

# Society on the Edge

*Social Science and Public Policy in the Postwar  
United States*

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## Introduction

### Whose Social Problems?

Philippe Fontaine and Jefferson D. Pooley

Since ... people variously located in the social structure differ in their appraisal of a particular situation as a social problem, we should be prepared to find ... that the “solutions” proposed for coping with these problems also differ.

Robert Merton and Robert Nisbet, *Contemporary Social Problems*, ix–x

#### 1.1 A Sociologist’s World?

At first glance, the 1961 collection *Contemporary Social Problems* is indistinguishable from dozens of similarly named textbooks. The volume, edited by Robert Merton and Robert Nisbet, was the latest installment in a long-running genre of works aiming to orient American sociology undergraduates to a range of “social problems.” Like its predecessors, the Merton and Nisbet collection featured a chapter-by-chapter march through a succession of named problems such as crime, drug addiction, and family disorganization. So the 1961 textbook was, in its form, unremarkable.

But this was no ordinary social problems textbook. The first clue was authorship: Columbia’s Robert Merton and the Berkeley-trained UC Riverside Dean Robert Nisbet were both theorists, known for grappling with European intellectual traditions. Merton was no stranger to empirical work, but his famous alliance with Paul Lazarsfeld at Columbia’s Bureau of Applied Social Research epitomized – even symbolized – a postwar shift away from sociology’s commitment to social problems in a reformist key. For his part, Nisbet’s only other book, *The Quest for Community*, was a dense and idiosyncratic work of intellectual history. In short, Merton and Nisbet were among the least likely American sociologists to take up the genre.

A second clue, linked to the first, was the editors' first-paragraph claim that a comprehensive theory of social problems was still lacking. What unified their book's chapters was merely a "theoretical *orientation*." That common framework was a loose-fitting version of Merton's functionalism: The volume's contributors, and the editors themselves, stressed consequences over causes, pointed to latent social problems, and placed "systemic interdependence" at the center of analysis.<sup>1</sup>

The book's table of contents was the third clue. Many of the usual problems were represented, with chapters on juvenile delinquency, mental disorders, and race and ethnic relations. But there were a number of unusual inclusions – chapters that had rarely, if ever, appeared in social problems textbooks. One was devoted to traffic and transportation, and another, dropped in the third edition (1971), to the "military establishment."<sup>2</sup> Most surprisingly, Merton and Nisbet commissioned a "disaster" chapter, focused on calamities such as tornadoes, floods, and earthquakes, which seem only glancingly social.

The 1961 collection was, in short, a surprising intervention. The phrase itself, "social problems," was a token for an approach to sociology from which Merton and Nisbet had distanced themselves. The "social problems" course, the "social problems" textbook: These were the hallmarks of an older, "amateur" phase of the discipline's history, the kind of sociology that Merton and his Harvard teacher Talcott Parsons had, after World War II, helped expel to the margins. The whiff of reform and Midwestern starch hung about the phrase. The discipline's new elite, forged in shared wartime service, favored systematic theory, sophisticated quantitative methods, and value-free scientific rigor – very much like its counterpart in economics.

It was around this time, indeed, that some of these new-style economists – notably Gary Becker – were claiming rights over those social problems Merton and Nisbet meant to make their own. So the appearance of *Contemporary Social Problems* in 1961 – on the edge of the decade's social unrest – was a revival of a peculiar kind. Merton and Nisbet, having won the battle for the discipline, were now claiming the vanquished tradition's core domain. They were likely aware of the economists' nascent and still-marginal enterprise. But their aim was redemptive, not defensive: to introduce sociological theory into a social problems literature that was,

<sup>1</sup> Merton and Nisbet, *Contemporary Social Problems*, vii, viii, x.

<sup>2</sup> Merton and Nisbet, *Contemporary Social Problems*, 3rd ed.

in their view, theoretically impoverished and hopelessly fragmented.<sup>3</sup> Economists interested in “noneconomic” problems would have endorsed the critique, but their remedy was of a different, and more auspicious, character.

Though these economists were not cited, references to psychologists abound throughout the book. The relative prominence of psychology can be explained by its more constructive relationship with sociology, as exemplified by the expansion of interdisciplinary social psychology after the war.<sup>4</sup> But arguably more important still was what Ellen Herman has described as a shift “toward a larger jurisdiction for psychology.”<sup>5</sup> With deep roots in the cross-disciplinary projects of the war, that shift marked psychology’s increased involvement with a wide range of social problems during the early postwar decades. The discipline’s protean character encouraged its broad application, and helped to spread its language to the other social sciences.<sup>6</sup> By 1961, psychology’s expanded remit had registered with sociologists studying social problems, as an individualistic complement to their approach.<sup>7</sup> The result was that psychologists’ efforts to expand their jurisdiction over social problems found a more receptive audience, at least initially, than did economists.

The appearance of psychologists and, more tentatively, economists on the social problems terrain was, in its way, a reminder that their study has always had a double character. The vocabulary of “social problems”

<sup>3</sup> Merton and Nisbet’s charge was itself, arguably, unfair: The prewar social problems textbooks, and other works by their authors, were steeped in theory – albeit of a distinctive mode (“history of social thought”), with its own touchstones, referents, and Spencerian residues. See Turner and Turner, *Impossible Science*, 121–28; and Hinkle, *Developments in American Sociological Theory*, 7–12, 186–90. Still, Erwin Smigel, in the preface to his 1971 *Handbook on the Study of Social Problems*, admitted that “[w]e have not been able to find a unifying theory for the study of social problems.” Smigel, “Preface,” vii.

<sup>4</sup> Sewell, “Some Reflections on the Golden Age”; and House, “Social Psychology, Social Science, and Economics,” 233–35.

<sup>5</sup> Herman, *Romance of American Psychology*, chap. 11. Herman’s title, as she notes herself, is inspired from Abraham Maslow’s title of part 1 in *Toward a Psychology of Being*.

<sup>6</sup> On the protean nature of psychology and its implications for the discipline’s relevance to a wide range of issues, see Capshew, *Psychologists on the March*, 54.

<sup>7</sup> See Merton’s comments on the “bridge-building game” between the two disciplines, delivered at a 1955 conference on juvenile delinquency: “The tactic that could be most helpful, it seems to me, would be for us to join together and fuse our respective sensitivities from time to time but, in the main, to continue to develop the conceptions most pertinent to each field.” Merton, “Concluding Comments and an Example,” 79.

developed primarily within sociology, but the study of the problems themselves was always and already a transdisciplinary endeavor. Sociologists have claimed the social problems label, but scholarship on the problems of society – the alternative phrase we adopt to signal this ecumenism – has featured the other social sciences too. In the decades after Merton and Nisbet’s volume, sociology’s always-partial claims for jurisdiction, if anything, weakened further.

The social problems literature in sociology had been ushered in fifty years earlier, in *Sociology and Modern Social Problems*, a 1910 “elementary text” authored by Missouri’s Charles Ellwood and designed for sociology courses centered on “current social problems.”<sup>8</sup> The volume treated the family as the main locus of social challenge, though a handful of late chapters addressed a series of related domains: population growth, immigration, the “negro problem,” the “problem of the city,” crime, and “poverty and pauperism.” The text’s basic orientation – even its reformist politics – would remain a staple of the social problems textbook for decades. The volume’s chapter-per-problem format as well as its catalog of named problems were embraced by the many competing texts published in the 1920s and 1930s to service sociology’s undergraduate curriculum.<sup>9</sup> Ellwood himself became the chief interwar proponent of “social problems” as the discipline’s anchoring orientation.<sup>10</sup>

American sociology was, in organizational terms at least, built on the idea of “social problems.” The phrase supplied a practical scheme to organize the would-be discipline. Even its subfields and journals began to mimic the problem-by-problem “sociology of” schematic pattern. The “social problems” construct, in short, helped sociology establish its distinctive identity – and its institutional foothold in the US academy. By the 1930s, however, advocates for a more rigorous, and resolutely quantitative, science of sociology took aim at the social problems paradigm. They lumped its textbooks and leading figures, Ellwood included, together with social work, public edification, settlement houses, and moralizing do-goodism.<sup>11</sup> This Depression-era assault on the social problems approach was, in its way, new.

<sup>8</sup> Ellwood, *Sociology and Modern Social Problems*, 3.

<sup>9</sup> Reinhardt, “Trends in the Teaching of ‘Social Problems.’”

<sup>10</sup> Turner, “Life in the First Half-Century of Sociology” and LoConto, “Charles A. Ellwood and the End of Sociology.”

<sup>11</sup> Turner and Turner, *The Impossible Science*, chaps. 2–3; Turner, *American Sociology*, chap. 2; and Bannister, *Sociology and Scientism*, chap. 13.

But the rhetoric of science – the boundary work with reform – had been a staple of the proto-discipline from its late nineteenth-century origins.<sup>12</sup>

What made the 1930s different was that sociologists turned on themselves. The rhetorical demarcation had, in preceding decades, existed in uneasy admixture with ongoing reform commitments.<sup>13</sup> Now advocates of a scientific sociology drew the border to exclude not just “outsiders” such as Christian temperance activists and social workers, but also those fellow sociologists insufficiently weaned from the discipline’s reformist past. The sometimes-belligerent campaign was waged by evangelists for statistical methods.<sup>14</sup> Ellwood and his reform-minded allies answered in kind.<sup>15</sup> There was an organizational flashpoint – the mid-1930s fight over the American Sociological Society (ASS) and the Chicago-based *American Journal of Sociology* (AJS). At the time, some of the discipline’s oldest and best-established outposts were scattered across the country’s non-elite universities and colleges, with particular strength in Catholic institutions. Many of these programs retained an ameliorist orientation, with close ties to social reform movements – Christian and nominally secular – long

<sup>12</sup> Sociology, together with its barely differentiated siblings in the American Social Science Association, was baptized in applied social reform during the last decades of the nineteenth century. There is an extensive literature on the proto-discipline’s engagements with, and resistance to, “reform” in all its typical (and often feminized) meanings. On the late nineteenth-century context, see, for example, Furner, *Advocacy and Objectivity* and Haskell, *Emergence of Professional Social Science*, esp. chaps. 9–10. On the settlement house movement, see, for example, Deegan, *Jane Addams and the Men of the Chicago School* and Lengermann and Niebrugge-Brantley, “Back to the Future.” On the social survey movement, see, for example, O’Connor, *Poverty Knowledge*, 26–44 and Gordon, “Social Survey Movement and Sociology.” On social work in particular, see Lengermann and Niebrugge, “Thrice Told.”

<sup>13</sup> Calhoun, “Sociology in America,” 10–19. Early figures in the discipline, such as Lester Ward, Albion Small, and Franklin Giddings, had made claims for sociology’s scientific character, even as they remained variously entwined with reform groups and initiatives. Second-generation sociologists like W. I. Thomas and Robert Park – indeed, Ellwood himself – adopted a similar rhetoric of scientific distance, likewise belied in practice by their on-the-ground alliances with philanthropists and Social Gospel reformers. The main strategy to square the science/reform circle – a tack also adopted by the social problems textbook authors of the 1920s – was to insist on a division of labor: The sociologist supplies the analytical guidance, while the reformers and politicians are on the hook for implementation. Turner, “Origins of ‘Mainstream Sociology’ and Other Issues,” 56–58.

<sup>14</sup> The backdrop to the struggle was mounting frustration with sociology’s apparent public ineffectuality as symbolized by its virtual exclusion from the New Deal bureaucracies that employed so many economists and political scientists. See Camic, “On Edge.”

<sup>15</sup> See, for example, Ellwood, *Methods in Sociology*.



after Chicago's department had rejected its reformist roots. In 1936, they won control of the ASS and launched a rival, ASS-sponsored flagship, the *American Sociological Review* (ASR). It was, however, a Pyrrhic victory: Soon enough the ASR itself became a platform for the very brand of rigorous, quantitative empiricism that Ellwood and his allies had earlier resisted.<sup>16</sup>

The Depression-era struggle over the discipline's future was not, however, resolved by the manifestos for quantification penned by advocates of a more scientific sociology. The key factors, instead, were generational turnover and World War II.<sup>17</sup> By the end of the war, when the academic job market picked up in earnest, many of the quantitative insurgents were nearing retirement. The result was a vacuum in disciplinary leadership that a younger cohort – figures such as Samuel Stouffer and Robert Merton – soon filled. The wartime mobilization was decisive for a number of mutually reinforcing reasons: Shared service in Washington and overseas, with all its agency-spawning cross-pollination, helped to connect young sociologists with each other and with like-minded social scientists from other disciplines. The team-based work itself, some of it employing new survey methods, was widely perceived as a down payment on a postwar social science of on-the-cusp promise.

There were other factors. Some members of the new elite, including Parsons (an erstwhile economist) and Lazarsfeld (an applied psychologist), were disciplinary outsiders with few commitments to prewar American sociology. There was, too, explicit postwar Congressional concern (buoyed by natural scientists) about the social sciences' alleged reformism, even before the "social"/"socialism" conflation became an early Cold War staple.<sup>18</sup> The post-World War II upstarts were, moreover, over-represented at Ivy League schools, maintained close ties to the New York foundation

<sup>16</sup> Lengermann, "Founding of the *American Sociological Review*"; Turner and Turner, *The Impossible Science*, 60–62, 81 n23; Abbott, *Department and Discipline*, 106–17; and Bannister, *Sociology and Scientism*, chaps. 14–15. As Lengermann's meticulous anatomy makes clear, the conflict was multi-dimensional, though it centered on a populist revolt against a perceived elite. The targets of the populists' ire included the Chicago department – in all its methodological diversity – as well as the discipline's leading evangelists for "scientific" quantification.

<sup>17</sup> Abbott and Sparrow, "Hot War, Cold War"; Steinmetz, "American Sociology before and after World War II"; Converse, *Survey Research in the United States*, chaps. 5–7; Turner and Turner, *The Impossible Science*, chap. 3; and Platt, *History of Sociological Research Methods in America*, 228–30.

<sup>18</sup> Solovey, "Riding Natural Scientists' Coattails."

world, and served as key brokers in the government/foundation patronage network of the 1950s.<sup>19</sup>

The relevant point is that sociology's "east coast fraternity," hitched to a cross-disciplinary movement to accent the science in social science, had displaced sociology's already-battered, teaching-oriented majority.<sup>20</sup> Clearly, the "postwar settlement," in its paradigmatic mix of survey methods and functional theory, was never accepted by the full discipline.<sup>21</sup> But the social problems tradition, in particular, was widely discredited.<sup>22</sup> Indeed, it served as a symbolic and field-defining rejected past. The movement to remake sociology as a science was won, in other words, through a series of repudiations, articulated in a litany of pejoratives: speculative, edifying, reformist, Christian, and impressionistic. Methodological rigor and theoretical sophistication were the proposed substitutes. For the theory, the Ivy departments turned to European sociology as an alternative genealogy, with such remarkable success that the field's classical pantheon was – after

<sup>19</sup> On the early Cold War brokerage and patronage networks, see Crowther-Heyck, "Patrons of the Revolution" and Solovey, *Shaky Foundations*, chaps. 2–3.

<sup>20</sup> Abbott and Sparrow, "Hot War, Cold War," 296. The standard story of mid-century American sociology treats the changes outlined here as (using the department shorthands) a displacement of Chicago by Harvard and Columbia. Chicago, in these accounts, is treated as a bastion of textured and qualitative empiricism then smothered by an alliance of quantitative technicians and high functionalists at Harvard and Columbia. This account is caricatural. Columbia's department, for instance, was already prominent decades before the fateful 1941 meeting of Merton and Lazarsfeld. For its part, the interwar Chicago department was both more plural, and less dominant, than the typical interwar account allows. (As Andrew Abbott has shown, the idea of the "Chicago School" was a retroactive creation of the early 1950s, when the department briefly embodied the traits it projected onto its past. Abbott, *Department and Discipline*, chap. 2.) But the main problem with the Chicago-Harvard-Columbia emplotment is that most of the country's interwar departments and programs, and many of its sociologists, are left out.

<sup>21</sup> Steinmetz, "American Sociology before and after World War II," 339.

