AN ACCIDENT OF MEMORY

EDWARD SHILS, PAUL LAZARSFELD AND THE HISTORY OF AMERICAN MASS COMMUNICATION RESEARCH

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Abstract

An Accident of Memory: Edward Shils, Paul Lazarsfeld and the History of American Mass Communication Research

The main memory of American mass communication research holds that scholars around Paul F. Lazarsfeld, in the years during and after World War II, dispelled the conventional wisdom that media marinate the defenseless American mind. According to the story, a loose and undisciplined body of pre-war thought had concluded naively that media are powerful—a myth punctured by the rigorous studies of Lazarsfeld and others, which showed time and again that media impact is in fact limited. This storyline, first narrated in Elihu Katz and Lazarsfeld’s *Personal Influence* (1955), remains textbook boilerplate and literature review dogma fifty years later. The dissertation traces the emergence of this “powerful-to-limited effects” disciplinary legend, with special emphasis on the surprising contributions of sociologist Edward Shils, the mandarin theorist and intellectual maverick with little interest in the empirical study of media. In the crucial postwar years, Shils provided an account of the disappearance and reemergence of “small group” research, which he framed as a contrast between pictures of society—between the mistaken European view of impersonal isolation as against his view, that *Gemeinschaft* elements endure. Shils’s treatment of small-group research, and especially his embedding of that story in terms of societal imagery, was essential to the field’s mnemonic emplotment. Shils had his own intellectual reasons for narrating the history in the manner that he did—reasons rooted in his evolving and deeply engaged
search for the underpinnings of modern social order. In a sense, however, his reasons did not matter once the narrative itself was released to the American sociological public; Lazarsfeld and Katz had their own reasons for adopting the historical picture that Shils put forward—reasons largely centered on scholarly competition and norms of originality.

The powerful-to-limited-effects narrative in *Personal Influence*, in turn, was so widely embraced in the late 1950s for a still-different set of reasons—because of the scholarly support it lent to the public intellectual defense of American popular culture, in the context of an evolving Cold War liberalism. The staying power of this limited-effects narrative was ultimately guaranteed, however, by the newly institutionalized, would-be discipline of “communication”—which retained the storyline as a usable, and teachable, past.
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Acknowledgements

The dissertation that I planned to write hardly resembles this, the completed manuscript. I set out to write a history of leftist media research, and ended up with an account of the field’s remembered history: from Herbert Schiller and Dallas Smythe, in my early conception, to Edward Shils and Paul Lazarsfeld here. My dissertation sponsor, James W. Carey, adapted cheerfully to the topical meandering. Few if any communication scholars share his broad-based intellectual literacy, and I benefited enormously from his erudition and guidance—inside and outside the classroom. I owe him the profoundest debt.

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INTRODUCTION

Fifteen Pages that Shook the Field

The main memory of American mass communication research holds that scholars around Paul F. Lazarsfeld, in the years during and after World War II, dispelled the conventional wisdom that media marinate the defenseless American mind. According to the story, a loose and undisciplined body of pre-war thought had concluded naively that media are *powerful*—a myth punctured by the rigorous studies of Lazarsfeld and others, which showed time and again that media impact is in fact *limited*.

If we were to trust the first chapter of *Personal Influence*—the landmark 1955 study by Elihu Katz and Lazarsfeld—the whole of pre-World War II research would seem to us naïve in its methods and crude in its conclusions. Katz and Lazarsfeld’s well-written, fifteen-page synopsis of the “ideas with which mass media research began” ascribes to past scholarship one of “two opposite inclinations”: The interwar body of work either decried the mass media as “instruments of evil design,” or else heralded those media as a “new dawn for democracy.” Both tendencies—the fearful *and* the ebullient—described the media message as a “direct and powerful stimulus.” Swept up by popular alarm or blinded by utopian rhetoric, both kinds of scholars based their judgments on intuition or folk wisdom or speculative European theory. None of this will do, write Katz and Lazarsfeld. Fortunately, a new body of work has emerged that rejects the folk wisdom and spurns the Europeans, and opts instead for a sober and quantitative approach.

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Katz and Lazarsfeld conclude their fifteen pages with a review of the new research, whose “greater precision” has generated “increasing skepticism about the potency of the mass media.”

In one short chapter, the field’s untidy past was neatly emplotted. A naïve, intuitive prehistory—given over to the mistaken belief that radio and film wield enormous power—got displaced by a calmer, scientific appraisal: These media, according to the new evidence, have only “limited” effects. This *Personal Influence* history is simple, direct, and meagerly sourced. Its clean narrative is resolved by the second act. And it was believed: Fifty years later, the “powerful-to-limited-effects” storyline remains textbook boilerplate and literature review dogma. Katz and Lazarsfeld’s fifteen pages have had more influence on the field’s historical self-understanding than anything published before or since.

All of the historiographical cliches of the decades to come—the interwar “magic bullet theory,” for example, or the idea of a “hypodermic needle” model—trace their origins to those fifteen pages. They themselves do not employ these terms—the “hypodermic” image, as it turns out, was first invoked in passing by their colleague at the Bureau of Applied Social Research, Bernard Berelson in his 1954 *Voting* study. But Katz and Lazarsfeld lay out the whiggish two-stage history that would become the common reference point—the default citational authority—for the many later elaborations of the “powerful-to-limited-effects” storyline.

It is a fitting irony that the sociologist Edward Shils, a mandarin theorist and intellectual maverick with no interest in the empirical study of media, supplied the coalescing field of mass communication research with a usable past. It was Shils who furnished, without intending to, *Personal Influence*'s basic plot.

While it is possible to discern inchoate gropings toward the “limited effects” storyline before Shils’s unwitting intervention, these were scattered and inconsistent, without narrative tightness. As early as 1943, Lazarsfeld and others started to note the difficulty of bringing about attitude change through media persuasion.3 But this observation—repeated often over the next ten years—wasn’t yet framed as a claim of minimal media impact, nor as a happy repudiation of precursor overreach. Indeed, the discovery of the obstinate audience was typically discussed as a technical problem, as an obstacle in the design of effective propaganda. Occasionally, especially in the years immediately after the war, the failure of straightforward persuasion was treated in broad, media-impact terms, but almost always in tension with the ongoing search for careful propaganda design.4 This two-track, schizophrenic framing—a concern, on the one hand, for finding out how to make persuasion work, and on the other, an effort to draw

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3. The earliest research synthesis that places an emphasis on audience intransigence is Lazarsfeld and Robert K. Merton, "Studies in Radio and Film Propaganda," *Transactions of the New York Academy of Sciences* 6(2) (1943). Slightly modified, the synthesis was included in Robert K. Merton’s enormously influential Social Theory and Social Structure: Toward the Codification of Theory and Research (Glencoe, IL: Free Press, 1949/1968).

sweeping conclusions about media impact—made it hard to formulate a clean, coherent statement of limited effect. With the Cold War dramatically hotter by 1949, Joseph Klapper, one of Lazarsfeld’s students, finally makes a muscular case for minimal media impact in free, plural societies in his influential synthesis of research to date.⁵ There is, in Klapper’s summary, a palpable tone of relief that anticipates the upbeat, celebratory cadence of the full-fledged “limited effects” storyline. But there’s still no plot in Klapper, no clear account of the triumph of careful observation over alarmist conjecture. This Shils would supply.

Shils’s help with the narration came in two installments. He provided, first, an account of the disappearance and reemergence of “small group” research that proved vital to the powerful-to-limited-effects account that gelled, finally, in Elihu Katz and Lazarsfeld’s Personal Influence (1955).⁶ Drawing on his authority as a major exegist of European sociology, Shils attributed the temporary small-group research fall-off to the influence of the misguided European tradition. “The great stream of [European] sociological thought in the nineteenth century,” he wrote in the crucial 1951 paper,

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⁵ Klapper, The Effects of Mass Media (New York: Columbia University Bureau of Applied Social Research, 1949) 210. In a fascinating foreword, however, Lazarsfeld is at pains to emphasize the limits of short-term persuasion studies to answer wider questions about media impact, which, he argues, must occur over time and through a complex back-and-forth with the social environment. Klapper, as in his crucial and much better known 1960 update to the 1949 summary, argues throughout that reinforcement of pre-existing beliefs and norms is the main, and salutary, effect of mass media. His 1960 synthesis, published after he had assumed the chief research post at CBS, incorporated the “limited effects” narrative in full, and contributed hugely to the diffusion and establishment of that history (The Effects of Mass Communication (New York: Free Press, 1960)).

developed a wrong-headed picture of the transition to modernity. For the Europeans—he mentions only Tönnies and Marx—that transition was a great unraveling of old communal bonds, a shucking off of tradition, to be replaced by an atomized and disoriented populace bound only by contract and mutual expediency. The Europeans greatly underestimated, Shils insists, the persistence of custom and primary ties in the modern world.

Shils stressed the Europeans’ influence on the younger American field. Though American sociologists, and Charles Horton Cooley in particular, had generated their own rich tradition of thought on primary groups in the early twentieth century, they regrettably succumbed to the European obsession with Gesellschaft: “[A]s a living trend in sociological research, the primary-group studies stimulated by the writings and teachings of Cooley…had come to an end by the early 1930s.” It was left, Shils concludes, to scattered research groups to start anew; their unwitting “convergence” on the small group had, by the late 1940s, fortuitously reclaimed an abandoned current of American sociology.

Katz and Lazarsfeld embraced Shils’s fall-and-rise account with alacrity. But, as it happened, the contrast between the pictures of society that Shils invoked—between the mistaken European view of impersonal isolation as against his view, that Gemeinschaft elements endure—had even greater influence. The distance between the “potent” and “minimal” media impact stances, Katz and Lazarsfeld argue, is the distance between

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7. Shils, “The Study of the Primary Group,” 44.
8. Ibid 47.
these contrasting pictures of modernity. Those with the superseded belief in media potency clung to that image of breakdown and anonymity of “the late 19th century European schools.”10 The more measured finding of limited effect, by contrast, has its roots in an image of society that recognizes the endurance of primary ties and a rich associational life. The wrongness of the interwar media analysts, in other words, rested on their warped view of modern life: “Their image, first of all, was of an atomistic mass of millions of readers, listeners and movie-goers prepared to receive the Message.”11 Katz and Lazarsfeld acknowledge their debt to Shils’s “excellent” essay early and often12: Their narrative contrast between interwar media analysts’ naïve belief in media potency and their own, more sober conclusions relies on the parallel distance between the two camps’ social image. And with this grounding, Katz and Lazarsfeld had elegantly narrated the history of the field, retroactively labeling Lazarsfeld’s body of communication research as falling under the “limited effects” position. Shils’s treatment of small-group research, and especially his embedding of that story in terms of societal imagery, was essential to the field’s mnemonic emplotment.

Shils’s own personal influence derived from his unusual, border-spanning perch: He had become an empirical social scientist with, however, a penetrating knowledge of social thought, in a social scientific milieu for which such knowledge was scarce. He was already, in 1948, claiming an interpretive authority in intellectual historical matters that was only to swell in the years to come. With one foot in social theory and the history of

11. Ibid 16.
12. “Several sections of this and the following chapter will draw extensively on Shils’ excellent essay” (Katz and Lazarsfeld, "Images of the Mass Communications Process," 17n).
social thought, and the other in the world of post-war empirical social science, Shils was able to take his knowledge of the former—with all of its self-validating prestige—and redeploy it to a besotted field.

Shils had his own intellectual reasons for narrating the history in the manner that he did in 1948 and again, with more clarity, in 1951—reasons rooted in his evolving and deeply engaged search for the underpinnings of modern social order. In a sense, however, his reasons didn’t matter once the narrative itself was released to the American sociological public; Lazarsfeld and Katz had their own reasons for adopting the historical picture that Shils put forward—reasons largely centered on scholarly competition and norms of originality. The powerful-to-limited-effects narrative in *Personal Influence*, in turn, was so widely embraced in the late 1950s for a still-different set of reasons—because of the scholarly support it lent to the public intellectual defense of American popular culture, in the context of an evolving Cold War liberalism. (In these same debates, Daniel Bell and Shils introduced the complementary “mass society theory” pejorative, which later merged with the powerful-to-limited-effects story.) The staying power of this limited-effects narrative was ultimately guaranteed, however, by the newly institutionalized, would-be discipline of “communication”—which retained the storyline as a usable, and teachable, past.

13. Shils and Morris Janowitz, "Cohesion and Disintegration in the Wehrmacht in World War II," *Public Opinion Quarterly* 12(2) (1948); and Shils, “The Study of the Primary Group.” Though Shils’s concern with the constituents of social order comes out, in the Wehrmacht essay, only in its last few lines, his clear fixation on this, the problem order, is clearly on display in other published essays from these years. His 1947/1948 survey of American sociology, for example, explicitly recommends the “fundamental problem of consensus” as the central research agenda for the field (*The Present State of American Sociology* (Glencoe, IL: Free Press, 1948) 55). In his 1949 reflective essay on the philosophy of social science, he proposes that the understanding of social order become the explicit value-relevant theme, in Weberian terms, that guides the selection and design of research projects ("Social Science and Social Policy," *Philosophy of Science* 16(3) (1949) 239-40).
There was nothing smooth nor linear about the chain of distinct purposes and contexts which led, eventually, to a standard “disciplinary” history for mass communication research. In this case at least, the memory got formed, altered, adopted, modified and re-adopted in disjointed succession. What was for Shils an intellectual coming-to-terms was for Katz and Lazarsfeld an artful re-packaging; for the mass culture debates it was scholarly ammunition, and for the communication discipline an origin myth and internal cohesive. That Shils would indirectly draft the lecture notes of journalism school instructors in 2005—this was an accident.

