What Were the Behavioral Sciences?

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Behavioral Sciences Today was published in 1963, when the behavioral sciences movement was in decline.[1] The essay collection, edited by Bernard Berelson, was based on a series of talks by social science luminaries, broadcast the year before on the U.S. Information Agency's Voice of America service. Berelson, the library scientist turned foundation officer, was the movement's self-appointed custodian. He had wrangled the twenty scholar-speakers, given the series its name, and arranged for the book's mainstream publication. So it was fitting that he devoted the book's introduction to the task of definition: "The term *behavioral sciences* is of such recent coinage, and the field itself so new, that it is desirable at the outset to explain the meaning of the term and the way the field came to be what it is today."[2]

Berelson struggled in the task. More than a dozen years after the Ford Foundation had launched the "behavioral sciences" label, self-described behavioral scientists had trouble defining the term. Berelson, who had run the foundation's Behavioral Sciences Division until its 1957 shuttering, had already proposed, and discarded, a handful definitions. Now in 1963 he had what was, he may have felt, his last chance.

"Perhaps the best way to start delineating the field," he wrote, "is to say what it is not." His first move was to distinguish the behavioral sciences from the more familiar social science label—a tent too big, but also not big enough. The behavioral sciences, he explained, "center" on just three social science disciplines: the "American versions" of anthropology, psychology, and sociology. Political science, he hastened to add, can be admitted too, though only that portion of the discipline concerned with "actual" political behavior. Bits of other disciplines can also claim membership: the corner of biology concerned with the physiological basis of human behavior, for example, or the subfields of economics that attend to consumer behavior, industrial morale, and related "empirical" topics.[3]

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[1] Bernard Berelson, ed., *The Behavioral Sciences Today* (New York: Basic Books, 1963).

[2] Bernard Berelson, "Introduction to the Behavioral Sciences," in *The Behavioral Sciences Today*, ed. Berelson (New York: Basic Books, 1963), 1.

Apparently unsatisfied, Berelson tried a different tack, a definition by inventory. The essay catalogs notable works in the three core disciplines, "in order to specify the matter somewhat further," followed by a long list of recently published article titles by discipline ("From anthropology: Japanese folk beliefs….," etc.)—a "reasonably good sample of what now goes on in the field."[4]

The strategy of the illustrative list was, Berelson seemed to realize, leaky. He turned next to a pair of demarcation criteria. To count as part of the behavioral sciences, a field must (1) "deal with human behavior" and (2) study its "subject matter in a 'scientific' manner." The scare-quotes around *scientific*, he admitted, refer to ongoing dispute over just what the "scientific method is as applied to human behavior," or "indeed whether it can be applied at all."[5]

Here and elsewhere, the essay is confident, pleading, and defeated all at once. In one breath, Berelson asserts that the behavioral sciences are scientific in the mold of physics and biology. "Admittedly," he pivots suddenly, "the edges of such a broad concept as the behavioral sciences are fuzzy—people have wasted a lot of time debating whether something is or is not properly included in the behavioral sciences—but the center of the concept is, I think, reasonably clear." Conceding that many scholars have been "put off by the term," his rejoinder is that the label is no less precise, nor more misleading, than the "social sciences."[6]

Berelsen pivoted once more: There is an "inherent distinction," he wrote, between the behavioral sciences and the "other" social sciences, citing the former's tendency to collect data on individuals or small groups, against the "more aggregative, indirect, and documentary practices" of economists, political scientists, and historians. The very next sentence:

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[3] Berelson, "Introduction," 1–2.

[4] Berelson, "Introduction," 5–7.

[5] Berelson, "Introduction," 3–4.

[6] Berelson, "Introduction," 3–4. The essay is pockmarked with statements of anticipated disapproval. "The newness of the field and its youthful vigor often produce unduly hostile counter-actions…. Accordingly I ask for my colleagues here the same detachment and sympathy that are given to students of other subjects that do not carry the emotional load of this effort…" 4–5. Or: Professionalization of behavioral science has "meant a high degree of quantification of data, so much so that the critics are afraid that the behavioral sciences are seeking to force human behavioral into a statistical straitjacket." 8.

> But it is not particularly useful, and usually it is downright tiresome, to argue over the definition in order to clear up every possible misconception, exception, or presumed contradiction. They cannot all be cleared up in any case, nor is there any compelling reason why they must be. For our purposes the term has a reasonably coherent meaning that distinguishes it sufficiently from related concepts, and this meaning will be increasingly communicated, I trust, by the papers that follow.[7]

Tacking between cocksure scientism and defensive resignation, Berelson punts. The behavioral sciences are what the reader will find in the book's chapters.

Each of the volume's authors is American, and in nearly every case a past recipient of Ford Foundation funding.[8] In that respect Berelson is right that the chapters are a proxy for the field. In his introduction, he repeatedly called out the American character of the behavioral sciences: "[T]he field has become technical and quantitative, segmentalized, 'modernized,' and 'groupized'—in short, Americanized."[9] The field of "American behavioral science," he added, could never have emerged without the "support and encouragement over the years of the major American foundations." He singled out Ford:

> The interest in clarifying the term, I might add, has not been altogether intellectual in character. Although the phrase 'behavioral science' was used from time to time over a period of years, it never caught on until about twelve years ago when the Ford Foundation used the term as a shorthand description of its program on Individual Behavior and Human Relations […] and supported this field with several millions of dollars. It was then that the term came into widespread use, and it was then that some people began to wonder whether they too were not behavioral scientists after all![10]

Berelson's decision to let the chapters speak for themselves was, in its way, an admission of definitional defeat. It was also a risky move, since the volume's essay-broadcasts are all over the map. Many of the authors avoid the "behavioral science" language altogether. Some even register dissent or distaste. And the book's concluding essay, by sociologist Robert Merton, comes off as willfully ambiguous about its object. Though Merton, Berelson's former colleague at Columbia, uses the "behavioral sciences" language throughout, the essay treats the term as a straight synonym for the "social sciences." Each time he invoked the former, in other words, the actual referent was the latter. Merton traced the fields' origins to nineteenth century Europe—downplaying their American character—while repeatedly including economics, history, and political science in the "behavioral sciences" ranks.[11] This was rhetorical bait-and-switch, with Merton—quietly skeptical of the term, as we now know—swapping in, but otherwise ignoring, the collection's ostensible object. And with writerly tact, so that the rebuttal of Berelson's demarcation project remained unflagged.

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[7] Berelson, "Introduction," 4.

[8] Eighteen of the book's twenty authors were male. The two exceptions are Cora DuBois's chapter on anthropology, and Anne Roe's co-authorship of the "Evolution of Behavior" chapter. On the Ford funding, see Berelson, "The Ford Foundation Behavioral Sciences Program Final Report, 1951–1957," 1957, Reports 0150548, Ford Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, NY, A1–A12. References to the Ford Foundation Archives are abbreviated to FFA hereafter.

[9] Berelson, "Introduction," 8. Here Berelson is quoting his own 1956 essay: "The Study of Public Opinion," in *The State of the Social Sciences*, ed. Leslie White (Chicago: University of Chicago Press, 1956), 304. With some irony, given that he had tagged the field as American throughout the essay, Berelson noted in the 1963 introduction that "the war cut down so heavily on behavioral science activity abroad that the United States emerged as far and away the major practitioner in the field—so much so that many observers feels that the field has an overly American character." Berelson, "Introduction," 10.

[10] Berelson, "Introduction," 10, 4.

This essay takes up the question that Berelson was unable to answer, set in the past tense: What *were* the behavioral sciences? My motivation is to furnish historians with a working definition. In the fast-growing literature on the postwar U.S. social sciences, most scholars, myself included, lap up against the "behavioral sciences" phrase. Many of us quote the label, or slot it into sentences, without remark or qualification. Is that bravado or diffidence? Either way it's a wise tack, a silent surrender to a baffling yet indispensable referent. The usual practice—to use the actors' definition—won't do for the "behavioral sciences." There is no stable, let alone coherent, meaning to adopt.

Another factor complicating the attempt to pin down a stable definition is that the formation which the term seeks to name predates its coinage. By the time the label was launched by what was—as even Berelson conceded—the Ford Foundation's perlocutionary fiat, the sense of a shared project was already felt. The term was, moreover, meant as a descriptive—to identify an on-the-ground vanguard. But it was also future-facing, as a promise or a project. The movement's aspirational character, I will argue, turns out to be one of its defining features.

A related complication is that some social scientists in the behavioral sciences mold resisted the label, often for its sudden, foundation-sponsored faddishness. Many others embraced the label opportunistically, with the aim to win space at the Ford trough.[12] Even figures who didn't much like the term, such as Merton or Edward Shils, were members by ascription.

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[11] Merton: "But contrary to what has sometimes been alleged, both by American chauvinists and by critics of these sciences elsewhere, it is *not* the case that the behavioral sciences are a distinctively American product." Merton, "The Mosaic of the Behavioral Sciences," in *The Behavioral Sciences Today*, ed. Bernard Berelson (New York: Basic Books, 1963), 250.

A further complicating factor is that there was, in this same period, a rival claimant to the "behavioral sciences" term, centered on the University of Chicago's James Grier Miller's efforts to establish a Committee on the Behavioral Sciences, beginning in 1949.[13] The Committee was disbanded with Miller 1955 departure for the University of Michigan. It was there that Miller's idiosyncratic, "living systems theory" rendering of behavioral science took root, at the Mental Health Research Institute he founded, as propagated through conferences and the institute's *Behavioral Science* journal. To add to the confusion, Miller repeatedly took credit for coining the phrase.[14]

A final issue is that the label itself persisted, Zombie-like, for three decades after the movement's mid-1960s dissipation. The federal government kept the phrase on life support, as the house synonym for social science at civilian funders like the National Science Foundation and in military agencies like the Advanced Research Projects Agency (ARPA). The catch-all phrase "social and behavioral sciences," meanwhile, became a commonplace in the secondary literature, alongside a definitional devolution of behavioral science to psychology alone. With the spectacular popularization of behavioral economics in the new millennium, the "behavioral sciences" phrase experienced a great revival. The label was restored to a place of professional pride by self-appointed behavioral scientists, then granted official status in U.S. and UK agencies devoted to nudging and choice architecture. This, however, was not Berelson's mid-century behavioral science.

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[12] Consider these two typical statements, both from presumed allies. In a 1955 speech, psychologist Gordon Allport said, "Personally, I am not entirely happy with [the term] since the science we seek is a science of feeling, of thought, of dreams and of silence, quite as much as of behavior. But philanthropic foundations seem to like the name *behavioral science*, and we shall raise no objection to it lest Cinderella miss her chance to ride in a golden coach provided by the Foundation." Quoted in Ellen Herman, *The Romance of American Psychology: Political Culture in the Age of Experts* (Berkeley: University of California Press, 1996), 133. In what was a Ford-sponsored report, economist Allen Wallis, wrote, "To be candid, however, I suspect that the rapid adoption of the term 'behavioral sciences' in the past five years is to be explained scarcely at all by such [intellectual] considerations but almost entirely by two facts about The Ford Foundation: (1) it uses the term; (2) it has large resources." W. Allen Wallis, "The 1953-54 Program of University Surveys of the Behavioral Sciences," April 18, 1955, Reports 002918, FFA, 3.

[13] See Philippe Fontaine, "Walking the Tightrope: The Committee on the Behavioral Sciences and Academic Cultures at the University of Chicago, 1949–1955," *Journal of the History of the Behavioral Sciences* 52, no. 4 (2016), for a detailed history of the Committee and Miller's central role.

[14] See, for example, James G. Miller, "Toward a General Theory for the Behavioral Sciences," *American Psychologist* 10, no. 9 (1955): 513

Despite these challenges, this paper makes the definitional attempt. My approach is to examine the Ford Foundation's 1950s Behavioral Sciences Program (BSP), with the aim to extract core features from the archival record. More important than the conflicting meanings that Berelson and others assigned to the phrase was what they did, how they came to do it, and with whom. I pay special attention to the divergent fates of political science and economics within the program. It is a messy story, filled with jurisdictional maneuvering. The benefit of lingering on these BSP developments is to draw out the *social* character of the behavioral sciences movement. Its intellectual commitments, the Ford record helps to show, were fashioned by a particular community of postwar U.S. social scientists. That small-world network was populated by sociologists, social psychologists, political scientists, and anthropologists, many of whom had served together in the World War II propaganda and morale bureaucracy. Economists, with notable and telling exceptions, had developed their own, largely segregated postwar community.