<sup>22</sup> In a soon-famous 1943 *AJS* study, a young C. Wright Mills surveyed interwar social problems textbooks to sketch out what he called American sociology's "common style of thought." He treated the books (over thirty of them) as a proxy for the discipline's "professional ideology" – as a more-or-less faithful register of its commitments. The texts, he added, are "empirically confused," fragmentary, and indifferent to structural patterns – and thereby leave out the "larger problems of social structure." The typical sociologist, in Mills' wartime portrait, was provincial, small-minded, and Babbitt-like. Mills, "Professional Ideology of Social Pathologists," 165–66. A similarly caustic account, published two years later, came from criminologist Edwin Sutherland: "The textbooks display a minimum of abstraction and a maximum of the commonplace." Sutherland, "Social Pathology," 430. See also Emil Bend and Martin Vogelfanger's quarter-century textbook survey, "New Look at Mills' Critique."

the war – almost completely repopulated by European figures like Max Weber and Emile Durkheim.<sup>23</sup>

The discipline's new, Eastern seaboard mainstream did not, crucially, rule out problem-oriented work. Indeed, many of its core commitments – around methodological rigor and theory-building, for example – were forged in the wartime mobilization. The lessons of the war, in turn, helped guide the self-identified “behavioral sciences” movement among sociologists and other social scientists during the early Cold War: Problem-oriented work, funded by foundation or government patrons, was especially well suited to the large-scale, team-based empirical projects that general theory-building required.<sup>24</sup> So work on *problems*, even those within the traditional social problems array, was perfectly compatible with the postwar formation – even if, in practice, the early Cold War mix of projects was heavy on overseas propaganda and morale topics. The point is that applied work was welcomed into the house of the ascendant behavioral sciences while the meliorist social problems tradition was not.<sup>25</sup>

The Society for the Study of Social Problems (SSSP) was founded in 1951 as a protest against the postwar marginality of that meliorist tradition.<sup>26</sup> The new group openly defined itself against the behavioral sciences vanguard. Its aim was to shelter a reformist alternative to the apolitical mainstream by then ensconced in the ASS leadership. The SSSP's founders, and its early membership, were largely drawn from the same Midwestern departments

<sup>23</sup> See, for example, Connell, “Why is Classical Theory Classical?” and Scaff, “Max Weber and the Social Sciences.”

<sup>24</sup> The best overview of the “behavioral sciences” movement – the self-understood clustering of sociologists, social psychologists, and political scientists (the latter with their own, complementary “behavioralism” moniker) in the early Cold War, with participation from some anthropologists and a handful of economists – remains Crowther-Heyck, *Herbert A. Simon*, chap. 5. For a history of the label including the crucial role of the modern Ford Foundation, see Pooley, “‘Not Particularly Felicitous’ Phrase.”

<sup>25</sup> This distinction helps explain our interpretative difference with Arnold Rose. In his posthumous 1971 history of social problems research, Rose discerned a drop-off of sociological interest in the interwar period, with a postwar revival linked to the war's boost to applied research. Rose, “History and Sociology of the Study of Social Problems,” 7–9. Our own review of the primary and secondary literature, however, suggests that the interwar period represented the heyday of self-conscious sociological engagement with “social problems” – and that, by the early postwar years, that tradition was marginalized. Rose's postwar narrative conflates a broader problem orientation, or openness to applied work, with the reformist social problems tradition.

<sup>26</sup> On the SSSP, see Skura, “Constraints on a Reform Movement” and Abbott, *Department and Discipline*, 78–79.

that had long incubated the discipline's undergraduate substrate.<sup>27</sup> Especially in its first decade, the SSSP was the organizational redoubt for the discipline's half-vanquished social problems tradition.

The SSSP was a backlash organization. The group took aim at the rising generation of Eastern seaboard sociologists who had refined and tested new quantitative methods, as well as affirmed the primacy of disciplinary issues – taken as “scientific” problems – over the older, reform-tainted social problems formulation. But the SSSP was not, by its own self-definition, bounded by a putative “social problems” subfield. The group, instead, represented an alternative – and besieged – orientation toward sociology as a whole. It was fitting, then, that Alvin Gouldner, Merton's former student, delivered the group's presidential address at its annual meeting in 1961.<sup>28</sup> The speech was an unbridled attack on the postwar sociological establishment. Gouldner assailed sociologists' claims to value freedom, in a line of critique that would – by the end of the decade – find wide appeal among student protesters.<sup>29</sup> The mantle of objectivity, to Gouldner, was a license to neglect real human problems in the service of professional self-interest. “In return for a measure of autonomy and social support,” he wrote, “many social scientists have surrendered their critical impulses.” The “dominant drift” of American sociology, he concluded, was a self-chosen segregation, a moral abnegation.<sup>30</sup> The antiseptic detachment of Merton and Nisbet's *Contemporary Social Problems*, published the same year, was a case in point.

By the early 1960s, then, the discipline's erstwhile social problems tradition had weakened, and alternative bids for the domain were gathering momentum. The SSSP claimed the interwar legacy, though with a leftward, minoritarian pitch. That claim was not, however, honored by the postwar mainstream. Over the subsequent, turbulent decade, American sociology settled into a

<sup>27</sup> The founders enlisted the University of Chicago's department as a symbolic ally, with cooperation from notable Chicago figures Ernest W. Burgess (the SSSP's first president) and Herbert Blumer (its third). Skura, “Constraints on a Reform Movement,” 71. Alfred McClung Lee was, however, the organization's real leader, and he assumed, from the beginning, a defiant posture toward the discipline's elites. Lee, based at Brooklyn College, positioned his pugilistic 1954 presidential address as a David-and-Goliath rejoinder to Harvard's Samuel Stouffer's speech as ASS president the year before. Lee, “Sociologists in an Integrating Society.”

<sup>28</sup> Gouldner, “Anti-Minotaur.”

<sup>29</sup> If the social problems tradition had supplied, for the early postwar elites, a useful symbolic contrast, the idea of an establishment-cozy “mainstream” furnished something similar for post-1968 sociologist-dissidents. See Calhoun and VanAntwerpen, “Orthodoxy, Heterodoxy, and Hierarchy.”

<sup>30</sup> Gouldner, “Anti-Minotaur,” 206, 207.

pattern of joint custody of the social problems terrain. The social movement activism of the early 1960s, at places such as Greensboro and Port Huron, had mushroomed into nationwide protest and unrest – including conservative backlash – by the decade’s end. Many of the demonstrations and disturbances centered on the “classic” social problems: race relations, urban life, crime, the family, poverty, education, and war. The political system registered the public clamor around these issues, by way of campaigns, commissions, policy proposals, programs, and legislation. The qualified interventionism of Lyndon Johnson’s Great Society platform was only the most visible stimulus.

One result was an efflorescence of sociological work on the conventional social problems, some of it sponsored by government agencies and the big foundations themselves. On the heels of the vast postwar expansion of the US university system, and sociology in particular, the discipline produced a massive wave of scholarship – much of it consciously policy-relevant. The 1960s unrest helped to yoke the behavioral sciences – the sociological elite included – to domestic problems. The RAND Corporation’s turn from war-gaming to urban poverty was, in that decade, a highly visible instance.<sup>31</sup> Radical interventions, allied with the New Left student movement and growing black militancy, cohabitated with the cross-tabulated sobriety of the discipline’s mainstream. This was, taken as a whole, a lively postscript to Merton and Nisbet’s improbable claim on the discipline’s social problems tradition.

But the main result of the 1960s for sociology was, if anything, the unraveling of the discipline’s claim to sovereignty over the “social problems” domain. By the end of the decade, sociology’s hold on social problems had weakened. The fall-off looked steeper still by the close of the twentieth century, at least as registered in political and policy impact.<sup>32</sup> By contrast, psychologists and economists won a larger jurisdiction for their disciplines in the wake of the 1960s. Psychologists benefited from the growing belief that problems of society could be approached with the techniques of individual diagnosis, while economists capitalized on the pervasiveness of the market metaphor. More generally, their work seemed congruent with the intellectual assumptions of the last third of the twentieth century. In Daniel Rodgers’ words, the disciplines contributed to and built upon “conceptions of human nature that stressed choice, agency, performance, and desire.”<sup>33</sup>

<sup>31</sup> Light, *From Warfare to Welfare*.

<sup>32</sup> For a merciless account of sociology’s general loss of policy influence in the two decades after 2000, see Turner, “More American Sociology Seeks to Become a Politically-Relevant Discipline.”

<sup>33</sup> Rodgers, *Age of Fracture*, 3.

The book's nine chapters explore this apparent shift in the postwar division of labor in social science, each refracted through a single "problem": the family, crime, the black ghetto, education, discrimination, poverty, addiction, war, and mental health. The authors' accounts address three major themes. The first is *problem status* itself: How and when did the issue (poverty, for example, or crime) come to be viewed as a problem of public significance, and to whom? For most chapters, this question implicates the interwar period and the wartime context itself. Each chapter traces its designated problem's career – the waxing and waning of social-scientific interest, public concern, and political intervention in that particular area. Documenting shifts in the *disciplinary division of labor* since World War II is the collection's second aim. The relative contributions of the various social sciences – in cooperation or rivalry, as these patterns shifted over time – are taken up by each chapter in accounts that, juxtaposed to one another, record the disciplines' contrasting fortunes. The book's chapters all take up, finally, the signal question of social problems' *explanation*. Did the leading approaches of one or another social scientist or discipline point to individuals as the source of the problem? Or did they stress social conditions that supersede and overlay the experience of the individual? To what extent, if at all, were structural or systemic accounts accorded priority in one period or another? The answers to these questions have obvious implications for policy. The contributors investigate, in the context of their problem-subject, the vexed and bidirectional relationship between social science and the political system over the postwar decades.

The balance of this introduction follows a similar format. We take up, first, the question of social problem status, move on to assess the redrawn boundaries of postwar social science, and conclude with a meditation on the paired questions of policy and explanatory resonance. Our aim is to make sense of a tension exposed by the chapters, taken as a whole. The authors document a divergence between developments *internal* to the social sciences, on the one hand, and the fate of competing disciplinary frameworks at the level of *policy and politics*, on the other. Crudely speaking – that is, with the variation across the problem domains bracketed – the core social sciences, sociology included, remain active contributors to the problem-specific literatures through the period under study. On the question of reigning explanatory mode, there is, again, no simple shift from, say, structural or systemic explanations to those that favor individual factors. And yet the chapters, taken together, register clear disciplinary "winners" at the level of policy and politics: namely, economics and psychology. The volume's

contributions, likewise, trace a postwar attenuation of social-structural ways of seeing problems like racial prejudice and poverty – though again, only at the intersection of politics and social science.

We conclude by grappling with this discrepancy. Disciplinary frameworks such as *homo economicus* and psychological individualism, we argue, both contributed to, and benefited from, a conception of society that, by the 1970s, was increasingly skeptical of social explanations and interventions. Likewise, academic discourse and public debate were embedded in, and helped to reinforce, a social imaginary that was already tuned to personal frames of understanding. Psychology and economics thrived because they resonated with the era's growing individualism, while also helping to underwrite it. The story is one of an elective affinity between these disciplines and a public imagination that was decreasingly social-structural and decidedly not sociological.

## 1.2 Problem Status

Sociologists, as the term “social problems” took hold in the early interwar years, raised the obvious definitional question: What counts as a social problem? The field's textbooks and coursework supplied an ad hoc answer, in their problem-a-week inventories: A social problem, according to their implicit argument, is one of the pathologies listed in the table of contents. A more deliberate literature of reflection, making the number of people affected a defining characteristic of a social problem, began to appear in the early 1920s, and then reappeared in regular installments – across commentaries, presidential addresses, and theoretical treatises – for the balance of the century.

The key issues, if not the problem-designates themselves, have remained consistent over the decades. Are social problems merely “what people think they are” – in the blunt words of an influential 1941 intervention?<sup>34</sup> Or are there objective criteria – actual social conditions, for instance – that scholars might use to qualify an issue as problematic? If indeed “objective” conditions are a legitimate yardstick, what is the measure of social *health* that specifies the pathology? The main (if often implicit answer) in the interwar years was *order* – stability in the face of rapid, centrifugal social change. “Social disorganization” was the ur-problem, in other words – the

<sup>34</sup> Fuller and Myers, “Some Aspects of a Theory of Social Problems,” 25. For all their subjectivist bravado, Fuller and Myers remained committed to a definition that includes an “objective phase” – a “verifiable condition, situation, or event.”

byproduct of modernity expressed through crime, poverty, and the other specific problems. An alternative approach, one that looked to prevailing *norms*, gained some traction in the 1930s: A social problem is defined by the gap between consensus values and conditions on the ground. If social scientists can identify the norms and their violation, they can name the pathology. A late-appearing rival to the *order* and *norm* theories, which won significant uptake only in the 1970s, dispensed with objective conditions altogether. A social problem, on the strong subjectivist view, is what people call a social problem.

The point is that a discourse on problem status – a running debate on the definition of a “social problem” itself – developed alongside the teaching and empirical study of specific pathologies. The disputes and definitional struggles took place, for the most part, among sociologists, but they are, nevertheless, relevant to the analysis of problems of society in general.<sup>35</sup> The act of naming problems, in patterned repetition, has helped to call those very problems into existence – and to attract the attention of a growing number of observers to their significance. There is a creative, illocutionary character to the designation of social pathology. So too with the running deliberation – the meta-discourse, as it were – over what elevates a topic to problem status. Our own approach, moreover, to the problems of society and their study – the object of this volume – is indebted to the discourse, in particular the subjectivist turn of the 1970s. So it is worth recounting here.

As we have seen, Ellwood’s (1910) textbook initiated the ad hoc tradition of social problem designation: Social problems, in effect, are what sociologists think they are. That there are identifiable “social evils” was, for Ellwood, a given, and so his stepwise elaboration required no warrant. The pathologies were announced by the chapter titles: “The Problem of the Modern Family,” “The Immigration Problem,” “The Problem of the City,” and on down the list.<sup>36</sup> Ellwood’s textbook was reissued in 1919 in an enlarged edition, and by the middle of the next decade four competitors had emerged to serve the

<sup>35</sup> There is no robust history of the “social problems” discourse in American sociology. The best overview is Senn and Senn, “What is a Social Problem?” See also Martindale, “Social Disorganization” and Spector and Kitsuse, *Constructing Social Problems*, chaps. 2–3, though the narrative is set up against the authors’ theory. Two other brief histories appeared in the 1971 *Handbook on the Study of Social Problems*: Rose, “History and Sociology of the Study of Social Problems”; and Chall, “Notes Toward a History.”

<sup>36</sup> Ellwood, *Sociology and Modern Social Problems*, 5–6, 13.



proliferating core course in the discipline.<sup>37</sup> These five texts, placed side by side, demonstrate the arbitrary (or at least eclectic) character of the problem selection process. Only the family is named as a problem across all five books, while just poverty and labor conditions appear in three. There is a long tail of singletons (like “The ‘revolt of the young’” in one, or “The drug habit” in the other), and just a handful of topics that appear twice: race, immigration, crime, health, education, and war. Perhaps it is fitting that a sociologist, writing in 1929, managed to identify 396 “major” social problems.<sup>38</sup>

A survey of American sociology departments, published that year, found that over half offered a generic “Social Problems” course, with many programs also reporting specialized courses such as “Drug Addiction and Alcoholism.” The departments, taken together, listed forty-four distinct problems covered in their core “Social Problems” courses, though a handful of topics stood out: poverty, crime, the family, race problems, population, immigration, and divorce were each reported by about two-thirds of respondents.<sup>39</sup> So some stability – perhaps achieved through self-generated momentum, cross-pollination, and the textbook/course interplay – was evident by the end of the decade.

We conducted our own analysis of textbooks published in the early postwar years, tallying the chapter-problems of the six leading volumes.<sup>40</sup> Some of the same topics were prominent: crime and the family appeared in all six books, with race and poverty included in all but one volume. A few of the leading problems in 1929 were less salient by the early 1950s: Only three books devoted a chapter to immigration, with still fewer devoted to population (two) or divorce (one). Newly prominent on the textbook agenda were mental health (five chapters) and education (four) – neither of which had made the top ten in 1929. Perhaps reflecting the temper of the time,

<sup>37</sup> Ellwood, *Sociology and Modern Social Problems*, rev. and enlarged ed. The others were Binder, *Major Social Problems* (1920); Parsons, *Introduction to Modern Social Problems* (1924); Beach, *Introduction to Sociology and Social Problems* (1925); and Queen and Mann, *Social Pathology* (1925). Two other major texts appeared in 1927: Odum’s *Man’s Quest for Social Guidance* and Bossard’s *Problems of Social Well-Being*. A 1929 survey of departments found that Queen and Mann, Odum, and Bossard were the most frequently used. Reinhardt, “Trends in the Teaching of ‘Social Problems,’” 383.

<sup>38</sup> Bowden, “Our 396 Major Social Problems.” Wrote Bowden: “We have many social problems.” 397.

<sup>39</sup> Reinhardt, “Trends in the Teaching of ‘Social Problems,’” 382.

