 2021b). In the case of Harry Alpert, for example, Solovey answers one of the questions that his previous book had left pending: did social scientists advancing the cause of the behavioral sciences really believe in their scientistic-oriented approach? Alpert's "carefully restricted" (and, in the end, successful) strategy for advancing the cause of the social sciences in a rather hostile environment at the NSF (Solovey, 2020, p. 55) is now completed with an analysis of his scholarly positions on the work of Émile Durkheim,

the relationship between sociology and social problems, and interpretive methods which aptly extend the sketch presented in Solovey & Pooley (2010). Alpert's image is now that of a clever strategist, one who understood the conditions under which his action took place without bowing completely to the ideology of scientism. As we said, this is an acknowledgment of the fact that cleavages ran (and run) not only between groups, parties, and institutions, but also within individuals.

2 Interpretive Social Science and (Cross)Disciplinary Stratification

Had Solovey given the same treatment to Clifford Geertz, the book would have acquired a deeper texture, one that would take the wider shape of the macro-field of American social science and the effects of academic stratification into full consideration. As introduced by the quote opening Chapter Nine (Solovey, 2020, p. 275), Geertz looks like a staunch adversary of the kind of social sciences championed by the NSF and its accompanying elite. In the words of a colleague, though he was "among the most distinguished anthropologists of his generation" (p. 283), Geertz would have not been able to receive funding from the NSF because of his anti-scientistic, interpretive approach — a kind of neglected foe that deserved much more. But the very fact that the quote itself comes from Geertz's 2001 retrospective account of the founding of the School of Social Science at the Princeton Institute for Advanced Studies — that is, the most revered academic institution in the world — should alert the reader that the story of the School's "long-time leader" must have been much more complicated than it seems.

While Geertz maintained a steady publication record from 1956 onwards, it was his 1973 collection, *The Interpretation of Cultures*, that made history: thanks to a fascinating methodological first chapter and an exceptional exemplar of what interpretive social science could do — that is, "Thick Description" and "Deep Play: Notes on the Balinese Cockfight" — the many essays collected in the book, written between 1957 and 1972, acquired a new twist and became the cornerstone upon which an interpretive brand of social science could be built (Swidler, 1996; Davis, 2009; Alexander & Smith, 2011; Cossu & Bortolini, 2015). And yet, looking only at post-1973 Geertz through his own retrospective accounts does not help to understand (1) how interpretive social science came into being and (2) how Geertz differentiated it from other strands of non-scientistic social science. A quick ride through the bare facts of his academic and intellectual trajectory shows Geertz as a Janus-like figure, from the point of view of both inter/disciplinary work and that of the relationship between scientism and interpretivism.

A graduate in philosophy at Antioch College, Clifford Geertz arrived at Harvard as a PhD candidate in social anthropology in 1950. He soon developed a close connection to Talcott Parsons — that is, one of the alleged champions of scientistic sociology and the man who was asked by the Social Science Research Council to write a "lost" essay to support the inclusion of the social sciences in the NSF (see Solovey, 2020, pp. 44–45, pp. 288–289)[3] — and became one of his most esteemed students (for a discussion of their relationship see Cossu, 2022). After a period of fieldwork in Java with his wife Hildred, also a graduate student in anthropology,

3. Today Parsons is generally considered as a strong supporter of the scientistic understanding of the social and behavioral sciences, a view that we are not going to challenge (see Abbott & Sparrow, 2007; Calhoun & VanAntwerpen, 2007; Steinmetz, 2007; Bortolini, 2021a, Ch. 4). At the same time, his positions have always been much more nuanced and problematic than Solovey seems to acknowledge, as shown by his many editorial statements in the *American Sociologist* (the professional journal of the American Sociological Association he founded in 1965) during the hot debates on the status and the vocation of sociology of the late 1960s. (See, e.g., Parsons, 1965, 1967)

as part of a multidisciplinary research group sponsored by the Ford Foundation, Geertz went back to Cambridge, where he was employed as a research fellow at Harvard and MIT. There he worked shoulder to shoulder with Parsons (and another of the latter's former students, Robert Bellah) on a theory of religion and, on his own, on a host of projects in the anthropology of development which were clearly embedded in modernization theory (Price, 2016). He then left in 1958 for a year at the Ford Foundation-sponsored Center for Advanced Studies in the Behavioral Sciences in Palo Alto (again, thanks to Parsons) and was given a post as an assistant professor at Berkeley. He moved to the University of Chicago Department of Anthropology in 1960, was promoted to full professor in four years, did more fieldwork in Indonesia and Morocco, and finally settled down at the Princeton Institute for Advanced Study (IAS), where he animated the School of Social Science from 1970 to his retirement in 1996 (and in fact until his death in 2006)(see Geertz, 1995, 2000).