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Lazarsfeld, in the *Personal Influence* narrative, characterized his own body of media research as the progressive unfolding of a counter-intuitive insight: that the media have only minimal effects. Todd Gitlin, in his Lazarsfeld narrative of the late 1970s, portrayed the Bureau findings for limited effect as a payment-in-absolution to its media industry patrons.14 Timothy Glander and Christopher Simpson have since challenged Lazarsfeld’s self-description more fundamentally: The Bureau was hardly concerned to show that media impact is limited, since it was in the business of making persuasion work for its commercial and especially government clients.15 Elihu Katz and Peter Simonson, most recently, have asserted that Lazarsfeld’s media research was far more sophisticated and even critical than his detractors ever admit.16

None of them is wrong. Lazarsfeld’s published research on mass media was all of these—and more still. To read through his 1940s media scholarship is to invite disorientation. This is not on account of the research findings themselves, which were relatively consistent. No, the dizzy feeling comes from the sheer variety of sense-making scaffolds that Lazarsfeld erected around the findings. Lazarsfeld, in these pages, comes off as a deeply serious scholar, an industry apologist, a public-information campaigner, an advertising strategist, an unblushing propagandist, and even an industry critic. On occasion, he is more than one of these in the same article.

This presentational flux stands out so markedly because the same core evidence is marshaled to such diverse argumentative ends. Ever since the initial, 1941 data analysis of what would become, in 1944, The People’s Choice, Lazarsfeld discovered—against his expectations—that short-term media persuasion does not, on its own, change minds or behavior very easily. From the same data, he realized that face-to-face influence works better than the mediated sort. He also surmised that the two kinds of persuasion may be complementary, or at least relatable in some way.

Over the next decade, Lazarsfeld would take this basic bundle of findings and give it variable shape—not randomly, but according to the particular audience he was to address. If educational broadcasters were the target, he might stress the importance of local, face-to-face promotion as a means to build a broadcast audience. If he was addressing scholars, he might concede the limits of short-term campaign studies and call for elaborate research into long-term effects. If his audience was the fretting public, he

might highlight the reassuring finding that propaganda often fails. If he was writing for
government propagandists, he might strategize about the most effective mix of
interpersonal and media tactics—turning his *People’s Choice* findings into a complicated
blueprint for two-step manipulation.

Lazarsfeld’s legendary resourcefulness revealed itself in any number of other
ways—in his contract-to-scholarship alchemy, in his re-analytic zeal for layabout raw
data, in his ability to harness particular student interests to larger Bureau goals. None of
this was wasted: His tenacious ingenuity in securing funds, for example, made possible
the Bureau’s awe-inspiring scholarly output—much of it his own and much of it
abundant with insight.

But that same adaptive cunning could also *impede* the pursuit of knowledge. This
happened in the case of mass communication research—and probably because his own
interests lay elsewhere. To some extent media research was a *means* to other ends: a
source for reputation and resources, for example, or a testing ground for new methods,
but in other respects an intellectual afterthought. His relative indifference was expressed,
in part, through his rather plastic treatment of the research findings. The decade-long
framing drift—the audience-dependent packaging of his media studies—revealed that, to
some extent, this research was an instrument for other goals. When stature and funding
opportunities permitted, he moved on.

*Personal Influence* was, as a result, his narrative last word—a final occasion to
make the case for his media research legacy. *Personal Influence*, unlike some of his other
published reflections, was addressed to his scholarly peers, more of whom than ever were
sociologists. The fall-and-rise small group account that Shils supplied was in many respects a perfect sense-making device: Some of *The People’s Choice* findings which had already been put to various use—the better performance of face-to-face over mediated persuasion in short-term campaigns, the hypothesis of a two-step flow—could be brought together in one coherent narrative. Not just that: The findings could also be assimilated into an exciting research field, the small group, which was, moreover, definitively sociological. Shils’s idea that the primary group had been abandoned on account of a mistaken, European-derived image of society helped, first, to emplot the narrative in terms of scientific progress. And “the *Gemeinschaft* after all” framing provided a legitimating link to the discourse of learned social theory.

The first chapter scene-setting that Katz and Lazarsfeld produced was very cleanly written, and delivered with confidence. It was, for all of its inaccuracies, an impressive feat of intellectual agility: Lazarsfeld had managed to convert the disappointments of *The People’s Choice* data into a convincing claim to original insight. The ahistorical culture of empirical sociology and survey research, and other related conditions, helped to secure the claim’s uncritical embrace. In the few years after *Personal Influence*, the “power-to-limited-effects” narrative was solidly established as the remembered history of mass communication research.

The storyline’s extraordinary staying power—it remains the bedrock account in most mass communication textbooks—was helped along by the new “discipline” of communication, busy colonizing journalism schools in the 1950s. With its own comparatively meager research traditions, the new discipline, under Wilbur Schramm’s
guidance, inherited and adapted the account as a usable past. The discipline’s field-
borrowing disconnect with its own remembered history contributed, ironically, to the
narrative’s widespread adoption: There were few remnants of contradictory memory to
stir up questions about its validity.

Lazarsfeld’s “limited effects” storyline did not, in the end, just blot out the many
strands of interwar media analysis through the ascription of naïve and unscientific
“powerful” findings. He also retroactively cast the Bureau’s media research in such a way
that most of its contributions—Lazarsfeld’s included—have long since been forgotten.
Most of the Bureau tradition’s media inquiries could not fit comfortably under the
“limited effects” banner, and very few, as a result, live on in the collective memory of
mass communication researchers. The exceptions all have particular explanations: The
1948 “Mass Communication, Popular Taste and Organized Social Action” paper by
Lazarsfeld and Merton survived because of its sheer conceptual brilliance as well the
combined prestige of its authors. The Bureau’s rich, proto-functionalist “gratifications”
studies—those by Herta Herzog, Rudolf Arnheim and Berelson especially—were
named predecessors to the “phenomenalist” functionalism called for by the Bureau’s

17. In the early 1960s, Schramm, champion of the new stand-alone discipline of “communication,” would
add Harold Lasswell, Carl Hovland, and Kurt Lewin to the roster of “founding fathers,” but in such a
way that their contributions were largely incorporated into the “limited effects” storyline.
the Study of Radio and its Role in the Communication of Ideas, ed. Paul F. Lazarsfeld (New York:
Duell Sloan and Pearce, 1940); Herzog, "What Do We Really Know About Daytime Serial
Listeners?" in Radio Research 1942-1943, eds. Paul F. Lazarsfeld and Frank N. Stanton (New York:
Duell, Sloan and Pearce, 1944); Herzog, "On Borrowed Experience," Studies in Philosophy and Social
Science 11(1) (1944); Berelson, "What Missing the Newspaper Means," in Communications Research,
1948-1949, eds. Paul F. Lazarsfeld and Frank N. Stanton (New York: Harper and Brothers, 1949); and
Charles Wright, Joseph Klapper, and, under the later “uses and gratifications” label, Elihu Katz.\(^{20}\) (The late 50s, early 60s call by Wright and Klapper was, fittingly, a direct outgrowth of the limited effects finding: Now that we know that media do not do much to people, let’s study what the people do with media.) Leo Lowenthal’s 1944 “Biographies in National Magazines” essay has lingered in the field’s memory too, certainly for its superb and still-relevant historical analysis, but also because it was incorporated into the Frankfurt-and-Bureau intellectual historical drama which, especially from the 1970s on, has gained a large interdisciplinary audience.\(^{21}\) Each of these “survivors,” for some of the reasons cited above, were included in early “readers”—which further helped to establish their mnemonic resilience.\(^{22}\)

But nearly all the rest of the Bureau’s media research has been assigned to the academy’s overflowing dustbin. Even *Mass Persuasion*, Merton’s brilliant Kate Smith war bond study, has barely limped along in semi-obscurity.\(^{23}\) The “limited effects” narrative, of course, cannot account for all of this forgetting; there are many other factors

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at work. But Lazarsfeld’s retroactive characterization of his own tradition was narrow
enough that much of it got excluded by default.  

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When Paul Lazarsfeld, with Elihu Katz’s help, set out to establish a sense-making
emplotment for mass communication research, he might have borrowed from any number
of available storylines that surfaced over the decade that *Personal Influence* gestated. Or
he might have framed the Decatur findings using any one of the several illustrative
scaffolds that he had, over the previous fifteen years, employed himself—many of which
were not cast in historical terms. But he and Katz chose to adopt the narrative packaging
that Shils had generated. Why was the storyline that Shils put forward, under his name,
the framing that Lazarsfeld and Katz selected for *Personal Influence*? Shils’s story was
selected, but why?

For a tentative answer, it is important to examine Lazarsfeld, to be sure, but also
the academic context around him. The best way to understand Lazarsfeld’s choices about
*Personal Influence* is, first, to recognize that the quest for scientific distinction—for peer
respect—was Lazarsfeld’s fundamental academic stimulus. This motivation coexisted
profitably with two genuine intellectual interests, methodology and the psychology of
decision-making—interests that were, however, well-suited to the generation of the

24. In this sense, Peter Simonson and John Durham Peters are surely right to stress the forgotten riches of
1940s media research, even inside the Bureau, though they do not stress the central importance that
Lazarsfeld’s *own* late portrayal in bringing about this forgetting. (See Simonson and Gabriel Weimann,
"Critical Research at Columbia: Lazarsfeld’s and Merton’s ‘Mass Communication, Popular Taste, and
Organized Social Action’"; and Peters and Simonson, eds., *Mass Communication and American Social
Thought: Key Texts, 1919-1968* (Lanham: Rowman & Littlefield, 2004)).

25. The Decatur field research for what would become *Personal Influence* was completed in 1945; the
book was published in 1955.
claims to *novelty* that underwrite scholarly reputation. The other crucial aspect for understanding Lazarsfeld is the *field* in which he staked his claims to credit; when he came to America in the early 1930s, he stumbled into the extremely peculiar and fast-evolving *public opinion cluster*. The public opinion research field became, for Lazarsfeld, an intellectual frame of reference, but also served as the institutional context in which he made his highly entrepreneurial career. The interwar world of public opinion research was both extra- and inter-disciplinary, *Gemeinschaft*-like in its personal networks, tethered to foundations and commercial firms and even the lay public, and centered intellectually on a set of evolving *methods*; with Lazarsfeld’s prominent help, these methods and the research shops to service them would, after World War II, somewhat improbably establish themselves at the center of empirical sociology more broadly.

From the beginning, the study of mass media was the opinion cluster’s most pronounced topical research area—to such an extent, in fact, that the various “communication”-related labels which emerged before and during the war were often paired, or used interchangeably, with the “public opinion” moniker. But this was an accident of funding and world crisis, and not the result of a conscious intellectual program or a received tradition of study. The field’s mass communication focus was a straightforward outgrowth, rather, of media- and advertiser-sponsored research, Rockefeller Foundation intervention, and the federal government’s wartime propaganda mobilization. Lazarsfeld’s own deep immersion in media research almost perfectly

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tracked these three successive, somewhat overlapping, funding windows. Like the rest of the field’s, only more markedly, Lazarsfeld’s engagement with the study of media was opportunistic. This he freely admitted on a number of occasions: The topic was attractive not for its own sake, but because funds were available—and because such studies were easily fitted to his real interests in methodology and the psychology of the decision act. One of the ironies of this disinterest is that it contributed to his ignorance and lack of curiosity about those currents of media analysis which had preceded his own. This ignorance, of course, was a fundamental pre-condition for the taut, denuded two-stage narrative that he and Katz put forward in Personal Influence. The same is true, in a slightly different sense, for the public opinion field more broadly: One of the reasons that it failed to correct the “powerful effects” construal was that it, too, was largely cut off from, and uninformed by, its media research predecessors.

It is useful here to establish a pair of analytic contrasts that, in the Shils-Lazarsfeld case at least, help to clarify the mnemonic transfer that took place. The first is the idea of a distinction between the context of formation and the context of adoption. When it comes to the making of disciplinary memory, the “formation” and “adoption” of specific storylines are often mutually enmeshed and indistinguishable. But in this case at least, the circumstances, intellectual conditions, and field-location that helped produce Shils’s narrative were quite foreign to the swirl of factors that led Lazarsfeld and Katz to go about its adoption. For all the intellectual and institutional coordinates that Shils and Lazarsfeld shared in common—and these were indeed significant—the fact remains that Shils had his own reasons and influences that in turn gave the history he wrote its
particular form. It is important, in cases like these, to recognize and separately treat the contexts that give rise to a narrative, and those that lead to its later adoption. This distinction, of course, is a relative one: From another angle, Lazarsfeld’s limited-effects storyline can be thought of as an already-formed narrative that, in turn, got adopted by still-other fields and conversations. This is indeed what occurred. The powerful-to-limited-effects narrative was frequently deployed in the late-50s public intellectual debate over “mass culture,” in the context of changing Cold War conditions. And the limited-effects story was adopted, too, by the emerging academic discipline of “communication,” as it colonized professional schools of journalism in the late 50s and 60s—complete with field-orienting survey courses and textbooks.

Lazarsfeld’s context-dependent credit-seeking helps explain not just his research program in mass communication, but also, I argue, his particular *Personal Influence* emplotment of the field. Over the fifteen years or so that Lazarsfeld conducted media research, he framed a fairly stable set of findings in a number of distinct and resourceful ways, depending especially on the main audience that any particular study was intended for. Put briefly, he took a bundle of findings that were, initially, disappointing in scientific terms, and *re-packaged* them repeatedly according to the intended audience. His core findings—that short-term, mass-mediated persuasion attempts often fail, that face-to-face efforts seem more successful, and that the two persuasion types may be connected—were most often, for example, put forward as technical advice to would-be persuaders in government, industry, and public advocacy. The findings were framed, moreover, one way when the audience was socially concerned liberal intellectuals, and another when the
audience was fellow media academics. Lazarsfeld was a masterful packager, chameleon-like in his audience adaptability. He used, in particular, book-length studies like *The People’s Choice* (1944) and *Personal Influence* to make the reputational case to his broadest academic frame of reference—the increasingly coterminous fields of public opinion research and empirical sociology. The book-length studies were used, that is, to advance carefully framed claims to originality, in both substantive and methodological terms.