<sup>40</sup> The six were selected on the basis of a total count of book reviews from 1930 to 1960 in JSTOR’s “Sociology” journal collection, with the earliest edition appearing in the 1950s chosen for analysis. The books are Horton and Leslie, *Sociology of Social Problems* (1955); Landis, *Social Problems in Nation and World* (1959); Merrill, *Social Problems* (1950); Neumeyer, *Social Problems and the Changing Society* (1953); Raab and Selznick, *Major Social Problems* (1959); and Weaver, *Social Problems* (1951).

chapters on civil liberties, natural resources, and mass communication each made an appearance.

The topical drift over the decades is one index of what long prevailed as the main method for identifying problems: the fiat of sociologist-authors.<sup>41</sup> This untheorized, taken-for-granted adhocism – together with an unexamined substrate of gauzy reformism – was already provoking critique in the interwar years.<sup>42</sup> The charge of reformism was, to a point, fair. The interwar social problems orientation was predicated on faith in guided progress. The common view was that – with the help of social science, moral uplift, and modest government intervention – modernity’s rough edges could be smoothed out. This bundle of assumptions was shared with other Progressive reformers, and consistent, too, with most of the American discipline’s late nineteenth-century pioneers. The basic idea was that the onrush of social change had outpaced society’s ability to hold itself together – but that “social control” could be re-established. The agitations of urban-industrial civilization, in other words, had so jostled the social order that social problems – byproducts of disorder – had shaken loose. These problems, crucially, were fixable: Social institutions and norms, with the aid of social science, just needed to catch up with social reality. Social problems, whatever their magnitude, were amenable to social intervention.

Summarized in the language of the time, social problems resulted from the “social disorganization” generated by “cultural lag,” thereby requiring the remedy of “social control.”<sup>43</sup> This worldview furnished, for

<sup>41</sup> The unarticulated selection process remains a fixture of the textbook market, as Joel Best observed in 2006: “publishers continue to churn out old-style, theoretically incoherent textbooks for traditional problem-of-the-week courses.” Best, “Whatever Happened to Social Pathology?” 536.

<sup>42</sup> This avoidance of theory was a point of pride to Ellwood back in 1910: The book, he wrote, “is not intended to be a contribution to sociological theory, and no attempt is made to give a systematic presentation of theory. Rather, the student’s attention is called to certain obvious and elementary forces in the social life, and he is left to work out his own system of social theory.” Ellwood, *Sociology and Modern Social Problems*, 3.

<sup>43</sup> “Social control” was already in wide circulation by the 1890s. “Social disorganization” was popularized by W. I. Thomas and Florian Znaniecki’s *The Polish Peasant in Europe and America* (1918), while “cultural lag” was not coined by Chicago sociologist William Ogburn until 1922. Ogburn, *Social Change*, Part IV. Even so, the ideas behind cultural lag and behind social disorganization were prevalent from the beginning of the social problems tradition. Take the case of Ellwood’s initial 1910 text: “old habits are usually not replaced by new habits without an intervening period of confusion and uncertainty. In other words, in the transition from old habit to the new habit there is much opportunity for disorganization and disintegration.” Ellwood, *Sociology and Modern Social Problems*, 131.

interwar American sociologists, an implicit theory of social pathology. Social problems were problems of modernity, brought on by the kinetic novelty of the factory floor and tenement congestion. Social order was the background against which problems were identified. What then counts as a social problem? Disorder, the unraveling of norms, the attenuation of social bonds, as expressed in the broken family or the juvenile delinquent. The task of the sociologist, in the view of interwar social pathologists, was to diagnose the breakdown of order – the deviations from the solidarity that society’s progress required. Theirs was, in other words, an under-articulated functionalism, with cohesion as the normative benchmark. The problems, moreover, were linked to solutions: The social scientist not only named the pathology, but also lent a hand in devising the treatment. For most interwar authors, the point was not explicit activism – which, after all, smacked of social work and moralistic reformism.<sup>44</sup> Instead, the sociologist, and perhaps social science as a whole, furnished expert guidance to the actual agents of social control.

So the mainline stance toward social problems was objectivist: Pathologies are real and discernible. Already by the mid-1920s, however, a dissenting view crept in, one that insisted on public *recognition* for an issue to count as a problem. Writing in 1924, the sociologist Clarence Case tethered problem status to the “attention of a considerable number of competent observers within a society,” who clamor for a social remedy. Case’s was the first articulation of a subjectivist alternative, one that (in his words) sees a social problem as “partly a state of the social mind and hence not purely a matter of unfavorable objective conditions.” Here public consternation was a necessary, if not yet sufficient, criterion for problem status: The “public mind ... *recognizes the existence* of the problem; and perceives also the fact that it must be collectively solved.” This alternative to the prevailing certitude – that there were problems out there that sociologists can identify – was, in a way, subjectivist twice over. The social scientist deferred to the public’s judgment that a state of affairs is problematic, which also entailed deference to a set of social norms – the ones that were violated. There was even a third dimension of relevant public belief: the shared view that a given problem is in fact addressable. For all the weight he granted to collective

<sup>44</sup> From Ellwood on, the social problems literature took exquisite pains to distinguish sociology and/or social science, with its scientific character, from the work of reformers, social workers, and legislators.

belief, Case preserved a role for objective inquiry: A social scientist must still ratify that “adverse social conditions” obtain.<sup>45</sup>

Case’s qualified subjectivism was subsequently endorsed by a number of other interwar sociologists who emphasized the importance of value *conflict* in the definitional process.<sup>46</sup> Indeed, Richard Fuller’s quarrel with the mainstream “social disorganization approach” concerned its neglect of clashing norms. Warring interest groups cannot even agree on what counts as a problem, let alone a solution, since they bring incompatible values to the battlefield. Consider the “problem” of unemployment, which many economists and employers take to be an inevitable byproduct of a market economy. Treating labor-market conditions, in this example, “as if they were objective states of social disorganization gets us nowhere.” The “clash of social interests” is the core of any social problem, and so the task of the social scientist is to analyze that definitional struggle.<sup>47</sup> Thus, according to Fuller and a co-author, social problems are “what people think they are.” Conditions on the ground matter, but they do not “assume a prominent place in a social problem until a given people define them as hostile to their welfare.”<sup>48</sup>

The objectivist view – that social problems exist regardless of whether people recognize them – persisted into the postwar decades. Merton and Nisbet, for example, assigned sociologists the task of identifying those *latent* problems that the public failed to acknowledge.<sup>49</sup> But proponents

<sup>45</sup> Case, “What is a Social Problem?” 268, 269, 271 (italics in the original). Case staked out his claim against Hornell Hart’s 1923 definition, which hinges on (objectively) measured impact: “A *social problem is a problem which actually or potentially affects large numbers of people in a common way so that it may best be solved by some measure or measures applied to the problem as a whole rather than by dealing with each individual as an isolated case, or which requires concerted or organized human action.*” Hart, “What is a Social Problem?” 349 (italics in the original). Hart, Case notes, defines a social problem “in terms of objective data.” For Case, “these are not social problems in the full sense, but simply the adverse conditions of life which form one side of social problems, the other being the more or less prevalent social attitude toward those conditions.” Case, “What is a Social Problem?” 272.

<sup>46</sup> Waller, “Social Problems and the Mores”; Fuller, “Sociological Theory and Social Problems”; Fuller, “Problem of Teaching Social Problems”; Fuller, “Social Problems”; and Fuller and Myers, “Some Aspects of a Theory of Social Problems.”

<sup>47</sup> Fuller, “Problem of Teaching Social Problems,” 421, 422.

<sup>48</sup> Fuller and Myers, “Some Aspects of a Theory of Social Problems,” 25. Every social problem, therefore, has “both an objective and a subjective aspect.”

<sup>49</sup> Writes Merton, in the volume’s conclusion: “For the sociologist to confine himself only to the conditions in society which a majority of people regard as undesirable would be to exclude study of all manner of other conditions that are in fact at odds with the declared values and purposes of those who accept or endorse these conditions. Such a limitation would require the sociologist to subscribe to an extreme subjectivism, under the self-deceiving guise of retaining the objectivity of the scientific observer.” Merton, “Social Problems and Sociological Theory,” 708.

of a more subjectivist cast were increasingly vocal, notably within the new SSSP. The emergence of labeling theory was especially important. As outlined in Edwin Lemert's *Social Pathology* (1951) and popularized by Howard Becker's *Outsiders* (1963), labeling theory proposed that normative majorities assign deviant status to the nonconformist minorities in their midst.<sup>50</sup> So deviance – and by extension other kinds of social pathology – has nothing to do with “bad” behavior itself. “The deviant is one to whom that label has been successfully applied,” wrote Becker in 1963. “Deviant behavior is behavior that people so label.”<sup>51</sup> Becker and other self-styled sociologists of deviance, in effect, turned the study of social problems on its head. The foundational moral calculus, that problems are *problematic*, was discarded in favor of an underdog morality – one that, at least implicitly, celebrated the outsider.<sup>52</sup> This shift reflected a more general drift away from the small-town meliorism that C. Wright Mills had mocked. The postwar “social problems” discourse, at the SSSP and within its *Social Problems* journal, was more insistently qualitative, sympathetic to the stigmatized, and sensitive to the social process of definition. The new orientation registered the moral energies of the Civil Rights Movement, the student New Left, and the counterculture, as well as the wider postwar culture of expressive individualism that, in myriad ways, celebrated nonconformity. The congealing, in the late 1960s and 1970s, of “symbolic interactionism” as a self-conscious alternative to mainstream sociology – erected around Herbert Blumer's influential reading of George Herbert Mead – was a crucial development too. Much of the subsequent sociology of deviance was conducted under its banner, and even the early postwar exemplars of the labeling approach were retroactively classified as interactionist.

The result was that much of the 1960s and 1970s “social problems” scholarship – if not the typical textbook – filtered its work through a subjectivist lens. Becker himself, in the introduction to his 1966 *Social*

<sup>50</sup> Lemert, *Social Pathology* and Becker, *Outsiders*. In an insightful 1962 paper, Kai Erikson argued that deviance – far from being a problem – is in fact crucial for the renewal and sustenance of social order. “Thus deviance cannot be dismissed as behavior which disrupts stability in society,” he wrote, “but is itself, in controlled quantities, an important condition for *preserving* stability.” Erikson, “Notes on the Sociology of Deviance,” 310.

<sup>51</sup> Becker, *Outsiders*, 9.

<sup>52</sup> In a remarkably scabrous 1968 takedown of his SSSP colleague Becker, Alvin Gouldner assailed the sociology of deviance for its romantic embrace of the stigmatized, characterized by a “collector's aesthetic.” Among other things, the Becker approach lets the “overdogs” – the truly powerful – off the hook. His is the “sociology of young men with friends in Washington.” Gouldner, “The Sociologist as Partisan,” 106–11. Gouldner's critique from the left is echoed in Liazos, “Poverty of the Sociology of Deviance.”

*Problems: A Modern Approach*, embraced Fuller’s “what people think they are” definition.<sup>53</sup> But Becker also registered a wider turn to the *process* of problem formation – to the messy, power-laden definitional struggle over a problem’s “career.”<sup>54</sup> To a significant degree, that is, sociologists shifted their gaze from the underlying social issues to the people and institutions that go about *naming* problems. This was, for Becker and others, contested terrain, pockmarked by definitional conflict among citizens, professional experts, politicians, and, of course, social scientists. The problematic people, so called, were in the mix too, shaped by but also reacting to their ascribed deviancy – an especially salient point within a racialized public and policy discourse that frequently blamed victims for their own plight and would increasingly do so in the years to come.<sup>55</sup> Becker, in 1966, called on sociologists to “take the point of view of those who are defined as causing the problem instead of those who so define it.”<sup>56</sup> His proposal was to assume the perspective of the “other” – to study, in W. E. B. Du Bois’s haunting phrase, how it feels to be a problem.<sup>57</sup> On this view there are no social problems as such, just ways of seeing people as problems – as nuts, sluts, and perverts, in Liazos’ critical shorthand.<sup>58</sup>

Problems, according to this new, more interactionist approach, have a natural history of sorts – a trajectory that the social scientist might trace.<sup>59</sup> Inchoate concern and organized advocacy, often transmitted through mass media, generate the widespread recognition that problem status requires. What follows, typically, is an institutionalization of the problem: the emergence of organizations and professionals who claim, in effect, custody. These are, to borrow a pair of prominent concepts in the literature, “control institutions” staffed by the “troubled persons professions” – such as social work or policy analysis – which subsist on the putatively troubled.<sup>60</sup> Once

<sup>53</sup> Becker, “Introduction,” 2–5.

<sup>54</sup> “The chapters of this book,” Becker wrote, “do not so much treat ‘social problems’ as they do areas in which various people – citizens, politicians, professional experts, cultural critics, social scientists, and other interested parties – define many kinds of social problems as arising.” Becker, “Introduction,” 7.

<sup>55</sup> On the politics of social scientific problem definition, around poverty, deficiency, and race in particular, see O’Connor, *Poverty Knowledge*, 10–16.

<sup>56</sup> Becker, “Introduction,” 27.

<sup>57</sup> Du Bois, *Souls of Black Folk*, 1–3.

<sup>58</sup> Liazos, “Poverty of the Sociology of Deviance,” 103. Leah Gordon suggested this point in conversation.

<sup>59</sup> The idea that social problems have a career, with identifiable stages, appears in a number of reflective accounts from the period.

<sup>60</sup> “Control institutions” are described in Erikson, “Notes on the Sociology of Deviance,” 310. Joseph Gusfield discusses the “troubled persons professions” in “Constructing the Ownership of Social Problems,” 432–33.

these are established and institutionalized, the contest over “ownership” – the authority to name the problem, suggest solutions, and marshal a response – continues along lines of stratified power.<sup>61</sup>

It was inevitable, perhaps, that the century’s drift toward a more definitional approach to the study of social problems would arrive at a pure and uncompromising subjectivism. In a pair of 1973 papers, sociologists John Kitsuse and Malcolm Spector staked out a strong constructionist position, one that proudly neglects “real” social conditions.<sup>62</sup> They even faulted apparent allies, like Becker and Fuller, for not going far enough – for clinging, needlessly, to the view that objective conditions help establish problem status.<sup>63</sup> The word “putative” is the decisive departure in their definition of social problems: “*the activities of groups making assertions of grievances and claims with respect to some putative conditions.*”<sup>64</sup> The task of the social scientist is to document and explain the claim-making, and nothing more than that. Spector and Kitsuse concede that actors often marshal evidence about social conditions, but those assertions are merely gist for the analytical mill: “Here *their* causal analysis, not the sociologist’s, is the crucial input.”<sup>65</sup> The process of claiming and naming – of calling out an issue as a problem – is what matters in the study of social problems. The intrinsic gravity of a problem is almost irrelevant; social problem status is a symbolic achievement, resulting from a complex and contested social baptism.

In a 1971 paper, Herbert Blumer – the Chicago veteran and godfather of symbolic interactionism – more or less anticipated the position that Kitsuse and Spector would so forcefully propound. Social problems are not born, Blumer claimed; they are made – legitimated, in other words, through a process of collective definition. There is, he wrote, a “selective process in which, so to speak, many budding social problems are choked off, others are

<sup>61</sup> Gusfield develops the idea of problem “ownership” in his important 1981 book, *The Culture of Public Problems*, 10–13, in the context of drunk driving.

<sup>62</sup> Kitsuse and Spector, “Toward a Sociology of Social Problems” and Spector and Kitsuse, “Social Problems.” Spector and Kitsuse codified their approach in a 1987 book, *Constructing Social Problems*.

<sup>63</sup> Kitsuse and Spector, “Toward a Sociology of Social Problems,” 412–13. Kitsuse and Spector were right that Case, Waller, Fuller, and even postwar labeling theorists such as Becker all carved out a role for the social scientist to, in effect, verify the public’s problem recognition by holding it up against actual social conditions. Becker: “Objective conditions are an important part of our conception of a social problem, then, because the definition of a social problem by participants in the society is likely to refer to a situation in society which can, as Fuller and Myers say, ‘be checked as to existence and magnitude (proportions) by impartial and trained observers.’” Becker, “Introduction,” 6.

<sup>64</sup> Kitsuse and Spector, “Toward a Sociology of Social Problems,” 414, 415 (italics in original).

<sup>65</sup> Kitsuse and Spector, “Toward a Sociology of Social Problems,” 414, 415 (italics in original).

ignored, others are avoided, others have to fight their way to a respectable status, and others are rushed along to legitimacy by a strong and influential backing.”<sup>66</sup> Blumer, like others before him, highlighted the *career* of the typical social problem – the series of stages through which a problem passes, from legitimation to (in some cases) government intervention.