As shown by Andrea Cossu (2021; see also Bortolini & Cossu, 2020), Geertz's relationship with Parsons was more than instrumental: in the mid-1950s the anthropologist was an expert practitioner of Parsonian systems theory and, at the same time, firm in indicating the need to incorporate various philosophies of symbolic forms and theories of literary criticism (Cassirer, Langer, Burke, Ryle, etc.) as the foundations of a full-fledged theory of culture. In this sense, the nod to the (not so seminal) work by Parsons and Edward Shils on values and motives of action at the beginning of his famed essay, "Religion as a Cultural System," was but a springboard towards a more hermeneutically-oriented approach to meaning (see also Bortolini, 2021a, p. 135 *ff*). At the same time, his finely-textured ethnographies (Geertz, 1963a, 1963b) and the repeated use of the "cultural system" tag well after his emancipation from Parsons's systems theory — "Art as a Cultural System" was published in 1976 — can be thought of as a relic of an early phase of Geertz's development only if his approach's clear match with Parsons's four-function model and the chronology of his work in the 1960s and the 1970s are blatantly ignored (Cossu, 2021).

This is not only to say that, as almost all sociological models of the intellectual field would predict, conceptual and paradigmatical innovation typically comes from the students of influential, powerful masters (Bourdieu, 1984; Collins, 1998; Frickel & Gross, 2005; Camic, 2021). It is also to underline that the individuals and research groups that were better placed in the various disciplinary and cross-disciplinary hierarchies of high modernist social science had sufficient symbolic and relational capital to secure access to sizable amounts of economic capital from diversified funders with relative ease and no strings attached (Heyck, 2015). One such example is the "Senior Research Career Fellow" grant that Geertz received in 1964 from the National Institute for Mental Health, a source of funding that assured him "stable career opportunities" as a recognized "scientist of superior potential" until 1970. This award would cover salary, fringe benefits, and other costs under the heading of "Comparative Studies in the Theory of Culture" — not exactly a "mental health" issue.[4] Second, the whole endeavor of the Committee for the Comparative Study of New Nations, of which he was a member, the secretary, and finally the chairman while at Chicago, was created under the auspices of typically

4. The expressions are cited in a standard letter that the chief of the research fellowship section of the NIMH, B.E. Boothe, sent Parsons to ask him to assess Geertz's promise in the Fall of 1963. Needless to say, Parsons's reply was enthusiastic. The grant was awarded in 1964 and lasted until Geertz moved to the Princeton Institute in 1970 (Parsons, 1963). The papers of the Chicago Anthropology Department show that Geertz was not the only anthropologist to receive NIMH money — apart from smaller grants, another champion of cultural and symbolic anthropology (and Parsons's former student), David M. Schneider, received a NIMH grant from 1961 to 1971. See, in particular, the papers of the Chicago Anthropology Department, University of Chicago Special Collections, box 100, folder 5 "Clifford Geertz (NIMH)."

high modernist circles but never hindered him from developing his interpretive paradigm — as the papers he wrote from 1960 to 1970, which make up the most part of *The Interpretation of Cultures*, clearly show (see Geertz, 1973; Warner, 2011). Third, the position he came to occupy in 1970 as the first professor at the IAS School of Social Science — an enviable place where faculty members enjoy unmatchable allocations of economic, relational, and symbolic capital — further demonstrates that the adjective "mainstream" has, in social science as elsewhere, at least two, non-perfectly overlapping, meanings (that is, structural and ideational).[5] Lastly, it is well known that after his arrival at the IAS Geertz was often called on to evaluate research proposals by both the NSF and NEH, and later became one of the main architects and animators of the SSRC's Joint Committee on Southeast Asia, which from the very beginning developed a strong "interpretive" orientation (see Szanton, 1976).