That pattern of self-segregation was echoed at Ford, with funding for economics relegated to its own division. Political scientists, meanwhile, won de facto membership in, and funding from, the BSP. The two disciplines' discrepant BSP experience helps to establish the broader point: The mainstream of economics developed—indeed flourished—outside the behavioral sciences movement.

I build out these and other claims about the movement's wider contours in the paper's second and third sections. The second section considers the work of Hunter Heyck, the historian of social science who has, more than any other, taken on the definitional task. Heyck's approach is an indispensable aid in making sense of the behavioral sciences movement. At the same time, I argue, his argument's grounding in the life and career of a singular figure, Herbert Simon, leads him to over-emphasize the importance, for the movement as whole, of mathematical formalism. One consequence is that Heyck, at least implicitly, sweeps economics into the behavioral sciences fold. Reading his more recent treatment of ”high modern social science” against his earlier attempts to define the behavioral sciences helps to show that the two formations only partially overlap.

The third and final section attempts to set the behavioral sciences' temporal scope. I argue that the movement was already underway in the immediate postwar era, years before Ford furnished the name. The program's fast decline came roughly two decades later, in the mid-1960s. I point to three major factors: a shift in the patronage system, the staggering expansion in the scale of U.S. social science, and high-profile revelations of secret military and CIA funding of behavioral science projects.

So what *were* the behavioral sciences? Here is a tentative definition, which the paper will refine. They were a self-understood vanguard of U.S. social scientists—all the mainline disciplines except economics—forged in shared World War II service, maintained through funder-enabled networks of the early Cold War, and characterized by a mix of nomothetic confidence and aspirational scientism, until the movement's decline in the mid-1960s. For a serious interval, the movement was, like Berelson's Voice of America series, "beamed in English throughout the world."[15]

I. The Behavioral Sciences at the Ford Foundation, 1948–1957

The rapid uptake of the "behavioral sciences" label was the direct result of the Ford Foundation's 1951 decision to name its social science unit the "Behavioral Sciences Program" (BSP). Thus the search for a stable definition must take account of developments at Ford. As Berelson's 1963 reflections attest, however, the foundation's officers and consultants never settled on a coherent meaning for a moniker they launched to remarkable, and enduring, effect.

One reason to recount the foundation's failed struggle to pin down its own label is that the story, convoluted as it is, furnishes the beginnings of a working definition. To extract that limited coherence requires some remove from the disputatious pointillism registered in the archives. Another strategy, a complementary one, is to read the foundation's behavioral sciences program as self-exemplifying—as a nesting-doll enactment-in-miniature of what was, however, a broader movement.

Read synoptically and with charity, the Ford story helps to show that the behavioral sciences had a dual character: The movement was a *social* formation, built up from a dense web of pre-existing interpersonal ties. At the same time, behavioral scientists were bound by a set of shared *intellectual* commitments. In practice and over time, these features proved to be mutually reinforcing.

Many of those social ties were forged in World War II service, in what was a proving ground for cross-disciplinary, team-based research in a quantitative key. The Ford christening of the "behavioral sciences" was, then, a new name for a movement already underway. That is a crucial caveat, a caution against treating Ford's short-lived BSP as a stand-in for a formation that arose before and carried on after.

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[15] Bernard Berelson, "Editor's Foreword," in *The Behavioral Sciences Today*, ed. Berelson (New York: Basic Books, 1963), v.

The Ford story is helpful, instead, as a refraction on the behavioral sciences' social and intellectual dimensions. At core, Ford's officers and advisors had in mind an *empirical* science, one grounded in the study of actual human behavior. This tenet was compatible with, and typically accompanied by, a view that empirical findings could be inductively accreted into general theories. A decisive corollary was that economics—its emerging neoclassical mainstream at least—did not belong. As I describe below, the feeling was largely mutual: Most economists opted out.

Mainstream economists' allegedly rival model of science—deductivist and reliant on utility-maximizing assumptions—was frequently invoked by both economists and other social scientists. The dividing line, at Ford but also elsewhere, wasn't merely intellectual. As I develop more in the next sections, the split had significant roots in what was a largely segregated experience of wartime service. If World War II was the economist's war, it was also—to use a deliberate anachronism—the behavioral scientist's war. Those already existing divisions shaped the Ford agenda as much as, even more than, Ford shaped the movement it named.

At Ford, the behavioral sciences stood for a more rigorous, *scientific* social science. As the BSP context shows, however, the member sciences were sociology, psychology, anthropology, and political science. By policy, and through the pattern of funded projects, economics was a science of, and on, its own.

A. The Gaither Report

The "behavioral sciences" term was already in limited circulation by the mid-1930s, deployed in distinct but overlapping ways by political scientist Arthur Bentley and psychologist Clark Hull. As I have recounted elsewhere, Hull's former student, Donald Marquis, played the pivotal role in introducing the label to Ford.[16] Marquis was the foundation's key social science planner during Ford's late-1940s transformation from a minor regional philanthropy into the world's largest foundation. For Marquis, the "behavioral sciences" label was a layabout alternative, an encumbrance-free near-neologism that could, on the one hand, avoid the recurrent conflation of "social science" with "socialism" by anti-New Dealers in Congress, but also signal a linguistic break with the speculative, unscientific legacy that—to Marquis and like-minded scholars—remained a drag on social-scientific progress.

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[16] Jefferson Pooley, "A 'Not Particularly Felicitous' Phrase: A History of the 'Behavioral Sciences' Label," *Serendipities* 1, no. 1 (2016). This section includes material drawn from that paper, as well as Pooley and Mark Solovey, "Marginal to the Revolution: The Curious Relationship between Economics and the Behavioral Sciences Movement in Mid-Twentieth-Century America," in *The Unsocial Social Science? Economics and Neighboring Disciplines since 1945*, ed Roger E. Backhouse and Philippe Fontaine (Durham: Duke University Press, 2010).

Ford's makeover was the result of Henry Ford's 1947 death, which left the foundation with 90 percent of Ford Motor Company's stock. With the dramatic recovery of Ford Motor's fortunes in the immediate postwar years, the foundation instantly became the world's wealthiest philanthropy by far, with an estimated $417 million in assets by 1951 (compared to the Rockefeller Foundation's $122 million and Carnegie Corporation's $170 million).[17]

To guide the transition, Ford commissioned a Study Committee led by H. Rowan Gaither to plot a vision appropriate to the foundation's new wealth and national stature. Gaither soon recruited the Committee's six members, all academics and each charged with representing a topical "division." Marquis's remit was the Social Science Division.

Meeting in 1948 and 1949, the Committee produced an early and surprising consensus that the foundation's mission should center on the social sciences.[18] The Committee's consensus on social science, however, was tempered by unease with the prevailing "social science" label. A key fear was the recurrent conflation of "social science" with "socialism," especially by anti-New Dealers in Congress.[19] As Marquis observed in his Social Science Division writeup, there is a "fairly common confusion of social science with 'social reform' or even 'socialism.'" In the report, he aggressively refutes the association: the "spirit" of a "total system" like Marxism is "foreign to that of the social scientist," who is "more akin to the physician … sober, pedestrian, undramatic."[20]

The other major motivation for a new label—clearly related to the first—was the Committee's desire to signal a clear intellectual break with the body of social science they deemed speculative and historical. In an early 1948 talk Marquis had referred to the "traditional social sciences" as a "mixture of common sense, speculative philosophy, historical scholarship, religion, wise advice, and some science." The identical sentence appeared in Marquis's Social Science Division report.[21] Talking points prepared for a presentation to Ford trustees refer to the "many shortcomings" of the existing social sciences. "In many ways they are not scientific enough… consist[ing] of ordinary common sense or personal views rather than verified knowledge," the document reads. "Too frequently" social scientists have proposed "some sweeping world reform which they thought good."[22]

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[17] Sutton, Francis X. "The Ford Foundation: The Early Years." *Daedalus* 116, no. 1: 41–91. See also two excellent overviews of Ford's social science investments in the 1970s,: Roger L. Geiger, *Research & Relevant Knowledge: American Research Universities Since World War II* (Oxford: Oxford University Press, 1993), 99–105; and Mark Solovey, *Shaky Foundations: The Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick, NJ: Rutgers University Press, 2013), chap. 3.

[18] "Notes for Discussions with Trustees," 14 January 1949, folder 19, box 2, series I, 20003, FFA, 2. The social science focus remained constant throughout the Committee's meetings. In the fourth and last, the minutes conclude that the final report will "indicate the importance of operation in the general area of the social sciences." "Staff-Committee Memorandum #14," 12 May 1949, folder 1, box 1, series I, 20002, FFA, 6.

[19] See Pooley, "A 'Not Particularly Felicitous Phrase," 52.

[20] Marquis, "Report of the Social Science Division," January 1950, FFA, 20–23

From the beginning, the place of economics and political science in the work of the Study Committee was ambiguous. Two organizational flow charts, from separate meetings, explicitly include both disciplines under the Social Science Division.[23] In the minutes of committee meetings and related documents, economics and political science are often included when social science disciplines are listed. At the same time, the Committee's Business Division, represented by Thomas Carroll, and its Political Science Division, represented by Peter Odegard, also had prima facie claims to economics and political science, respectively, a fact that would prove crucial during often-serpentine committee deliberations. Carroll, a business school dean and Gaither's cousin, would go on to play a decisive role in the foundation's relationship to economics. Odegard, by contrast, was a marginal Committee member whose avowed internationalism was plainly out of sync with the deliberation's Cold War temper.[24]

Throughout their meetings, the Committee cycled through a series of terminological stand-ins for social science—though not as yet "behavioral sciences." Gaither soon took the Committee's still-nebulous mandate to the Trustees. A summary of his oral report indicates that he pitched "human relations and social organization" as the "central problem of our times."[25] There is no evidence that the Trustees objected to the social science focus, and memos exchanged between Gaither and his staff still indicated a social science–dominated report, with explicit references to the inclusion of economics.[26]

Gaither's staff assistant, William McPeak, took the lead in drafting the Committee's report in the summer of 1949. A field staff director in Samuel Stouffer's wartime Army Research Branch,

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[21] Donald Marquis, "Scientific Methodology in Human Relations," *Proceedings of the American Philosophical Society* 92, no. 6 (1948), 411; Marquis, "Report of the Social Science Division," 10.

[22] William McPeak, "Presentation of Program Five," 13 February 1950, folder 74, box 7, series V, 20046, FFA, 8–9

[23] "Organization of the Study," 30 November 1948, and "Organization for a Study of Policies," 17 December 1948, both in folder 24, box 3, series I, 20003, FFA.

[24] See Pooley and Solovey, "Marginal to the Revolution," 206–8, 229.

[25] "Memo to the Trustees," 23 May 1949, folder 20, box 2, series I, FFA, 2.

[26] Dyke Brown, "Comment on Ford Study to Date," 10 June 1949, folder 20, box, 2, 20003, FFA, 2.

McPeak would go on to play a major role in the BSP and other Ford initiatives as a long-serving and well-regarded officer.[27] Together with Marquis and Gaither, McPeak helped win Trustee approval for what became the Behavioral Sciences Program.

The story of how the Committee's initial endorsement of a social science mission morphed into a broader blueprint—inclusive of economic, political, educational, and international issues—is described elsewhere.[28] For our purposes the important point is that, in McPeak's 1949 draft report, support for social science shared billing with four other named programs: Area One ("The Establishment of Peace"), Area Two ("The Strengthening of Democracy"), Area Three ("The Strengthening of the Economy"), and Area Four ("Education in a Democratic Society"). To designate the social science–oriented Area Five, Gaither, Marquis, and McPeak selected an ungainly phrase, "Individual Behavior and Human Relations."[29] Area Five was the future BSP, positioned here as a basic scholarly unit intended to service the other four, more substantive areas.