So Blumer, in a sense, furnished a digest of the literature of reflection on social problems, or at least its terminus in subjectivism. What distinguished his 1971 contribution, however, was its claim that social scientists rarely have anything to do with that process of collective definition. Blumer chided his fellow social scientists for their policy pretensions and inflated sense of public efficacy. Social-scientific knowledge, to the extent it enters the definitional stream at all, is often “ignored, distorted, or smothered by other considerations.”<sup>67</sup> Powerful interest groups, politicians, media coverage, public opinion polls, outside shocks, and legislative horse-trading, among other factors, are normally far more decisive than social science over the course of a problem’s career.<sup>68</sup>

We take issue with Blumer’s last point, even as we endorse the subjectivist program that his paper encapsulates. The chapters of this volume, like the classic sociology textbook, are devoted to specific problems of society, one after another. It is true, too, that our selection process for the chapter topics honors the view that social problems are what people say they are. But for us, and for this project, the social scientist is decisive. The chapters’ attention, in other words, lingers on those problems that economists, psychologists, and the rest – even sociologists – took to be most trenchant over the postwar decades. Major research projects, commissions and reports, works of public resonance, expert testimony – moments in which social scientists and their work joined the definitional fray – are at the heart of the volume’s topical chapters. These interventions often withered on the vine of official indifference, as Blumer observed – or else registered their impact at the more subterranean level of language. But social scientists, in each of the postwar cases, were among the significant definers. They helped to designate the problems and – in complex interplay with other social and political currents – to give shape to the policy responses.

The point, contra Blumer, is that social scientists were in the mix, as postwar America grappled with its increasingly visible challenges. The

<sup>65</sup> Blumer, “Social Problems as Collective Behavior,” 303.

<sup>66</sup> Blumer, “Social Problems as Collective Behavior,” 304.

<sup>67</sup> Friedrich Hayek, in a well-known 1949 essay, already pointed out the growing influence of these “professional secondhand dealers in ideas.” Hayek, “Intellectuals and Socialism,” 417.



chapters, with that baseline, attend to the *relative* contributions of the social science disciplines – their shifting prominence and claims to topical “ownership” vis-à-vis their counterparts. Here the case of sociology is instructive: For all its preoccupation with the “social problems” domain, the discipline has watched its rivals – notably psychology and economics – lay increasingly credible claims to the problems themselves.

### 1.3 Disciplinary Division of Labor

By the time Richard Swedberg considered the history of interaction between economics and sociology, in 1991, economics imperialism, with Gary Becker as its flag bearer, had made a name for itself. The uncoordinated attempts that had characterized its beginnings in the late 1950s had now coalesced, creating new momentum for the discipline’s inroads into the realm of other social sciences. Becker’s “economic approach to human behavior” was now complemented by a similar effort in sociology with the publication of James Coleman’s monumental treatise in rational choice sociology – *Foundations of Social Theory* – in 1990. Disciplinary boundaries had inexorably shifted and jurisdictional changes were not temporary or accidental.<sup>69</sup>

As Swedberg described the nature of these changes, he offered a four-fold periodization of the relationship between economics and social analysis: the time of political economy (late eighteenth to late nineteenth-century); the *Methodenstreit* (1880s–1910s); mutual ignorance and distortion in the social sciences (1920s–1960s); and economic imperialism and the challenge of redrawing the boundaries in the social sciences (1970s–).<sup>70</sup> As a broad characterization of changing boundaries, Swedberg’s periodization fares well, but its third and fourth periods suffer from imprecisions. It is surprising indeed that the opening years of the “mutual ignorance” period coincide with the creation of the Social Science Research Council (SSRC) and that its end dovetails with what can be considered as the closing of the most prolific period for interdisciplinary work in the US social sciences. Likewise, placing the start of economics imperialism in the 1970s illustrates the frequent emphasis on the movement’s heyday at the expense of its first significant inroads in the late 1950s and early 1960s. To some extent, the shortcomings of Swedberg’s last two periods reflect his own biases as an economic sociologist: the minimization of the role of political science in the redefinition of the division of labor in the US social sciences in the early

<sup>68</sup> Swedberg, *Economics and Sociology* and Coleman, *Foundations of Social Theory*.

<sup>69</sup> Swedberg, *Economics and Sociology*, 14–15.

1920s, and the temptation to cast sociology as the main rival to economics in the 1970s.

The changing division of labor among the US social sciences since 1918 cannot be understood without bearing in mind that their practitioners viewed disciplinary boundaries through the prism of solving important problems of society. Clearly by the first decade of the twentieth century, the departmental structure of the American university as we know it was in place – with the caveat that many departments remained bi-disciplinary.<sup>71</sup> It was, even into the 1930s, common for sociologists to coexist with either economists or political scientists in the same department. Likewise, most social sciences had their own professional societies, which strengthened a nascent sense of identity among disciplinary brethren, but did not stand in the way of organizing common annual meetings, which remained the norm until the 1940s.<sup>72</sup>

By the early 1920s, increasing specialization was already raising fears. The tendency to disciplinary isolation accentuated those fears, with the benefits of specialization often contrasted with the costs of excessive departmentalization and compartmentalization. Specialization itself was not regarded as evil, but its acceleration in a time of rapid social change could, according to this view, prove counterproductive. The concern was that social scientists, confined to their disciplines, were badly positioned to grasp and address problems of a complex, interrelated, and fast-changing society. Specialization encouraged intellectual particularisms that made synthesis – the integration of research findings from different social sciences – more problematic. Increased specialization accompanied the professionalization of social science disciplines, but it would be an exaggeration to suggest that their practitioners saw problems of society as falling under the exclusive jurisdiction of any particular discipline.

To varying degrees, each of the core social sciences – economics, sociology, and political science – addressed problems such as crime, addiction, the family, prejudice, poverty, and education. And in the 1920s, at least, they stood on similar footing, as measured by prestige, legitimacy, and co-equal membership in the SSRC. By providing flexible institutional structures supporting multidisciplinary research work, the SSRC, created in 1923, helped address concerns raised by increased specialization. Its primary purpose was to advance the research methods of political science and related social sciences by encouraging greater cooperation between existing disciplinary associations, including the American Statistical Association. By the mid-1920s,

<sup>70</sup> Abbott, *Chaos of Disciplines*, 122–31.

<sup>71</sup> Young, “Emergence of Sociology from Political Economy.”

the American Psychological Association, the American Anthropological Association, and the American Historical Association had all joined the SSRC. As political scientist and SSRC co-founder Charles Merriam pointed out, the organization was established to support “cases where problems overlap the boundaries of one or more of the special fields concerned.”<sup>73</sup>

By the late 1920s, the sociologist William Ogburn and the anthropologist Alexander Goldenweiser drew a portrait of the US social sciences in which their interrelations, more than their general nature, stood out: “The problems of living society,” they wrote, “do not range themselves so as to fit the artificial isolation forced upon the social sciences by differences of specific subject and method. These problems are what they are. If they are to be solved, whatever knowledge we possess about society must be called into service, wherever needed.”<sup>74</sup>

Ogburn and Goldenweiser offered a handful of examples that, with the exception of taxation – described as the concern of economics and political science – would have slotted into most social problems textbooks of the time. Thus, poverty was described as falling in the domains of psychology, economics, sociology, political science, ethics, and education. With a few variations, the same argument was repeated for immigration, race problems, and crime. Ogburn and Goldenweiser’s book stood as a good illustration of the position of leading social scientists on disciplinary specialization in the wake of the SSRC’s creation. Increased specialization allowed for greater professionalization and more “scientific” approaches, but social problems themselves did not fall under the exclusive preserve of any single discipline.

In the eyes of Ogburn and Goldenweiser and many others, synthesizing the research findings of various social sciences appeared much more important than sanctifying their modes of expertise. Synthesis was supposed to open new horizons for future developments in the US social sciences, but with the unprecedented economic crisis of the 1930s, the sense of urgency thrust certain disciplines to the forefront. In the process, it became even more obvious that the pressing problems of American society not only did not “fit the artificial isolation forced upon the social sciences by differences of specific subject and method,” but actually played a role in redrawing the boundaries attached to these differences.<sup>75</sup> Until the late 1920s, social scientists had tended to emphasize interdisciplinary

<sup>72</sup> See Worcester, *Social Science Research Council*, 14 and Merriam, “Annual Report of the Social Science,” 185.

<sup>73</sup> Ogburn and Goldenweiser, *Social Sciences and Their Interrelations*, 7–8.

<sup>74</sup> Ogburn and Goldenweiser, *Social Sciences and Their Interrelations*, 7.

cooperation as a prerequisite for solving social problems. With the crisis of the 1930s, however, uneven participation in policymaking marked the disciplines' diverging fortunes.

With the Great Depression and the New Deal, the pace of social change continued to occupy the minds of social scientists, if only because there was so much uncertainty as to the kind of social order that would emerge from those troubled years. Yet, the threat that large-scale economic disorganization posed to capitalism itself quickly took precedence. As Charles Camic has shown, the 1930s were not a happy time for sociologists, whose nascent public stature suffered at the expense of other social scientists, economists in particular. Among the significant changes in the substance and organization of American culture, Camic listed "a fundamental reordering of the prestige hierarchy of academic disciplines as a result of trends that predated the Depression."<sup>76</sup>

Even before economic hardship struck the whole US society, indeed, there was a gradual shift in emphasis toward economic problems following the depression of 1920–1921. Though social problems still concerned the community of social scientists as a whole, the centrality of economic problems in American society, together with Secretary of Commerce Herbert Hoover's marked trust in economic expertise, granted economists a comparative advantage over other social scientists. As early as August 1927, plans were discussed for a study of recent economic changes, which would improve the understanding of the American economy as a whole with the aim to draw down unemployment. Following the recruitment of collaborators, work and discussions were undertaken under the directorship of economic historian Edwin Gay and empirical economist Wesley Mitchell from the National Bureau of Economic Research (NBER). Both Gay and Mitchell were known for promoting the collection of data as a major resource in the search for the solutions to society's problems.<sup>77</sup>

That two institutional economists found themselves at the center of a national survey commissioned by the federal government need not be taken as evidence that policymakers favored one stream of thought over another, or that economics alone carried professional expertise. The growing faith in professional expertise was tethered to statistical methods more than disciplinary background, so differences in recognition depended on social scientists' familiarity with these methods as much as their activism in the

<sup>75</sup> Camic, "On Edge," 226.

<sup>76</sup> It is worth remembering that Gay and Mitchell played an important role in the creation of the NBER in 1920 and the SSRC in 1923.

service of society, with Mitchell illustrating the former trait and Gay the latter. When it came to advising policymakers, however, differences in theoretical orientations within economics could prove influential, because different conceptions of disciplinary boundaries implied differential attention to noneconomic factors in devising solutions to economic problems. Institutional economists such as Mitchell and Gay showed greater sensitivity to the relationships between economic and social organization than their neoclassical counterparts, a tendency that helped give quantitative economic research greater public relevance than it would have had otherwise.

The Committee on Recent Economic Changes' final report was completed in February 1929. As Gay noted in the introduction, the report emphasizes the causes of American prosperity and suggests that its current features resembled those of "former major periods of prosperity." As he referred to maladjustments of economic growth, Gay likewise found similarities with the past, but also noted that "the rapidity and vigor of growth of some elements is so great as seriously to unbalance the whole organism." In an argument reminiscent of the cultural lag hypothesis, Gay observed a shift in the "psychological attitude" toward change in material conditions, with "quick adaptation and rapid mutation" increasingly regarded with "more social concern." The economic historian hinted that "something distinctly different from our former experience is taking place" and that "there seem now to be differences of degree which approach differences in kind."<sup>78</sup> Gay's remarks should not be taken as prophesizing the coming economic crisis; instead, they reflected acute perception that change in the adaptive culture failed to synchronize with change in the material culture, making the perspective of a new type of social organization altogether likely. It is ironic that such concern, which emerged in a time of accelerated changes accompanying economic prosperity, became even more paramount when policymakers began to ponder the best way to adjust social organization to the economic dislocations of the late 1920s and early 1930s.

From that perspective, the decision of now-President Hoover to create the multidisciplinary Research Committee on Social Trends in the autumn of 1929 is less perplexing. The Committee, the culmination of the growing stature of the social sciences throughout the 1920s, seemed especially suited to dealing with the undesirable social consequences of economic prosperity. Yet by the time its report was published in 1933, after four years of economic hardship, its motivation was hardly in phase with current preoccupations.

<sup>77</sup> Gay, "Introduction," 10–12.

In keeping with the spirit of the 1920s, the report viewed the “outstanding problem” of American society “as that of bringing about a realization of the interdependence of the factors of our complicated social structure, and of interrelating the advancing sections of our forward movement so that agriculture, labor, industry, government, education, religion and science may develop a higher degree of coordination in the next phase of national growth.” In a time of severe recession, that concern seemed incongruous to say the least. More importantly, in keeping with the emphasis on the social consequences of economic prosperity, the report presented itself as a study of social change, pointing to the interrelations between various social trends at a time when economic changes were on everyone’s mind. Of course, the contributors to the volume saw economic changes, like many other changes, as parts of a broader pattern of overall social change, but the “central view of the American problem as revealed by social trends” cut against the inclination to concentrate on economic concerns in the midst of a depression.<sup>79</sup>

Whatever interest sociologists and other social scientists might have had in economic matters, it is understandable that economics, which was still methodologically pluralistic at the time and certainly more open to the other social sciences than it became after World War II, acquired the status of a reference discipline in the eyes of policymakers. Though the merits of economists and the impact of their ideas on economic policymaking were already recognized in the pre–New Deal era, the kind of stature the discipline achieved during the New Deal was of a different nature. Its practitioners’ strong commitment to objectivity and quantification combined with a sense of economic urgency to create unprecedented expectations.<sup>80</sup>

In economics, the great figure of the 1930s was John Maynard Keynes, but the message of *The General Theory of Employment, Interest, and Money* reached the United States much later. If one wants to form a clear idea of the influence of economists on the public stage in the United States, a good place to start is with US economics itself. By the mid-1930s, the discipline encompassed a variety of perspectives – a pluralism inadequately captured

<sup>78</sup> President’s Research Committee on Social Trends, *Recent Social Trends*, xii, xiii. Comparing its work to Hoover’s Committee on Recent Economic Changes, Mark C. Smith presents the Committee on Social Trends project as a “similar but more ambitious study of American society.” Smith, *Social Science in the Crucible*, 71.

<sup>79</sup> Barber, *From New Era to New Deal*. Likewise, Michael A. Bernstein’s *A Perilous Progress* offers a detailed story of the years prior to 1939, pointing to the gradual strengthening of the economics profession, its sensitivity to objectivity, and its participation in the realization of a public purpose.

by the sharp distinction between neoclassicism and institutionalism.<sup>81</sup> Economics allowed for a variety of theoretical approaches at the same time that its policy recommendations, being less systematized, suffered from inadequate disciplinary demarcation. As a result, the distinction between abstract and empirical work lacked the clarity it later achieved. The absence of a common theoretical framework encompassing pure and applied economics meant that the shared identity of economists in the 1930s resulted more from efforts at “shaping an authoritative community,” to use Michael A. Bernstein’s phrase, than from attempts at defining an orthodoxy around a certain conception of scholarly research. The identity of economists built upon a professionalizing, not a disciplinizing, vision.

While economists, together with political scientists and legal scholars, filled top New Deal policy posts, sociologists continued to publish “social problem” treatises to a curiously indifferent public. Though plainly relevant to the decade’s struggles, sociology’s litany of social problems fell outside the main New Deal frame.<sup>82</sup> Sociologists were not long in registering the weakening of their position in academia and among policymakers. Throughout the 1930s, they missed no opportunity to voice their concern about the discipline’s lowly place in society. They variously lamented the “almost complete disregard of the depression in the programs of important sociological meetings,” argued for increasing attention to the sociology of economic relations, attributed the lesser influence of sociologists in the current national crisis to an “inferiority complex,” and pointed to the relatively greater role of economists in contemporary life and the weak presence of sociologists in the bureaus, divisions, offices, and sections of the Roosevelt administration.<sup>83</sup>

These concerns, voiced in a 1934 special issue of *Social Forces*, illustrated sociologists’ perceived need to deal with wider social and economic problems, which, as sociologist Luther L. Bernard put it, “are not treated specifically under the heading of conventional economics.” Bernard, the ASS’s president in 1932,

<sup>80</sup> Roger Backhouse notes: “In the 1920s and 1930s, US economics was pluralistic.... Classical economists (Frank Taussig at Harvard) and institutionalists (John Commons at Wisconsin and Wesley Mitchell and John Maurice Clark at Columbia) flourished alongside neoclassical economists (Irving Fisher at Yale) and Marshallians (Edward Chamberlin at Harvard). Some individuals defied classification (Frank Knight at Iowa and then at Chicago). By 1960, all this had changed, and neoclassical economics, or at least the neoclassical synthesis of Paul Samuelson’s textbook, was unquestionably dominant.” Backhouse, “Transformation of US Economics,” 85.