At least in top cases such as Geertz's, sheer prestige — itself dependent on the participation in, and the support of, the "right" networks — might account for access to funding and grants, rather than any particular methodological or epistemic approach (for a similar assessment of Bellah's career see Bortolini, 2019). This is not to belittle Geertz's genius or originality nor to generalize the conditions of the top echelons to all social scientists. The point, however, is clear: social structural positions were as important as ideas and paradigms in accounting for the likelihood of being lavishly funded (and enjoying the intellectual freedom ensured by such funding). Obviously, this means that there were many different funders in the field — a fact that any reader of Solovey's 2013 book, *Shaky Foundations*, and other fine volumes in the history of postwar social science knows all too well (Turner & Turner, 1990; Parmar, 2012; Turner, 2014). Here we would like to briefly call attention to the National Endowment for the Humanities (NEH), one important but neglected source of money and prestige, whose history up to 1985 (the end of Reagan's first term) might help the reader in better assessing the relationship between scientistic and interpretive social science — the latter, again, taken as but one example of a minority, excluded, or maverick approach. At the same time, we would like to survey some of the implications that the distribution of various kinds of capital throughout the field had on the shape and content of the macro-field of American social science.

3 The Funding of "Humanistic" Social Science

Created in 1965, the NEH "provided some support for humanistic social research," immediately granting US$ 150,000 to "sociology, political science, anthropology, and government, plus $1.6M spread across [...] archaeology, history, jurisprudence, and linguistics." While Solovey (2020, pp. 130–131) passingly acknowledges that this source of funding "may have led some legislators to believe" that the support of humanistic social inquiry could be achieved in a third way — that is, neither inside the NSF nor through a national foundation dedicated to the social sciences — *Social Science for What?* remains silent on the NEH's impact on American social science. And yet, this agency had a pivotal role as an alternative source of federal funding *and* symbolic capital for the interpretive strands of the American social science

5. This is not the place to discuss Geertz's adventures at the Princeton IAS. Suffice it to say that although he and IAS director Carl Kaysen failed to coopt sociologist Robert Bellah as the second professor at the School of Social Science in 1972 (Bortolini, 2021a, Ch. 10), all subsequent picks were interpretive and interdisciplinary scholars: Albert O. Hirschman (in 1974), Michael Walzer (in 1980), and Joan Wallach Scott (in 1985), thus consolidating a general "soft" orientation of what has consistently been considered one of the top ten academic institutions in the world (Scott & Keates, 2011). For a general assessment of institutes for advanced study, see Padberg (2020).

that did not find a place in the NSF — the presence of which shaped the NEH since its very early steps. In his 1984 book, *Excellence and Equity*, Stephen Miller reconstructs the story of the first twenty years of the NEH by highlighting the Endowment's complex relationship with Congress and its role as the gatekeeper for the humanistic disciplines, focusing on some of its undertakings (including educational activities directed to the general public, the conservation of books, and the funding of libraries) to underscore how these influenced the form and the status of the humanities in the United States.

According to Miller (1984, p. 4), "some observers, including several who played major roles in the founding of NEH, believe[d] that [...] the true counterpart of NEH [was] the National Science Foundation." The NSF was brought into the debate right at the beginning of the process of imagining a federal agency to support the humanities, as testified by Barnaby Keeney — at that time the president of Brown University — who wrote, "the scientists have their Foundation; the humanists should have theirs" (quoted in Miller, 1984, p. 9). The National Science Foundation, however, was not only cited as a rhetorical means of justifying the creation of a "twin" agency for the humanities. The relationship between the two federal institutions, and its likely effects on the development of the social sciences in the United States, was a cause for considerable doubt. As historian John Highman commented a year after the establishment of the NEH, "to conceive of [the humanities] as distinct from the social sciences in the exercise of qualitative judgment is to perpetuate stereotypes colored by the dislocations of a generation ago" (quoted in Miller, 1984, p. 19). Hard scientists were worried too, as Miller forcefully demonstrates by commenting on a letter written by NSF director, physicist Leland Haworth, in 1965. While in favor of the federal funding of the humanities, Haworth expressed his contempt for "those who spoke glibly of humanistic or humane values, thereby implying that the sciences were inhumane" (quoted in Miller, 1984, p. 20).

When it comes to its purpose, the NEH looks strikingly similar to the NSF — that is, a "grant-making agency" which "devises categories of support and invites individuals or institutions [...] to apply for grants in particular categories," awarded after a peer-review process. According to Miller (1984, p. 28), this organizational structure reflected a precise political intent: legislators strived to prevent the National Endowment for the Humanities from having too much leeway when it came to distributing funds under pressure from lobbyists and politician — mirroring the fears that emerged during the founding of the NSF, as Solovey carefully shows throughout his book. The National Science Foundation and its history thus worked as organizational models for setting up other federal agencies.