The Committee's final report, better-known as the *Gaither Report*, was completed in late 1949. The "individual behavior and human relations" language remained dominant in the Area Five section, but "behavioral sciences" appeared four times—all on a single page.[30] The significant development, for our definitional purpose, is that economics was assigned to Area Three, while the other social sciences—with the ambiguous exception of political science—were separately placed in Area Five. The inclusion of a program area devoted to economic issues is itself unsurprising. Throughout their meetings Committee members had listed problems like business cycle fluctuations, unemployment, labor relations, and inflation as potential targets of foundation activity. In the Cold War context, the felt need to demonstrate the American economy's strength vis-à-vis its Soviet rival, together with the potential of a revived war economy, ensured that economic policy was a widely discussed national security issue. The Area Three write-up, like the other program area narratives, leads off with a list of activities that the "Foundation should support." The first item, for example, calls on Ford to encourage "a growing economy characterized by high output, the highest possible level of constructive employment, and a minimum of destructive instability."[31]

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[27] "Background on Personnel," 20 December 1949, folder 19, box 2, series I, 20003, FFA; John A. Clausen, "Research on the American Soldier as a Career Contingency," *Social Psychology Quarterly* 472, no. 2 (1984): 212; William McPeak, "BSP [1951–1957]: Report and Appraisal," December 1961, Report No. 003156, FFA, 3.

[28] See Pooley and Solovey, "Marginal to the Revolution," 202–15.

[29] An undated "Table to Rank Program Areas," circulated by Gaither at some point in the summer, refers to "Individual Behavioral and Human Relations." Table to rank program areas, n.d., 1949, folder 25, box 3, series I, 20004, FFA.

[30] H. Rowan Gaither, Jr., et al., *Report of the Study for the Ford Foundation on Policy and Program* (Detroit: Ford Foundation, 1949).

What is striking about the report's Area Three write-up, written by Carroll,[32] is that the narrative that follows does not address any of these substantive issues; this narrative is, instead, focused mainly on describing plans for a program of basic economic research. By contrast, for each of the other four areas the lead-off list corresponds to its write-up. The Area Two narrative, for example, is devoted to the strengthening of democracy; neither political science nor indeed any program of research appears in the text.

Even by the report's internal logic, the segregation of economics from the other social sciences is puzzling. Area Five is described, in effect, as the basic research arm of the other four areas, as the source of core scientific advances that should in turn inform the substantive goals of Areas One through Four. The report's mandate for Area Five is sweeping and inclusive of all scientific study of "man." Only two sorts of research are excluded: applied work related to the specific initiatives of the other four areas and "polemical, speculative, and pre-scientific" strands of social science, the invocation of which, here as elsewhere, provided a defining contrast to the resolutely scientistic approach that Area Five planned to support.[33] Yet economics is unambiguously claimed by Area Three. The report contains, then, a fundamental ambiguity as to what counts as the "study of man": Although Area Five claims all scientific approaches, one of these, economics, has already been ceded to Area Three.

A relevant irony is that Areas Three and Five advance nearly identical arguments on behalf of their respective sciences. Economic theory that does not face the "acid test of verification"—"test hypotheses with bodies of evidence and thereby develop useful general propositions"—is deemed "speculative" and criticized by the Area Three narrative.[34] In addition, the writeup deploys the same interdisciplinary rhetoric found in its Area Five counterpart: there is a "growing recognition that man's economic behavior ... cannot be abstracted and studied in isolation." Fields like psychology and sociology, with their "new methods and approaches," should be applied to the study of economic life, the write-up argues. Notably, the Area Three summary includes an aggressive dismissal of neoclassical economics: “Dominant ‘schools’ of economic thought have from time to time constructed overall ‘systems’ through the use of convenient but unrealistic abstractions, such as ‘other things being equal’ or the fiction of the ‘economic man,’ and these systems have subsequently been adopted uncritically and consequently misapplied by economists and the lay public.”[35] Because of its denunciation of neoclassical theory, the Area Three account is often misinterpreted as a brief on behalf of institutional economics. But this account was written by a noneconomist (Carroll) and seems inspired by the same empiricist social science convictions that inform the Area Five write-up.

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[31] Gaither et al., *Report of the Study*, 70.

[32] James L. Cochrane, *Industrialism and Industrial Man in Retrospect: A Critical Review of the Ford Foundation's Support for the Inter-University Study of Labor* (New York: Ford Foundation, 1979), 50.

[33] Gaither et al., *Report of the Study*, 95.

[34] Gaither et al., *Report of the Study*, 70; see also Robert Leonard, *To Advance Human Welfare! Economics and the Ford Foundation, 1950–1968* (Durham, NC: Institute of Policy Sciences and Public Affairs, Duke University, 1989), 4–5.

The sequence of steps that led to the anomalous Area Three narrative remains mysterious. What is clear is that McPeak wrote the Area Five narrative and that its contents reflect the full-throttled empiricist-scientistic sentiments that had dominated the Committee’s deliberations. We also know that Carroll drafted the Area Three section and that its contours reflect his particular interest in bringing social science research to business schools. Carroll was no economist, but in the absence of an economics division he had a claim to the discipline. Economic research had been treated in Carroll’s committee proposals far more extensively than in the memos Marquis circulated, an imbalance that became relevant in the decision to devise a separate program area devoted to economic issues.

In a 2010 paper, Mark Solovey and I speculated that the energetic Carroll had maneuvered his way into the Area Three writing slot, as a jurisdictional gambit.[36] It is also possible that economics was pushed out of Area Five plans by Marquis and McPeak. In his Social Science Division report, Marquis had described economics with respect but in terms that placed the discipline outside the other social sciences’ verificationist worldview. “Modeled in part after philosophy,” he wrote, “economic theory has been characterized by logical rigor and great generality. The next step of verification has, however, presented unusual difficulties because the concepts are not such as can be directly measured by observation.”[37] Perhaps the Area Five write-up’s emphasis on empirically tested generalization made economics an awkward fit—although for Carroll this issue posed no problem in his Area Three narrative.

At any rate, Marquis introduced the “behavioral sciences” label around this time, in his Social Science Division report.[38] By early 1950, Gaither, McPeak, and Marquis had settled on the

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[35] Gaither et al., *Report of the Study*, 72, 71.

[36] See Pooley and Solovey, “Marginal to the Revolution,” 212–13.

[37] Marquis, “Report of the Social Science Division,” 16.

[38] Marquis, “Report of the Social Science Division,” 34.

"behavioral sciences" phrase to describe Area Five. McPeak prepared an 18-page script for Gaither to present to Trustees—a remarkable document in its own right and notable for its definitional labors.[39]

In the script, McPeak offered a somewhat tortured explanation for the Committee's aversion to the "social science" label. "Just as those who do research to get knowledge of nature are called natural scientists, those who try systematically to learn about man and his behavior are frequently called social scientists," he admitted. But "here the parallel breaks down," because the "social sciences" term, "as we would like to use it, appears to be loosely constructed." He noted that in addition to the major academic social science disciplines, the term often refers to a broad range of professionals, including lawyers, accountants, public relations counselors, "or many other things." But since Area Five's emphasis is on knowledge of "human behavior," the Study Committee is "not concerned with all of these groups to the same extent." Rather, the focus is on those social scientists who are "becoming more scientific all the time," who have "borrowed many techniques from the natural sciences, and … devising more of their own." Rather than refer to "social scientists," McPeak explained, "we would like to use the term behavioral scientists." In a nod to Trustees' likely disinterest in such terminological nuances, McPeak half-apologized: "If you will let us use this term for the rest of the discussion, we won't use more technical terms."[40]

McPeak offered two additional, and telling, justifications. He noted, first, that the foundation hoped to also support "certain medical men, and psychiatrists, geneticists and other natural scientists, and social workers"—all of whom research behavior, but do not identity with the "social science" label. In a fateful move, he also justified the new term by restricting its application to just three of the social science disciplines, to the exclusion of economics and political science. "We are," he said, "very much interested in the sociologists, the anthropologists and the psychologists, particularly the more scientific ones." Economics and political science had appeared on the longer list of "various interpretations" of "social science" that McPeak had already cited as evidence of the term's overly broad connotations.[41] The Area Five write-up in the published Study Committee report also explicitly lists sociology, psychology, and anthropology, with no mention of the other two disciplines.[42]

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[39] William McPeak, "Presentation of Program Five," 13 February 1950, folder 74, box 7, series V, 20046, FFA.

[40] McPeak, "Presentation of Program Five," 7–9.

[41] McPeak, "Presentation of Program Five," 7–8.

[42] Gaither et al., *Report of the Study*, 95.

This three-field formula introduced a definitional ambiguity with surprising staying power, beyond the foundation.[43] Even at this early stage, the decision was, or so it seems, motivated by the structure of Ford's program areas. Political science was presumed to fall under the jurisdiction of Area Two ("The Strengthening of Democracy"), and economics under Area Three ("The Strengthening of the Economy"). But the Area Two writeup makes no mention of political science, let alone funding for the discipline. So political science's omission in the Area Five narrative proved more troubling. Soon after the *Gaither Report* appeared in 1950, complaints about the discipline's apparent exclusion began to surface.

As we shall see, the two disciplinary cases evolved in very different directions. Political scientists would go on to win de facto membership in Ford's behavioral science club. Economists, meanwhile, declined the invitation.

B. The early Behavioral Sciences Program, 1951–1953

Whatever the machinations and maneuverings, two of the five mainline disciplines found themselves outside the behavioral sciences umbrella, as tentatively defined by Ford. What happened next—their divergent fortunes at the foundation—helps to make sense of what the term actually signified. Political science was, in effect, smuggled in. Economics remained aloof, happy to claim Area Three as its own funding domain. Though Ford Foundation micro-politics were unquestionably important in these developments, such a split was conceivable only in the context of the pre-existing gulf between economics and the other social sciences.

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[43] One example of the durability of the three-field definition from 1964: "In some circles, membership in the behavioral science club is limited to psychology, sociology and anthropology. In other circles, membership is extended or perhaps limited to mathematics, psychiatry, and neuron-physiology. In many circles, however, membership is denied to such fields as history, economics and political science." Bernard M. Kramer, "Social Context as a Determinant of Behavior," in *Unfinished Tasks in the Behavioral Sciences*, ed. Arnold Abrams, Harry H. Garner, and James E. P. Toman (New York: Williams & Wilkings Co., 1964), 192. In its 1965 official report to a Congressional science committee, the National Academy of Sciences adopted the three-field definition, complete with awkward qualification: "The term behavioral sciences is of relatively recent origin and emphasizes those parts of social science that attempt to solve their problems by empirical and scientific methods. It includes most of contemporary psychology, sociology, anthropology, and certain aspects of political science and economics." National Academy of Sciences Committee on Science and Public Policy, *Basic Research and National Goals* (Washington: National Academies, 1965), 203. Even the *History of the Behavioral Sciences Newsletter*—predecessor to the *Journal of the History of the Behavioral Sciences*—used a modified form of the three-field formulation: "It deals only with historical aspects of the behavioral sciences and is directed toward all those working in this area—primarily anthropologists, psychiatrists, psychologists, and sociologists, but also biologists, neurologists, historians and any other interested individuals." Quoted in Peter R. Senn, "What Is 'Behavioral Science?'—Notes Toward a History, Journal of the History of the Behavioral Sciences," 2, no. 2 (1966): 107–8.

By the time that planning for Area Five picked up in the spring of 1951, the "behavioral sciences" label was already established as Ford's term of choice. Throughout 1951 Gaither shepherded the program's planning process, retaining Marquis and sociologist Hans Speier—the head of RAND's Social Science division—as consultants. In late summer, Gaither, Marquis and Speier were joined by Berelson, hired to lead the program. The four men collaborated on a detailed planning document, whose extensive revisions were informed by a frenetic series of cross-country interviews and conferences in the fall of 1951.[44] Ford Trustees formally approved the plan in early 1952, and the new Behavioral Sciences Program (BSP) was announced to the world.

It is significant that the three-field definition was nowhere referenced in the approved plan. Indeed, Ford documents in this period—through to 1953—make no mention of the sociology-psychology-anthropology triptych. The formula was only revived in the mid-1950s, after the place of economics at Ford had been definitively settled. This was after a failed, early 1950s attempt at rapprochement, one that economists ultimately rebuked.