*Personal Influence*, published after he had effectively left the field of media research, was in this sense a last-word reputational sealant—a chance to establish, retroactively, the novelty and relevance of his fifteen-year effort. Claims for originality are easiest to make in narrative terms, especially with before-and-after contrasts. The powerful-to-limited-effects storyline was deployed in just these terms, as a summative and retroactive claim to the novelty and coherence of his body of media research. The Shils small-group story, in this context, became an irresistible emplotment aid: It provided a ready-made narrative contrast that helped, moreover, to re-frame the failures of media persuasion as, instead, a constitutive contribution to an exciting research trend. The fact that Shils, as perhaps only he could plausibly do with confidence, set his narration in broad form—with reference to the history of the American field, the century-old European theoretical influence, the big-picture contrast of clashing images of society—rendered his story all the more attractive in novelty-establishing, reputational terms. Lazarsfeld’s zeal for distinction, the public opinion field’s specific contours, the features of Shils’s narratives—these combined to produce the most influential fifteen
pages ever written in American media research.

The storyline supplied glue to an emerging “communication” field with bricks but no mortar. Scholars oriented themselves, and their graduate students too, on its foundations. Disputes within the field were framed with appeals to its authority. The world outside communication studies—the world of deans and the established social sciences, in particular—was exposed to its plot. The story of the discipline’s past became a common idiom in a field without much else in common: Even leftist detractors in the late 1970s took the history as the main thing to detract from.27

The story has such staying power because it’s a great story: Dramatic clarity, big men, the frisson of breakthrough, vivid (and violent) metaphors like “magic bullets” and “hypodermic needles”—the elements, in short, that make for riveting narrative. It’s also proven plastic, flexible enough to bend without losing its form. And it’s benefited from a kind of self-reinforcing mnemonic inertia.

Like all narratives, the story that communication studies has told itself is partial—bashed at and edited down. And like other narratives, the field’s chronicle has, in turn, doubled back on the reality that it ostensibly describes. The history drafted by Shils, Lazarsfeld, Katz and the others, to put it more bluntly, helped shape the emerging field itself—marked off its boundaries, plotted its future. That history, after all, was drafted in a specific context: early Cold War American social science, with its cocksure

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27. Gitlin’s critique of the Lazarsfeld “dominant paradigm” is an important case in point. Katz and Lazarsfeld, wrote Gitlin, “conceptualize the audience as a tissue of interrelated individuals rather than as isolated point-targets in a mass society...As a corrective to overdrawn ‘hypodermic’ notions, as a reinstatement of society within the study of social communication, the new insistence on the complexity of the mediation process made good sense” (“Media Sociology: The Dominant Paradigm” 207).
scientism and choose-the-West assurance. Even as the Cold War cooled off—and even as faith in cross-tabulated renderings of the world flagged—the storyline had already anchored in the field’s consciousness.
CHAPTER ONE
The Young Shils and the Downcast Intellectual

Edward Shils’s contributions to the history of mass communication research weren’t the product of self-serving careerism. He didn’t set up the two-stage plot, adopted by Katz and Lazarsfeld with such enthusiasm, in order to satisfy academic norms of kill-your-father originality. He wasn’t bullied by corporate funders to supply his upbeat prognosis. He was, like most everyone else in postwar social science, swimming in the waters of Cold War liberalism, but somewhat against the current. He worked at times for the new national security state, like many others—but his cheerful take on American society, his scabrous dismissal of critics, owed little to that service’s blend of rewards and constraints. This is not to say that careerism, the Cold War consensus, corporate sponsorship, and government service were irrelevant factors in the development of a disciplinary history for communication studies. These factors were often, as I argue at length elsewhere, of profound importance. But they don’t help us understand Shils’s role very well.

Shils led an unconventional intellectual life, at deliberate remove from the mainstream of American sociology. This, it turns out, is important not just because he developed his take on the history of social thought in, so to speak, self-exile—through a unique set of personal and intellectual encounters. It’s also true that he became, in essence, an ambassador to American sociology, passing along a particular version of these alien intellectual coordinates. And for all of his self-imposed marginality, the specific intellectual terrain that he mapped for sociologists—European social thought,
past and present—made for high-status exegetical authority, especially in the context of the post-war milieu of sociological theorizing. One of the ironies of all this is that Shils’s border-dwelling uniqueness, in turn, has probably deflected attention from him and his astonishing influence—if only because he doesn’t fit in any of the well-worn grooves in the standard historiography of sociology.

The point isn’t just that Shils had, to borrow a term fashionable in post-war social psychology, a distinct frame of reference from most of his colleagues. It’s that the particular world of ideas which he represented, with all his spread-out erudition, earned him a high degree of interpretive sway. The postwar era’s American sociological consensus was built on a division of labor between theorists like Shils (and Parsons) fluent in European thought, and ahistorical, resolutely quantitative empiricists like Lazarsfeld—with the latter deferring to the former in intellectual historical matters. All of this meant that Shils, as a border-dweller, was propitiously placed to influence the rest of the field’s picture of European sociology, and even the Western intellectual tradition more broadly. He possessed, by virtue of his interstitial role, a great deal of hermeneutic license.¹

It’s worth thinking about Shils’s interpretive influence in parallel with the sociology of cross-national intellectual translation. A translated work, released into a brand-new national intellectual current, is plucked out of its original context. This means that a work can take on very different meanings in its new intellectual home, and new

meanings too for interpreters who themselves often occupy dissimilar roles to the original
exegists in the source nation.² Those who do the translating, in particular, wield
considerable power, by way of selection, word choice, and first-mover interpretive clout.
The translator’s colleagues suffer from a knowledge asymmetry; they regress, in a sense,
to lecture-attentive pupils—susceptible to the translator’s framing and coloration.³

Shils was a border-crossing cartographer in just this sense. The territory that he
helped map for his American colleagues in sociology was especially vast, and, until his
and a few others’ interventions, mostly uncharted. The maps that Shils drafted, at least
sometimes, contained the intellectual equivalent of exaggerated coastlines and partially
submerged dragons. There was nothing random about his misleading cartography, nor
was it born out of ignorance.

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Shils’s renderings of the history of social thought are bound up in his liberal politics. He
was a defender of a specific kind of liberal order, which he came to identify with the
United States and, especially, Great Britain. His deeply serious commitment to the study
of social order more broadly was formed in tandem with this emergent appreciation of the
fragile liberal achievements of Anglo-American societies. For the most part, these two

² A student of Pierre Bourdieu, Michele Lamont, has written a superb, barbed study of the cross-Atlantic
embrace of Jacques Derrida in American literature departments that treats the uprooting of context in a
sophisticated fashion ("How to Become a Dominant French Philosopher: The Case of Jacques
Derrida," American Journal of Sociology 93 (1987).) Pierre Bourdieu is characteristically astute but
vague on the topic of cross-national intellectual translation; his scattered writings seem driven in part
by pique over his own patchy American reception. (See, for example, "Passport to Duke,"
International Journal of Contemporary Sociology 33(2) (1996).)

³ For a relevant example, consider the enormous influence of Talcott Parsons on the received image of
Max Weber and Emile Durkheim as literal translator and, more importantly, as key early interpreter.
The picture of Weber and Durkheim that he put forward were both particular, hardly surprising given
the convergence-on-action-theory thesis that he fit them into in The Structure of Social Action.
projects—his defense of a particular liberal order, on the one hand, and his truth-seeking zeal to comprehend the general constituents of social solidity, on the other—were intertwined and even complementary. But there was also a tension between the two. And it is this tension that explains, in the end, his otherwise surprising abuses of history.

During his undergraduate years in the late 1920s, Shils had already become fascinated with, and repulsed by, the tendency that he observed among intellectuals to despise their own societies. Shils’s aversion to intellectual disloyalty was a constant throughout his adult life, though his specifically “Shilsian” take on the intellectual and her society would only cohere, in a sophisticated, original, and consistent way, in the late 1950s. But he had, while a very precocious college student, already come to distrust the utopian aspirations that underpinned, he thought, intellectuals’ gloomy pictures of their own societies.

It is important, for understanding Shils, to realize that his God never failed; that he was born only once; and that he chose the West from the beginning, without any of the Cold War agonizing of ex-radicals. But like so many others, Shils experienced a crisis in his own liberalism during the 1930s, which he spent as a still-precocious graduate student and instructor at the University of Chicago. The Great Depression was important to Shils’s crisis, as it was for so many others. But Shils filtered the decade’s turmoil through his own, distinct experience and intellectual prisms.

He formed his profound reverence for German academic achievement just as his beloved Weimar conversation was coming to a violent close. His friendships, in the 1930s, with traumatized German Jewish refugees amplified his despair over the rise of
National Socialism. At the same time, he came to link European intellectuals’ other-worldly aspirations with the barbarous collapse of their own societies; he held them partly responsible for the calamities in Germany, Italy, and Soviet Russia.

In his partially formed analysis of fascism and Soviet communism, he came to accept a downcast picture of modern society, especially in the lead up to World War II. In a complex, overlapping composite of his own readings and the diagnoses of Karl Mannheim and Harold Lasswell, respectively—both friends and close contacts—Shils came to subscribe to a view of modern life that stressed the dangerous attenuation of belief systems, the threat posed by unanchored masses, and a general fear of social disorder. Along with Mannheim, Shils helped articulate this view of dangerous disintegration with reference to Tönnies’s Gemeinschaft-Gesellschaft contrast—which Shils took to be implicit in the broader stream of late nineteenth and early twentieth century German sociology. The illiberal regimes, he came to believe, had exploited these isolated, meaning-starved masses by supplying them transcendent ideals and the comforts of order. The basic outline of this analysis was, of course, developed by many other scholars, especially emigrés from across the political spectrum, before, during and after the war. It should be obvious, too, that this diagnosis is substantially the same picture of modern society that Shils and Daniel Bell come to dismiss—and historicize—as the “theory of mass society” in the mid-1950s. What later gets called “mass society theory” was, in the late 1930s, Shils’s own view and the view, too, of those close to him.

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4. Shils’s wholesale conflation of the Gemeinschaft-Gesellschaft construal, as he found it in Tönnies, Simmel and even Weber, with the distinct kind of revolt-of-the-masses fear expressed by Shils’s and many others before and since, would remain a central weakness of his postwar “theory of mass society” label.
At first glance, this seems odd. But by the 1950s, when Shils was attacking so-called “mass society theorists,” he had replaced his old interwar fears with a strikingly different image of modern, and especially Anglo-American, life. The crucial years of revision were 1942 to 1945, when Shils worked in the London branch of the Office of Strategic Services, precursor to the CIA. His contacts there, with English and emigré intellectuals, helped along his new understanding, as did his intense study, in this period, of literature on early modern English religious tolerance. Above all, his wartime research on German prisoners-of-war contributed to his changed view: In trying to explain the extraordinary tenacity of the Wehrmacht in the face of certain defeat, Shils came to realize that close primary ties among small units—and not diffuse attachments to Nazism or the Fatherland—accounted for the soldiers’ will to fight. On reflection, Shils came to see the persistence of primary ties as an overlooked feature of modern life—a feature that helped to undermine the more dissensual picture he and those around him had clung to in the pre-war years.

Shils’s observations, readings, and contacts in wartime London also gave him, crucially, a new and half-formed appreciation for the distinct and particular achievements of the English liberal tradition. He came to admire England’s civil and consensus-driven politics, and their exclusion, especially, of the totalizing worldviews that on the Continent had proven so destabilizing. British intellectuals were, for the most part, averse to Promethean politics; they tended to accept, he observed, the limitations of politics, and they displayed a healthy attachment to their nation, its elites, and its traditions. Britain seemed to him a living riposte to the Schmittian gloom and cynicism about liberal
democracy so common among German intellectuals fleeing the disastrous denouement of the Weimar experiment—a cynicism that he, too, had flirted with in the late 1930s.

Shils’s appreciation for Britain’s achievement, which he saw essentially confirmed in the United States’s own English traditions, was amplified in the immediate postwar years. He was appointed reader in sociology at the London School of Economics in 1946, where he deepened his exposure, in particular, to the intellectual defense of the self-stabilizing English political order that was part of the LSE-based critique of Mannheimian planning. Friedrich Hayek and Karl Popper were especially important here. Shils’s own foray into the post-war politics of nuclear technology at the University of Chicago—where he remained on a half-time basis—supplied for Shils, in the form of a living community of responsible scientist-intellectuals, a counter-ideal to the reckless, antinomian intellectual he had so long despised. Through one of the Chicago nuclear scientists, Leo Szilard, he met Michael Polanyi, the Hungarian chemist-turned-philosopher-of-science. Shils’s sensitivity to order-maintaining liberal traditions was reinforced in his close friendship with Polanyi, in his LSE years and beyond.

In this postwar period, Shils’s remained convinced that utopia-drunk intellectuals had contributed to the disastrous collapse of Continental societies. He was especially put off by the persisting claims of intellectuals, many of them leftist emigrés traumatized by National Socialism and the Holocaust, that modern society—and by implicit and explicit extension, the U.S. and Britain—were plagued by dangerously suggestible and atomized masses who could, yet again, yield to totalitarian demagogues. This was, to a significant degree, his own fear in the years leading up to the war, but he had since come to
recognize the more complex, *Gemeinschaft*-persisting integration of all modern social orders, as well as the particular defenses built up by the liberal Anglo-American societies.