In the case of the NEH, however, legislators spelled out the disciplines that were going to benefit from the agency's funding from the very start. Along with "language, both modern and classical; linguistics; literature; history; jurisprudence; philosophy; archeology; the history, criticism, and theory of the arts," the earliest official definition of the humanities included "those aspects of the social sciences which have humanistic content and employ humanistic methods." In 1968 Congress expanded the definition to include "the study and application of the humanities to the human environment," and in 1970 added: "With particular attention to the relevance of the humanities to the current condition of national life" (Miller, 1984, p. 29). Though the budget increased slowly during the first years — from US$ 2.5 million in 1966 to US$ 6.05 million in 1970 — there was a "dramatic growth of NEH's budget" under the chairmanship of Ronald S. Berman, a Nixon appointee — from US$ 11.1 million in 1971 to US$ 72 million five years later (p. 32). The Reagan administration continued along these lines and increased the agency's budget "from $77M in 1981 to $96M in 1982 and $112M in 1983" (p. 56). In other words, it was precisely during the period that Solovey calls the "dark days" for

the American social sciences at the NSF, that the NEH budget grew most spectacularly.

To complement Miller's (1984) reconstruction, we conducted an exploratory survey of the NEH database to investigate the place social scientists had carved out for themselves in this federal agency.[6] For this overview, we looked at information about grants allocated from the agency's debut up to 1985, considering all the different grant programs (i.e., basic research, fellowship, summer stipends, challenge grants) and restricting the disciplinary fields of the projects financed to include only the social sciences.[7] In doing so, we collected information on 1626 social scientific projects financed either by the NEH alone or by the agency and private foundations through matching funds and/or gifts from 1967 to 1985.[8] Total funding allocated to projects in the social sciences by the National Endowment for the Humanities in this timeframe amounted to US$ 65 million. The smallest grant, US$ 122, went in 1979 to historian Paul K. Conkin for a seminar for college teachers (grant FS-136378-79), while the biggest project financed, presented by Soviet specialist Allen H. Kassof, received US$ 2.5 million for a seven-year period (grant RI-2139-81). Looking at the distribution of funds, the latter project is something of an outlier. In fact, the first quartile is US$ 3,118, while the median — which divides the distribution in half — is US$ 15,000. The value of the third quartile is somewhat more than twice the median: 75% of the social scientific projects funded from 1967 up to 1985 received less than US$ 36,286. This means that, compared to the size of the budgets reported by Solovey, it is evident that the NEH *cannot* be considered a player at the level of the NSF in funding the social sciences.[9]

At the same time, we believe that looking at the distribution of funds over time can provide the reader with insights on the role the NEH had in the development of the American social sciences in the second half of the last century. In 1969, for example, social scientific projects at the NEH received nearly US$ 250,000 — with anthropology and sociology receiving US$ 15,240 and US$ 21,000 respectively, and the rest of the sum going to general social science — while the NSF social sciences had a budget of US$ 15 million (Solovey, 2020, p. 87). This massive imbalance was maintained throughout the 1970s. In 1975 — when the Social Science division at the NSF was abolished — the agency allotted US$ 27 million in funds. In the same year, NEH social scientific projects received US$ 3.8 million, of which almost US$ 400,000 was for anthropological research. The trend started to change at the end of the 1970s, when the budget for social scientific projects at the NEH increased dramatically, while at the NSF social sciences were in crisis after their division was closed and the "dark days" of the Reagan administration were about to start. In 1980 social scientific projects at the NEH received US$ 15.6 million — which, if compared to the US$ 52.5 million provided by the NSF, starts to look significant. In the following year, the NEH granted US$ 9M to social scientific research projects — more than one-fifth of the by-then diminishing NSF social science budget, which in that year amounted to US$ 43.7 million. In 1982, the NEH granted US$ 4.6M to the social sciences, while the NSF budget for social scientific research dropped dramatically to US$ 32.6

6. Available at https://securegrants.neh.gov/publicquery/ (last accessed November 24, 2021).

7. In particular, we selected the following disciplines: anthropology, cultural anthropology, economics, social science: general, geography, linguistic anthropology, social science: other, psychology, and sociology.

8. Projects financed through private funds were also submitted to a peer review process, and they typically received a combination of program ("outright") funds and matching funds. (See Miller, 1984, pp. 33, 162). In 1966, the agency's efforts were devoted almost entirely to planning. In fact, the budget for that year was spent in 1967 (p. 30).