The 1951 plan treats economics with ambiguity. The discipline receives explicit praise for its advanced stage of development, and the text gestures at its inclusion in the BSP's agenda. Yet the authors restrict their explicit endorsement to the study of the "behavioral aspects" of economics, in what comes off as an exclusions-by-omission of the discipline's neoclassical mainstream. The treatment of political science is superficially similar, in that the plan rules out broad swaths of the discipline for potential funding. But the excluded fields, in political science, are the discipline's "unscientific" remainder: political theory, legal history, and the like. So political science gets the same proscription as anthropology, psychology, and sociology: Lop off the "speculative," pre-scientific past. For economics, by contrast, it's the discipline's "advanced," scientific center that's earmarked for exclusion.

The plan strikes all the familiar, new-rigorist keys, albeit with a distinctly empiricist cast.[45] The "concept of the behavioral sciences," the authors wrote, "encourages the acquisition of behavioral knowledge under conditions which, so far as possible, ensure objectivity, verifiability, and generality." Interdisciplinarity is prominently called out, without restriction to "traditional academic disciplines." Another major theme is the compatibility of basic research and applied problems—"basic scientific research oriented to major problem areas," in the authors' words.[46]

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[44] Bernard Berelson et al., "Proposed Plan for the Development of the Behavioral Sciences Program," December 1951, Report No. 002072, FFA.

[45] Carl E. Schorske, "The New Rigorism in the Human Sciences, 1940–1960," *Daedalus* 126, no. 1 (1997).

[46] Bernard Berelson et al., "Proposed Plan," 2–4.

The plan invokes the behavioral sciences' on-the-cusp promise in stirring prose:

> As with other such ambitious enterprises, this approach has experienced certain defeats, distortions, and excesses. But at the same time, it has produced a body of technically verified knowledge that is not unimpressive. While it is not yet a 'mature' science, it is certainly more than 'promising.' […] While no one can say that this development will flower into a genuinely mature science of man, nevertheless there is here a cultural movement of some scope and power. The Foundation has a real opportunity to further this movement.[47]

Marquis and his co-authors singled out economics as the "only one in which the stage of propositional theory has been reached to any large extent." The uptake of "proper methods and scientific procedures" has stalled in psychology, is "currently active" in sociology, and "just beginning" in anthropology and political science. Some of these disciplines are making efforts toward the "systematic formulation of propositions," though the "over-all integration of the behavioral sciences is still far ahead." The plan analogizes the social sciences to an earlier stage of medical research: "there are informed people who believe that an opportunity now exists to make headway in the behavioral sciences comparable to the progress made in the medical sciences."[48]

Whatever its praise of economics, the document's only relevant program proposal is restricted to the "behavioral aspects of the economic system." The writeup is prefaced with a *Gaither Report* quote on "non-logical factors" in economic life, and opens with a veiled reference to economists' self-chosen isolation: "Social and psychological factors are involved at a number of points in economics, although the relations have traditionally been obscured by university departmentation." The program area, the plan adds, will be developed together with Area Three, "which is, of course, directly concerned." A proto-proposal for what would become the CASBS is floated later in the document, one that positions economics and the humanities as "neighboring fields."[49]

The plan's proposed program on "political behavior," by contrast, aims at the very center of that discipline's self-proclaimed scientific vanguard. In what reads as reassurance in the absence of political science's formal BSP inclusion, the plan states that the "Foundation's interest in an increased understanding of political behavior is implied by several of its principal program goals as well as by some of the activities which it is presently supporting." The point was laid on thicker still: “The problems of political and social order and change are central to the behavioral sciences.”[50]

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[47] Bernard Berelson et al., "Proposed Plan," 4–5.

[48] Bernard Berelson et al., "Proposed Plan," 11–12.

[49] Bernard Berelson et al., "Proposed Plan," 22–23, 39.

By 1953, the BSP’s ambiguity around economics had given way to flat jurisdictional surrender. In his 1953 report on the new division, Berelson explained that Ford’s adoption of the “behavioral sciences” term is “not equivalent” to the “usual” five-discipline definition. “This is so partly because the Divisional program does not include traditional studies in economics (which fall within Program Three of the Foundation) but mainly because the term is not meant to designate single conventional fields of knowledge or combinations of such fields.” Berelson lingered on the latter point, citing political science, anthropology, and psychology as disciplines with subfields excluded from the program’s remit. Political science, he wrote “contains legal, historical, philosophical, and professional interests,” in addition to its relevant “scientific component.”[51]

So by 1953 economics, to Ford, was not a behavioral science, while political science—or at least its scientific component—had won membership. The BSP, Berelson wrote, hopes to sponsor dialogue between the behavioral sciences and its neighbors: “history, economics, social and political philosophy, and humanistic studies.” Economics, needless to say, is unlike the “unscientific” others in the list. If anything, the discipline remained a high-achieving model to emulate. The report’s writeup on the BSP’s energetic promotion of mathematics, through summer institutes and training materials, illustrates the point: “Just as the tools of mathematics have contributed to the natural sciences and more recently to economics, so it is hoped they can give assistance to the behavioral sciences.”[52] Economics, here and elsewhere, was an object of aspirational envy—but not a behavioral science.

The final break, such as it was, had only just unfolded over the previous year. As Mark Solovey and I have documented in much more detail, Berelson had launched an initiative in 1952 to widen the BSP’s mandate to include economics.[53] The idea was to cut through the “sharp” and “arbitrary” line that set off the discipline from the other social sciences.[54] The BSP’s effort to incorporate economics, or at least its “behavioral aspects,” was carried out by an Advisory Group on Economics and the Behavioral Sciences appointed by Berelson.[55] In 1952 the group met periodically, circulated a proposal for comments, and convened three conferences to solicit input from economists. The effort failed, and the main reason was opposition from economists.

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[50] Bernard Berelson et al., “Proposed Plan,” 16–17.

[51] Bernard Berelson, “The Ford Foundation Behavioral Sciences Report,” June 1953, Reports 002750, FFA, 12–13.

[52] Berelson, “The Ford Foundation Behavioral Sciences Report,” 36, 38.

[53] Pooley and Solovey, “Marginal to the Revolution,” 219–29.

[54] Bernard Berelson, summary of second meeting, 11 July 1952, Herbert Simon Collection (hereafter HSC), Series VI-41, 6.

The advisory group's reports, minutes, and correspondence offer a chronicle of diminished ambition. The group, composed of scholars opposed to the growing neoclassical orthodoxy, occupied what was fast becoming the discipline's heterodox margins: a mix of institutionalists, business school affiliates, and PhDs from outside the discipline. The group's initial statements were filled with muscular denunciations of the divide between economics and its social science peers, together with attacks on economists' rationality assumption and their deductivist neglect of empirical studies of behavior. Indeed, the group's early statements are strikingly reminiscent of the *Gaither Report*'s Area Three write-up, with its promise to initiate an overhaul of the discipline. The group's unorthodox agenda was correctly inferred by mainstream economists who presented a formidable and ultimately effective critique.

After the advisory group first met in the spring 1952, Berelson observed that there was "unanimous agreement that cooperation between economics and the behavioral sciences was highly desirable."His summary records an ambitious program of integrated theory-building, research support, and cross-disciplinary graduate training. Still, his reference to a "border zone" between the behavioral sciences and economics is in keeping with the BSP's 1951 "Proposed Plan," with its call to incorporate only the "behavioral aspects" of economics into the BSP. "Economic behavior" appears throughout his summary and signals the group's focus on empirical research into actual behavior, in explicit contrast with neoclassical economists' assumption of rationality.[56]

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[55] The foundation even considered a complete transfer of economics into the BSP. A mid-October memo from Speier, Marquis, and Berelson states at one point that the BSP will probably "require the addition of two more professional staff members," based on the assumption that "research in economics will be handled in Program III." However, the memo then mentions the possibility that the BSP would "take responsibility for it [economics]," in which case "another staff member will be required (and budget adjustment will be necessary)." Hans Speier, Donald Marquis, and Bernard Berelson, "Proposed Development of Program V," 15 October 1951, folder 75, box 7, series V, FFA, 2. McPeak later confirmed that the foundation considered folding Areas Two and Three into the BSP, though the timing is unclear: "We on the staff were of course not completely sure that the Foundation should have set up Program V as a separate and independent program in the beginning. Our history clearly showed unrelenting stress and strain and caused us to wonder whether the independent program was a convenient unit for operation. Accordingly we suggested the possibility of combining the Behavioral Sciences Program with Programs II and III, which, through their heavy emphasis on the application of the social sciences, seemed close kin. I can not [sic] say for sure why this course was not taken but it is my understanding that, however wise it might have been in the beginning, such a course was not, in the context of the 1957 [closure], a practicable one." McPeak, "BSP [1951–1957]: Report and Appraisal," 6.

[56] Bernard Berelson, "Report of First Meeting," 13 May 1952, HSC, 2, 4.

Berelson's summary, in some tension with its stated interest in just a "border zone," declares the entire divide between economics and the other social sciences to be an arbitrary product of history:

> The present situation is due to an historical separation between the disciplines. A group of specialists began to work on a set of "economic problems" which were defined in limited terms for purposes of simplicity and manageability. They invented their own psychology and sociology, as needed. This form of intellectual organization became institutionalized through university departments, and then became traditional. As time went on, the intellectual claims and achievements of economics relative to the behavioral sciences promoted a kind of academic "pecking order" within the social sciences, with economics at the top, and this made cooperation all the more difficult.[57]

This historical account, its thinly veiled criticism of neoclassical model building, and its reference to a "pecking order" led by economists: all of these would prove offensive to even sympathetic economists.

After a second meeting, Berelson expanded the initial summary such that it was even bolder than the initial write-up. Moving beyond the call for stimulating a "border zone," the first of the summary's list of "next steps" proposes that the foundation "let it be known that it regards the sharp line now existing between economics and the behavioral sciences as arbitrary, and that it is interested in knowing about projects aimed at cutting across the line." In a list of proposed criteria for foundation support, Berelson writes that preference should be given to projects "which propose to study actual behavior of persons in economic situations, as opposed to the end products of behavior (e.g., 'dollar' behavior)."[58] At the suggestion of Herbert Simon, who more than anyone else shaped the advisory group's intellectual agenda, the references to theory were also strengthened.[59] "Collaboration," reads the summary, "means that behavioral science should not only be 'applied' to economic problems wherever feasible, but should also include economic phenomena as one area out of which a general theory of behavior must be developed."[60]

This document was circulated to over sixty economists and other social scientists, from a list generated at the second meeting.[61] The distribution list is dominated by economists and business scholars known to be sympathetic to cross-disciplinary collaboration, including Allen Wallis, Kenneth Boulding, Walt Rostow, Clark Kerr, John Nef, Wight Bakke, and George Katona. Yet a significant minority represents various strands of postwar neoclassicism, including Paul Samuelson, Milton Friedman, Ted Schultz, Franco Modigliani, and Armen Alchian. There was, too, a separate, much shorter list of "behavioral scientists," including Merton, Daniel Lerner, and Robert Dahl. In all, nearly sixty scholars replied to the request for comments.[62]

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[57] Berelson, "Report of First Meeting," 2–3.

[58] Berelson, summary of second meeting, 6, 5, 2.

[59] Herbert Simon to Bernard Berelson, 24 May 1952, HSC, 1–2. On Simon's overall influence on Berelson and the BSP, see Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: Johns Hopkins University Press, 2005), 152–56

[60] Berelson, summary of second meeting, 2.

[61] Thomas Carroll to Herbert Simon, 11 July 1952, HSC, 1.