<sup>81</sup> Camic, “On Edge.”

<sup>82</sup> All these points are made in a 1934 *Social Forces* symposium devoted to examining questions concerning the role of sociology in the New Deal.

added: "In fact such problems as those of poverty and relief, social welfare, the standard of living, profits and social service, the function of capitalism in civilization, collective ownership and control, wages and welfare, child labor and child welfare, conditions of work, and social reform can be treated adequately only from a sociological standpoint." The reference to conventional economics implied that sociology could join with alternative streams of thought, institutional economics in particular, which were closer to the other social sciences and took these problems seriously. At the same time, Bernard's chauvinist claim that a "sociological standpoint" was required suggested that sociologists should hold the upper hand in this potential collaboration.<sup>84</sup>

In the late 1930s, the growing significance of ideologies of economic planning notwithstanding, social scientists outside economics had not relinquished the hope for a brighter future.<sup>85</sup> Moreover, the role of institutional economists in the New Deal left the door open for consideration of solutions that took the contributions of other social sciences into account. As we have seen, sociologists showed growing awareness of the need to make a special effort to demonstrate the public significance of their field. In 1937, a number of "monographs appeared in print, under SSRC's imprint, with the titles standardized in the form of *Research Memorandum on [X Topic] in the Depression*. The thirteen topics covered were family, religion, education, rural life, internal migration, minority peoples, crime, health, recreation, reading, consumption, social work, and relief policies." With a few exceptions, the topics addressed resembled those appearing in the social problems literature of the time, even though the contributing authors were not all associated with that tradition.<sup>86</sup> This initiative did not produce durable results and eventually failed in its attempt to strengthen a sociological vision of the Depression on the public stage. The divisions in sociology did not help, but economics was not especially unified either. Its greater public recognition by the end of the 1930s derived from a special combination of disinterested scholarship, unmistakable resonance with current problems, and a sense of identity based on professional expertise.

<sup>83</sup> Bernard, Contribution to "Questions for Sociology," 167–68. Camic places the arguments of sociologists within two broader, distinct strategies: one that made economic and political changes associated with the Depression and the New Deal expressions of long-term cultural trends, and the other that emphasized mores, customs, values, ideals, and attitudes and the particular institutions that supported them. Camic, "On Edge," 277–80.

<sup>84</sup> Balisciano, "Hope for America."

<sup>85</sup> Camic, "On Edge," 268.



By the time the war started, the division of labor in the core social sciences was mostly unchanged. In political science, from the early 1920s, various efforts to bring a scientific viewpoint to the study of politics and to define the discipline as a policy science won its practitioners more recognition on the public stage. Different disciplines were nominated as suitable complements to political science in its analysis of political questions: psychology and statistics for Charles Merriam, physics for Bennett Munro, and classical economics for George E. G. Catlin.<sup>87</sup> As the role of political scientists in the New Deal amply demonstrated, however, the discipline enjoyed enough professional credit for its own expertise over political systems to be recognized.<sup>88</sup>

In sociology, too, avowed appeals to science and quantitative methods gained traction from the 1920s and, as in political science, that aspiration coexisted with a professed belief in the necessary involvement with public issues. Up to the late 1920s and early 1930s, sociology to a large extent centered on the study of social problems, which more or less coincided with the problems of US society. The New Deal marked a turning point, as we have seen, with economic problems acquiring increased political salience. Sociologists suffered from their relative neglect of economic issues, but eventually reacted by arguing that their discipline offered a broader treatment of the problems affecting US society. Reorganizing the insights of economics and political science within a broader sociological framework could help restore sociologists' public stature.

While it is undeniable that the 1930s marked an important stage in the quantification of economics, the consolidation of its professional authority, and the affirmation of its relevance for the treatment of public problems, it would be an exaggeration to suggest that its estrangement from other social sciences had already begun. Part of its success derived from its concentration on economic problems at a time when those called for special attention. Yet, in the late 1930s and early 1940s, economics was still methodologically pluralistic and its internal diversity comparable to those of political science and sociology. Within the division of labor in the social sciences, the place of economics had not changed much from the 1920s, even though its position in their hierarchy had certainly improved.

<sup>86</sup> Somit and Tanenhaus, *Development of Political Science*, 110–17.

<sup>87</sup> Writing on the nature of the social sciences in the mid-1930s, Charles A. Beard noted that political science “is that division of social study which is concerned with government.... It is usually a plane-surface description of those aspects of human nature and human activity which pertain to government.” Beard, *Nature of the Social Sciences*, 73.

The war itself created an even more challenging combination of circumstances for the social sciences, with new problems coming to the fore. Professional societies reacted to this sea change with variable success, which eventually impacted the relative prestige of social science disciplines on the public stage. The impact of the war on social science is well known. The government's need for rapid solutions together with its lack of patience for departmental boundaries took prewar efforts to mobilize social science to a new scale and encouraged a move in the direction of problem-oriented, cross-disciplinary team work. That does not mean that the government had a definitive idea of its actual needs, or that social scientists knew precisely the nature and scope of their possible contribution. But overall, the two parties found their bearings and established a form of collaboration that survived the war for two decades before a new turn toward specialization, helped by a new patronage system, began around the mid-1960s.<sup>89</sup>

Rather than changing the division of labor entirely, the war confronted its practitioners with new questions and new forms of research organization that made that division less constraining. In the process, wartime service favored greater awareness of applied work and more opportunity for disciplinary interaction. The prewar social sciences did not lack applied and interdisciplinary ambitions, but war problems, of a different nature from those of adjusting to modernity, provided even more incentive for social scientists to cross disciplinary boundaries. Leaving their departments to join governmental agencies, social scientists were encouraged to curb their natural inclination to consider problems – economic, political, and sociological – primarily associated with their own discipline. The enthusiasm for interdisciplinary work came together with a conception of what it meant to be scientific based on methods of investigation rather than objects of study.

Many social scientists, notably economists, sociologists, and political scientists, were recruited to the war effort, working in various capacities and locations.<sup>90</sup> Economists worked as general, technical problem-solvers, collaborating with natural scientists and engineers as well as with other social scientists. They were especially active in the Office of Strategic Services (OSS), the Enemy Objectives Unit, and the Statistical Research Group, where they dealt with problems related to military strategy and tactics as well as more traditional economic topics.<sup>91</sup> It was during the war

<sup>88</sup> Crowther-Heyck, "Patrons of the Revolution."

<sup>89</sup> Backhouse and Fontaine, *History of the Social Sciences*, 186–88.

<sup>90</sup> Guglielmo, "Contribution of Economists to Military Intelligence."

that economists began to develop a sense of separation from the other social sciences – a self-segregation of the privileged that sharpened in the postwar decades.<sup>92</sup>

Sociologists' experience in the war "comprised two types of service – in war-related research and in the military." Like economists, sociologists worked in a number of agencies, including the OSS Research & Analysis Branch, the Army's Research Branch, the Department of Agriculture (where they represented a significant contingent), and the Labor Department.<sup>93</sup> To some extent, war service helped sociology regain academic standing vis-à-vis its neighbors, in part through ambitious cross-disciplinary projects with social psychologists, anthropologists, and political scientists working in the federal government's sprawling propaganda bureaucracy.

Political scientists, with their ties to law and public administration, likewise participated in military activities and civilian work in connection with national defense.<sup>94</sup> As of 1941, they were especially active in two federal agencies: the Office of Price Administration, where, together with economists, they helped keep prices under control and organize the rationing of consumer goods; and the Office of the Coordinator of Information, an intelligence organization later divided into the OSS and the Office of War Information, where they interacted with sociologists in particular.

By 1945, political scientist Pendleton Herring, who held several positions in Washington during the war, was already drawing conclusions from the mobilization of his peers: "As professors of a distinctive discipline, we have taught our courses and expected of our colleagues in other departments that respect for jurisdictional boundaries which serves as the greatest safeguard to our scholarly mysteries and the readiest protection of academic amenities. Changes are already upon us that promise to alter greatly these familiar and pleasant arrangements." Like many of his colleagues who had worked in wartime service, Herring was aware of the growing interest of other social scientists in understanding governmental activities and, as a result, concluded that "the study of governmental problems cannot be the concern of one discipline to the same extent as in the past." At the same time, he noted that problems formerly associated with the subject matter of

<sup>91</sup> Pooley and Solovey, "Marginal to the Revolution." The self-segregation, though real and traceable, was by no means absolute. A number of economists made serious efforts to work with other social scientists throughout the early postwar decades.

<sup>92</sup> Abbott and Sparrow, "Hot War, Cold War," 26.

<sup>93</sup> Ogg, "News and Notes."

other social sciences had piqued the interest of political scientists. Overall, Herring viewed the process of cooperation among the social sciences as a way of inventing new tools of analysis and of creating new sources for data.<sup>95</sup>

Herring's conclusions for political science could be easily extended to the other social sciences, whose practitioners had similarly experienced the increased permeability of disciplinary boundaries during the war and its beneficial effects on knowledge creation and policy relevance. Social scientists had objective reasons to think of themselves as active participants in the war effort, but their recognition in the early postwar years did not match their actual contribution and hardly equaled that of physical scientists. With the exception of economists, whose recognition in high circles was made obvious with the creation of the Council of Economic Advisers in 1946, social scientists took a painful measure of the gap between their sense of accomplishment and their invisibility to policymakers. As the war came to an end in Europe, George A. Lundberg, a former president of the ASS, betrayed these mixed feelings when he complained "that social research was [seen as] a kind of luxury to which surplus funds might be devoted as a sort of advertising stunt reflecting the benevolence of donors, or in any event as a side issue not vitally concerned with the serious business of managing society. If social research is really to flourish," the quantitative sociologist concluded, "this view must change. Sooner or later it will change."<sup>96</sup> How long it took, exactly, is difficult to say, but it is clear that the decade following World War II was not an easy ride for the US social sciences.

Economics' gradual shift toward hypothetico-deductive modeling marked its entry into a new era, with greater methodological agreement and lessened contacts with other social sciences as the new trends. For social sciences other than economics, the late 1940s reinforced the view that they were lacking in comparison with the natural sciences. According to Michigan psychologist and academic entrepreneur Donald Marquis, the social sciences (economics included) followed imperfectly the sequence of six steps supposed to characterize the scientific process.<sup>97</sup> The idea that the social sciences lagged behind the natural sciences became the rhetoric of social scientists willing to push the agenda of the so-called "behavioral

<sup>94</sup> Herring, "Political Science in the Next Decade," 758, 759. A decade later, political scientist David Easton continued to deplore overspecialization and the disintegration of social knowledge "into a multitude of intellectual feudalities," but remarked: "Today this condition has stimulated a movement towards a re-integration of our compartmentalized knowledge." Easton, *Political System*, 101.

<sup>95</sup> Lundberg, "Social Sciences in the Postwar Era," 138.

<sup>96</sup> Marquis, "Scientific Methodology in Human Relations."

sciences.” Self-identified behavioral scientists endorsed a cross-disciplinary, problem-oriented approach – compatible, in their view, with theoretical and methodological sophistication akin to the natural sciences. Disciplinary divides were routinely, and as a matter of principle, breached, but the shared domains of study were not, for the most part, oriented to the litany of social problems that had preoccupied their interwar predecessors.

From the late 1940s onward, the political context, notably the “antifoundation sentiment in American politics,” was not especially favorable to the social sciences, as “they became deeply entangled with the domestic and international dimensions of the Cold War.”<sup>98</sup> As earlier criticisms about their confusion between social advocacy and scientific objectivity resurfaced and took on a new political sharpness, the wartime accomplishments of the social sciences were gradually sidelined. From the late 1940s to the mid-1950s, McCarthyism impacted the social sciences and natural sciences alike, but the hierarchy of the sciences, reinforced by the growing prestige of physics after the war, protected the latter more than the former. The various attacks against the social sciences tarnished their reputation, but they did not stall – and may have even spurred – the effort to establish legitimacy through natural-science mimicry. As a result, as suggested by Roger Geiger, the social sciences were “far more vigorous by 1956, and much less in need of reformation, than had been the case at the end of the 1940s.”<sup>99</sup>

The porousness of disciplinary boundaries in war work and the effort to turn the experience of interdisciplinary interaction into a model for future research did not, in themselves, shift any jurisdictional claims nor reorder the prewar hierarchy of prestige. The war helped create new methods, new subfields, and new collaborations, to be sure, but, overall, these changes were compatible with the existing disciplinary structure. As it turned out, cross-disciplinary research ventures represented an alternative form of production and dissemination of social-scientific knowledge more than an effort to loosen the disciplinary yoke.<sup>100</sup>

From that perspective, it is not surprising that the first serious challenges to the prevailing disciplinary division of labor came from social scientists who, with unblushing ambition, plied their tools in the traditional domains of their disciplinary rivals. The opposite tack – to assert that a discipline

<sup>97</sup> Solovey, “Cold War Social Science,” 4. See also Solovey, *Shaky Foundations*, 119–27.

<sup>98</sup> Geiger, *Research and Relevant Knowledge*, 104. On the postwar campaign for scientific legitimacy in social science, see Haney, *Americanization of Social Science*, chap. 2.

<sup>99</sup> Fontaine, “Introduction.”

might learn from its peers – was, perhaps predictably, less significant.<sup>101</sup> Likewise, it is understandable that these ambitions developed within a discipline – economics – that stood aloof from a movement – the behavioral sciences – that presented comparable intentions.<sup>102</sup>

The first significant incursions of economists into the domains of the other social sciences came with Gary Becker's work on discrimination and Anthony Downs' on government action.<sup>103</sup> Both projects concerned topics that had long been on the agenda of the other two core social sciences, sociology and political science, respectively.<sup>104</sup> Other works in this vein followed in the 1960s and 1970s, encroaching on the jurisdiction of other disciplines and culminating with the publication of Becker's *Economic Approach to Human Behavior* in 1976.

Some sociologists, in fact, received Becker's work on discrimination with interest, on the grounds that it furnished theoretical structure to a domain of study that could appear as inadequately systematized. Some economists, by contrast, were lukewarm – suspicious that microeconomic tools were appropriate for the study of discrimination, when their application within microeconomics itself posed problems.<sup>105</sup> In studying discrimination, Becker was entering the “social problems” scene at a time when this tradition was subject to strong competition within sociology itself. As they discussed “Change and Controversy in Recent American Sociology” in 1961, sociologists Seymour Martin Lipset and Neil Smelser located a tension between the scientific emphasis of present-day sociology and its earlier policy orientation within the social problems tradition, which they believed had “brought it [sociology] rapidly to the fore as a major subject of research and study in the United States.” In sociology, they argued, the shift away from a policy-oriented approach toward a more scientific position was

<sup>100</sup> Economists nonetheless kept an eye on other social sciences, if only because they offered insights to enrich their own discipline's behavioral assumptions. See Backhouse and Fontaine, “Economics and Other Social Sciences.”

<sup>101</sup> Pooley and Solovey, “Marginal to the Revolution,” 199.

<sup>102</sup> Sociologists Richard Swedberg and Mark Granovetter noted “that scholars like Becker and Downs were extremely important in being the first to challenge the peculiar division of labor between economics and the other social sciences that had developed in the twentieth century.” Swedberg and Granovetter, “Introduction,” 2.

<sup>103</sup> Swedberg, “The Battle of the Methods,” 25, also mentioned the work of Alfred H. Conrad and John R. Meyer on the history of slavery in the late 1950s, but the use of the “economic approach” in economic history cannot be put on the same footing as its uses in sociology and political science, since economic history is an organized subfield of economics. See Cole, “Economic History in the United States.”

<sup>104</sup> Fleury, “Wandering through the Borderlands of the Social Sciences,” 19–29.

associated with the use of more sophisticated methods of analysis, and a view of problems of society “as part of an interdependent whole.”<sup>106</sup> As we have seen with Merton and Nisbet, the study of social problems could be adapted to a functionalist approach. Though Becker’s effort did not go unnoticed, its microeconomic focus and mathematical apparatus were poorly fit to the picture of functional interdependence held by many sociologists.