9. A more recent discussion of the structure of funding for the social sciences is presented in Grossmann, 2021, Ch. 5.

million (p. 228). This trend continued in subsequent years: in 1983 and 1984 the NEH allotted US$ 6 million per year to social scientific research projects.

We recognize that the funds for the social sciences granted by the NEH were far from matching what the NSF could provide, but we would also call attention to the fact that different styles of research might require different budgets. Though it is true that in 1966 the NSF's budget for oceanography alone amounted to US$ 29.1 million, while the whole of the social sciences received a mere US$ 13 million (Solovey, 2020, p. 87), it is also true that the two research endeavors were dependent on completely different technological equipment and instrumentation — this marks a crucial difference between the two approaches to research, and one that completely changes the timing of the discovery and the creation of a general scientific consensus that gravitates around genealogies of research technologies (Collins, 1994). A close inspection of the research projects funded by the NEH from the late 1960s to the mid-1980s might add depth and thickness to the picture sketched in Chapter Nine of Solovey's book — especially when one considers that anthropological disciplines had the lion's share of this federal agency's social science funding. In 1973, for example, research projects categorized as anthropology, cultural anthropology, and linguistic anthropology received almost one third of the social sciences budget (US$ 427,200 out of US$ 1.3 million), while in 1976 projects that indicated anthropology and cultural anthropology as their first discipline were assigned one fifth of the total funding for the social sciences (US$ 211,372 out of US$ 972,786). Interestingly, two of the three best-funded 1973 projects promoted fieldwork in a crucial country for Cold War global politics — namely, Afghanistan — well before Senator Proxmire awarded Geertz's student Sherry Ortner his Golden Fleece Award in 1979 for her research on Nepal's Sherpas (Solovey, 2020, p. 150), triggering a debate that shed light on the role of social scientific research for the development of American foreign policy.[10] In 1977, eight top anthropologists — including Clifford Geertz and his then-wife Hildred — publicly criticized the NSF anthropology program top officer, Nancie Gonzalez, for what they viewed as a scientistic bias in the allocation of funding. Responding to the eight scholars calling for recognition, Gonzalez wrote that "much respectable social anthropological research addresses humanistic, rather than scientific questions" (quoted in Solovey, 2020, pp. 283–284). That same year, research projects in anthropology presented to the NEH received grants for another US$ 200,000.

Indeed, during the 1970s and 1980s, the humanistic style of research that challenged the scientistic approach to social sciences did receive institutional support from the National Endowment for Humanities, an agency that, at least in the eyes of the Congress, had the same status as the National Science Foundation. Even when budgets for social scientific projects at the NSF were rapidly decreasing, the NEH allocated a steady flow of economic and symbolic capital to a niche group of researchers who did not conform to the NSF style of social science.[11] Just to

10. The three projects were entitled "A Film Record of the Pashtoon People of Afghanistan," "Pashtoon Nomads of Afghanistan Project," and "The Apacheans: A Comparative Study in Culture and Cultural Dynamics." Three years later, the best-funded projects were "The Agnicayana Ritual," "An Analysis of the Anthropological and Narrative Functions of the Multiple Frames in the Decameron," and "The Transmission of Cultural Traditions into Space."

11. Just as the natural sciences have the President's National Medal for Science (instituted in 1959, since 1980 it can be awarded to a social scientist), the NEH awards the National Medal for Humanities, created in 1997 after the Charles Franker Prize in the Humanities (1988). Winners of the former award for "Behavioral and Social Science" include R.K. Merton in 1994 and William Julius Wilson in 1998. Although Geertz was repeatedly nominated for the National Medal for the Humanities to no avail, the award was given to at least four scholars who had strong ties with him and/or, for better or worse, the Princeton IAS: Robert Bellah (2000), Bernard Lewis (2006), Robert Darnton (2011), and Natalie Zemon Davis (2012). The symbolic function