A narrative "Digest of Replies" records general support for the idea of a Ford-sponsored "behavioral aspects" initiative, although with significant reservations and a series of specific complaints about the Berelson summary. About ten scholars raised serious objections, nearly all economists: "Dissent and qualification came chiefly, although not solely, from the economists". Some of the resistance from economists was plainly tied to confusion about the behavioral sciences neologism. A number of econometricians, including Harold Hotelling, insisted that economics already is a behavioral science. "Quite a number" of economists were "deeply concerned" about the idea of a "general theory of behavior"; Ted Schultz suggested that the advisory group take two or three pages to spell out what it means. The incendiary historical passage was "the most generally criticized," with about twenty replies registering an objection; one labeled it "fashionable foundationese."[63]

A telling set of warnings issued by economists spoke to the high costs of interdisciplinarity within the discipline. Milton Friedman, the only respondent to express out-and-out opposition, suggested that the "interdisciplinary fad" could divert economists from the specialization necessary for the discipline to become a "cumulative science." A proposal for cross-disciplinary graduate training met lukewarm support at best, according to the "Digest": "The lack of experience in interdisciplinary training apparently makes economists cautious on the subject." Max Millikan, for example, pointed to the "long time required to become a competent economist" as his reason for resisting cross-field graduate training . A "large number" of economists stressed the price young economists would pay for venturing outside the discipline. Boulding called it the "demand problem." Richard Heflebower cautioned that the "'reward system' in economics is unfavorable to those who challenge its methods or isolation from other sciences," which in his view is the "fundamental problem to the program proposed." Kuznets, in a similar vein, advised that the foundation must make cross-disciplinary research "attractive enough to an important group of economists to divert them from the strong present pressure in other directions," referring solemnly to "those economists who are spiritually ready for crossing boundaries."[64]

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[62] Berelson, summary of second meeting, 7–8.

[63] Milton Heath, "Digest of Replies to Letter Concerning Report of Advisory Group on Economics and the Behavioral Sciences," n.d. 1952, HSC, 2, 3, 6–7; see also "List of Repliers," n.d. 1952, HSC.

Also striking in the "Digest" are the quoted comments from economists who fully supported the advisory group initiative, and sometimes took direct aim at neoclassical theory. In a favorable tone Walter Weisskopf called the search for a general theory of behavior "the crux of the entire matter," citing close integration with psychology, anthropology, and sociology. Clark Kerr complained about his discipline's fixation on a "single type of motivation—the responsiveness to pecuniary costs and rewards"—and observed that "even economists have come to feel the necessity of drawing upon non-pecuniary ones." Wight Bakke separated "economic behavior" from "behavior of the 'economic man,' that is, the behavior postulated in theories of rationality," and then called for "extensive further development" of its study. The economists in the advisory group along with their heterodox supporters recorded in the "Digest" invoked the other social sciences as ammunition in their battle with the neoclassical mainstream—in much the same way that interwar institutional economists had appealed to psychology and sociology in *their* version of the dispute.[65]

The mixed response to the advisory group summary foreshadowed the emergence of stronger opposition in the three conferences chaired by Thomas Carroll in the late fall, held in San Francisco, Chicago, and New York Here, concern about the relationship between Areas Three and Five surfaced for the first time and seems to have dominated the three gatherings. So prominent was the economists' concern over Area Three's dormancy that the first four paragraphs of Carroll's conference summary recount what he called a "major 'stumbling block.'" "Hope was strongly expressed," he wrote, "that provision will be made in Program III for support of fundamental economic research." There is "some real and apparently widespread apprehension" that Area Three would only support applied policy work and public education campaigns, which may "crowd out desirable support of efforts specifically directed toward the creation of new economic theory and knowledge."[66] The advisory group's focus on the "behavioral aspects" border zone comes off here as a sideshow irritant to economists awakened to Area Three's threatened potential:

> Attention was called to the "danger" of diverting economists to activities "they are not well-equipped to do" if they are under the impression that "new knowledge" is supported only through Program Area V. The New York group proceeded on the stated assumption that the framework of the Study

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[64] Heath, "Digest of Replies," 2, 16, 12.

[65] Heath, "Digest of Replies," 11, 10, 5. See Yuval P. Yonay, "When Black Boxes Clash: Competing Ideas of What Science Is in Economics, 1924–39," *Social Studies of Science* 24, no. 1 (1994): 54–58.

[66] Thomas Carroll, "Report of Three One-Day Conferences," December 1952, HSC, 2.

> Committee Report for Program Area III will continue to be Foundation policy and that "theoretical research in economics" will be supported. The Chicago group requested that they be recorded as favoring "fundamental theory support in Program III" soon and as substantially as possible.[67]

The "theory" in concern of course is not the cross-disciplinary "general theory of behavior" previously touted in the advisory group's proposal.

On the core question of interdisciplinarity—a key concern for the behavioral science–orientation taking shape—economists offered some support, but this was qualified by doubts and warnings. "There was," wrote Carroll, "a feeling that interdisciplinary activity should not be supported *as such*, but rather that problem-oriented activities which are inherently interdisciplinary in character should be supported." Attendees referred repeatedly to disciplinary punishment for wayward economists who dabbled in other fields. In contrast to some of the "Digest" replies that indicated support for interdisciplinary efforts should be strengthened, economists at these meetings generally recommended against an explicit focus on interdisciplinary graduate training. According to Carroll, it was the "general opinion that 'improvement of training will follow when there is something to study'"; the economist attendees favored research, "with training as a by-product."[68]

The overall impression from Carroll's summary document is that conference participants were largely indifferent to the advisory group's proposals. Expect other economists to be dismissive or even hostile to the "economic behavior" program, the attendees warn throughout. "It was brought out several times that many economists are 'prejudiced against this sort of thing,'" records Carroll. New York attendees urged the foundation to select a "broadminded economist" to direct Area Three, not somebody "who is 'against' involvement in activities such as are encompassed in Program Area V." The economists' main preoccupation throughout is with Area Three, and their recommendations for the advisory group focus on preventing relations with the BSP from distorting Area Three activities. According to the summary, there was a clear push to limit the BSP director's independent authority to mete out grants and other awards. Participants suggested the creation of an advisory group that would include representatives from the American Economic Association (AEA), Social Science Research Council (SSRC), and other bodies, with the foundation officers left to name a minority of the membership "'in their own way.'"[69]

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[67] Carroll, "Report of Three One-Day Conferences," 2.

[68] Carroll, "Report of Three One-Day Conferences," 2–3, 4.

[69] Carroll, "Report of Three One-Day Conferences," 5, 3, 10–11.

By the time the BSP issued its report in June 1953, the "Behavioral Aspects of the Economic System" was not even listed as a substantive area. The division report does refer to the advisory group in a single paragraph, but concludes that its "recommendations will be progressively considered for implementation as the general economic program of the Foundation is developed."[70] The "border zone" had been ceded to Area Three. In July, Gaither—who had assumed the Ford Foundation presidency earlier in the year—recruited Carroll to take the helm of Area Three, which spent close to $80 million dollars before closing in the mid-1960s, thus outliving and outspending its BSP counterpart.[71]

C. The late Behavioral Sciences Program, 1954–1957

Though the place of economics was now a settled fact, the BSP continued to cast about for a coherent definition for its named domain. The three-field formulation was revived, though now sometimes prefaced with a "core" designation. The adjective's first appearance, fittingly, was in the Advisory Group's report, which refers to the "so-called 'core' [behavioral sciences] of sociology, anthropology and social psychology." In the same document, the authors professed ignorance of political science's funding home at Ford, even as they endorsed immediate Area Three support for economists: "We are not well-informed about the support of parallel activity in the area of political science but wish to express our firm conviction of a similar need for support in that field, if it has not been planned or is not contemplated by the Foundation."[72]

William McPeak, in a 1961 appraisal of the BSP, marshaled the three-field definition to explain the label that he had, back in 1949, helped shepherd into ubiquity. McPeak had also rejoined the foundation with Gaither's 1953 appointment as president, and served as the BSP's supervising officer over its subsequent lifespan.[73] The *Gaither Report*, McPeak explained, had used the "behavioral sciences" term because, "despite our close attention to the matter, no better one was forthcoming." Social science wouldn't do, he added, since it "includes at least three major disciplines—economics, political science, and history—that were not typically included in the 'behavioral sciences.'" This was, McPeak must have realized, a questionably recursive rationale.

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[70] Berelson, "The Ford Foundation Behavioral Sciences Report," 49.

[71] "Program in EDA," folder 68, box, 6, series II, 20025, FFA, A9, 1.

[72] Thomas Carroll et al., Advisory Group on Economics and the Behavioral Sciences report, February 18, 1953, folder 6, box 1, series 1, Behavioral Sciences Division, Office Files of Bernard Berelson, FFA, 4, 3.

[73] McPeak, "BSP [1951–1957]: Report and Appraisal," 3. McPeak, like Gaither, had been advocating for the program since its late 1940s conception, a practice that continued during his tenure as the BSP's supervising officer: "at least once a year until the Program was terminated in 1957 I led a general discussion with the Board concerning the status and direction of the Program."

He hastened to add that those disciplines, "at least in their applied aspects, were dealt with elsewhere in the Foundation"—an accurate claim only in the case of economics. "In the end," he wrote, "the 'behavioral sciences' got defined essentially as psychology, anthropology, sociology, and closely related activities."[74]

McPeak's appraisal came four years after the BSP was ignominiously shuttered by Ford's Trustees. Ironically, it was the Cold War—or at least its McCarthyite by-product—that sealed the BSP's fate. The "behavioral sciences" language—chosen to avoid the "socialism" conflation and to signal scientific rigor—failed to shield the program. The socialism accusation was made anyway, and the avowed objectivity only attracted charges of amoral "scientism" from right-wing critics. In the mid-1950s, aggressive scrutiny from conservative lawmakers in Congress and controversy over a handful of BSP grants and proposals convinced skittish Trustees to shutter the program in 1957.[75]

In a bitter postmortem, Berelson laid some of the blame on the "behavioral sciences" term itself. Since it was "not only unfamiliar but associated with a center of power, it disturbed a number of people and undoubtedly made a certain amount of trouble for the Program both in and out of the Foundation." He complained of the "endless problem of defining, explaining, and justifying the term and our field of operation." It was, he admitted, a "not particularly felicitous" phrase.[76] Thus Ford's sponsorship of the term—so plainly the source of its widespread adoption—also contributed to the demise of the program it named. Even so, the term was already sufficiently lodged in the linguistic sediment to survive without the foundation's sponsorship.

In his 1957 postmortem, Berelson suggested that turf disputes were responsible for the narrow, three-field definition. He observed that the "behavioral sciences" term was chosen over "social science" because other foundation program areas had jurisdiction over key social science fields: "The familiar term 'social sciences' includes at least three major disciplines—economics, political science, and history—that were not typically included in the 'behavioral sciences,' if for no other reason simply because they were dealt with elsewhere in the Foundation." As Berelson surely knew, it was only economics that was separately funded. "[I]n the end," the term "did get defined essentially" as psychology, anthropology, and sociology. He seemed to blame what he called "the jurisdictional problem."[77]

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[74] McPeak, "BSP [1951–1957]: Report and Appraisal," 3.

[75] See the detailed detailed account in Solovey, *Shaky Foundations*, 119–27. In the Reece Committee hearings Berelson himself was labeled one of the foundation's "dubious staff," for once serving on a committee at the University of Chicago that welcomed the "Red dean of Canterbury," Hewlett Johnson. U.S. Congress, House, Special Committee to Investigate Tax-Exempt Foundations and Comparable Organizations, Tax-Exempt Foundations, Hearings, 83d Cong., 2d sess. (Washington: GPO, 1954), 36.

[76] Bernard Berelson, "The Ford Foundation BSP Final Report, 1951–1957," September 1957, Report No. 010548, FFA, 3.

But Berelson also presented a case, a brief one, that the term had a real intellectual referent. The behavioral sciences "came to refer primarily," he wrote, "to the objective and empirical observation and analysis of the behavioral of human beings, especially of a non-economic kind." There "was and is," he continued, "a certain coherence in the central concerns of these [behavioral science] disciplines and to some extent the term has caught on to describe this core of the scientific study of man."[78]

Berelson had a point. As I want to draw out now, Ford's patronage of the movement it named furnishes some tentative insight into what the behavioral sciences were. For all the jockeying and muddling through—and for all the term's infelicities—it is possible to extract the outlines of a working definition from the foundation's activities and self-description.