Leading political scientists such as Charles Lindblom and Edward C. Banfield received Downs’ *An Economic Theory of Democracy* rather favorably, pointing to its elegant effort to systematize ideas through hypothetico-deductive modeling, but noting as well that – even though they had not yet been presented in the form of an explicit theory – some of its conclusions were already known to political scientists.<sup>107</sup> Economists did not pay special attention, but those who did mentioned the book’s emphasis on the need to integrate political and economic theory.<sup>108</sup>

Though Downs’ book did not address a social problem per se, it did consider the role of modern democratic governments in economic matters and, as such, related to a number of earlier debates which opposed a problem-oriented perspective to a more “scientific” approach. The book came out at the end of a period of debate for behavioralism, an approach that linked scientific analysis in political science with the use of quantitative methods – survey methods and statistical analysis in particular. As noted by James Farr, some of the most significant studies associated with behavioralism “placed voting at the center of a model of representative democracy that downplayed the individual citizen and highlighted the competition for votes among elites within an overall system of liberal institutions.”<sup>109</sup> Robert Dahl, one of the movement’s originators, recalled that the behavioral approach was intended to furnish the discipline with “empirical propositions and theories of a systematic sort, tested by closer, more direct and more rigorously controlled observations of political events.”<sup>110</sup>

<sup>105</sup> Lipset and Smelser, “Change and Controversy,” 41, 43.

<sup>106</sup> Lindblom, “In Praise of Political Science” and Banfield, “Review of *An Economic Theory of Democracy*.”

<sup>107</sup> Bergson, “Review of *An Economic Theory of Democracy*.” See also Diamond, “Review of *An Economic Theory of Democracy*,” 208, which speaks of “the assimilation of politics to economics.”

<sup>108</sup> Farr, “Political Science,” 323.

<sup>109</sup> Dahl, “Behavioral Approach in Political Science,” 766. Dahl did just that in his 1961 *Who Governs?* The book was not just another contribution to community research. For all its grounding in the political system of New Haven, Connecticut, the volume’s reflection on the relationships between the theory of democracy and empirical results, with its special attention to the fragmentation of influence and the intricacies of local politics, could not but shed light on America’s challenges in exporting democracy.

Like sociology, in the early 1960s political science was marked by “a mood of sympathy toward ‘scientific’ modes of investigation and analysis.” Published at a time when a number of political scientists were trying to promote a “scientific outlook” through the analysis of individuals, Downs’ reliance on rational man provided a nice illustration of the explanatory power attached to simple behavioral assumptions for the study of political behavior. Yet, as Dahl noted, behavioral political scientists had a greater ambition, namely, “understanding the psychological characteristics of *homo politicus*: attitudes, belief, predispositions, personality factors.” And here he mentioned examples from sociology, psychology, and political science – not economics.<sup>111</sup>

Throughout the 1960s, the continuing incursions of economists into the domains of sociology and political science marked a shift in the disciplinary division of labor in the three core social sciences. Before Becker’s and Downs’ work, sociologists and political scientists’ belief in the benefits of interacting with economics rested on the experience of war service and multidisciplinary team work. Within certain limits, that attitude continued to reflect the spirit of the SSRC enterprise: Political scientists and sociologists alike held that learning from other social sciences was a necessary step in the vexing effort to solve society’s problems.

With economists’ first attempts to investigate “noneconomic” topics, differentiating the various social-scientific approaches – the economic, the political, and the sociological – to problems of society took precedence over their integration. To some extent, Becker’s and Downs’ effort in the late 1950s, and those of others in the early 1960s, marked the beginning of a declining concern for a general understanding of society. That decline coincided with the waning of a cross-disciplinary age in social science, with the individual disciplines set in new, competitive relief. It is no exaggeration to suggest that economists, who had been used to cultivating their difference with other social sciences, benefited from that reorientation, which later encouraged the affirmation and propagation of economic reasoning within society at large.<sup>112</sup> In policy circles, too, economists revealed ambitions that would have appeared excessive only a few years earlier. By 1963, as suggested by Bernstein, “federal economists were now very much part of an effort to stimulate social and political change in modern American society.”<sup>113</sup>

<sup>110</sup> Dahl, “Behavioral Approach in Political Science,” 766, 769.

<sup>111</sup> Craig Calhoun regards economics as the main competitor to sociology in the project of a general understanding of social life. It should be made clear, however, that Calhoun talks about claims not achievements, for neither sociology nor economics offers an integrative approach. Calhoun, “Sociology, Other Disciplines,” 180,

<sup>112</sup> Bernstein, *Perilous Progress*, 139.



As Hamilton Cravens rightly put it, “the social sciences were more the creature of politics than the other way around.”<sup>113</sup> This explains why the launch of the Great Society programs exacerbated tensions within social science. The wave of programs and initiatives testified to the recognition that persisting social and political ills deserved special attention at a time when, by their own admission, sociologists largely neglected social disruption and political scientists paid inadequate attention to political crises. For their part and despite notable achievements, economists were reminded that policy shifts could diminish their comparative advantage over the other social sciences – making it crucial to reaffirm their jurisdictional claim over a variety of social problems.

By the late 1960s, the core social sciences found themselves in contrasting situations. Just as political scientist David Easton deemed behavioralism in need of revamping, and hoped to see more intellectual resources devoted to studying the problems of the day, so Alvin Gouldner pointed to the crisis of Western sociology and hoped to find a better reflection of social conflict in social theory. At about the same time, economist Mancur Olson, then Deputy Assistant Secretary for Social Indicators, felt the need to remind the readers of *The Public Interest* that “if poverty is not an economic problem, then nothing is.” And in case this was not clear enough, he insisted: “it is futile to attempt to determine the division of labor between social science disciplines in terms of the objects they are supposed to consider. Reality cannot be divided into departments the way universities are, and no logically defensible division of subject matters is possible. The various disciplines are, however, distinguished by their prejudices and their methods.”<sup>115</sup>

Social scientists outside economics had good reason for their frustration. The effort to make their disciplines more publicly appealing in the past decade had produced mixed results, as if the shift away from social advocacy had eventually diverted them from problems themselves. While the scientific accomplishments of economics translated into increased prestige, with the Council of Economic Advisers standing as the epitome of economic expertise in policy circles, those of other social scientists found lesser resonance among policymakers – and sometimes occasioned skepticism, as illustrated by the repeated failures of Senator Walter Mondale’s bills to create a Council of Social Advisers in the late 1960s and

<sup>113</sup> Cravens, “Have the Social Sciences Mattered in Washington?” 129.

<sup>114</sup> Easton, “New Revolution in Political Science”; Gouldner, *Coming Crisis of Western Sociology*; and Olson, “Economics, Sociology,” 97.

early 1970s.<sup>116</sup> If the prosperity of US capitalism in the 1950s and 1960s was often credited to economists, the social and political crises of the 1960s seemed to underscore the lack of relevance and meaningfulness of other social sciences for understanding contemporary problems of society.

From the late 1960s through the 1970s, changes in the economic and political context, together with cultural shifts, fueled the ongoing redefinition of the boundaries between the social sciences. Becker's *Economic Approach to Human Behavior*, published in 1976, included subjects such as discrimination, democracy, crime, fertility, marriage, and, more generally, social interactions. Potentially, such a range of issues could bring economics closer to sociology, especially as the latter had experienced notable transformations in the late 1960s and early 1970s. With the invention of a "mainstream" in sociology came repeated critiques of its orientations and calls for reform. The revolt against the mainstream implied a greater role for issues such as race, class, and gender, which partly paralleled Becker's own effort in economics.<sup>117</sup> Yet, the heart of the "economic approach" – the "combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly" – suggested less collaborative prospects.<sup>118</sup> Moreover, Becker's approach was in tune with more general trends in the realm of ideas in a way that sociology, with the emergence of neo-Marxian perspectives, was not. Those trends historian Daniel Rodgers has aptly described as the result of a process whereby "conceptions of human nature that in the post-World War II era had been thick with context, social circumstance, institutions, and history gave way to conceptions of human nature that stressed choice, agency, performance, and desire."<sup>119</sup>

#### 1.4 From Social Problems to Individual Problems?

How do social problems come to exist and persist? The question has threaded through the reflective literature on social problems that sociologists have produced since the interwar years. Writing in 1941, for example, Richard Fuller proposed a "natural history approach," with the aim to trace the "common order of development through which all social problems pass."<sup>120</sup> Some thirty years later, Malcolm Spector and John Kitsuse rephrased the

<sup>115</sup> Solovey, "To Measure, Monitor, and Manage."

<sup>116</sup> Calhoun and VanAntwerpen, "Orthodoxy, Heterodoxy, and Hierarchy," 376–85.

<sup>117</sup> Becker, *Economic Approach to Human Behavior*, 5.

<sup>118</sup> Rodgers, *Age of Fracture*, 3.

<sup>119</sup> Fuller and Myers, "Natural History of a Social Problem," 320, 321.

point with antiseptic precision: The “central problem for a theory of social problems,” they wrote, “is to account for the emergence, maintenance, and history of claim-making and responding activities.”<sup>121</sup> The idea that social problems have careers is, in short, a recurring theme.

Most of the major mid-century social problems remain, as it were, recognized. Crime, poverty, addiction, and the others were, into the new millennium, still considered problems of society. What had changed, for many problem domains at least, was their public etiology – the prevailing frame by which they were explained. Trends in US politics and policy since the mid-1960s, registered by the social sciences too, had challenged their “social” dimension.

C. Wright Mills, back in 1959, furnished a useful vocabulary to address this shift. In *The Sociological Imagination*, he lamented the claustrophobic consciousness of everyday life in America. For most people, problems are experienced as private – as a “series of traps.” Problems that are, to Mills, manifestly social come across, to the average American, as personal. Caught up in “private orbits” and the “close-up scenes” of family and work life, the public has the nagging sense that something is off.<sup>122</sup> But they have lost the power, Mills thought, to link their private struggles to structural forces – the kind of linkage that the Great Depression, for example, had made conspicuous.

Mills’ project was to encourage a new consciousness, a “sociological imagination,” that restores the relationship between “the personal troubles of milieu” and “the public issues of social structure.” Employment, war, marriage, the city: The key to the sociological imagination, he claimed, is the awareness that these perceived problems are “caused by structural changes” – and therefore resistant to personal solutions. Mills’ complaint was that private troubles are no longer translated into public issues: “Much private uneasiness goes unformulated,” he wrote in 1959, “much public malaise and many decisions of enormous structural relevance never become public issues.”<sup>123</sup> The sociological imagination is Mills’ solution to a meta-problem: The process by which society’s problems come to be recognized has, he claimed, broken down.

The distinction between public issues and private troubles, trenchant as it is, implies that the former – recognized social problems – present a structural explanation by default. For Mills, a problem’s private enclosure

<sup>120</sup> Spector and Kitsuse, “Social Problems,” 146.

<sup>121</sup> Mills, *The Sociological Imagination*, 1.

<sup>122</sup> Mills, *The Sociological Imagination*, 8, 10, 12.

just means that it is understood as personal. Once a concern wins public recognition, its systemic nature is (or so Mills suggests) revealed too. But the lesson of the post-1960s era is that public resonance and personal frames of explanation can co-exist. Social problems can, in effect, be stripped of their social character. As a whole, the social dimension of the problems has thinned out over these decades. The prevailing explanatory schemes, and especially those policy interventions with meaningful purchase, have been cast in increasingly individual terms. And the declining significance of the social has a correlate in the disciplinary division of labor: psychology and economics lay claim to political territory once occupied by sociology.

The waning of the social, as a cross-problem pattern, demands an explanation. One clue, we think, resides in the broader US political economy. Scholars of the postwar social-problems array have remarked on the decisive importance of the welfare state – even in its limited, US form. Joseph Gusfield, in his 1989 SSSP presidential address, argued that the very concept “social problem” is “embedded in the development of the welfare state.” The idea that societies have fixable problems – and that they have a responsibility to go about fixing them – is, he noted, a recent development. Private troubles can only become public issues once the modern, democratized state has emerged. Its moral substrate is the “optimism of a sense of progress” according to which “most of life’s difficulties are inherently remediable.”<sup>124</sup> For Gusfield, this entailed, among other things, the rise of the “troubled persons professions,” who service – and profit from – the public’s will to intervene. For our purposes, the crucial point is that the very conceivability of social problems as such – as social and resolvable – presupposes an interventionist state.<sup>125</sup>

Since the mid-1970s, of course, the US version of the postwar Keynesian welfare state has come under unremitting attack. Sharp cuts to safety net programs, selective deregulation, free-falling marginal tax rates, and enfeebled union protections were secured, at different registers, in both

<sup>123</sup> Gusfield, “Constructing the Ownership of Social Problems,” 432.

<sup>124</sup> The linkage of the welfare state, sociology, and social problems is a major theme of Alvin Gouldner’s (1970) *The Coming Crisis of Western Sociology*. Writes Gouldner: “The needs of the new Welfare State, then, constitute both the growth opportunities and the limiting conditions that shape modern Academic Sociology as an institution; Academic Sociology flourishes in a period when Keynesian economics permit effective intervention with respect to the more traditional economic factors. Sociology is thus the N + I science of the Welfare State, providing it with an expert, university-based staff which addresses itself to the ‘other,’ the noneconomic social problems: racial conflict, deviant behavior, delinquency, crime, the social consequence of poverty.” Gouldner, *Coming Crisis of Western Sociology*, 161.

Republican and Democratic administrations through the 1990s. This bundle of changes, paired with steep upticks in income inequality and labor precarity, is often called “neoliberalism” – a term that, given its political freight and competing definitions, we use sparingly.<sup>126</sup> For now, the welfare state’s partial rollback suggests a tantalizing question: If social problems, as such, presume an interventionist state, what is their fate when intervention takes on an entirely new meaning? Might, in other words, a relationship obtain between the prevailing US political economy and the country’s designated problems? If so – and this is our crucial question – how are the social sciences implicated, if at all?

We can only gesture at some tentative answers, supported by the volume’s chapters. There is, to be sure, nothing neatly causal to report: The social sciences, in their changing configuration, did not convert, as it were, public issues into private troubles in the quarter-century since the Carter Administration. But the social sciences, economics and psychology in particular, are in the explanatory mix. To even approach the issue is to back into a sociology-of-knowledge thicket since there are so many layers of mutual entanglement: popular beliefs, the political system, the policy-making process, the influence industries, and the social sciences themselves. There are, moreover, elective affinities – resonances – among the layers as they have co-evolved in the postwar era, so that claims of directional influence are crippling hard to sustain.

The chapters nevertheless document an overall drift since the 1960s: an attenuation of social ways of seeing society’s problems, and their replacement, to some extent, by economic and psychological framings. Writ large, problems once cast as social were refracted, by the 1980s, through individualistic prisms, especially at the level of policy and public debate. The social sciences contributed to this shift, but were also – in their patterns of public prominence – remapped by the changes they helped bring about. The process was complicated by the suffusion of social-scientific concepts to the general populace and throughout the political system, alongside the spread of certain tools – such as cost-benefit analysis – into the policy-making arena.

For our purposes, three dimensions of the social sciences of social problems are worth isolating: *explanation*, *prescription*, and the *frame of analysis*. Along each dimension, a social scientist’s program might have, at the very least, implications for the way that problems are understood

<sup>125</sup> See Rodgers, “Uses and Abuses of ‘Neoliberalism,’” and the linked forum: Ott et al., “Debating the Uses and Abuses of ‘Neoliberalism.’”

and addressed. For example, we can distinguish between explanations for a problem such as crime that are structural – rooted in, say, the history of institutionalized racism or the economy’s lopsided distribution of wealth – and more individualistic frameworks that assign blame to personal failures or even crude self-interest. Interventions – proposed or implied – might hinge on that diagnosis, with a remedial strategy such as redistributive spending arising from a structural explanation. Or, on the individualistic account, a “family values” public information campaign or stiff mandatory prison sentences might follow. All of this (the explanation and even the remedy) could be guided by the mode of analysis – the methods chosen, paired with key disciplinary assumptions.

Words such as “structural” and “individualistic” are, of course, blunt designators. We have already referred to explanation – the realm of causes – as well as remedies (implied or prescribed). The question of method we have invoked too. At all three levels we are keen to maintain a distinction – a soft one – between the *structural* and the *individualistic*, but without imposing our definition on the volume’s authors – who have their own reasons to frame the stakes differently. Nor do we mean to invoke the contrast in the ontological sense – that is, in terms of what the real stuff of society is (e.g., individuals, systems, or social relations) – even if those commitments might ground explanatory accounts or methodological decisions.<sup>127</sup>

If the question is how problems arise and why they persist, one set of answers looks to the aggregated beliefs and actions of individuals. Mental illness, from one angle, is quintessentially private, possibly biological – if also affected by immediate social experience like early family history. A more structural account might, by contrast, stress the social conditions and institutions (notably including the “psy-disciplines”) that help produce the diagnoses and treatment regimens that organize individual lives. Thus for explanations to qualify as structural in the loose sense we invoke here, they must point to the hard cake of law, institutions, or political economy. Attitudes and actions take primacy in the more individualistic accounts.