make a couple of examples which would admittedly need much more research, in 1984 Geertz was awarded a grant for providing temporary fellowships at the Princeton IAS (1985–1988) for $211,860 (grant FC-20053-84).[12] A year later, Geertz was given another $300K to provide for additional fellowships at the IAS (1988-1990), again under the banner of general Social Sciences (grant RA-20005-86).[13] Along with money for fellowships and/or individual projects, the NEH provided social scientists with funds to promote professional development and the circulation of ideas. One example was the much-appreciated program of summer and yearly seminars for college teachers, through which scholars from peripheral institutions might study and work with top-notch scholars at leading universities. To remain in the vicinity of Geertz, Berkeley sociologist (and Parsons's former student) Robert Bellah was repeatedly granted the funds needed to run summer seminars, and in 1976 could lead a year-long residential seminar for college teachers on "Tradition and Interpretation: The Sociology of Culture," which yielded not only an influential reader entitled *Interpretive Social Science*, but also some of the theoretical scaffolding of the research project which ultimately led to the publication of Bellah's co-authored humanistic (in method and orientation) best-seller, *Habits of the Heart*, a multi-year program that was financed by the NEH (grant FS-10190-76) and by Rockefeller and Ford Foundation matching grants for a total of US$ 250,000 (Rabinow & Sullivan, 1979; Bellah et al., 1985; Bortolini, 2021a, p. 219 *ff.*).[14]

Inspired by the very existence of the NSF, the NEH sponsored many research projects in the social sciences, providing institutional, economic, and symbolic support to enable scholars who did not fit in the scientistic framework that operated in the social science division at the NSF — before it was dissolved — to practice a different style of social science that, eventually, came to challenge the scientistic approach favored by the NSF. However, the distribution of economic, social, and symbolic capital tells only half of the story — the other being the influence that these dynamics had on the division of labor in the intellectual field, the timing of discoveries, and the creation of a consensus within the scientific community of sociologists.

4 From Funders to Fractals and the Sociology of Sociology

As limited as our research has been, it is clear that the conditions of the macro-field of American social science were (and still are) much more complex than *Social Science for What?* (Solovey,

of the NEH, especially in increasing humanistic research's prestige with the general public, is emphasized in Turner & Turner (1990, p. 143).

12. According to former IAS director Carl Kaysen (1976, pp. 18–19), the IAS School of Social Science had been originally funded by the Ford Foundation, the 1907 Foundation, and a few private donors. Besides the funds given to Geertz, the NEH gave another US$ 161K to the School for funding temporary fellowships following applications by Albert O. Hirschman (FC-0401-79) and Michael Walzer (FC20034-83). A further US$ 1.5 million for fellowships and US$ 1.1 million for individual projects were granted to permanent members and/or directors of the IAS.

13. The funding was provided under the category of general social science. Incidentally, this means that this grant was not considered under the heading of anthropology or cultural anthropology in the previous calculation, as well as contributing to show that many of the projects funded by the NEH in the period considered do not fall into a distinct disciplinary framework.

14. It should be added that up until that moment Bellah had been a typical armchair scholar, working solely with books and texts with the generous support of the Ford Foundation. When he decided to "go empirical" with a research group of five who used "qualitative" methods to interview slightly over 200 people and run several rounds of participant observation, he was able to secure a sum corresponding to 1M in 2021 dollars. This serves to show that scholars at the top of their field never had problems in finding money to finance their humanistic or interpretive approach. (See NCH, 1975).

2020), with its focus on the National Science Foundation, would suggest. The book was admittedly, and correctly, limited in its scope, so this is not a criticism that detracts from the merits and strengths of Solovey's work. At the same time, had Solovey presented bibliometric or scientometric data on the shifting proportion of scientistic and non-scientistic approaches over time, or a detailed analysis of the research projects submitted to the NSF for evaluation, his overall picture would have gained some strength. That kind of data would also help to assess the effective timing of the rise of scientism in American social science and the National Science Foundation's impact on the various fields. Again, our intent is not to criticize Solovey's theses, but to call attention to the complexity of the wider macro-field of American social science — and to the lack of sound and extensive sets of data that might help historians and social scientists assess the development of various styles of thought and/or methodological approaches.

As far as empirical research goes (as opposed to personal opinion on the part of social scientists or disciplinary lore, which tend to focus on the "happy few"), only a few assessments of the relative proportions of "qualitative" and "quantitative" research and/or publications in sociology and the other social sciences are available — and this is unfortunate, for such measures might serve as proxies for evaluating the impact of the NSF. The complexity of the field emerges clearly from even a cursory survey of the copious "sociology of sociology" literature typical of the early 1970s — itself a sign of the rising unrest at the boundaries of the discipline at the time of the epistemic and disciplinary revolts of the late 1960s (Sica & Turner, 2005; Bortolini, 2021a, Ch. 9). In their 1969 evaluation of the state of sociology, Neil J. Smelser and James A. Davis underlined the impetuous growth of the discipline as gauged by the increase in both students (graduate and undergraduate) and funds, which grew from US$ 2.5 million in 1959 to US$ 36.6 million in 1966 (pp. 132–139). Like Smelser & Davis (1969, pp. 147–148), James L. McCartney (1971, p. 385 *ff.*) underscored the importance and the perils of applied research financed by the NIMH, NSF, and the US Department of Agriculture. At the same time, in the early 1970s the field was a patchwork of methods, theories, journals, and departments lacking a shared definition of what counted as prestigious (Glenn, 1971; Bode, 1972).