First, a handful of the BSP's intellectual commitments might be extended to characterize the movement as a whole:

> **1. Scientific Rigor:** Throughout its career, inclusive of planning pre-history, the BSP was an unambiguous advocate for the "scientific" side of social science. This was a self-understood ideal, often modeled on the example of the natural sciences. Just as crucially, the BSP's commitments to science was articulated, repeatedly, as an intellectual break with a "speculative," pre-scientific social science past. The BSP's mantle of science included professions of value freedom and evangelism for quantitative methods.
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> **2. Nomothetic Theory-Building:** In its planning and reporting documents, Ford's program made the pursuit of general theory a cardinal goal. The idea was to accumulate empirical support for universal laws of human behavior, or at least stable, law-like patterns. New and established quantitative methods, such as survey research and experimentation, were expected to establish support for general propositions, with the hope that these might be integrated into a coherent "theory" of human behavior.
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> **3. Empiricism:** By "empiricism" I mean a loose and unreflective commitment to empirical findings as the cumulative foundation upon which to erect general theories. Nowhere in the Ford records is there an elaborated, philosophy-of-science defense of induction, nor any real grappling with the theory-ladenness of observation. There was, instead, a constant refrain that the study of actual human behavior is fundamental to the "behavioral sciences" project. Of course the split with mainstream economics reflected, and perhaps helped to sharpen, this empiricist self-understanding.
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> **4. Aspirational Mathematization:** The "aspirational" modifier is crucial here, since the BSP's energetic promotion of mathematics was rarely reflected in the program's funded projects and list of accomplishments. What mattered, instead, was the *idea* of a mathematical social science—an unrealized

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[77] Berelson, "The Ford Foundation BSP Final Report," 4, 8.

[78] Berelson, "The Ford Foundation BSP Final Report," 4.

promise expressed by economics and a handful of non-economist outliers like Paul Lazarsfeld and Herbert Simon.

5. Interdisciplinarity: Peans to interdisciplinarity have, somewhat notoriously, accompanied every phase of professionalized social science since the core disciplines' late nineteenth and early twentieth-century differentiation. In that respect, the BSP's repeated endorsement, like many of its commitments, were hardly novel. Even so, the rhetoric of interdisciplinarity plainly informed much of the division's programming and grant-making, even in terms of projects directed at "neighboring" fields like economics and the humanities.[79]

6. Problem-Based Inquiry: From the *Gaither Report* forward, the foundation's behavioral sciences program stressed that genuine, "basic" findings and theory could be generated from applied projects. The conviction, as expressed, is that the many applied projects that Ford funded could be the setting for more fundamental knowledge gains.

Evidence for each of these six commitments is abundant in the BSP records and activities. Taken together, they amount to a loose scientific-intellectual worldview, one shared among the officers and most of the program's small army of consultants. It is true that some of the program's prominent advisors—Talcott Parsons, Hans Speier, and Edward Shils come to mind, though in very different ways—dissented from one or more of these values. But there was, at Ford at least, a baggy consensus on a bundle of intellectual commitments.

I want to make the claim that these epistemic virtues were embraced, by and large, by the wider behavioral sciences moment. To support that claim, however, it is important to draw out the social character of the BSP's activity. The main justification for this section's march through the archival trenches, indeed, was to draw out the program's social underpinnings, listed here:

1. American: To acknowledge the Ford Foundation's U.S. character is not to exhaust the sense in which the BSP conceived of itself as an American project. As Berelson observed in his 1957 postmortem, "It is not too much to say that the behavioral sciences are … in large part an invention of this country."[80] The program's entanglements with the early Cold War struggle are well-documented. Their significance, however, is less for their knowledge-shaping character—which was arguably limited—but instead for the sustained social interaction that the Soviet struggle justified. The BSP was avowedly missionary, eager to spread the behavioral science gospel around the world. The program's commitments to social-scientific universalism were compatible with this evangelism. The Cold War pretext of many of these activities was, moreover, relatively unproblematic given the shared conviction that problem-based inquiry could yield genuine insight.

2. Elite: The small-world character of the BSP is vividly demonstrated in the program's cast of advisors and its grant-making activity. The advisors, and the awards, were overwhelming drawn from, and made to, social scientists at a handful of elite U.S. universities, notably Harvard, Columbia,

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[79] On the efflorescence of cross-disciplinary ventures in this 1945 to 1965 period, see Roger E. Backhouse and Philippe Fontaine, "Toward a History of the Social Sciences," in *The History of the Social Sciences since 1945*, ed. Roger E. Backhouse and Philippe Fontaine (Cambridge: Cambridge University Press, 2010).

[80] Berelson, "The Ford Foundation BSP Final Report," 420

> Chicago, Michigan, and Stanford.[81] The vast majority of working U.S. social scientists were beyond the program's orbit. Indeed, in keeping with the program's commitment to promote scientific rigor to otherwise-besotted fields, the program positioned its constituency as a scientific vanguard.
>
> **3. Generational Cohort:** The BSP's documentary history invites an incestuous reading, so frequently do the same three-dozen or so names appear on panels, commissions, and advisory boards. These figures were, as I revisit below, already well-known to each other long before the BSP's formal launch, having served in shifting wartime posts and then again in sundry projects and panels in the early Cold War. With notable exceptions like political scientist Harold Lasswell, the social scientists in the BSP orbit had launched their careers, in earnest, only after World War II. They formed something like a generational cohort.

One benefit of looking at the BSP through the double lens of its social and intellectual dimensions is that the program's otherwise-baffling messiness comes into sharper relief. My close attention to the discrepant place of economics and political science, in particular, is intended to help account for the BSP's struggle to furnish a coherent definition of its self-named domain. The program's fate, in short, was colored by both kinds of factors. The eventual settlement—political science smuggled in, economics in self-chosen exile—was a product of jurisdictional in-fighting, unquestionably. But there were, too, important intellectual stakes at play, as well as social formations that pre-dated the BSP.

Consider the case of political science. The postwar cluster of political scientists committed to the scientific study of political behavior really were a better intellectual fit for the BSP's brand of quantitative empiricism than was the economics discipline's various neoclassicisms. They were committed, after all, to the empirical study of behavior, in line with the BSP's professed remit. But they were also much more interwoven into the wartime- and early postwar networks of three disciplines that constituted the BSP's putative core. For this pair of reasons—one intellectual, the other social—it is unsurprising that political scientists received a far larger proportion of BSP grants than anthropologists.82

The contrast with economics is instructive. Some economists, especially early on, were indeed included in the program's advising and grant-recipient ranks. But those economists, by and large, shared intellectual and social coordinates with the BSP's much larger pool of sociologists, psychologists, anthropologists and, yes, political scientists.[83] The BSP-affiliated economists were, in most cases, distinguished by dissent from their discipline's emerging orthodoxy. They were also among the relatively small number of economists who had worked alongside other social scientists in the World War II morale and propaganda bureaucracy.

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[81] The point is made by a quick glance through the BSP's list of consultants, as well as its grant awardees. For the former, see Berelson, "The Ford Foundation Behavioral Sciences Report," 60–67; for the latter, Berelson, "The Ford Foundation BSP Final Report," A1–A12. The same small handful of universities appear in both lists—the same institutions, for the most part, that were the subject of well-funded, BSP-commissioned self-studies. See Wallis, "The 1953-54 Program of University Surveys of the Behavioral Sciences."

[82] Berelson, "The Ford Foundation BSP Final Report," A1–A12.

Many economists experienced the war differently. They worked alongside natural scientists and mathematicians, and emerged from the war with a sense of confidence that owed little to collaboration with other social scientists. That self-assurance, moreover, had grown up around a model of science—quasi-deductivist, steeped in modeling, and committed to utility-maximizing assumptions—inconsistent with the other social sciences. So the fact that the discipline's mainstream muscled its way out of the BSP, and into the richer Area Three domain, reflected pre-existing, and notably interwoven, social and intellectual differences from their social-scientist peers.

So the BSP example turns out to be a useful aperture to make tentative sense of what the behavioral sciences were. At the same time, its brief lifespan and eponymous identification with the movement risk obscuring the behavioral sciences' temporal scope. The BSP's definitional disarray, moreover, has contributed to confusion in the secondary literature. The siloed history of political science "behavioralism"—its "behavioral revolution"—is one index of the BSP's checkered historiographical legacy. The persistent ambiguity about the place of economics in the movement is another. A third legacy is conflicting claims about the movement's duration.

To take up these issues, I turn to the work of Hunter Heyck, before, in the final section, moving to establish the movement's temporal bookends.

II. Hunter Heyck on the behavioral sciences movement

Hunter Heyck, more than any other historian of social science, has taken on the definitional project. His main contributions appear in his 2005 intellectual biography of Herbert Simon and a linked *Isis* article the following year on postwar patronage.[84] In those works, Heyck presents the behavioral sciences as an interdisciplinary "movement," bounded in time—a social web formed by war and sustained by funders, and characterized by a series of loose commitments to mathematics, cross-disciplinary team work, and the commingling of applied and basic research. Heyck's key insight is to define the behavioral sciences along a pair of mutually reinforcing dimensions. The movement, he argues implicitly, was both intellectual and social—marked, that is, by shared beliefs *and* a web of interpersonal and patronage connections.

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[83] See, for example, the instructive case of Kenneth Boulding. Philippe Fontaine, "Stabilizing American Society: Kenneth Boulding and the Integration of the Social Sciences, 1943–1980," *Science in Context* 23, no. 2 (2010).

One of Heyck's important claims is that patrons were decisive enablers of the behavioral sciences. His point isn't that funders dictated topics or steered scholarly agendas. The influence, if anything, went in the opposite direction. It was behavioral scientists, arguably, who guided the social science agendas of the Ford Foundation, the Office of Naval Research, and similar funders. The crucial role of the patrons, instead, was to underwrite, and sometimes coordinate, joint activity. Whatever their specific purposes, the workshops, conferences, summer institutes, advisory boards, working groups, consulting gigs, and cross-campus research projects served to maintain social ties that, in many cases, were formed in shared World War II service. Funders like Ford furnished a social-networking infrastructure: gatherings and projects that served as episodic and cross-cutting moments of interpersonal renewal for a community that was, after all, comparatively small. Ford's Center for the Advanced Study of the Behavioral Sciences (CASBS), explicitly designed for cross-pollination, was only the most expensive instrument of this kind.

Heyck used Simon's remarkable career as a prism onto the broader world of the behavioral sciences. Simon was, famously, a one-man interdiscipline, and in this respect a metonym for what was a networked movement.[85] Extracting from Simon, Heyck identified four shared commitments that typified the kind of social science that behavioral scientists sought to promote:

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[84] Hunter Crowther-Heyck, *Herbert A. Simon: The Bounds of Reason in Modern America* (Baltimore: Johns Hopkins University Press, 2005), esp. 149–65; Crowther-Heyck, "Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Science," *Isis* 97, no. 3 (2006). At key moments in these works, Heyck deploys "behavioralism" as the umbrella term. "I will use the term 'behavioralism' to refer to this set of commitments, following the usage of the day." "Patrons of the Revolution," 431. It is an unfortunate choice, because—outside of political science—the "behavioral sciences" phrase was far more common, outside political science. That discipline's distinctive language of "behavioralism" and "behavioral revolution" has, among other factors including the BSP's definitional disarray, help produce a siloed historiography in the subfield of history of political science. Like many other historians, Heyck erroneously attributes the "behavioral sciences" term's coinage to James Grier Miller's Chicago committee. "Patrons of the Revolution," 439.

[85] Simon was, Heyck, writes, both unique and representative: "If we want to understand the postwar transformation of the sciences, especially the behavioral sciences, we need to understand Herbert Simon." Crowther-Heyck, *Herbert A. Simon*, 11.

1. **Mathematical:** For behavioral scientists and their patrons, there was "no question that a reformed social science would be mathematical." Heyck cites Simon's 1957 *Models of Man*, a collection of "Mathematical Essays on Rational Behavior in a Social Setting," as a case in point. Mathematical modeling entails abstraction, a willful simplification predicated on a "finite set of functions."[86]
2. **Behavioral-functional:** As a kind of pre-condition—a "key step on the road"—for mathematization, Heyck proposes "behavioral-functional" as a catch-all label for systematic approaches in general, but especially those that consider individual behavior and their containing systems as mutually constitutive. He cites work by Robert Merton, James Grier Miller, and Simon as "exemplary" of this link between behavioralism and functionalism. Formal theory and formal models were, he adds, are typically deployed.[87]
3. **Problem-centered:** Behavioral scientists and their patrons had the conviction, Heyck claims, that practical and applied projects could have significance for general theory. "To men like Bernard Berelson or Talcott Parsons or Rensis Liberty or Herbert Simon, theory could advance only by being put to the test in experimental situations, and practical problems could be solved only through the advance of theory."[88]
4. **Interdisciplinary:** The appropriate setting for such problem-oriented research, according to behavioral scientists, was in large, funded team-based projects bringing together researchers from diverse disciplines, to work on the applied projects so fruitful for yielding knowledge gains.[89]

This "intellectual agenda" plainly overlaps with the six-fold list I identified with Ford's BSP. Indeed, Heyck's basic approach—to identify a family of intellectual commitments—inspired my own BSP-derived set.