<sup>126</sup> See the interesting discussion in Tilly, *Durable Inequality*, 17–24. Of course, a commitment to viewing social life through the prism of the individual makes it harder to even conceive of supra-individual factors such as institutions or political-economic dynamics. Likewise, an ontology that privileges systems or groups is ipso facto averse to explanations at the level of individual minds or behavior. Only those ontological schemes that foreground social relations, social process, or the dynamic, mutual constitution of subjectivity and structure are, as it were, truly agnostic on the explanatory questions we are interested in.

Attempts to address social problems might, accordingly, target structural impediments or, instead, dispositions and behavior. It's true that there is, strictly speaking, no necessary overlap between a given causal story about a problem and its proposed solutions. Individualistic accounts, after all, often seek to explain socially patterned outcomes. Those conditions – a racial wage gap, for example – could accommodate a range of would-be remedies, including policies, such as social welfare spending, that do not target the underlying “cause.” Nevertheless, interventions may be suggested, or discouraged, by the kind of explanation that elicits the greatest public resonance.

In other words, our two explanation types – individualistic and social-structural – in effect nominate their causal agents as targets for intervention. So if the public tends to see crime through an individualist prism, social-scientific explanations supporting individual agency will generate more appeal among policymakers. Making the crimes cost more (through harsher sentences or stepped-up enforcement), for instance, can appear as the most sensible policy response. If, instead, social forces find greater public resonance, social-scientific explanations of crime centered on social structures can attract policymakers much more effectively. Boosting the economic fortunes of potential perpetrators, for instance, will appear as a more fitting response. Explanations and policy interventions have affinities, in other words, along the structural/individualistic lines we have drawn. In the context of politics, moreover, policies must be promoted and justified on the basis – at least in part – of causal accounts. Explanations furnish distinctive kinds of rhetorical resources: Most individualistic explanations invoke, at least implicitly, intention, while many structural explanations point to unintended consequences. A remedy such as school-funding redistribution, for example, is hard to defend if the education problem is really about bad teachers and indifferent parents.

There is, finally, the frame of analysis – by which we mean methodology in the broad sense, inclusive of underlying assumptions. Here again a crude distinction may be drawn between approaches that position individual agency as the unit of analysis and those scholarly strategies that attempt to account for structural forces first and foremost. These methodological choices are, at the same time, dueling ways of seeing society. A commitment to methodological individualism, in other words, entails a social aperture whose exposure is narrowed. Survey methods that treat populations as individuals-in-aggregate, for example, might boost a more individualist problem diagnosis – as might a framework that, in the microeconomic mode, assumes rational maximizers. A political-economic analysis will,

with its focus sharpened to pick out large-scale systems, be prone to see the pivotal role of institutions. There is nothing determinate about these methodological nudges, but because they come with specific questions, they privilege certain understandings of a problem.

The point of drawing this contrast, between the structural and the individualistic, is to better specify patterns of change in the postwar social science engagement with social problems. As sketched across these three dimensions (explanation, prescription, and frame of analysis), the claim that “social” problems have lost some of their social character can be more precisely articulated. The draining out of the social is really about the declining fortunes of structural approaches to US society’s problems – and the ascendancy of more individualistic alternatives.

Consider economics. The discipline has maintained, at least since the 1930s, outsized policy prominence relative to its social science peers. But that fact obscures an array of relevant changes that, taken together, mark the period since the late 1950s and mid-1960s as distinctive. The economists who staffed the New Deal were, after all, much more pluralistic by the standards of the postwar discipline, with intellectual coordinates, in many cases, that were hard to distinguish from their sociologist or political science peers. The story of economists’ war-won prestige in the early postwar decades is well known, but the discipline’s mathematized orthodoxy was only secured at the end of the 1950s.<sup>128</sup> Well into the 1960s, moreover, the field’s mainstream paired its microeconomic neoclassicism with a Keynesian macroeconomics. Already by the late 1950s, however, Chicago figures like Gary Becker were taking microeconomics on the road – applying a utility maximization framework to domains well beyond the discipline’s inherited jurisdiction. By the early 1970s, the Keynesian consensus in the macroeconomic realm was, in the face of stagflation, unsettled. It is easy to exaggerate, but the trend, since then, has been the ascent of the “economic approach” within the discipline itself.

Meanwhile, the political climate for economists had improved, reflecting a trend toward “economization” – a way of framing political issues in economic terms.<sup>129</sup> The concept of “the economy,” in the relevant sense as the sum total of economic activity, did not gain public or political

<sup>127</sup> Backhouse, “Transformation of US Economics.”

<sup>128</sup> Berman, *Creating the Market University*, 174–77 (rendered as “economic rationalization”); and Berman, “Not Just Neoliberalism.” Berman’s use of “economization” is distinct from, for example, Murphy, *Economization of Life or Callon*, “Embeddedness of Economic Markets in Economics.”



purchase until the early postwar decades.<sup>130</sup> By the late 1970s, as Elizabeth Popp Berman demonstrates in a variety of US policy contexts, thinking about government as mainly in the business of improving the economy had become pervasive. From regulatory rule-making all the way through to campaign discourse, policymakers and politicians increasingly framed their talk and their decisions around economic impact. Government's role is to prime and pump the economy – an assumption, by the late twentieth century, embraced across the political spectrum and reflected, too, in everyday talk. The belief, almost unquestioned, was that a strong economy is the public goal that matters most. To Berman, this form of economization is much more ubiquitous – and ultimately significant – than the market fundamentalism espoused by the political right with special vigor since the 1980s. Economization is, indeed, an enabling condition, a seedbed from which the far narrower “neoliberal” worldview could grow.<sup>131</sup>

There was no single engine driving the post-1960s process of economization, but US economists undoubtedly contributed, if only because they were also active in policy institutions like the Council of Economic Advisers and in public debates.<sup>132</sup> They were, at the same time, its main academic beneficiaries. Over this period they won unequalled levels of policy influence, even by the standards of the discipline's existing prominence.<sup>133</sup> While some of that sway took the form of promoting market liberalization and market creation – the “neoliberal” facet – the policy leverage was secured on other, more mainstream grounds too. Economists' elevated influence, moreover, was felt indirectly: through political and popular language, for example, and through the spread of policy instruments. These dynamics, in practice, generated feedback loops: The discipline's authoritative stature helped economists obtain positions – often prominent ones – in policy institutions, which then boosted the field's prestige.

<sup>129</sup> Berman, “Not Just Neoliberalism,” 408; see also Mitchell, “Fixing the Economy”; Suttles, *Front Page Economics*, chap. 9; and Smith, *The Right Talk*, chap. 3.

<sup>130</sup> Writes Berman: “While neoliberalism is still an influential worldview, economization is in many ways a more powerful, and potentially more durable, trend.” Berman, “Not Just Neoliberalism,” 419.

<sup>131</sup> Berman, *Creating the Market University*, 46, 50 and Berman, “Not Just Neoliberalism,” 408–9.

<sup>132</sup> There is a large literature that chronicles the post-1960s influence of economists on US politics and policy, only some of it related to the rise of so-called “neoliberalism.” See, for example, Bernstein, *A Perilous Progress*, chap. 6; Hirschman and Berman, “Do Economists Make Policies?”; and Appelbaum, *Economists' Hour*.

That legitimacy, and those institutional footholds, helped in turn to promote and spread an economic way of thinking – a “cognitive infrastructure” that, once in circulation, did not depend on actual economists or their scholarship.<sup>134</sup> At varying levels of depth and sophistication, politicians, public intellectuals, and policy elites ventriloquize economists. Even barstool banter is sprinkled with economic concepts. So the idea, for example, that we are all calculating individuals – strategic managers of our own best interests – has seeped deeper into the social imaginary with the aid, at least, of economic thinking.<sup>135</sup> Put differently, the country’s individualistic spine was arguably straightened in the homo economicus mold. And if economists helped to underwrite the culture’s entrepreneurial ethos, they also reaped its legitimizing rewards – as an everyday warrant for the discipline’s distinctive style of reasoning.<sup>136</sup> That way of thinking, suffused with disciplinary argot such as “externalities” and “marginal cost,”

<sup>133</sup> This analysis draws heavily from Hirschman and Berman, “Do Economists Make Policies?” Angus Burgin’s and Daniel Stedman Jones’ analyses of neoliberalism – *The Great Persuasion* and *Masters of the Universe*, respectively – show that economic ideas, and not just economic theory, change society, and that they do it from within. So what matters is the seeming convergence between those ideas and the political climate. See Fontaine, “Other Histories of Recent Economics,” 400–401.

<sup>134</sup> We are using the notion of “social imaginary” in Charles Taylor’s sense: “By social imaginary, I mean something much broader and deeper than the intellectual schemes people may entertain when they think about social reality in a disengaged mode. I am thinking, rather, of the ways people imagine their social existence, how they fit together with others, how things go on between them and their fellows, the expectations that are normally met, and the deeper normative notions and images that underlie these expectations.” Taylor, *Modern Social Imaginaries*, 23. Taylor, focused on the broad sweep of modernity, claims that a major economic dimension was stitched into the Western social imaginary centuries before Becker’s *Economic Approach to Human Behavior*. See Taylor, *Modern Social Imaginaries*, chap. 5.

<sup>135</sup> Hirschman and Berman borrow the “style of reasoning” concept from Ian Hacking, “Statistical Language, Statistical Truth, and Statistical Reason.” The economic style of reasoning, according to Hirschman and Berman, “includes basic concepts such as incentives, growth, efficiency and externalities. It includes economic ways of approaching problems: by using models, systematically weighing costs and benefits, analysing quantitative empirical data, considering incentives, and thinking marginally. It suggests causal policy stories ... linked to economic theories: that investing in education will increase human capital and thus raise wage levels, or that increased government spending will stimulate the economy. And it makes certain methodological assumptions: about the importance of quantification and the possibility of using monetary value as a means of commensuration, for example.” Hirschman and Berman, “Do Economists Make Policies?” 794. See also Berman, *Thinking Like an Economist*, chaps. 1–2.

has taken special hold among policy analysts, most of whom are not card-carrying economists.<sup>137</sup>

A related development, stressed by Berman and Daniel Hirschman, was the rapid uptake of “policy devices” such as cost–benefit analysis or Congressional Budget Office bill-scoring.<sup>138</sup> Some of these devices, created or co-authored by economists, settled into the policymaking sediment in this period. Once lodged in rule-making practices, or even established by statute, the instruments – at least the successful ones – enjoy an effortless and durable legitimacy. As authorless doxa, their status as containers of particular values, often economic ones, goes mostly unseen. But the devices bear those values in consequential ways, Frankenstein-like. Their assumptions ricochet across the policy landscape – the antitrust fixation on price, for example, or the auctioning off of the public spectrum to the highest bidder.

The point is that economics – with its surging fortunes since the mid-1960s – has helped to undermine the idea that problems of society are social. To do so, economists did not have to deny the social character of these problems. Instead they drew attention to their microeconomic dimension

<sup>136</sup> Hirschman and Berman, “Do Economists Make Policies?” 795. Policy analysis is an especially amorphous, cross-disciplinary academic field whose history has yet to be written. With roots in public administration and Daniel Lerner and Harold Lasswell’s mid-century “policy sciences” program, policy analysis as a self-understood label took hold in the late 1960s and early 1970s, with especially heavy contributions from political scientists housed in public policy schools, newly established or re-branded from public administration in this period. See Torgerson, “Policy Analysis and Public Life,” 235–43; DeLeon, *Advice and Consent*, chap. 2; and Fleishman, “New Framework for Integration,” 734–38. By the mid-1970s, the think tank sector, often tied to funded political agendas, was growing dramatically, employing policy analysts and the emerging policy evaluation toolkit. See Medvetz, *Think Tanks in America*, chaps. 2–3 and Smith, *The Right Talk*, chaps. 4–5. Economic modes of analysis have been fundamental to the academic field’s approach and curricula, and to the practices of policy analysts in government and the think tank sector, ever since. See Fleishman, “New Framework for Integration,” 739–41; Haveman, “Policy Analysis and Evaluation Research,” 193, 197–202; House, “Social Psychology, Social Science, and Economics,” 237–39; and Berman, *Thinking Like an Economist*, chap. 1.

<sup>137</sup> Hirschman and Berman, “Do Economists Make Policies?” 796–800. They concede that economists were co-authors, as it were, with other disciplines in the creation of many widely deployed policy instruments. They adapt the “policy device” concept from the “market device” idea outlined in Fabian Muniesa, Yuval Millo, and Michel Callon’s “An Introduction to Market Devices.” Hirschman and Berman develop an interesting distinction between “devices for seeing” and “devices for choosing.” The “seeing” kind is about understanding, usually through a quantitative prism. “Choosing” devices, by contrast, grease the wheels of decision-making. Hirschman and Berman, “Do Economists Make Policies?” 797.

at a time when governments, confronted with the limits of the welfare state, found increasing merit in rational pricing. The social problems tradition within sociology had embraced the view that poverty, crime, and the other issues are fixable dislocations of modernity. That worldview had indeed helped underwrite the wider belief that government has an obligation to intervene – as expressed, to some extent, in US social policy through the Great Society. The claim that a problem like racism has social roots, with attendant social remedies, has since become less legible. As we have suggested, the discipline of economics was both a contributor to, and beneficiary of, a way of seeing society's problems through a cost-benefit prism. That worldview did not rule out the possibility of government intervention, but did contribute to reorienting governmental action toward creating favorable conditions for market pricing. Economists lay claim to "ownership," in Gusfield's sense, of no single problem in particular, but instead the whole array. Those jurisdictional bids were contested, of course, and strands of post-1960s quantitative sociology have – in some problem domains – remained a significant presence. It is also true that work on the full spread of society's "problems" has continued apace within sociology. The scholarship, however, failed to gain much policy purchase through the 1990s. If anything, sociologists positioned their interventions as a rearguard defense against policy rollbacks. Likewise, it was the rare work of sociology that resonated in the political sphere or with the wider public. By the late 1970s, in short, it had become harder to think like a sociologist and get noticed.

The case of psychology is harder to pin down, though the discipline's swelling influence in certain problem domains – like addiction and mental illness – is well documented by the volume's chapters.<sup>139</sup> The more crucial influence, however, was indirect, at the level of everyday discourse. The American vernacular, by the second half of the twentieth century, was littered with psychological concepts – the ideas, to some extent, and even the diagnostic language. Psychology supplied, as it were, a vocabulary of popular knowledge – and a way of seeing problems. Viewed through the prism of popular psychology, problems are personal. They are, in C. Wright Mills' phrase, "private troubles." Structural explanations fall outside the frame, and remedies, too, have a narrow, therapeutic character.

<sup>138</sup> The historiographical debate over whether psychological social psychology has shed much of its "social" character over the twentieth century operates at a different academic register than the popularization dynamics that we gloss here. See Greenwood, *Disappearance of the Social in American Social Psychology*.

It is true that the line from popular psychology to the postwar politics and policy of society's problems is hard to draw. Still, the therapeutic ethos – its baseline individualism – has helped seed the ground conditions for political intervention. Indeed, we refer to *psychologization* to capture this dynamic – with the parallel to, and example of, economics very much in mind. If the ends of public life have come to be recognized – by politicians, policymakers, journalists, and voters – as economic, something analogous can be said about private life: The injunction, issued with new urgency since the 1960s, has been to seek personal fulfillment. What counts as pathology, in a culture of self-actualization, is the individual's state of mind – her mental life as shaped by upbringing and immediate social experience. To the extent that psychological categories define the boundaries of the problematic, they limit the range of imagined explanations – and interventions – to the personal sphere.<sup>140</sup>

Neither the popularization nor the self-fulfillment culture was entirely new. The language of Freudian psychoanalysis, for example, had entered into popular circulation in the 1920s.<sup>141</sup> And cultural historians have documented expressions of yearning for “authentic” experience among late nineteenth-century American elites, which – by the 1920s – had spread in contradictory coevolution with mass consumerism.<sup>142</sup> But developments in the postwar decades were of a different character: Psychotherapy, as a practice and as an idea, took off, as psychologists broke psychiatry's monopoly on professional therapy in the late 1940s.<sup>143</sup> Federal legislation – the National Mental Health Act of 1946 and the Community Mental Health

<sup>139</sup> There is no treatment of “psychologization” comparable to Berman's elaboration of “economization.” Jan De Vos has employed the term in a roughly analogous, though heavily theorized, way to the sense we invoke here: “Psychologization is the overflow of the knowledge of psychology into society altering the way in which ‘man’ is present with himself, others and the world. Psychologization is the process in which psychological signifiers and discursive schemes result in the typical dualism within modern humankind which reflects upon itself having adopted the academic, psychologizing gaze.” De Vos, “From Milgram to Zimbardo,” 158. See also De Vos, *Psychologization and the Subject of Late Modernity*. The term also appears, in passing, in other academics' works, to refer to the popular incorporation of psychological concepts. Rutherford, *Beyond the Box*, 12. Nikolas Rose, in his Foucauldian genealogies of the psy-disciplines, occasionally invokes the term, and with a similar meaning. Rose, *Governing the Soul*, 38, 248.