It is of vital importance that, for Shils, these critics of modern life were not just wrong. They were wrong, of course, but their misguided complaints were also dangerous. In Shils’s view, their dystopian tracts resulted from bitter, morning-after disappointment, rooted in a contrast with their impossibly utopian beliefs. The distance between intellectuals’ transcendent ideals and their actual, earthly societies, Shils had observed all the way back to his undergraduate years, helps to explain their typical disgust for their own nations. In the lead-up to 1789, 1917, and 1933, the savage critiques of perfectionist intellectuals had in each case destabilized the social order; many of the same intellectuals had gone on, blinded by ideological zeal, to help bring about human catastrophe. Even in the ashes of these disastrous experiments, many intellectuals either gave in to a homeless, roving idealism in search of new utopias, or else succumbed to an icy, disillusioned “realism.” Either way, the tendency among intellectuals has been to attack existing society and, in particular, exaggerate its faults.

In the postwar years, many American and emigré intellectuals on the left—after giving up, in light of the all-too-many Kronstadts up to and including the formation of the post-war Eastern bloc, their socialist convictions—became disappointed radicals of the “realist” stripe. Despite and because of their renunciation, they remained embittered by transcendent contrast, and prone to find their earthly societies deficient. In practice, Shils observed, their inveterate griping tended to take the form in these years of what he would come to call the “theory of mass society”—an overemphasis on the *Gesellschaft*-like
character of modern life, an exaggeration of consumer-culture soullessness, an inflated fear of the “masses”’ susceptibility to demagogic propaganda. For Shils—the one-time adherent to a version of this view—the picture put forward by these 1950s intellectuals was a profoundly inaccurate displacement of their own disappointed ideals.

The crux, for Shils, is that the fragile Anglo-American liberal order depends on faith in its civil traditions of politics—a faith that the embittered ex-leftists threaten to corrode. Even before he codified this view in the late 1950s in relation to his emergent categories of “center” and “periphery,” Shils came to regard these intellectuals’ grim critique of American society and culture as a potentially cancerous element in America’s plural politics. The risk, for Shils, was that the critics’ mistaken picture would end up a kind of tragic self-fulfilling prophecy—by poisoning the well of public sentiment that the precious Anglo-American liberal order drinks from, either directly or by producing nativist, anti-intellectual backlashes whipped up by spurned and opportunistic politicians.

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In defense of this American (and English) pluralism, Shils bludgeoned critics of society with his erudite polemics. Many of his historical writings, including those that influenced the history of mass communication research, came as part of this ongoing effort to shame dangerously naïve colleagues on the left. He used his historical erudition as a weapon in a one-man war against the reckless intellect—and in the process narrated the small-group rediscovery and, later, the catch-all pejorative “mass society theory” to place his contemporary disputants in a discredited (and elitist) intellectual lineage. The result was, very often, polemical history that most other sociologists, however, couldn’t distinguish
from good faith synthesis.

In the end, Shils’s passionate defense of a particular liberal order came into conflict with his deeply held commitment to truth. In that defense, he engaged in his own kind of recklessness with the past. He committed a series of historical conflations that remain commonplace, under a crude and ill-fitting “mass society theory” rubric: Interwar American sociology (and especially the “collective behavior” tradition around Park and Blumer), turn of the century German sociological theory, certain emigré analyses of conditions under fascism, liberal and ex-radical critics of mass culture—all get thrown in with de Maistre, Le Bon, and Sorel. Shils became so hypersensitive to the faintest whiff of social criticism, especially if it implied dissensus, that he grossly exaggerated the affinities of the offending thinkers.⁵

Shils, as we have seen, had his own distinct reasons for assigning misguided intellectuals to a discredited tradition. Quite apart from this, his portrayal happened to possess great intellectual fitness for other Cold War liberals.⁶ The idea of a “mass society theory” was an eminently useful strawman in the social scientific and public intellectual defense of American pluralism in that decade. And his contrasting imagery—a nation small-scale fellowship—proved attractive to sociologists of mass media, who incorporated the contrast into their narrative, as we have seen, early on.

By the time the Cold War had cooled off a bit, the “rediscovery of the small

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⁵. To take one isolated example, Shils faults Merton’s Sociological Ambivalence and Other Essays (1976) for its index’s dissensual-topic-to-consensual-topic ratio, which Shils estimated at 7 to 1! (“Social Science as Public Opinion,” Minerva 15 (1977)).

⁶. The term “intellectual fitness” is a modification of Robert K. Merton’s “symbolic fitness,” used, for example, in his superb monograph on Kate Smith. (See Mass Persuasion 76.)
group,” along with the “mass society” pejorative, had already hardened into axiomatic cement. These had become constantly recycled shorthand, no longer tethered even to the peculiar context of Cold War liberalism. What began as a knowing polemic—what became, thereafter, a useful, straw-built contrast—had become by the mid-1960s a taken-for-granted history. Shils, perhaps, knew better. Most of those who followed after him—certainly those authoring mass communication textbooks—did not. The “received history,” in its formative stages, got emplotted the way it did in part due to the unintended consequences of Shils’s particular history-cum-screed polemics.

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Shils oriented himself to a set of intellectual traditions, unearthed through “wide reading, often too random,” that had no place in the disciplinary socialization of American sociology. He was an intellectual self-isolate, which was reflected in his institutional choices. Though he remained affiliated with the University of Chicago for his entire career, he gravitated to the University’s Hutchins-derived anti-disciplinary experiments—first at the undergraduate College and then, after the war, at the Committee on Social Thought. Indeed, for many years Shils dropped his affiliation with the University’s sociology department altogether, and he never earned a Ph.D. Captivated by his London


8. Shils left the department (and the undergraduate College) in 1947 to join the Committee on Social Thought. He rejoined the department in 1957, while remaining on the Committee. (See Andrew D. Abbott, Department & Discipline: Chicago Sociology at One Hundred (Chicago: University of Chicago Press, 1999) 38.)
wartime experience and his five-year stint at LSE, Shils spent half the year at Cambridge from 1960 onward, away from his American colleagues. He was famously indiscriminate, by discipline and nationality, in his wide circle of friends around the world. If American sociology could, for argument’s sake, be thought of as “local,” then Shils’s own orientation was definitively “cosmopolitan.”

Shils was an outsider of a very peculiar sort—a self-exile, in his mind, to a more important “field.” He wasn’t primarily oriented to the American discipline, or even to academic sociology in general. He saw himself, instead, as part of a much broader, and much older, discourse that, regrettably, most sociologists neglect through ignorance. *His* frame of reference was, as he put it in 1982, composed of “many powerful, wonderfully studious intelligences”—the especially sensitive and serious minds that have, over the centuries, wrestled with the same sort of profound questions as Shils himself: What holds societies together? What is the proper role of the intellectual in that order?

Shils was never marginal to the field, in the way that leftists like Alfred McClung Lee and C. Wright Mills became. He never occupied a place of symbolic leadership in the discipline either, despite his mid-century flirtation with Parsonsian grand theory. He was, to borrow his own metaphor, at the center *and* on the periphery, though never clearly in either location. It was on the periphery—at the edge, that is, of the American


discipline—that he formed his particular worldview, and there too that he earned a specific kind of intellectual authority. And it was this authority that doubled back onto the field, to give Shils access, in limited ways, to the discipline’s center.

Shils surfaces all over the 20th century and its intellectual scenes. His ubiquity, when stumbled upon at random, is always startling, mostly because his interventions, large and small, never registered in sociologists’ memories. His role as translator of Karl Mannheim’s two major works is occasionally remembered, but his behind-the-scenes involvement in Mannheim’s uneven 1930s American reception is almost never acknowledged. Shils was a key participant in the pre- and post-war “Weber industry”—the competitive effort to translate and interpret Max Weber to an American audience.

Indeed, he was instrumental in establishing, along with mentors Frank Knight and Talcott Parsons, a partial and scientistic “Weber” that prevailed, for a time, in the American sociological mind. His own extensive, wartime Weber translations nearly scuttled Hans Gerth and C. Wright Mill’s 1946 *From Max Weber* volume—and when they didn’t, he was forced to narrow his own project, published in 1949 as *Max Weber on the*


12. Shils, for example, largely wrote Louis Wirth’s preface to the 1936 *Ideology and Utopia* translation, and drafted, too, a memo that Wirth relied upon in defending Mannheim from Robert M. MacIver at a crucial Sociological Research Association meeting during the 1937 ASS conference in Atlantic City. Kettler and Meja, in their masterful treatment of Mannheim, place Shils’s important involvement in richly detailed context (see *Karl Mannheim and the Crisis of Liberalism* 194-246).

Methodology of the Social Sciences.\textsuperscript{14} (Here again Shils proves a largely forgotten scene-setter: At Mannheim’s urging, he helped Gerth, Mannheim’s former student, establish himself in the U.S., and indeed shepherded (and heavily edited) Gerth’s paper on the “Nazi Party” through publication in the American Journal of Sociology—which was crucial for Gerth’s own appointment to the University of Wisconsin, where he later taught Mills.\textsuperscript{15} Shils was important, too, for the establishment of the pedagogical canon of European sociology, and its displacement (on syllabi and in memory) of the early American theorizing he knew so well.\textsuperscript{16} As a young instructor, Shils helped shape the justly famous Social Science sequence at the Hutchins-transformed College of the University of Chicago; without explicit permission, he overhauled the Social Science syllabus and helped recruit an extraordinary staff to teach it, including Daniel Bell, Nathan Glazer, Philip Rieff, Philip Selznick and David Riesman.\textsuperscript{17} Bell later recalled Shils’s preeminent stature on the College staff: “…. There was always some secret notion


\textsuperscript{15} See Oakes and Vidich, "Gerth, Mills, and Shils" 413-414. There’s no dispute that Shils extensively reworked Gerth’s atrocious English prose, but Shils later claimed, in a 1987 letter to Gerth’s wife, to have devised the thesis and drafted the paper, using only Gerth’s data and refusing, for Gerth’s reputational sake, Gerth’s offer of co-authorship. As Oakes and Vidich note, Shils’s 1938 correspondence only claims extensive editing.

\textsuperscript{16} He encouraged Jeremy Kaplan, founder of the Free Press, to publish his postwar stream of sociological classics, and assisted his friend and colleague Morris Janowitz throughout his important “Heritage of Sociology” series editorship. (For the Free Press, see Martin Bulmer, "Edward Shils as a Sociologist," \textit{Minerva} 34(1) (1996) 11; for the Heritage series, see Irving Louis Horowitz, "Max Scheler and the Heritage of Sociology," \textit{Society} (1993).

[among us] about the man who knows…. Edward Shils used to always play this role. That was Shils’ power. If he said yes, fine. But if he said swish swish, that was the end of it.”

Few remember that Shils co-founded in 1946, with Leo Szilard and others, the *Bulletin of the Atomic Scientists*, and that he argued consistently for the international control of nuclear energy in its pages and elsewhere. His high-profile partnership with Parsons is frequently recalled, but rarely is his important role in establishing sociology as a legitimate discipline in postwar Britain. His backstage work in planning and organizing the Congress for Cultural Freedom’s famous 1955 Milan conference—which he reported on in his “End of Ideology?” recap in the CCF’s *Encounter*, helping to start the eponymous debate—along with his intense involvement with the organization in the late 1950s and early 1960s, are mentioned in CCF histories, but with little context.

close friendship and intellectual cross-pollination with Michael Polanyi seems well-known, but much less has been written about the friendship and mutual influence of Shils and Karl Popper. Shils’s decisive role in bringing Leo Strauss to the University of Chicago seems largely unknown; likewise, Shils’s successful effort to secure Polanyi a post at Chicago, thwarted only by paranoid State Department intervention, is hardly ever mentioned.23 The “transatlantic polymath,” as the Times Higher Education Supplement called Shils in the mid-1970s, also travelled to Castel Gandolofo for Pope John Paul II’s annual seminars; the two men were said to hold each other in high esteem.24

I refer to all of this, in quick succession, to contrast his border-dwelling influence—from the important to the trivial—with his remarkably small place in the memory of American sociology. (His main bundle of arguments about social order are similarly marginal.) As it is, there are only traces, scattered fragments, of Shils’s life and influence registered in larger histories, supplemented by a few surprisingly insubstantial essays and memorials by a small circle of admirers. There’s no biography or book-length study of his work, though both are richly deserved.25

The challenge of fitting Shils into certain classificatory tropes of post-war American intellectual history—Cold War liberal, New York intellectual, “Standard


American Sociologist,” pluralist, modernization theorist—means that he is, by default, often relegated to the footnotes in most histories. When Shils is included in specific histories of sociology, it is normally in reference to his Parsons collaboration, with the implication that he was a consistent Parsonsian. This is, as Stephen Turner notes in his masterful treatment of Shils, a “profound misperception.” Some of the neglect is self-perpetuating, a kind of reverse Matthew Effect: Jennifer Platt, to take one example, excludes Shils from her study of the limited Weber reception in interwar American sociology because, as she explains in a footnote, of the “absence of any information about his career in the usual works of reference.”

The Parsons association, which became such a reputational liability in the late 1960s and especially early 1970s—just when Shils was gathering his essays into collections—surely explains much of the inattention. But some of Shils’s own habits and practices undoubtedly contributed as well. In part, Shils’s reliance on the essay form gave off the impression that his thinking lacked consistency and coherence. It’s relevant, too, that he was an essayist not by choice, but as a consequence of a debilitating

26. See, for example, Richard Pells’s generally impressive overview of American intellectual life in the 1940s and 50s (The Liberal Mind in a Conservative Age: American Intellectuals in the 1940s and 1950s (Middletown, CT: Wesleyan University Press, 1989)). Shils is entirely absent from the book.