In Heyck's account, the patronage system that took shape in the late 1940s and early 1950s was distinguished not just by scale—never had the social sciences been on the receiving end of such largesse—but also by its small-world character. The major foundations, with Ford as the gigantic new arrival, agreed on the kind of social science they sought to support. It was a vision shared, in its main features, by the military agencies too. This rough consensus was not, to stress again, an imposition from funders onto social-scientist grantees. After all, the foundations' program officers and military officials—many of them social scientists themselves—were enmeshed in the same postwar web of mutual acquaintance as their funding recipients. The resonance of grantor and grantee on what should count as good behavioral science was, as it were, reproduced through their ongoing interaction, built up from a shared postwar baseline of belief. As Heyck notes, a handful of university-based social scientists became central nodes in the patronage network of the 1950s—"brokers," as Heyck calls them, who were repeatedly appointed to advisory boards, consultancies, and evaluation teams.[90]

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[86] Crowther-Heyck, "Patrons of the Revolution," 432; Herbert A. Simon, *Models of Man: Social and Rational* (New York: John Wiley & Sons, 1957).

[87] Crowther-Heyck, "Patrons of the Revolution," 432; Robert K. Merton, *Social Theory and Social Structure* (Glencoe, IL: Free Press, 1949); Miller, "Toward a General Theory for the Behavioral Sciences"; and Simon, *Models of Man.*

[88] Crowther-Heyck, "Patrons of the Revolution," 433.

[89] Crowther-Heyck, "Patrons of the Revolution," 433.

Heyck's portrait of the behavioral sciences is extraordinarily useful; it is the foundation upon which refinements should build. He points, crucially, to the interweaving of social and intellectual factors, and casts funders as conveners rather than puppeteers. In a similar way, Heyck acknowledges the Cold War backdrop, but without conceding too much. Of course foundation and military-agency budgets and funding agendas were profoundly affected by the Soviet struggle. But Heyck helps to show that the behavioral sciences movement was invented by social scientists, and also sustained by them; *they* were responsible for most of its research priorities and nearly all of its intellectual commitments. It's true, Heyck admits, that a significant share of funded projects in the 1950s and early 1960s were hitched to the Cold War effort. Some of the social scientists on the grants' receiving end were hardened Cold Warriors; many others were funding opportunists. Either way, the behavioral sciences creed, such as it was, had as its predicate that problem-oriented research could yield broader theoretical insights. Heyck helps show that the Cold War dollars, and the applied projects they underwrote, were the means by which behavioral scientists pursued what were, for the most part, their own ends.[91]

For all its virtues, Heyck's definition-cum-summary has some flaws, as both the BSP case helps to reveal. Heyck, in the more recent *The Age of System* and his earlier work on Simon, is committed to drawing out the postwar mania for formalization—for ambitious system-building and universalist modeling. In *Age of System*, Heyck calls this intellectual style "high modern social science," centered on a "bureaucratic worldview," itself rooted in the country's twentieth century "organizational revolution." It's a sweeping thesis, supported by a content analysis of flagship journals from the 1920s to the 1970s. The "apogee" of high modern social science, Heyck concludes from his journal study, spanned 1955 to 1970. Simon, unsurprisingly, is

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[90] Simon, unsurprisingly, was one such broker. Heyck identified five especially ubiquitous brokers, based on an appointment count of Ford-sponsored advisory committees. "Paul Lazarsfeld, Robert K. Merton, Herbert Simon, Ralph Tyler, and Thomas Carroll—the organization elite of the social sciences during the 1950s and 1960s—all served on at least four of these advisory committees…" Crowther-Heyck, "Patrons of the Revolution," 439.

[91] For a view broadly sympathetic with these claims, see David C. Engerman, "Social Science in the Cold War," *Isis* 101, no. 2 (2010).

Heyck's exemplar of the high modern style—just as he served as proxy for Heyck's portrait of the behavioral sciences a decade earlier.[92]

The problem, already present in Heyck's earlier work, is that the mathematical formalization that Simon exemplified isn't a good fit for the behavioral sciences as a whole. It is both too broad and too narrow. Heyck's formalist-heavy definition sweeps in, for example, the large swath of economics that had, by the early 1950s, self-segregated from the other social sciences. The behavioral sciences, in an important sense, were defined *against* economics in its various postwar neoclassicisms. In this respect Simon, with his decades-long engagement with mainstream economics, was an outlier among self-identified behavioral scientists.

Heyck is right to stress the privileged place of mathematics in the movement's self-understanding. But he largely sidesteps what was, for most behavioral scientists, an *aspirational commitment*, unrealized in actual research practice. Thus Heyck's accent on mathematical formalization is—in its inclusion of neoclassical economics—too broad and—in its exclusion of most behavioral scientists—too narrow.

Heyck's sensitivity to theoretical system-building causes similar demarcation issues. The emerging, overlapping fields of cybernetics, information theory, and systems theory are granted top billing by Heyck when they were, instead, marginal for most behavioral scientists. The prominence, in Heyck's accounts, of James Grier Miller's idiosyncratic, systems-theory vision for "behavioral science" is one index of the problem. Miller's program, and his *Behavioral Science* journal, stood outside the behavioral sciences mainstream. Heyck also over-emphasizes Talcott Parsons' high-altitude structural-functionalism, built up in the late 1940s and early 1950s. As with Miller's program, Parsons' structural-functionalism was welcomed as a real tenant in the house of the behavioral sciences. But its actual uptake and application by practicing U.S. social scientists in the 1950s and 1960s was never significant. Far more common was a commitment, sometimes unspecified, to a lower-grade species of functionalism. Very few behavioral scientists, regardless, ever endorsed Parsons' brand of analytic realism. Many, perhaps most, subscribed instead to an unreflective empiricism, on the expectation that accumulated findings would yield law or law-like generalities—a lay epistemology exemplified by Berelson's inventories project at Ford.

Heyck's "behavioral-functional" category illustrates my point. He stresses behavioralists' deliberate simplification—their abstraction from the messiness of social reality. In one respect, he's right: Quantification based on the measured relationship of variables was indeed widespread in elite U.S. sociology, political science, and social psychology. But this brand of abstraction wasn't typically in the service of the "development of formal theory and formal models," as Heyck claims. It's true that behavioral scientists sought to build up general theory from data-gathering. But the mainline approach, Parsons aside, wasn't formalist nor even systematic, except in the prospective distance. There is, in Heyck's definition, a slippage between highly elaborated systems modeling, on the one hand, and the much more pedestrian commitment to rigorous theorizing, on the other. What Heyck has mainly in mind, in other words, is something more mathematical, more formalist, and more systematic—a "mathematical, behavioral-functional, systems-theoretic approach," to quote one of his formulations—than was the typical, on-the-ground research practice.[93]

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[92] Hunter Heyck, *Age of System: Understanding the Development of Modern Social Science* (Baltimore: Johns Hopkins University Press, 2015).

This isn't to say that there weren't systematizers and formalists among the behavioral sciences ranks—Parsons and more obscure figures like Charles Dodd surely count as such. A loose tradition of formalist linguistics, centered in Cambridge, Massachusetts, and with ties to some Harvard psychologists, was also on the behavioral sciences' periphery. Certainly many behavioral scientists, and their funders, were math-and-modeling *aspirants*, but that is an important distinction.

The contours of what Heyck, in 2015, labels "high modern social science" were there in his earlier work. Without assessing his core high-modernist claims, it is enough to concede that he is on to something significant—that he has identified, and given a name to, an intellectual temper, one inclusive of economics and the variety of postwar systems theories. My claim is that this formation, with its 1955 to 1970 apogee, only partially overlaps with the behavioral sciences movement, which roughly spanned 1945 to 1965.[94] There is, of course, a significant Venn space, populated by figures like Simon, Parsons, and Merrill Flood, as well as projects like Columbia's

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[93] Crowther-Heyck, "Patrons of the Revolution," 431. The conflation comes off in passages like this: "The final element in the behavioralist intellectual agenda was the development of formal theory and formal models. The behavioralists sought to unite the empirical data-gathering characteristic of American social science with the philosophically rigorous theorizing associated with European social thought." 432. He cites the Harvard-specific context of Parsons, as influenced by L. J. Henderson and A. N. Whitehead. On that Harvard context, see Joel Isaac, *Working Knowledge: Making the Human Sciences from Parsons to Kuhn* (Cambridge, MA: Harvard University Press, 2012), chap. 5. But behavioral scientists' marriage of data-gathering and European social thought—much of which was hardly philosophically rigorous—is much better illustrated by Parsons' student, Robert Merton, whose middle-range functionalism and proposed union of European theory and American empiricism was set in polite relief against Parsons' formalist ambitions. See Merton, *Social Theory and Social Structure*, Part III.

[94] Heyck seems to admit the partial-overlap point in *The Age of System*: "The advocates of this type of research," he writes, in an echo of his mid-2000s works, "usually called it 'behavioralism' and began to refer to the social sciences as the 'behavioral sciences.' This behavioralist approach was an important part of the high modern vision for a a new social sciences." The book includes a revised version of the 2006 article as its second chapter; nearly all the book's references to the phrase appear in this revised-reprint chapter. Heyck, *The Age of Systems*, 62, chap. 2.

ONR-sponsored Behavioral Models Project. There is, nevertheless, a traceable distinction, a crucial one for spelling out just what—and when—the behavioral sciences were.

III. The behavioral sciences movement, 1945–1965

The most important contribution of the secondary literature is to provide temporal bookends on the movement. The late arrival of the "behavioral sciences" term, together with the premature closure of Ford's program, makes the task of establishing the movement's temporal scope especially vexing. My approach is to draw on the large literature on the World War II mobilization of U.S. social scientists to make the case that, label aside, the movement got underway during and after the war. As with the Ford case, I cite a blend of social and intellectual factors to support the argument. The core claim is that the psychologists, anthropologists, social scientists, and political scientists who would, after the war, form the nucleus of the behavioral sciences movement, were first mobilized to serve in the federal government's sprawling propaganda and morale bureaucracy. A second claim is that many economists found themselves in distinct wartime networks, often working alongside natural scientists and mathematicians. The upshot is that many economists emerged from the war with weaker connections to their social science counterparts. Many of the same economists, I begin to show, were also poised to campaign, after the war, for an economics discipline centered on modeling, deduction, and the rationality assumption. The burden of my claims, taken together, is to show that the behavioral sciences, as they were eventually called, were already substantially there, with premonitions of the economics-behavioral science divide in place too.

The other bookend is, perhaps, easier to establish. Drawing on the work of Heyck and others, I argue that the behavioral sciences movement declined sharply in the early to mid-1960s. I point to three major factors: a shift in the patronage system in the wake of Sputnik, staggering expansion in the scale of U.S. social science, and high-profile revelations of secret military and CIA funding of behavioral science projects.

The wartime mobilization of U.S. social scientists is extensively documented. Hundreds of social scientists temporarily left their academic posts during the war, to take up direct employment or consultancies for dozens of government and military agencies. Scholars, most of them in Washington, were scattered about an always-evolving acronymic tangle of programs and departments. Significant for our purposes, sociologists, psychologists, and political scientists, along with a smattering of anthropologists, were recruited into agencies tasked with working on Allied propaganda and morale. Economists were typically staffed elsewhere, in bureaus devoted to economic planning but also in prominent natural science–dominated initiatives.