<sup>140</sup> See, for example, Burnham, *After Freud Left*, part 1.

<sup>141</sup> Lears, *No Place of Grace*; Lears, “From Salvation to Self-Realization”; Susman, “‘Personality’ and the Making of Twentieth-Century Culture”; and Leach, *Land of Desire*.

<sup>142</sup> Herman, *Romance of American Psychology*, chap. 9 and Cushman, *Constructing the Self*, *Constructing America*, chap. 8.

Centers Act of 1963 – helped finance an army of trained psychotherapists. Clinical psychology, a specialty that scarcely existed before the war, came to dwarf the discipline’s academic ranks.<sup>144</sup> Therapy for the “normal” American – the merely neurotic – quickly lost its stigma, producing an explosive growth in demand.

By the 1960s psychology had, in Ellen Herman’s words, become “public culture,” infused into social movement politics and mass media.<sup>145</sup> The postwar growth of humanistic psychology – epitomized by figures like Carl Rogers and Abraham Maslow – directly fed a popular fixation on self-fulfillment.<sup>146</sup> The 1970s efflorescence of encounter groups, New Age spirituality, and the human potential movement was itself just one tributary in a broad-stream therapeutic culture expressed – with inescapable ubiquity – in mass-circulation magazine advice, confessional talk show television, and the self-help book trade. So there was, in the decades after World War II, a lava-like flow of psychological concepts into the postwar American lexicon – transforming the meaning of the social world to its actors themselves. Of course, the postwar American social imaginary was already tuned to personal frames of understanding, long before the publication of the *Diagnostic and Statistical Manual of Mental Disorders – III* in 1980.

If anything, disciplinary frameworks such as psychological individualism and homo economicus took hold, at the level of everyday life, because they resonated with preexisting currents of American atomism. But the disciplines were not simple beneficiaries of the country’s Lockean and bootstrapist legacies. They also helped to rechannel the social imaginary in distinct, if overlapping, ways: through the spread of an entrepreneurial ethos, for example, or the thorough-going embrace of psychological talk. As a shared substrate of common sense – of a “worldview” in the everyday sense – the social imaginary helps to set the parameters of political possibility. It is easier, in other words, to treat government as the problem, or to accept that there’s no such thing as society, when the social is imagined as a collection of self-determining individuals. It is here, at the indirect and hard-to-measure level of popular sentiment, that psychology and economics may have their greatest import. Even if psychology’s direct policy influence was more intermittent and domain-specific than that of economics, the two disciplines helped, in tandem, to peg the boundaries of the politically thinkable to the postulate of self-reliance. By the 1980s it was harder to see

<sup>143</sup> Capshew, *Psychologists on the March*, chaps. 6 and 8.

<sup>144</sup> Herman, *Romance of American Psychology*, 310. See also chaps. 9–11.

<sup>145</sup> Grogan, *Encountering America*.

private troubles as expressions of social problems, or to target those troubles with collective intervention.<sup>147</sup>

The new prominence of economics and psychology coincided with the hardening of disciplinary boundaries among the social sciences. By the 1970s, partly due to a shift in the patronage system, specialization gained ground, with disciplinary concepts and techniques gradually supplanting the use of broad, integrative concepts and theories. Economists overtook other social scientists in studying society's problems and advising policymakers, while psychologists registered their social-problems influence at the level of everyday language and the spread of a popular therapeutic ethos.

The volume's chapters, taken together, reveal a paradox – or at least a discrepancy. In the realm of policy and politics, the postwar story has an arc: Social problems became economic problems, personal problems, or both. Economists, and to a lesser extent psychologists, displaced sociologists at the public center of the social science of society's problems. That is the more-or-less consistent takeaway from the chapters' problem-by-problem accounts – but mainly, as it turns out, for the high-stakes domain of policy, politics, and public discourse. The chapters tell a different story about the social sciences themselves. Debates that in the early postwar period were plural and polyphonic remained so – at least in many cases – by the late 1980s.

Social scientists, as Savina Balasubramanian and Charles Camic show in their chapter, were already fretting about the family early in the twentieth century. The institution, according to sociologists through the late interwar period, was especially vulnerable to the disorganization wrought by modern social change. Concern for the family's fate took on a new, international cast in the early Cold War period, in conjunction with the rise of demography as a cross-disciplinary field. In the contest with the Soviets, the drive to modernize the “new states” had, as its demographic dimension, a concern for large family size. The claim that economic development and insulation from socialism hinged on fertility control was advanced by demographers, many but not all housed in sociology. By the mid-1960s, a domestic-facing research landscape was gathering momentum, supported by government and foundation interest in the family's role as transmitter of inequality. The domain of family demography, a resource-intensive specialty clustered in

<sup>146</sup> Indeed, the two disciplines' implicit models of selfhood had, by the last quarter of the century, formed a curious cocktail – of strategic self-objectification mixed with authentic self-fulfillment. The injunction, fully realized on the new millennium's social media platforms, was to treat yourself as a product promoted through the calculated appearance of authenticity. Pooley, “The Consuming Self.”

cross-disciplinary centers, spread throughout the remainder of the century – dwarfing other claimants, including Gary Becker’s microeconomics of the family.

In his chapter on education, Andrew Jewett traces the peculiar – and sometimes hands-off – relationship of the mainline social sciences to education research over the course of the twentieth century. The existence of low-status education schools, operating as standalone units on the professional margins of the US university, colored the shape and volume of social-scientific inquiry in shifting ways. Into the 1950s, Jewett observes, education was typically positioned as a solution for other problems of society, rather than its own focal concern. With the Cold War and the federal government’s new mandate to steward economic growth as backdrop, “fixing” the nation’s schools took on special urgency, as exemplified in the early 1980s by a policy and political climate increasingly oriented to national competitiveness. In Jewett’s account, social scientists from the main disciplines move in and out of the education domain, sometimes yielding jurisdiction to “ed school” faculty whose radicalism has tended to marginalize their contributions since the 1960s. From the 1970s on, meanwhile, the policy prominence of economics has increased. The human capital framework, in particular, supplied an individualistic and vocational lens to assess the school system, one that sidelined the stratification and inequality concerns of other social scientists and educational researchers. Jewett’s account, in other words, documents the post-1960s split-screen dynamic outlined above: social-scientific pluralism juxtaposed with the overarching policy influence of economics and, to a lesser extent, psychology.

Alice O’Connor’s chapter on poverty describes the broad arc of twentieth-century poverty knowledge in terms of “disembedding.” Progressive era social research had cast poverty as a structural problem, one that would require structural solutions. When journalists and politicians thrust poverty back onto the social-scientific agenda in the 1960s, the issue was framed – by economists and other social scientists – in narrow and absolute terms, to the explicit exclusion of inequality. Economists in the postwar policy firmament were decisive and notably aloof, but the behavioral science-orientation of their noneconomist colleagues contributed to the War on Poverty’s circumscribed ambitions too. By the time the political currents shifted in the 1970s, the stage was set for a further disembedding – a re-pauperization of the poverty problem that culminated in Bill Clinton’s mid-1990s welfare rollback. O’Connor’s account foregrounds the often-determinate role played by politics and – in the case of the neoconservative think tank – mezzo-level policy discourse. But social scientists were not



impotent bystanders in the disembedding process. They had, in the War on Poverty years, laid the groundwork for the dodging of inequality questions – and, ironically, for the personal-responsibility moralism that, in the Clinton era, marked a full retreat from liberal social provision.

Leah N. Gordon, in her chapter, recounts how social scientists, during and after the war, tended to treat discrimination as a system – one with interlocking legal, political, and economic dimensions. By the 1950s systemic frameworks had receded in favor of more individualistic explanations for the “race problem.” The study of discrimination remained strikingly cross-disciplinary, but the lens of prejudice – individual attitudes in the aggregate – was newly prominent, supported by philanthropy and Cold War discretion. Gary Becker brought microeconomics to discrimination in this period, too, in an approach that, like the psychology of prejudice, stressed the causal priority of dispositions. The announcement of formal equality in the civil rights legislation of the mid-1960s complicated the study of race for the balance of the century. Systemic accounts were partially revived, and evidence for persisting racial inequality was widely documented. But causal factors proved harder to identify, Gordon concludes. In the wake of *de jure* segregation, even radical critics of “institutional racism” and “internal colonialism” conceded that discrimination’s effects were easier to describe than its causal dynamics. Quantitative sociologists and economists deployed a cascade of measures that demonstrated disparate outcomes, though again without clear explanatory accounts rooted in discrimination. By the 1980s the conservative rhetoric of “colorblindness” had, as it were, turned the discrimination question on its head.

George C. Galster’s chapter addresses the “black ghetto” – the persistent concentration of African-American poverty in the country’s inner cities. The interwar Chicago School’s race-agnostic paradigm was, Galster shows, challenged by a handful of high-profile sociologists and economists in the mid-1940s, in works that, though they documented a discriminatory thicket, failed to win public or policy traction. Only with the televised urban riots of the mid- to late 1960s, in the midst of Johnson’s Great Society, did the black ghetto attain full-fledged problem status. Social scientists registered the new stakes in a wave of studies that, in effect, established the battle lines for decades. Works that stressed the spatially concentrated legacy of racial discrimination were pitted – in a highly charged political climate – against culture-of-poverty accounts. The research lines, in turn, informed competing remedies, notably geographic dispersal, community development, and – in a reflection of the country’s rightward drift – outright disengagement. The broad, if uneven patterns in the post-1960s scholarship,

according to Galster, are a de-emphasis of race on the one hand, and the strengthening of individualistic frames on the other. There is, moreover, a rough disciplinary divide: Sociologists, he shows, have tended to highlight spatial and social factors, with economists and political scientists favoring, for the most part, more individualist and class-based accounts.

The story of crime, as related in Jean-Baptiste Fleury's chapter, has a familiar arc. Interwar criminology was dominated by sociologists, who cited modern social conditions as the main source of crime. Together with social psychologists and legal scholars, sociologists had swapped out environmental explanations for the biological accounts prevalent in the late nineteenth-century. In the early postwar decades, the study of crime came to center on juvenile delinquency, with a rough division of labor established between sociologists and psychologists. The overall explanatory frame remained environmental, though low-income and subcultural factors had, by the 1950s, largely supplanted immigration and urbanization. New philanthropic attention to delinquency helped guide the federal government's adoption of community action programs under the Democratic administrations of the 1960s, with heavy involvement from social scientists. Johnson's twinned Wars on Poverty and Crime were, at least initially, predicated on the postwar consensus that the root causes of crime were social. As Fleury recounts, an uptick in crime and the urban riots of the mid- to late 1960s put Johnson and the Great Society's social policies on the defensive – as Republicans refined a racialized backlash politics of “law and order.” By the late 1960s a handful of social scientists had launched high-profile attacks on the prevailing criminological mainstream, coinciding with a federally sanctioned turn toward “crime control” and standalone programs in “criminal justice.” Though still prominent, sociologists shared jurisdiction with other social scientists, including a growing and influential contingent of economists. By the 1980s, crime had been sheared off from other social issues, with the field now centered on crime's efficient management.

Nancy D. Campbell's chapter takes up the case of drug addiction. The social science of the “opium problem” (an early label) was, from its 1910s beginning, entangled with the federal government. Federal institutions generally promoted research that located addiction in personal psychology or the properties of drugs and their effects on the brain. There was, from the 1930s onward, a marginal but persistent alternative – exemplified by sociologists Alfred Lindesmith and Howard S. Becker – focused on the social process of definition, one that involved “addicts” themselves interacting with their social environment. An avalanche of new interest in

the 1960s and 1970s – a response to a perceived drugs crisis and follow-on funding and policy mandates that brought into being the National Institute on Drug Abuse (NIDA) – brought epidemiologists, economists, and anthropologists into the research mix. In Campbell’s account, the social sciences of addiction is a century-long jurisdictional melee, with the notable inclusion of fields bordering on, or fully within, the natural sciences. And so her conclusion – that the least “social” among them, neuroscience, came to dominate by the 1990s – provides support, with a natural-science asterisk, for the volume’s social-attenuation thesis.

In his chapter on mental health, Andrew Scull traces the social sciences’ relatively light pre-World War II engagement with mental health issues, with the partial exception of sociology. The postwar expansion of federal research funding, paired with explosive treatment demand for returning veterans, transformed psychology, swelling its ranks (as we have seen) with clinicians. Federal largesse, especially from the new National Institute of Mental Health (NIMH), underwrote a surprisingly diverse range of projects, with psychologists, psychiatrists, and, to a lesser degree, sociologists as the main beneficiaries. As psychology swelled under its postwar “scientist-practitioner” settlement, sociology remained comparatively small and, by the 1960s, increasingly critical of psychiatry and the country’s mental health institutions. Meanwhile, as Scull describes, psychiatry had rapidly shed its psychoanalytic character by the 1980s, a response to dried-up funding and the psychopharmacological revolution. The chapter addresses the relative neglect of mental health by economists up through the 1990s – even as the discipline (as documented by the volume’s other chapters) marched its toolkit through many other social problem domains. Scull’s explanation for the anomaly is that mental health, in all its stigmatized irrationality, was a step too far for a discipline committed to the everyday fact, and scholarly application, of reason. With its tight discipline and established policy sway, economists had “no need to chase after scraps from the table served up by NIMH” (p. 352).

The social science of war, as Joy Rohde demonstrates in her concluding chapter, is a curious case. If education *gained* problem status in the early postwar period, war followed the opposite arc, shedding its social problem framing. From the interwar years through to the late 1940s, war was a public-facing problem whose solution – the eradication of armed conflict – seemed within reach for many social scientists and their internationalist allies. Quincy Wright’s magisterial and multidisciplinary 1942 *A Study of War* exemplified the social-scientific ambition to foster peace through an expert-guided world order. The Cold War, however, abruptly stalled war’s brief career as a social problem. The Soviet threat, and the national security

state erected in response, helped to reframe the social science of war in management terms. For the next two decades most social scientists of war – though split on methodology and approach – hitched their study to the Cold War struggle. By the late 1960s the Vietnam debacle had implicated Defense-sponsored work on counter-insurgency and psychological warfare, leading to a public backlash against military entanglements. Many social scientists, Rohde shows, abandoned the study of war in Vietnam's wake, ceding the domain to political science in general and international relations in particular. The result was a social science of war that remained centered on statecraft and security into the 1980s.

So the volume's problem-specific accounts describe, from a bird's-eye view, an intriguing gap between developments internal to the social-scientific discourse on the one hand, and public prominence on the other. The picture is one of academic pluralism juxtaposed with a lopsided resonance at the level of politics and policy. In the second domain, the more public constellation, economics and psychology won an outsized influence that, however, was not always reflected in the problem-specific academic literatures traced by the book's authors.

What are we to make of this discrepancy? One answer, which we reject, is a variation on the theme that Herbert Blumer, in his 1971 paper, put forward: Social scientists and their work are side-shows in the political process. What matters, in the end, are the prevailing political winds; politicians and policymakers will, perhaps, raid the academic storehouse, but for their own justificatory ends. The policy shelf is well stocked with academic literature fit for any particular political platform to, in effect, check out. All the agency on this view lies with the politicians and their house intellectuals and functionaries. If governments are constrained by organized interests and more diffuse publics, then something gauzier – like the prevailing social imaginary – furnishes those limits. On this account, social science is an inert and ineffectual bystander – a supplier of raw materials, at best.

The problem with this position, for all its obvious truth, is that the constitutive contributions of social scientists to the underlying conditions of reception are written off. We have, by invoking the pair of ungainly process nouns "economization" and "psychologization," tried to gesture at the dynamism – the mutual shaping – that enmeshes social scientists in the politics of American social problems. The very seedbed of legibility is fertilized, in part, by the circulation of social-scientific knowledge. If the individualisms of economics and psychology have resonated in the century's last twenty-five years, the explanation is not merely their good

fortune. If their normative, explanatory, and methodological frameworks are a good match for the prevailing politics, one reason is that psychology and economics have helped to sculpt the very popular and political space in which they flourish. The disciplines have, crucially, helped set the conditions for their own success.

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