27. Stephen Turner, “The Significance of Shils,” Sociological Theory 17(2) (1999) 126. Turner’s essay is by far the best published study of Shils and his ideas, and I am indebted to his sweeping interpretation. He puts, though, too much emphasis on Shils’s early reading of Hendrik de Man (an admittedly important influence), and his attempt to place Shils in a loose English wartime conversation centered on the limits of reason and total politics overplays, in the absence of clear evidence, the importance of Michael Oakeshott and T.S. Eliot, at the expense of Hayek, Popper and Polanyi. His thesis is that Shils put forward a full-fledged defense of liberal democracy comparable in scope and power to the critiques of liberal democracy advanced by Jürgen Habermas and Michel Foucault. In this recovery effort, I think that Turner is successful, but the thrust of the argument leads him to paint a too-constant portrait of Shils’s thought, and in particular to project Shils’s “mature” views back onto his earlier periods.


29. See Turner, “The Significance of Shils.”
perfectionism: He left at least four books unpublished, in addition to a half-finished manuscript that A.H. Halsey described as a “breathtakingly ambitious, Weber-like project on the ‘movements of knowledge’ down the ages and across cultures and religions.”

He was willing to issue, in essay form, what C. Wright Mills referred to as “controlled releases,” but could rarely satisfy his own high standards for completed books.

From the late 1950s on, he adopted an omniscient, footnote-free prose-style, which was elegant but often filled with his own terms of art which, out of context, must have baffled many readers. His notorious nastiness, a product in part of high standards, was not infrequently aimed at fellow sociologists. He referred, in his important 1961 history, “The Calling of Sociology,” to the “literary cumbersoness and ineptitude of many sociologists,” which, he added, “have long been the laughing stock of literate persons.”

He seemed to have recognized the costs of such incivility in a late-life letter regretting the “rigidity of my beliefs.” He tended, especially in his later years, to shun sociological journals for generalist outlets like *Encounter* or *The American Scholar* or his

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30. A.H. Halsey, "Edward Shils, Sociology and Universities," *Minerva* 37(4) (1999) 393. In the early fifties, he had composed a “full draft” of *Consensus and Liberty: The Social Psychological Foundations of Political Democracy* and a rougher draft of *Primary Groups in the Social Structure*. A year or two later, he composed “an almost wholly new manuscript,” *Love, Belief, and Civility*. Of the latter, Shils in 1975 admitted, “I was not at all satisfied with it and I am still not satisfied with it” (see "Introduction," in *Center and Periphery: Essays in Macrosociology* (Chicago: University of Chicago Press, 1975) xxviii). His first draft of his planned systematic treatment of intellectuals was drafted in 1952; when the manuscript was “last revised for the fourth or fifth time in the early 1970s,” it was around 1500 pages. In 1982, Shils acknowledged that he “still hope[s] to come back to it, to reduce it and to release it from its present seclusion” (see "Introduction," in *The Constitution of Society* (Chicago: University of Chicago Press, 1982) xxiii). The book was never published.

31. Invited to present the keynote address at a 1959 Poconos conference on mass culture, Shils’s remarks are built around his newly-minted concepts like center and periphery and the dispersion of charisma without, however, explaining them. Most of the audience surely failed to follow his argument. The essay was later published (“Mass Society and Its Culture,” in *Culture for the Millions? Mass Media in Modern Society*, ed. Norman Jacobs (Princeton, NJ: Van Nostrand, 1961)).


own *Minerva*. He had few distinguished students, and failed to cultivate a “school” or even a circle of intellectual disciples—so crucial for mnemonic longevity.34

Shils applied his principled respect for individual privacy to his own life. Lord Dacre, Master of Shils’s Cambridge college, remarked in a eulogy that Shils was a “very private person, not very easy to know.”35 Shils himself contributed to his biographical neglect by sealing his personal papers for 50 years after his death.36 His often vitriolic denunciation of Vietnam war protesters and the student New Left—and his active intervention at the University of Chicago in the late 1960s—did not endear him to the discipline’s left-drifting faculty ranks.37

Shils’s perch on the border, the wellspring of his historiographical clout, was at the same time the cause of his relative obscurity. This paradox renders the attempt to trace his formative intellectual roots especially frustrating, as it yielded a tiny and unreliable secondary literature.38 Even the best treatments tend to assume a retroactive continuity to Shils’s thought, which obscures its important evolution.39 As Shils

34. Two exceptions were his “dear students” S.N. Eisenstadt and Joseph Ben-David.
36. See Abbott, *Department & Discipline* 27. Shils’s somewhat paradoxical penchant for pre-publication secrecy seems to have produced the embarrassing overlap in the wartime Weber translations of Shils and Hans Gerth, which forced Shils to dramatically narrow his own published translations. See Oakes and Vidich, “Gerth, Mills, and Shils.”
37. He often referred to the “savage attacks” on universities by students “and then by teachers and publicists” (“Introduction," in *The Intellectuals and the Powers and Other Essays* (Chicago: University of Chicago Press, 1972) xii). Working with the Chicago president, Edward Levi, the “two Edwards” blocked many student demands that were granted elsewhere (see Altbach, "Introduction: Edward Shils and the American University" x; and Joseph Epstein, "My Friend Edward Shils," *American Scholar* 64(3) (1995) 119).
38. Erroneous references to Shils’s life and thought range from the trivial—Dacre claims he graduated from the “State University of Pennsylvania”—to the significant: Nathan Liebowitz, in his study of Daniel Bell, claims that Shils in the 1930s had been “a registered communist”! (Dacre, "Edward Shils (1910-1995)" 90; Liebowitz, *Daniel Bell and the Agony of Modern Liberalism* 21.) Regrettably, much of the better commentary on Shils is published in the elusive *Tradition & Society*, the publication of the Michael Polanyi Society. The journal has no online presence and is missing from the stacks of even the best American research libraries, including Columbia, University of Pennsylvania, and Princeton.
39. See note 27, on Turner, “The Significance of Shils.” Harold Orlans’s theory survey of his thought
sometimes stressed, his own progression hasn’t been “in a straight line”; he was at pains to emphasize the decades-long “process of the author’s self-education.” His own intellectual self-narrativizing, because of his sealed papers and the spare secondary literature, assumes a dangerous prominence in any attempt to account for his influences over time. His languid, melodious chronicles of his own intellectual comings-of-age, scattered about in introductory essays and portraits of other scholars, are especially seductive—in part because they are so genuinely useful. Aided by a prodigious memory, his reflections are unusually rich in detail, which merely contributes to their distinctly Bildungsroman character. The task of reconstruction is further complicated by his tendency, which Turner observes, to incorporate readings and thoughts into his own work many years after his own original encounter with them. My own solution has been to cross-check his remembrances against one another, and also against the evidence embedded in his early published works. The risks of retroactive clarity and whiggishness remain, but I have come to regard his memoirs as unusually reliable. The temptations of reputational management seem to have lost out, in Shils’s case, to his commitment to good-faith self-disclosure of intellectual tradition. I do not mean to imply that his narratives are enough in themselves; he neglects contextual influences and tends to

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42. Turner, “The Significance of Shils.”
downplay, in particular, the temporary, late 1930s importance of Karl Mannheim and Harold Lasswell to his own thinking then.\textsuperscript{43} Given his later aversion to the “mass society” picture, his consistent rejection of Mannheim’s and Lasswell’s technocratic elitism, and Mannheim’s tainted postwar reputation, Shils succumbed in this instance to prudential “predecessor selection,” in Charles Camic’s sense.\textsuperscript{44} A further complication arises from the relative obscurity, today, of certain books and scholars that were, for Shils in the late 1920s and 1930s, quite important—including Hendrik de Man, Georges Sorel, Robert Michels, Hippolyte Taine, Gaetano Mosca, Werner Sombart, and Vilfredo Pareto, many of them neglected for their own reputational stains.

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Without straightening out Shils’s crooked intellectual itinerary, I want to place Shils within a current of liberal thinkers that shares a broadly similar response to the dangers and limited promise of modernity. This line of chastened liberalism is not, in the conventional sense of the word, a tradition, but rather a common pattern of response to the fate of individual liberty in a violent, unstable era. I have in mind thinkers like Edmund Burke, Alexis de Tocqueville, Taine, Le Bon, and Ortega y Gasset, who are often labeled conservative for their aristocratic nostalgia and their fondness for tradition. They all stress the danger of popular passion, and the special threat posed by the abstract and utopian thought of the Enlightenment. In different ways, and with distinct language,

\textsuperscript{43} Though even in this respect, he does put forward a fuller picture of Lasswell and, in particular, Mannheim: See Shils, "Introduction," in Center and Periphery; and especially his posthumously published memoir of Mannheim (“Karl Mannheim”).

they all associate the French Revolution with a more general and ongoing threat to liberty. The tragedies of 1789 (and 1793) were fertilized by hyper-rationalist philosophes and Rousseau’s romantic chiliasm; the Encyclopédie and the Contrat social eroded the traditional substrate, dissolved elite legitimacy and unleashed Robespierre and the guillotine. Most of these thinkers admired the English contrast, with its flexible yet tradition-bound stability. All of them imagine some kind of corrective politics, either in a limited form of liberal democracy in the cases of Burke or Tocqueville, or in the anti- (or pseudo-) democratic recourse to elite-driven symbolic manipulation of the masses, as with Taine, Le Bon, and y Gasset. This latter option is, of course, paradoxical—illiberal and comfortable with the rationalism of an elite.

It’s no accident that the anti-democratic elitists flourished in the last quarter of the nineteenth-century and on into the twentieth. The emergence of a sometimes-radical industrial working class, revolutionary movements, and the constant threat of unrest coincided, in different ways, with distinct but related national crises of culture, especially in France and Germany. The gathering interest in the unconscious mind, especially as applied to crowd phenomena by the enormously influential popularizer Le Bon was, in a sense, complemented by a realist critique of the underpinnings of democratic theory, both in the Franco-Italian elite theories but also, especially after the World War, in British and then American social science. Max Weber’s coldly realist analysis of politics—

particularly his stress on the emptiness of electoral and parliamentary rhetoric—was pushed further by Carl Schmitt, whose decisionist cynicism Mannheim and, in a different way, some of the Frankfurt School scholars were to absorb after the Weimar collapse.

This was the cross-buffeted intellectual milieu that Shils—and virtually no other American intellectuals—plunged into, first in college, then more intensely in the 1930s. He spent the fifteen years from his enrollment at the University of Pennsylvania in 1927 to his wartime departure for London in 1942 grappling with the worrisome fate of liberalism. Like other chastened liberals, he fretted about mass disorder and Promethean rationality—though he couldn’t imagine the sort of corrective politics of manipulative symbolism that Le Bon, Shils’s colleague Harold Lasswell and indeed the exiled Karl Mannheim embraced. Shils, in the late 1930s, shared their worries but not their solution. It was only London and the Wehrmacht’s fellow-feeling that pulled Shils back from the pessimistic abyss. Still wary—still worried about popular passion and apoplectic reason—but on much firmer ground, the postwar Shils came to view the pessimism itself as part of the problem. Indeed, he came to view it as the disappointed doppelgänger of intellectual antinomianism.

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Shils was born in Philadelphia in 1910 to a working-class family of Russian Jews. (Robert K. Merton was born in the same neighborhood, the same year—also, as it turns out, to a working-class family of Eastern European Jews.) In Shils’s relatively non-
observant household, Yiddish was regularly spoken, but not much else is known about his early family life. Like Merton, Shils was a frequent visitor to the local public library, where he established his lifelong habit of indiscriminate reading.49

By his own account, Shils’s undergraduate years at the University of Pennsylvania were, in essence, a four-year immersion in self-guided reading. The sheer volume that he recalls—Durkheim’s entire oeuvre, for example—is only plausible in light of Shils’s lifelong and voracious bookishness.50 He majored in French literature, and focused on nineteenth century writers and, in particular, Gustave Flaubert. In the course of his literary readings—mostly French, but also American, English, German and Russian51—Shils also consumed a “vast amount of nineteenth-century political and belletrist literature.”52 It puzzled him that so many of these writers and thinkers, Flaubert above all, rejected their own societies with such venom. Intrigued and repulsed, Shils went about trying to understand these writers’ scabrous alienation: “Why did writers, historians, philosophers and other intellectuals, some great and all interesting, feel such revulsion for their own societies, for the institutions through which they were ruled and the persons who ruled them?”53 In many ways, this question became Shils’s core

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49. Ibid.
50. “At the beginning of the thirties while I was still an undergraduate I read through all of Durkheim’s works” ("Introduction," in Center and Periphery xix).
53. Ibid. In another memoir, Shils describes a more focused motivation: “I took it as my task to understand why Flaubert hated his country and his fellow countrymen as passionately as he did” (“Karl Mannheim” 223).
preoccupation throughout his career, though his explanation for intellectuals “comprehensive rejection of modern society” would move through several iterations before settling, in the late 1950s.54

It was this initial puzzle that drove Shils to the literature of Marxism and its critics. Based on his later remembrances, it is difficult to place encounters with specific authors in sequence, but he claims special importance for Hendrik de Man’s *Psychology of Socialism* (1928 [1926]), Robert Michels’s *Political Parties* (1915 [1911]), and the works of Georges Sorel, especially *Réflexions sur la Violence* (1908).55 These books, read alongside Marxist tracts and the work of French counter-revolutionary historian Hippolyte Taine, helped Shils arrive at a tentative explanation for all that intellectual discontent.56 Shils’s thesis was that intellectuals’ attraction to Marxism, as well as the broader intellectual contempt for society, had as their source a chiliastic moral impulse to heroic transcendence—expressed, in its pure form, by Sorel. (Here it is important to resist the anachronistic urge to project back onto the college-age Shils a clear understanding of the intellectual milieu from which these works arose. He may, for example, have been aware of the connections between Sorel, Gaetano Mosca, Michels and Pareto, as this link