The roots of this divide are complex, and can only be gestured at briefly here. Economists fared far better in the wake of the Rockefeller Foundation's 1930s retrenchment, securing New Deal federal government employment and contract work. In the mid-1930s, meanwhile, a group of entrepreneurial political scientists, sociologists, and psychologists seized on new sampling-based survey methods developed by commercial pollsters. The members of this "public opinion" field would go on to form the nucleus of the enormous propaganda bureaucracy mobilized for World War II. They were mobilized early, owing to the Rockefeller Foundation's tacitly endorsed effort to create a propaganda bureaucracy in waiting, before Pearl Harbor. The public opinion scholars soon joined the official war bureaucracy, alongside many of the rising stars of experimental psychology and quantitative political science in the Army's Research Branch, the Office of Strategic Services, the Office of War Information, the Library of Congress, the Departments of Justice and of Agriculture, the Federal Communications Commission, and many others. The employment overlap of constantly shuffled scholars produced networks of contacts, friendships, and acquaintances that constituted, after the war, the behavioral sciences core.

Economists were conspicuously absent from these agencies' ranks. The single exception is telling: Many economists served side-by-side with other scientists in the OSS's Research & Analysis branch, but maintained a superior and even belligerent attitude toward their non-economist co-workers. The great bulk of economists worked in other agencies connected to managing the enormous economic-planning machinery. An important subset worked alongside natural scientists in Vannevar Bush's Office of Scientific Research and Development (OSRD), forging close ties to the postwar science establishment. Of all the social scientists, only economists served under the OSRD umbrella in significant numbers—and their successful contributions earned them the respect of the natural science establishment as well as key military figures.

The example of the Applied Mathematics Panel (AMP) is an instructive case in point. Formed in 1942 under the leadership of mathematician Warren Weaver, the AMP's mandate was to help solve mathematical problems related to the war effort. A unit of the AMP, the Statistical Research Group (SRG), brought mathematicians and statisticians together with economists like Harold Hotelling, Allen Wallis, Milton Friedman, and George Stigler. The important point for our purposes is that the economists were central contributors to the SRG work, and earned the respect of the military officials and scientists in the OSRD orbit. Their war-won credibility was a direct contributor to the divorce, at the postwar Project RAND, of economics from the other social sciences.

After the war, economics and the other social sciences were already on distinct social tracks. The divide was also, at least incipiently, intellectual. The wartime network of psychologists, political scientists, and sociologists subscribed to each of the six intellectual commitments that would characterize Ford's BSP. Those economists who shared these commitments were, by and large, already enmeshed through wartime service in the same networks. But a larger and more influential segment of the discipline was committed to one or another version of the emerging neoclassical mainstream. There were shared allegiances, around scientific rigor and nomothetic theory. Still, the combination of distinct social networks and the divide over the rationality assumption was decisive.

When the Cold War heated up in 1947 and 1948, the network of sociologists, psychologists, and political scientists that had worked on propaganda and morale topics were, in effect, remobilized.[95] They served on a range of government- and foundation-sponsored projects, some of them directly modeled on the wartime example. It was this group of social scientists—committed to the idea that science and service could be joined—that was the core of the behavioral sciences movement, as anointed by Ford in the early 1950s.

Economists, with some prominent exceptions, weren't remobilized into the Cold War propaganda and morale network. By the early postwar years, economists already had their distinct networks, intellectual coordinates, and funders. All of this played out in the messy divorce between economics and the other social sciences in the BSP.

Three interrelated factors contributed to the movement's mid-1960s decline. One was a major shift in the patronage system that had, for over a decade, served to congregate and underwrite the small-world networks of psychologists, sociologists, anthropologists, and political scientists who formed the behavioral science core. As Heyck draws out brilliantly, the earlier system was centered on the big three foundations and military funders, with its heyday from the early postwar years through to the early 1960s. That system was gradually displaced after Sputnik, which spurred explosive budgetary growth at the civilian funding agencies, notably the National Science Foundation and the National Institute of Mental Health (NIMH). This second system had, by the mid 1960s, largely displaced the first. The stress was on disciplinary funding, especially at the NIMH whose spending came to dwarf foundation budgets. Where the earlier system had favored problem-centered inquiry, the newer regime tended to distinguish between "basic" and "applied" research. The cross-disciplinary brokerage network that so typified the 1950s patrons had, by the mid 1960s, given way to discipline-specific peer review committees managed by civilian-agency bureaucracies.

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[95] This argument is developed in Jefferson Pooley, "The Remobilization of the Propaganda and Morale Network, 1947–1953," working paper, MediArXiv, https://doi.org/10.33767/osf.io/g9rp4.

The behavioral sciences, as a cross-disciplinary movement, also faced the headwinds of scale and generational turnover. The U.S. university system was already growing rapidly throughout the 1950s. That growth accelerated by the end of the decade, with every major social science discipline more than doubling in size by the mid-1960s. What had been a small-world network of mutual acquaintance collapsed under the sheer scale of new entrants into the faculty ranks. At the same time, many of the movement's key figures had come of scholarly age during or after World War II. They were, by the 1960s, starting to retire and otherwise slow down.

A final factor was related to the first two: Camelot. Revelations about covert military and CIA entanglements in social science research, beginning with Project Camelot in 1964, kept cascading through New Left journals and *The New York Times* for years. Even Ford-funded initiatives, like MIT's Center for International Studies, were exposed as secret recipients of CIA dollars. It was no longer feasible to maintain contract work for the Vietnam-era national security state at the elite universities that had incubated the behavioral sciences movement. As a result, military behavioral science, as Joy Rohde has documented, went largely underground.

By some appearances, the behavioral sciences were just getting started. Federal funders were adopting the "behavioral sciences" language with surprising alacrity. In 1962, the "Behavioral Sciences Subpanel" of the President's Science Advisory Committee published a statement on "Strengthening the Behavioral Sciences" in *Science*. In the statement, the subpanel of social-science luminaries called for a boost in research funding and policy consultation, with "behavioral sciences" defined in vague but resolutely scientific terms. The next year, the National Academy of Sciences renamed its Division of Anthropology and Psychology to the Division of Behavioral Sciences.

Beginning in the late 1970s, the label took on a ghost-like afterlife in federal funding nomenclature, as a pejorative, and in the vestigial names of journals and scholarly associations. The term remained in circulation throughout the 1980s, mainly as an unreflective synonym for psychology (often with a biological inflection). Even historians of social science began adopting a compound variant—"social and behavioral sciences"—as an apparent hedge.

More recently, the behavioral sciences have enjoyed revival of sorts, largely on the back of the wildly successful new field of behavioral economics. In 2015, then-President Barack Obama issued an executive order formally establishing a "Social and Behavioral Sciences Team" charged with improving government through behavioral science "insights"—defined as "research findings from behavioral economics and psychology about how people make decisions and act on them."

Perhaps Berelson, who died in 1978, would be pleased with the term's staying power. But there are no behavioral scientists today, not of the kind that the term once named.

IV. Conclusion

In his 1957 postmodern, and again in the 1963 *Behavioral Sciences Today* collection, Berelson wrote that the behavioral sciences are "here to stay."[96] So it is ironic that his 1964 book, *Human Behavior: An Inventory of Scientific Findings*, stands as a coda for a movement that was by then in sharp decline. *Human Behavior*, co-authored with Gary Steiner, was the quixotic culmination of the BSP's stalled "inventories" project.[97] In its original 1951 formulation, the initiative was described as a "Systematic Inventory of the State of Existing Knowledge in Particular Problem Areas." The "time is ripe," Berelson and his co-authors wrote then, for the "systematic codification of propositions in several fields." In its ambition and sweep, the project was quintessential behavioral science: the "rigorous formulation of existing speculations and research results into major generalizations about human behavior and for estimates of the degree of verification for each generalization." The project, if done well, will "help to unify the study of man by incorporating into one theoretical system the contributions of different disciplines and of both speculation and empiricism."[98]

Berelson commissioned six such inventories, on subjects ranging from "Communication" (assigned to psychologist Leon Festinger) to "Organizational Theory" (Herbert Simon) to "Social Stratification" (Edward Shils).[99] The results, as Berelson complained in 1957, did not live up to their original promise. He listed the inventories project as among the BSP's "particularly disappointing" initiatives: "They are just too hard to do; at any rate, no one seems quite to know how to go about it." He expressed puzzlement at the project's failure: "all we wanted was for the scholars to set down in a scientific form what they know or what they think they know, and I still cannot understand why that cannot be done." It must, he concluded, be a 20-year project, rather than a five-year one.[100]

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[96] Berelson, "The Ford Foundation BSP Final Report," 18; Berelson, "Introduction to the Behavioral Sciences," 11.

[97] Bernard Berelson and Gary A. Steiner, *Human Behavior: An Inventory of Scientific Findings* (New York: Harcourt, Brace & World, 1964).

[98] Bernard Berelson et al., "Proposed Plan," 43–44.

[99] Berelson, "The Ford Foundation Behavioral Sciences Report," 35.

[100] Berelson, "The Ford Foundation BSP Final Report," 15.

Berelson, who left his Ford post for stints at Chicago and Columbia, resolved to continue the project. Seven years after the BSP, *Human Behavior* was the published result, dedicated to none other than William McPeak. The 712–page book was organized around fourteen topical chapters, including "The Society," "Culture," and "Motivation." It was a proudly integrative work, as symbolized by 11 pages of small-type credits placed before the book's table of contents. The introduction is prefaced by a full-page of epigraphs, from John Stuart Mill, Samuel Butler, James Bryce, Arnold Toynbee, René Dubos, and George Homans. The book is, Berelson and Steiner wrote, a fulfillment of the original Ford project, an "inventory of the behavioral sciences." Each chapter contained a list of "verified generalizations," arranged in numbered order.[101]

Berelson and Steiner positioned the book as a "milestone on the developmental path" of the behavioral sciences, specified here as "anthropology, psychology, and sociology—minus and plus." The propositions, they admitted, are "presented in an austere and perhaps stark form … simply set down, one after the other, without elaboration, orientation, illustration, speculation, repetition, conceptualization, theorization, and transition that typically accompany such material." The behavioral sciences—"a major intellectual invention of the twentieth century, and largely an American one"—are "still in early process."[102]

Human Behavior was, instead, a last word of sorts. Just a few years later, Donald Marquis, the movement's original Ford champion, issued something more like an obituary:

> I was eager to make a distinction between soft science and hard science. That happens to be my bias to approach psychological and social problems with the standard methods of science and a different label enabled us to define an area rather than to accept largely defined areas. So, behavioral science, was in our thinking, in fact, interpreted as a scientific approach to social problems and originally much too narrowly. This view was shared by Berelson who was appointed to head it. I wouldn't try to do that now, I would put in things like policy and history and so forth. But I was young, eager, overconfident, and not nearly as disillusioned as I am now that the scientific approach to social problems would be the best way to attack them. I wouldn't defend that now.[103]

* * *

What were the behavioral sciences? They were a social and intellectual formation with a mission —a movement, in other words. They were U.S. sociologists, political scientists, psychologists, and anthropologists, joined by a small number of like-minded economists. Most them had worked alongside one another in the World War II propaganda and morale bureaucracy, and were soon brought back together in the early Cold War by foundations and the military funders. They defined themselves against what they saw as a pre-scientific, speculative, meliorist social science, and—to a significant if lesser extent—against the emerging mainstream of postwar economics. Their aim was to promote an alternative vision for social science, one characterized by scientific rigor, nomothetic theory-building, and a broadly empiricist picture of knowledge accumulation. They aspired to fold mathematics into their methodological toolkits. They embraced the view that team-based interdisciplinary projects centered on applied problems could contribute to theoretical progress. They were a small, tight-knit community of American social scientists, clustered at elite institutions and in relative generational synchrony. Their movement, owing to shifts in patronage, the scale of the U.S. university system, and revelations about clandestine ties to the U.S. national security state, was in sharp decline by the mid-1960s.

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[101] Berelson and Steiner, *Human Behavior*, 3.

[102] Berelson and Steiner, *Human Behavior*, 667, 10–11, 13, 12, 667. Berelson and Steiner: "*Minus* such specialized sectors as physiological psychology, archaeology, technical linguistics, and most of physical anthropology; *Plus* social geography, some psychiatry, and the behavioral parts of economics, political science and law." 11.

[103] Oral history interview with Donald Marquis, 27 October 1972, Ford Foundation Oral History Project, FFA, 7–8.

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