What is interesting about the "sociology of sociology" of the early 1970s is that the quantitative/qualitative divide was seldom a topic for reflection. Most research focused on subspecialties and their relative status (Smelser & Davis, 1969, p. 148 *ff*; McCartney, 1971, p. 393 *ff*; Cappell & Guterbock, 1992), or on the relationship between applied and basic research, on the one hand, and the values and the political positioning of sociologists, on the other (Lipset & Ladd, 1972). Research done on major journals in sociology (i.e., the *American Journal of Sociology* and the *American Sociological Review*) estimated that there was an appreciable rise in quantitative and statistics-based sociology between the early 1940s and the late 1960s (Brown & Gilmartin, 1969; McCartney, 1970), but rarely did these data give rise to a current of (self-)reflection on what Solovey (2020) calls scientism.[15] More recent research on article abstracts from major journals highlighted the existence (or the persistence) of a methodological divide between qualitative and quantitative areas (Traag & Franssen, 2016; Schwemmer & Wieczorek, 2020), a trend that is somehow softened by the fact, about which most major players in the American sociological field seem to agree, that in the 2010s and 2020s most sociologists would use whatever method they deem fit to advance their research interests, without any strong prejudice (see Chen, 2020; Abbott, 1999).

Wider historical and theoretical research on American sociology seems to confirm not only

15. Given the different traditions in sociological specialties, it is clear that the quantitative/qualitative divide was more or less reproduced not only within but also across the sub-disciplines. Empirical data remain, however, scarce. On political science, see Kuehn & Rohlfing (2016).

the field's pluralism but also the counterforces and the many epistemic "revolts" that have punctuated the last seventy years. In her pioneering work on the emergence of research methods, Jennifer Platt (1996) suggested using an approach based on "incoherence, eclecticism, and lack of pattern" to study such a complex and decentred field as sociology (p. 236). In *The Impossible Science*, Stephen Turner and Jonathan Turner (1990) argued that the increased availability of research money and the diversity of funding agencies consistently produced more epistemic and methodological pluralism. Others have emphasized the continuous emergence of intrinsically inter- and multi-disciplinary fields and specialties where scientistic and humanistic approaches would mix almost by definition: to name a few, we could point to area studies in the 1950s and women's, ethnic, and cultural studies from the late 1960s onwards (Wallerstein, 1998, p. 36 *ff.*; Blasi, 2005; Rhoades, 1981). All of these were based on a rejection of (the sometimes-incompatible forms of) positivism typical of "mainstream" social science (Platt, 1996, p. 103). When combined with the many autonomous systems of stratification and allocation of material and immaterial capital, these dynamic elements contribute to a more complex, less univocal picture of the macro-field.

More recently Andrew Abbott and James T. Sparrow (2007, p. 295–297) noted that the need to map (and control) nation-wide populations typical of wartime research boosted a positivistic, survey-based approach whose roots went back to the inter-war period, and in fact, at least to the epistemic premises of the Culture and Personality movement in anthropology and Ernest Burgess's "adjustment school" in sociology (Abbott & Sparrow, 2007, p. 306). "The war furthered and perhaps consummated sociology's love affair with science," they wrote, but did not produce "what might seem the logical outcome — an applied science of openly recognized social planning based on esoteric knowledge" (p. 300). While Solovey (2020) is right in attributing the failure to advance a pragmatic version of social science to the same skepticism against political planning that sealed the fate of the social sciences at the NSF, Abbott & Sparrow (2007, p. 293) are particularly effective in shedding some light on other dynamic elements of the field, such as the demographics of PhD holders in the social sciences who came to occupy almost all the top and middle-rank positions available in the 1940s and 1950s, thus reproducing for some thirty years a very specific understanding of what proper social science looked like.[16]