55. Hendrik de Man, *The Psychology of Socialism* (New York: Henry Holt and Co, 1928 [1926]); Robert Michels, *Political Parties: A Sociological Study of the Oligarchical Tendencies of Modern Democracy* (New York: Hearst's International Library Co, 1915 [1911]); and Georges Sorel, *Réflexions sur la Violence* (Paris: M. Riveáere et cie, 1909). I cite the English translations for de Man and Michels because he almost certainly read these in translation; he was only beginning to struggle with German at the end of his undergraduate years. For this reason, I have excluded here Werner Sombart’s *Der proletarische Sozialismus: ‘Marxismus’* (1924), which is the never-translated 10th revised edition of *Sozialismus und soziale Bewegung* [Socialism and the Social Movement] (*Der proletarische Sozialismus: ‘Marxismus’* [Jena: 1924]). He did claim, in one memoir, to have read the Sombart book while an undergraduate, but his other accounts point to his intervening year in New York—after college and before Chicago—when he began to read German in earnest (and imperfectly) (see "Introduction," in *The Constitution of Society* xxii; and “Karl Mannheim” 221-223).
was made explicit in both Sorel and Michels. But he probably did not then know of the
debt Mosca, Sorel, Le Bon, and Pareto owed to Taine, for example, nor the crucial links
between Michels and Max Weber—whose only works in English translation were Frank
Knight’s *General Economic Theory* (1927) and, after 1930, Talcott Parsons’s *Protestant
Ethic*. 57)

Shils claimed that his reading of the first volume of Taine’s *Origines de la France
contemporaine* (1875-1893) was especially important to his emerging analysis of
intellectuals. Taine, the philosopher and amateur historian, wrote his six-volume history
of the Revolution after the humiliations of 1870-1871—the Prussian defeat and the Paris
Commune aftermath. 58 Taine, whose historiographical care was quickly and decisively
impugned, was nevertheless the leading Orleanist critic of the Third Republic and its
liberal rationalist leadership. 59 He used Tocqueville’s analysis of the *ancien regime* as his
starting point, but he went much further than Tocqueville (or Burke) in laying the blame
for the Revolution on Enlightenment ideals. 60 Like Tocqueville, he pointed to the abstract

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between Michels and Weber was highlighted in Marianne Weber’s memoir (1926), which Shils read
with relish in the early 1930s (“Introduction,” in *The Intellectuals and the Powers* xi). But the full story
of Michels’s deep debts to Weber—and Weber’s own to James Bryce!—wasn’t known, at least in the
English-language world, until fairly recently, with the publication of Lawrence Scaff’s important 1981
essay on the pair. Michels, after the first German edition, removed his acknowledgment of gratitude to
Weber after the two had a falling-out. It is fascinating to note that Weber, though he admired Michels,
took him to be the prototypical *Gesinnungsethiker*—an adherent to what he would later famously call
the “ethic of absolute ends” (Lawrence A. Scaff, "Max Weber and Robert Michels," *American Journal
of Sociology* 86(6)).

58. I am especially indebted to Alan Pitt ("The Irrationalist Liberalism of Hippolyte Taine," *The Historical

59. See, for an overview, Henri Peyre, "The Influence of Eighteenth Century Ideas on the French

Tocqueville, too, puts blame on “abstract” Enlightenment ideals, but with none of Taine’s
psychological muckraking. Tocqueville also lay greater stress on the Bourbon monarchy’s own role in
its collapse, in its absolutist dissolution of aristocratic intermediary institutions, which created the
space for activist intellectuals.
rationalism of the *philosophes* as a major progenitor to what Taine famously called the “Jacobin mind.” But Taine extended his irrationalist philosophical anthropology—his “beast in man” pessimism—from the masses to the Enlightenment standard-bearers themselves: Their humanitarian politics, their rationalist educational zeal, their law-making hubris—all of these, for Taine, were actually driven by unconscious resentment. The beast was still there, only—to use anachronistic language—*repressed* and *sublimated* into Enlightenment and Revolutionary ideals. The masked irrationalism of the Revolutionary leadership and their *philosophe* forbears was all the more dangerous for its concealment. Taine’s recourse to unconscious (and irrationalist) explanation for outwardly humane and rational thought was one direct and indirect source for many of the “psychological” analyses of socialism in the late 1890s and into the twentieth century—including Le Bon’s *Psychologie du Socialisme* (1898) [*The Psychology of Socialism*].

There is no evidence that Shils absorbed the particulars of Taine’s analysis of the Enlightenment unconscious. Shils’s end-of-college explanation for the societal hatred of literary men did not, for example, rest on a Taine-like thesis of atavistic beastliness. Indeed, Shils would come to attribute intellectuals’ crankiness and sometime Promethean political impulses to a perfectionist *moral ideal* that rendered protean politics or

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61. *The Psychology of Socialism* (New Brunswick, NJ: Transaction Books, 1982 [1898]). Taine’s ideas about the unconscious, mass irrationality, the importance of elites—and his corrective politics of symbolic manipulation paired with elite technocratic guidance—had varying influences on Sorel, Pareto, Mosca and Le Bon. For an excellent discussion, see Nye, *The Anti-Democratic Sources of Elite Theory* 10-11, 14.

62. Though in a general way, Shils seems to have absorbed the broader *philosophes*-French-Revolution thesis, anticipated by Tocqueville and Burke. He made frequent passing reference to the argument. For example, in his important 1977 essay “Social Science as Public Opinion,” he argued that disgruntled American sociologists are the “contemporary equivalents of the *philosophes* of eighteenth century France” (*Minerva* 15 (1977)).
bourgeois culture repugnant by contrast. But Shils did claim that this moral ideal was often unconscious or at least unacknowledged, and here Taine’s reliance on misrecognized intellectual motivation may have contributed.

More than anything, it was Taine’s obsessive focus on Rousseau as chief antecedent villain of the Revolution that, in combination with Shils’s own reading of Sorel and de Man, helped identify the heroic ethical fervor that, to Shils, drove radical politics and this-worldly disdain. Taine wasn’t the first, nor certainly the last, to link Rousseau’s writings to catastrophic revolutionary ideals, but his lengthy and detailed denunciation in the *Origines* probably influenced Shils’s own Rousseau reading. Shils’s reaction to Rousseau was, however, ambiguous and, in one respect, even admiring: For all his “abhorrence” with the notion of a *volunte generale*, he was intrigued by the idea of individual absorption into some transcendent order. Shils reacted in a similar, almost schizophrenic manner to Sorel and to the ethical socialism of de Man and R.H. Tawney—in various memoirs, he claimed that these thinkers helped to stir his appreciation for the necessity of moral ideals in binding societies together.63 But the ideal of complete absorption, linked as it was for Rousseau, Sorel, and indeed Marx to the promise of total individual freedom, struck him in these undergraduate years as “both repugnant to me morally and unrealizable as well.”64 Rousseau’s “noble savage” romanticism, his reverse-Hobbesian chastisement of *society*, his fiery denunciations of inequality and ancient privilege, his popular consent-derived contractualism, and above all his apoplectic


64. “Introduction,” in *Center and Periphery* xiv.
moralism\textsuperscript{65}—Shils recoiled from these, but came to understand the ethical impulse, in particular, as key to his original question.

This ethical impulse was clarified for Shils further in his encounters with the works of Michels, de Man, and especially Sorel.\textsuperscript{66} All three thinkers, in distinct arguments, relayed to Shils important critiques of orthodox Marxism and, in Michels and Sorel, of parliamentary socialism too. De Man and Sorel amplified his interest in the cohesive role of shared belief, an interest that, of course, would prove central to his later analyses of the underpinnings of social order. But the core insight that Shils gleaned from his readings was his sense for the underlying moral drives that help explain intellectual discontent.

On a number of occasions, Shils referred to Michels’s \textit{Political Parties} (1915 [1911]) as an important undergraduate encounter.\textsuperscript{67} The book is, of course, a key contribution to the Franco-Italian tradition of elite theory, with its realist assault on democratic possibilities.\textsuperscript{68} But Michels’s study is, first and foremost, an empirical debunking of Germany’s Social Democratic Party—which, in contrast to its democratic socialist ideals, Michels found machine-like, bureaucratized, and dominated by a self-serving bourgeois elite. Michels famously extrapolated from his findings the “iron law of oligarchy”—that hierarchy is an unavoidable feature of all organizations. Under the

\textsuperscript{65}. From \textit{The Social Contract}: “The State, set aflame by civil wars, is so to speak reborn from its ashes and regains the vigor of youth in leaving the arms of defeat” (Jean-Jacques Rousseau, \textit{The Essential Rousseau} (New York: New American Library, 1974) [1762] ii, 8)).


\textsuperscript{67}. See, for example, "Introduction," in \textit{The Intellectuals and the Powers} vii.

\textsuperscript{68}. See Nye, \textit{The Anti-Democratic Sources of Elite Theory} for an excellent survey.
influence of James Bryce’s *The American Commonwealth* as well as ideas on fundamental intellectual inequality and mass predilection for leadership from Le Bon, Mosca, and Pareto, Michels arrived at a sharply pessimistic prognosis for socialism and for democracy.⁶⁹ His downcast take on what he called the “problem of democracy” in 1911 had, by the 1919 second edition, given way to full alliance with the “scientific opponents of democracy.”⁷⁰ The realist critique of the democratic (and socialist) ideal, Michels’s as well as Mosca’s and Pareto’s, remained a life-long influence on Shils’s own thinking about democratic politics, but after World War II Shils’s endorsement of their picture was always qualified.⁷¹

It’s probable that Michels’s disillusioned radicalism itself—the linked contrast, that is, between his initially high ideals and his Machiavellian conclusions—helped Shils along in his effort to understand intellectual alienation. Michels’s “realism” was so bleak and despondent because, measured against his Herculean ideals, it amounted to a cold bath.⁷² Michels had joined the Social Democratic Party as a reform-minded “ethical”

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⁷⁰. *Political Parties* (New York: Free Press, 1962 [1919]); quoted in Scaff, ”Max Weber and Robert Michels” 1281. Here’s Michels in *Political Parties*: “The incompetence of the masses, which is in the last analysis always recognized by the leaders, served to provide a theoretical justification for the domination of these… From the democratic point of view this is perhaps an evil, but it is a necessary evil”; quoted in Nye, *The Origins of Crowd Psychology* 155.

⁷¹. See, for example, ”Introduction,” in *Reflections on Violence*: “There is undoubtedly much truth in these writers, but they only saw a part and claimed to speak for the whole” (19).

socialist; his differences with Party leaders led Michels to an association, starting in 1905, with Sorel and syndicalism. Sorel, in this period, was composing the Reflections, and attracted Michels to his anti-institutional politics of heroic direct action. On the encouragement of Max Weber—who dismissed Michels’s syndicalism, but was impressed by his analytic acumen—Michels published his first study of the Party in Weber’s Archiv für Sozialwissenschaft und Sozialpolitik in September 1906, deliberately timed as a provocation to his erstwhile Party colleagues gathered at their annual Congress. As early as 1907, Michels drifted from the syndicalists too, for their failure to live up to their own high ideals. The 1911 publication of Political Parties registered Michels’s steady disillusionment with the failures of political reality as well as actually existing radicalism. Weber, in correspondence, repeatedly chastised Michels for his naïve hopes and his despairing conclusions about democracy, which Weber found far too sweeping. Michels’s bitter disappointment, Weber observed, is the predictable flipside to his absurdly perfectionist ideals. He was, in Weber’s term, a Gesinnungsethiker gripped by the desire for ethical purity, and predictably chastened at the world’s corruption. Michels’s own shifting allegiances over the years—from ethical socialism, to syndicalism, to scientific elitism, to nationalism, and then to fascism—seem to bear out Weber’s observations.73

Shils implies that Michels’s book served as a model of disappointed radicalism only in the context of his undergraduate project to understand intellectual alienation.74 He

73. Scaff speculates that Michels was one of the models for Weber’s warnings against the “ethic of ultimate ends” in “Politics as a Vocation” (“Max Weber and Robert Michels” 1272).
74. Referring to his undergraduate reading of Michels and de Man, Shils writes: “These writers made me ask the question: Why did writers, historians, philosophers and other intellectuals, some great and all interesting, feel such revulsion for their own societies, for the institutions through which they were
certainly didn’t know about the Weber connection until a few years later—and then only through Marianne Weber’s memoirs. He also probably lacked knowledge of Michels’s political evolution on first reading, though the book itself openly betrays its author’s radical disillusionment. In any event, Shils made the pendulum-like connection between radical hope and bitter disappointment in his reading of Sorel. And the category of the postwar God-that-failed ex-Communist gets analyzed by Shils, in the 1950s, in just these terms—with real significance for the “mass society theory” label.

Hendrik de Man, the Belgian socialist thinker and prominent Depression-era parliamentarian and party leader, wrote his Zur Psychologie des Sozialismus [The Psychology of Socialism] in 1926 while studying in Weimar Germany. (De Man became famous for his 1930s “plan-de-Man” calling for wide-scale government intervention in the Belgian economy—and infamous for his 1940 collaboration with the Nazis, which probably accounts for his obscurity today.) Psychology of Socialism, as Stephen Turner points out, was one expression of a general crisis of Marxism in the 1920s in the aftermath of the failed post-World War Central European revolutions, Lenin’s vanguardist “revolution against Kapital,” and above all the apparent quiescence

 ruled and the persons who ruled them?” (“Introduction,” in The Intellectuals and the Powers vii).

75. On the strength of the book and other writings, he was appointed to the chair in social psychology at Frankfurt-am-Main, but returned to Belgium in 1933 when the Nazis came to power (Erik Hansen, "Depression Decade Crisis: Social Democracy and Planisme in Belgium and the Netherlands, 1929-1939," Journal of Contemporary History 16(2) (1981) 304).

of the working class. De Man’s socialism was, in the first instance, a rejection of key
tenets of Second International-style orthodox Marxism. In line with the pre-war
revisionism of Edouard Bernstein, de Man rejected armed revolution in favor of an
evolutionist and parliamentary strategy. Like Bernstein, but also in line with Rosa
Luxembourg, Georg Lukács, Antonio Gramsci and Lenin himself (by example), de Man
dismissed the economic determinism of the orthodox position, and stressed, in its stead,
the active and contingent nature of the socialist project.

The originality of de Man’s books rests on his critique of Marxism’s
psychological myopia—its failure to understand (and thereby cultivate) the traditional
moral underpinnings of working-class discontent and its corresponding blindness to the
distinct motivation of (mostly bourgeois) socialist intellectuals. Tradition, de Man
argued, did not weigh like a nightmare on the brain of the living as, in the Marxist view, a
residual form of false consciousness. Unfulfilled, often Christian ethical principles, for de
Man, are the motivation for workers’ embrace of socialism. “The motives which make
him (or her) a socialist,” de Man wrote, “are not created by the present: they are rooted in
that distant past.” Ingrained, largely Christian values of justice and decency were
already rendered critical given the obvious contrast in workers’ daily lives—indeed, the
gap between traditional ethics and workers’ experienced reality accounts, de Man

77. Turner, “The Significance of Shils” 127-29. The “revolution against Kapital” is, of course, Gramsci’s
phrase. The importance of these events for various members of a loose tradition later dubbed “Western
Marxism”—especially Georg Lukács, Antonio Gramsci, Karl Korsch and the Frankfurt School
scholars—is surveyed in Martin Jay, Marxism and Totality: The Adventures of a Concept from Lukács
to Habermas (Berkeley: University of California Press, 1984).

78. See Turner for a short but lucid overview of de Man’s book (“The Significance of Shils” 127-29); and
Peter Dodge’s intellectual biography of de Man for a more in-depth exposition (Beyond Marxism: The
Faith and Works of Hendrik de Man (The Hague: Martinus Nijhoff, 1966)).

79. de Man, Psychology of Socialism; quoted in Turner, “The Significance of Shils” 128.
insisted, for the attraction of socialism in the first place. De Man argued that socialist
strategy should center on the deliberate tapping of this deep-rooted ethical substructure—
to actively cultivate, that is, a socialist mentality on its foundations. “It is then necessary,”
he wrote, “instead of motivating socialism by class interest, to motivate class interest by socialism.”

De Man also dismissed the notoriously thin account of the intellectual in orthodox
Marxist theory. De Man, like many others, was haunted by socialist and Communist party
“embourgeoisement” and bureaucratization, in part on account of Michels’s realist
critique of the German Social Democratic Party. De Man hoped that by unmasking the
revolutionary intellectual’s distinct motives he could dampen their “chiliastic
expectations” and thereby recruit them to his evolutionist socialist vision. Leftist
intellectuals, he argued, are motivated by resentment of their “unrecognized genius”;
though they often embrace the “science” of Marxism, they are driven, ultimately, by the
Fabian ideal that in the future “the acquisitive motive of the capitalist and the worker will
be replaced by a new motive, that of service to community.” De Man insists that this
ideal is really a “desire to make all members of the community into intellectuals.”

De Man wants to expose the paradoxically bourgeois sources of this ideal in order to

80. de Man, Psychology of Socialism; quoted in Sakari Sariola, "[Book review] Beyond Marxism: The
81. See Dick Pels, "Socialism Between Fact and Value: From Tony Blair to Hendrik de Man and Back,"
complained about de Man having appropriated his main ideas without due acknowledgement” (The
82. de Man, Psychology of Socialism; quoted in Allan H. Kittell, "[Book review] Beyond Marxism: The
83. de Man, Psychology of Socialism; quoted in Turner “The Significance of Shils” 129.
84. de Man, Psychology of Socialism; quoted in Turner “The Significance of Shils” 130.
convince intellectuals to scale back their short-term ambitions and work, instead, to help maintain and amplify the tradition-bound socialist mentality of the working class.

De Man’s critique of Marxist orthodoxy was, perhaps, revealing in itself to Shils, though he nowhere indicates this. Shils does acknowledge that de Man’s sensitivity to society-wide traditional beliefs had an impact on his then-embryonic thinking about social order. But De Man’s most important contribution, especially in light of Shils’s own undergraduate project to understand intellectual antinomianism, is undoubtedly his analysis of the unconscious motives of left intellectuals. De Man suggests that intellectual chiliasm derives from a contrast between a perfectionist moral ideal and the grubbiness of everyday politics and culture. This is precisely the contrast that Shils observed, in apotheotic terms, in Georges Sorel’s “heroic sublimity.”

It was Sorel who provided Shils the key to his puzzle of the discontented intellectual—though not because Sorel’s general-strike syndicalism was especially influential. For Shils, Sorel was important, instead, as an especially clear window into the intellectual soul, unobstructed by the apathy, resignation, or bohemianism that is, in so many others, its surface manifestation. Sorel’s ethical extremism was, in a sense, the purest expression of a moral ideal that other intellectuals harbor too in less apparent ways. Sorel, Shils recalled in a memoir, “made an unpleasant but nonetheless deep impression on me.”

Along with de Man, Taine and others, Shils credits Sorel for helping to reinforce his nascent belief in the “notion that a society has a set of moral and

cognitive beliefs, adherence to which is a condition of its survival."^87 But it was Sorel’s pure chiliasm, his unambiguous orientation to ultimate values, and his all-or-nothing moral temperament that provided, for Shils, a glimpse into the intellectual psyche—and, by extension, a way to understand intellectuals’ “oppositional mentality” as derivative of a transcendent moral ideal. As Shils later argued, in 1950 Cold War terms, Sorel’s “connection with moral criteria and the expectations of an apocalyptic change is more visible than in other Socialist writers—and this is one of the reasons why Sorel’s writings are valuable for discerning the basic patterns of socialist thought.”^89

Sorel’s rejection of existing society was near-total. He was, in a sense, a Flaubert engagé who despised, especially, bourgeois decadence and corruption. His heightened sensitivity to hypocrisy and hallow idealism made him a critic, too, of Marxism and especially parliamentary socialism. Indeed, his observations about the inevitability of elite dominance famously fueled the democratic “realism” of Mosca and Pareto—only, for Sorel, the corruption of bourgeois and socialist parties could be elided, in apoplectic violence. He rejected not just Marxist economism but even its implicit utopia as unrealistic and intellectualist; he remained a Marxist only if “social poetry” counts as Marxism.

Especially in his late nineteenth century years, Sorel was a conservative moralist and always, even as a syndicalist, rejected the typical leftist call for sexual emancipation. He claimed respect for ancient tradition and especially religious sentiment, for they draw

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87. Ibid.
88. See, for example, "Personal, Primordial, Sacred, and Civil Ties” 138-39.
89. ”Introduction,” in Reflections on Violence 16.
on the fundamental human capacity to believe, the “facultie mystique.” Sorelian myths, and the revolutionary myth of the general strike in particular, are meaning-drenched calls to action, designed to appeal to that nonrational will to believe. Sorel largely accepted Le Bon’s portrait of crowd psychology, but without Le Bon’s trepidation; in a violent mob, man has reached closest to his irrational essence—closest, that is, to that “profounder region of our mental life.”[^90] The myth of the general strike was to be a rousing call to purifying violence, a striving for “total and simultaneous emancipation.”[^91] For Sorel, the ethic was in the act itself, the cleansing violence—and not in any imagined, placid end-state. The general strike, in this sense, was for Sorel an uncompromising refusal to live in the world as it is, in all its grubby corruption.[^92]

In his unceasing quest for ethical regeneration, Sorel moved through conservative moralism, Marxism, syndicalism, authoritarian traditionalism (in 1910) and then on to sympathetic Bolshevism before his 1922 death. Common to all his political guises, Shils observed in 1950, was a single principle: “the highest good is the heroic (i.e., aggressive) action performed with a sense of impersonal consecration to the ends of a restricted, delimited group bound together in fervent solidarity and impelled by a passionate confidence in its ultimate triumph in some cataclysmic encounter.”[^93]

Sorel’s impact was so profound that Shils outlined a book on Sorel while an


undergraduate, which he was still planning to write when he arrived in Chicago, in 1932. Shils’s picture of the discontented intellectual, when he left the campus and Philadelphia, was by no means fully formed; his “mature” theory of the intellectual, with reference to the “sacred” and “earthly centers,” would emerge more than twenty-five years later, and only after a second intense encounter with Sorel in the late 1940s. But his youthful reading of Sorel—and also, in different ways, of Michels and de Man—provided an answer to that original enigma: “why Flaubert hated his country and his fellow countrymen as passionately as he did.” In a 1982 memoir, he recalled that his undergraduate reading of Sorel, de Man and others helped him gain a “better understanding” of intellectuals’ “comprehensive rejection of modern society,” on the grounds, he wrote, of its “materialistic disregard for the realm of transcendent things.” It was Sorel’s chiliastic politics of heroic violence which, in its purist clarity, helped disclose the transcendent moral impulse that, to varying degrees, leads intellectuals to judge their societies harshly. When, after World War II, the moral ideal seemed spent even within socialist movements, Shils observed its traces in the complaints of the ex-radical apostates—“disappointed and broken-down Trotskyites and Edelmarxisten, most of Frankfurt provenance.” They became, in turn, the prime targets of Shils’s “mass society” pejorative.

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95. “Karl Mannheim” 223.


CHAPTER TWO
Shils and the Interwar Crisis of Liberalism

Shils spent the first half of the 1930s acquiring detailed knowledge of twentieth-century German social thought. He spent the second half of that decade fretting alongside German refugees about the fate of modern society. Both encounters helped shape his aggrieved response, after the war, to the criticism of American culture by fellow intellectuals.

By the time Shils graduated from the University of Pennsylvania in 1931, he had already developed a deep reverence for German scholarship. He later claimed that his interest went back all the way to his childhood: He remembered visiting the University’s museum of Middle Eastern antiquities when he was about ten, and noticed then that many of the exhibit descriptions indicated that the originals were in Berlin.1 By the late 1920s, as an undergraduate, he began to teach himself the language by struggling, “word for word,” through Weber’s posthumous Wirtschaftsgeschichte [General Economic History] (1923) and the first volume of Werner Sombart’s Der moderne Kapitalismus (1916) [Modern Capitalism].2 He recalls spending his evenings “looking up most words in the Muret Saunders dictionary,” without much comprehension: “I would vaguely decipher a whole sentence sometimes.”3 He first encountered Karl Mannheim’s name while

3. “Karl Mannheim” 221.
browsing through Weber’s *Archiv für Sozialwissenschaft und Sozialpolitik*—“the journal of my intellectual promised land.”

After graduating from Penn in the midst of the Depression, Shils spent a year in New York—he later called it an “enthusiastic *weltschmerz*”—as a social worker in training. He apparently devoted most of his time to reading: “That year was spent in delighted discovery of the scintillating insights of Georg Simmel, helpless pondering on Dilthey’s *Einleitung* (Introduction) and, in the salt-mine-like labor in reading word for word Weber’s *Das antike Judentum* (Ancient Judaism), which caused me to have to look up almost every word.” His German gradually improved over the course of the year, so much so that he reports reading Rilke’s *Notebooks* with “some reasonable understanding.” By the time he left New York, he had acquired the “notion that German professors were the true professors, the very idea of professors…” The Humboldtian university would remain, throughout his life, his academic ideal.

Shils left for Chicago in the fall of the following year, 1932, with the hope of studying at the University of Chicago, and soon found work as a social worker in what was then called the “Black Belt.”

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4. Ibid.
6. “Karl Mannheim” 222.
7. Ibid. He probably read Sombart’s *Der proletarische Sozialismus: 'Marxismus' during his *weltschmerz.*” See note 55, Chapter One.
8. “Some Academics, Mainly in Chicago” 179.
10. Shils: “I had come to Chicago with the generous backing of an enthusiastic friend, himself nearly impecunious, and was soon supporting myself as a social worker in the Black Belt, working for the Cook County Bureau of Public Welfare” (“Some Academics, Mainly in Chicago” 179, 186).
at the University, had “enticed” Shils by his account of the economist Frank Knight’s “somber and unremitting search for truth.” Shils was also drawn west by the sociologist Robert E. Park, whom he had read as an undergraduate. With Park traveling in Asia at the time, Shils arranged to meet Louis Wirth, the young sociologist and former Park student, on his first day in Chicago. Shils had been impressed by Wirth’s *The Ghetto*, which he had read—of course—at Penn. Though Shils would later come to think of him as a relative lightweight, Wirth’s recent trip to Germany was proof enough of his intellectual stature. “Meeting with Wirth was, for me, to be in contact with Germany. To be in contact with German universities was to be in contact with the great tradition of learning.” Just a few months later, in early 1933, Shils started to attend Wirth’s course on the history of German sociology. It was, he later recalled, one of the “oddest” courses he ever took, as Wirth spent most class periods reading excerpts from Franz Oppenheimer’s and Gottfried Saloman’s *Soziologische Lesestucke*—in German, to a monolingual class. Shils was not impressed.

Wirth recognized his student’s grasp of German and invited Shils a few months later to serve as his research assistant. Wirth billed the assistantship as part of a funded

11. “Karl Mannheim” 222.
14. Shils: “He told me that he had recently returned from Germany—as far as I was concerned, no experience could have been more desirable. The Nazis were not yet in power and I occasionally allowed myself the luxury of a daydream of studying in Germany” (Ibid).
15. Ibid.
16. Shils notes that it might have been a course in the history of sociology more broadly (Ibid 187).
17. Shils: “Yet the students adored Wirth and not least for his reading to them in German, even though they could not understand what he was reading. They thought him a true scholar” (Ibid).
18. Oddly enough, the offer came at a “large demonstrative meeting” of social workers at the Goodman
study on the “methodological presuppositions of German sociology,” and Shils accepted the post with enthusiasm, despite a sharp pay cut: “I felt myself elevated into the intellectual empyreum.”