It might be, then, that the main problem of Solovey's book does not depend on his narrow empirical focus on the NSF or his neglect of other players and processes. It may be that the book's limits are mainly theoretical — i.e., a better "background theory" of the intellectual and academic field would have produced a more balanced analytical story. Bortolini and Cossu (2019) have sketched the main threads of the debate elsewhere. Here we will only call attention to the model advanced by Andrew Abbott (2001) in *Chaos of Disciplines*, a book where the discipline of sociology is used to illustrate a wider theoretical pattern of intellectual life. According to Abbott, the intrinsic pluralism of a field where position-taking and diversity is the norm (Bourdieu, 1984; Collins, 1998; Baert, 2018) gives rise to waves of success and decline that have a more or less thirty-year time frame, becoming a "generational paradigm" (Abbott, 2001, p. 23). More interestingly, this cycle of rise and decline has a specific "fractal" form: since debates in the social sciences revolve around a handful of "irresolvable" basic prob-

16. In a "twin" essay, George Steinmetz (2007, pp. 319–324) argues that an "epistemological stalemate" between ethnographic and survey techniques took place in the 1930s, and then goes on to depict the rise of positivism in the following decades. Calhoun & VanAntwerpen (2007), who also write about the postwar period, maintain that so-called "mainstream sociology" (i.e., that depicted by Solovey in his book) had less of an influence on the field at large than is generally thought.

lems, each intellectual or epistemic victory is, in fact, a remapping of the field of sorts. "The triumph of a position in intellectual life," Abbott (2001, p. 18) writes, "usually guarantees that position's downfall by playing it in a new context of fractal comparison." Since the winners cannot encompass all the problems (and the solutions) that their former competitors had envisioned from their own point of view, the losing position re-emerges *within* the winning one as an "internal opposition" of sorts. These new, originally minority positions become an interface and a point of exchange with external fields and disciplines, and promote innovation, hybridization, and, finally, the start of a new cycle of rise and decline where old ideas don new clothes to respond to new/old challenges.

In a sense, the story of Geertz and Bellah "inventing" hermeneutic and interpretive social science from within the Parsonian network is a tale of fractal cycles. As the best students of one of the masters of mainstream scientistic social science, they developed an interest in one of the most cutting edges of research — that is, culture — but found themselves unsatisfied with the formulaic approach to symbols typical of systems theory. They thus cast their gaze outside social science proper, taking the philosophy of symbolic forms from the humanistic fields of literary criticism, and drawing on religious studies for the tools and methods needed to start a (new version of a recurrent) epistemic revolution. At the same time, and contrary to what some critics have maintained, neither Geertz nor Bellah gave up their identification with social *science* — they never wanted to "move" once and for all to the humanities, thus giving ammunition to both more radical (i.e., humanistic) and more conservative (i.e., positivistic) shifts from the compromise "fractal" position they had elaborated as the centripetal heirs of the mainstream network (Bortolini, 2014b; Cossu, 2021).

Maybe a re-telling of Solovey's (2020) story based upon this *canovaccio* of Abbott's (2001) fractal cycles would help make sense of some unexpected phenomena, such as the late-1960s revolts against the introduction of further statistics requirements in graduate curricula or the mushrooming of alternative, anti-positivist, and politicized approaches at a time — the 1970s — when the social sciences as a whole suffered from a general haemorrhage of funds and students.[17] These "sustainable revolutions" (again, a term coined by Turner & Turner, 1990) are only the latest example of a cyclic opposition to value-free, "purely scientific" research styles that started, for the period under consideration, with the establishment of the Society for the Study of Social Problems in 1953 (Skura, 1976). It might be, as we have admitted, that sociology is not representative of social science in general — after all, as a generalist "container" of scientific interests and sensibilities, it has never been good at excluding objects, methods, and other "things" (Abbott, 2001, pp. 5–6). At the same time, it seems to us that a combination of Abbott's self-proclaimed "internalist" approach, complete with his ecological understanding of the complex relationships between departmental and disciplinary forms of organization, and Solovey's (2020) interest in the relationships between the scientific and the political fields (and the wider public sphere, where general debates take place) would greatly improve a historical work whose inventiveness and accuracy are beyond doubt. If the various (and sometimes competing) structural and cultural models of scientific success and disciplinary primacy we briefly sketched are persuasive, the (alleged) primacy of positivist social sciences is not only due to political decisions or strategies of "passing" on the part of social scientists, as in Solovey's history of the National Science Foundation, but also to the very structure of the intellectual field of modern science — itself a social and cultural construction which might, one day, disappear.

17. Both points are emphasized in Turner & Turner (1990, pp. 143, 172–176).

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