Wirth’s project, as it happened, was “not well thought out,” but this merely freed Shils to spend a year in the stacks, acquiring an intimate knowledge of recent German social thought. Shils “repeatedly applied” to Wirth for guidance, which was however never forthcoming. “When I did find him,” Shils remembered, “he would tell me that he was interested in the ‘presuppositions’ of social science.” With such a vague mandate, Shils converted the project, by default, into a year-long self-guided education. He began reading Weber “in earnest,” including *Gesammelte Aufsätze zur Wissenschaftslehre* (1908) [Collected Essays on Methodology] “until their pips squeaked.”

It was during this period, too, that he first read Weber’s “Wissenschaft als Beruf” (1918) [“Science as a Vocation”], which, as he noted frequently, made “a great impression on me.” As for so many others, Weber’s Heidelberg address would become one of Shils’s guiding texts, then and for the rest of his life. Weber’s warnings against the “miserable monstrosities” of the “academic prophecy”—alongside his wary treatment of the “ethic of ultimate ends” in “Politics as a Vocation”—would provide, for Shils, a standpoint to critique the political Prometheanism of the intellectual left. Indeed, as he

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19. “Some Academics, Mainly in Chicago” 187; and “Karl Mannheim” 224.
22. Ibid 225.
23. "Introduction," in *The Intellectuals and the Powers and Other Essays* vii. Shils: “It helped to define a standpoint from which the fellow-traveling and the communist intellectuals of the 1930s were to be
later remarked, the essay helped make sense of his own undergraduate thoughts on the antinomian intellect.24 Weber’s fatalistic, daemon-seizing justification for science as an end in itself would also inform Shils’s own more-or-less consistent defense of academic autonomy oriented around seeking basic truth rather than utilitarian ends. Indeed, Shils’s first full-fledged journal article, published in 1938, was an impassioned, richly empirical survey of violations of academic freedom, based on case details gathered by the American Association of University Professors and the ACLU, with unmistakably Weberian undertones.25 Long before “Science as a Vocation” became available in English translation, Shils included his private translation of the essay as part of his revamped syllabus for the College of the University of Chicago’s “Social Science 2” course.26

In this same Wirth-sponsored year of German immersion, Shils read Rickert, Spranger, Erich Becher, Gustav Schmoller, Tönnies, more Sombart, Carl Menger, Ludwig von Mises, Otto Bauer, Max Adler, and struggled through Dilthey—whose works, “despite my fantasies about what they contained, I found fairly impenetrable.”27


25. "Limitations on the Freedom of Research and Teaching the Social Sciences," Annals of the American Academy of Political and Social Sciences 200 (1938). For details on the research, see Shils, "The University: A Backward Glance," in The Order of Learning: Essays on the Contemporary University, eds. Shils and Philip G. Altbach (New Brunswick, NJ: Transaction Publishers, 1997) 334. Shils ends his 1938 essay with: “The intellectual integrity of the social sciences can, however, be maintained only to the degree that every responsible scientist himself succeeds in distinguishing his judgment of a colleague’s scientific competence from his agreement or disagreement with his political, economic, and moral views… [which is] the foundation stone for the protection of the social sciences from whatever limitations outside groups—administrative, government, and private—might seek to impose on their freedom” (164).


27. “Karl Mannheim” 225.
He even read, as he put it in 1995, “the horrible Horkheimer.” Shils’s research for Wirth had been meant to supply material for Wirth to write a book, and Shils even wrote “several long articles” on related topics, a few of which got published under Wirth’s name. With some bitterness, Shils later claimed that these articles “enhanced [Wirth’s] already high reputation for being a person who knew a lot about many diverse things.”

Though Wirth’s project floundered, Shils had managed to acquire an “intimate knowledge” of late nineteenth and early twentieth century German social thought. And Wirth’s original promise to study the “methodological presuppositions” of German sociology had, by 1934, morphed into a translation project: Karl Mannheim asked Wirth to translate Ideologie und Utopie—which became Shils’s translation and the beginning of his serious engagement with Mannheim and his work.

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Of his early Chicago influences, Shils has consistently pointed to Park and Knight. In his final years, he increasingly singled out Park as the profound influence, at least among living contacts. (Shils ended his reverent 1991 memoir of Park with, “I never met anyone else who impressed me as lastingly as he did. He was a marvelous man.”) Shils had already, before Park’s return from Asia, taken a course with Park’s friend and

28. Ibid.
29. Ibid 226.
30. Shils: “This had become a substitute for the ‘methodological presuppositions…’ which soon ran into the ground because neither Wirth nor I knew what we were looking for” (“Some Academics, Mainly in Chicago” 190).
31. Shils: “… the two best men at Chicago at that time…” (Ibid 179). Shils will sometimes include Lasswell (see, for example, "Introduction," in The Constitution of Society, ed. Shils (Chicago: University of Chicago Press, 1982) xxx), but then fails to admit any substantive influence.
32. "Robert E. Park” 396.
collaborator Ernest Burgess, who Shils remembers with condescending praise.\textsuperscript{33} The course was centered on Park and Burgess’s famous 1921 textbook, known as “the Green Bible,”\textsuperscript{34} which indirectly exposed Shils to the full range of Park’s unruly mind. The exposure became direct soon after, when Shils enrolled in Park’s last course at Chicago, on “Collective Behavior” in the fall of 1934.\textsuperscript{35} (The next year, Park departed for Fisk University, where he remained until his 1944 death.) Shils, as each of his later, lovingly etched portraits of Park makes clear, was entranced by his spasmodic eruptions of insight—his “unquenchable wonderment and curiosity,” manifested in the classroom with Park “growling” like a bear “as he paced up and back across the front of the room.”\textsuperscript{36} After Shils delivered a presentation on socialist and communist movements for the class, Park singled him out “for special attention.”\textsuperscript{37} Shils took to walking with Park to his apartment after class: “he spoke to me all the way home and stood there sometimes for a few minutes before going in.”\textsuperscript{38}

In later years, Shils would isolate two of Park’s contributions for praise: His rich,

\textsuperscript{33} Burgess was a “gentle little man, with a soft, shy smile. He spoke in a whisper” about his and Park’s “Green Bible”, “expounding it in a way which showed that his heart was elsewhere” (“Some Academics, Mainly in Chicago” 184-5). See also Shils’s memoir of Burgess, which paints the same portrait (“Ernest Burgess,” in Remembering the University of Chicago: Teachers, Scientists, and Scholars, ed. Shils (Chicago: University of Chicago Press, 1991)).

\textsuperscript{34} Robert E. Park and Ernest W. Burgess, Introduction to the Science of Sociology (Chicago: The University of Chicago Press, 1921).


\textsuperscript{36} “Some Academics, Mainly in Chicago” 190. Shils relates how Park, in his last months at Chicago, would drop in “several times a day” to Louis Wirth’s office, where Shils and Wirth were working to revise Shils’s translation of Ideologie und Utopie. He was, Shils remembers, “usually very excited about some new subject” (190).

\textsuperscript{37} Ibid 188. Shils: “That is the way he was. If a student’s subject interested him, he did not limit his interest to the classroom” (188).

\textsuperscript{38} Ibid.
street-level empiricism, and also his “inchoate” thoughts on moral order. Park had, Shils observed, a “passionate sensitivity to the humble facts of ordinary life.”

39 Park’s street-wandering curiosity was channeled into the department’s extraordinary set of student monographs, many of them published classics, which Shils credited to Park’s active supervision and inspiration. So central was Park’s investigate élan—his “vivid, energetic curiosity about the rich and mysterious texture of metropolitan life”—that his departure for Fisk in 1935 marked the end of Chicago’s sociological dominance. 41 At the height of his postwar Parsonsian enthusiasm for systematic theorizing, Shils’s esteem for Park’s sensitive empiricism was bracketed, however, by stern criticism of his teacher’s failure to reflect theoretically on all the rich documentary data. 42

The other debt to Park that Shils later acknowledged was a half-formed understanding of the complex social bonds that underlie modern order: “In his idea of moral order, inchoately expounded by him and inchoately apprehended by me, he went more deeply than any other sociologist of his time or since into the fundamental substratum of the bonds which form individuals into societies.” 43 Shils, in this 1981 reflection (repeated verbatim in a 1991 memoir), attributes to Park his early interest in the problem of order and even certain substantive elements of Shils’s eventual theory of

39. Ibid 189. “He loved to walk the streets to watch ordinary human beings, to strike up conversations with them, to ask them questions, and to ponder on their lives” (189).
41. “Robert E. Park” 394. This is a widely held view.
42. See The Present State of American Sociology 10-11; and Shils, "Social Science and Social Policy," Philosophy of Science 16(3) (1949) 221.
43. “Some Academics, Mainly in Chicago” 188.
loose consensus—which, of course, Shils regarded (as he stated many times) as his singular intellectual preoccupation. There is a fascinating irony here, in that Shils, in his mid-century historical surveys of social thought—including his 1951 essay on primary groups that proved so important to the narration of mass communication research trends—presents a European-smitten Park beholden to that picture of *Gemeinschaft* dissolution which he, Shils, thinks is so wrong-headed, so inattentive to the endurance of certain *Gemeinschaft* elements. Indeed, Shils’s important 1957 essay “Personal, Primordial, Sacred and Civil Ties,” the first summary of his post-Parsonsian scheme of order as well as his first intellectual self-narrative, argues that Park and other Chicago sociologists clung to a mistaken image of anomic individualism and moral dissensus. Here and elsewhere, Shils invokes Park’s interest in “collective behavior”—and the derivative studies of Wirth and especially Herbert Blumer—as a fixation on crowd or mob irrationalism, rooted in the French social psychology of the crowd. This portrayal of the “collective behavior” tradition gets grouped later, by Shils and others, under the “mass society” label.

44. Even earlier, in a 1975 introduction, Shils stressed this side of Park’s contribution: “... I got a clearer understanding that very little could be accounted for in society unless beliefs were considered” ("Introduction," in *Center and Periphery: Essays in Macrosociology* xix). In this and another memoir, Shils claims that Park’s “human ecology” studies—especially as they applied to metropolitan dominance—later played a role in the formation of his “center” and “periphery” concepts (Ibid xxxi; and "Center and Periphery: An Idea and Its Career, 1935-1987" 280-1). In both memoirs, Shils points to the influence of W.I. Thomas too, whose studies of the Polish peasant, in terms of old- and new-country attachments, disorganization and reorganization, helped along his understanding of social order (Ibid 281).

45. Shils in his 1948 survey: “For Park, as for so many other modern sociologists, the loosening and disruption of communal bonds and the increase in personal freedom were the main facts of modern society” (*The Present State of American Sociology* 10). See also Shils, "The Study of the Primary Group," in *The Policy Sciences: Recent Developments in Scope and Method*, eds. Daniel Lerner and Harold D. Lasswell (Stanford, CA: Stanford University Press, 1951) 46; and Shils, "Personal, Primordial, Sacred, and Civil Ties," *British Journal of Sociology* 8(2) (1957) 133.

46. "Personal, Primordial, Sacred, and Civil Ties" 133.

47. Herbert Blumer, "Collective Behavior," in *An Outline of the Principles of Sociology*, eds. Robert E. Park and Edward B. Reuter (New York: Barnes & Noble, 1939); and, less often, Louis Wirth,
behavior” is, however, absurdly partial, as both contrasted the ephemeral crowd (or “mass”) with the otherwise stable and healthy “public.”48 This is abundantly clear, for example, in Park’s 1904 dissertation, *Masse und Publikum*—which was, ironically, many years later published in English at Shils’s urging.49

Though Shils’s esteem for Park as a “seeking” scholar is undeniable, it’s not clear, in the end, what Shils retained from Park’s “vast, bewildering intellectual universe.”50 He openly rejected Park’s thinking on the social application of ecological competition, with its biological residue, as well as Park’s shifting commitments to positivism. And Shils seems to have passed over Park’s own, admittedly scattered reflections on the mass media provision of a kind of over-the-wire *Gemeinschaft*.51 Shils’s paper on socialist and communist movements for the “Collective Behavior” class, at least, was an important development in his analysis of left politics. The paper, drawing on Sombart, Arthur Rosenberg, and a close reading of the *Daily Worker*, contrasted the total and electoral platforms of the Communist and Social Democratic parties; it was, he remembers, his first attempt to formulate his distinct conception of ideology. Park was impressed enough to invite Shils to assume an assistantship under him for a planned book

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50. “Some Academics, Mainly in Chicago” 179.
on the communist movement; Park’s decision to leave for Fisk, of course, scuttled these plans. The seminar paper also seems to have led to Shils’s first publications, three 1936 reviews of various Marxist books in the American Sociological Review and the American Journal of Sociology. His pugnacious prose-style already on display, Shils’s reviews are nevertheless fair and open-minded readings of a doctrine that he clearly regards as flawed but rich with minable insight.

Shils credits Frank Knight, the maverick economist, Weber enthusiast, and classical liberal, as his other major interwar influence. Soon after he arrived in Chicago in 1932, Shils noticed that Knight was to lecture, under the title “An Ex-Liberal Looks at Communism,” to the Communist Club on campus. Shils remembers an audience dumbfounded by Knight’s unapologetic denunciation of communism, alongside his principled defense of classical liberalism. Knight’s pessimism, his famously relentless skepticism, had, for the time being at least, undermined his faith in the tenability of liberal democracy. In an analysis strikingly similar to Karl Mannheim’s post-emigration diagnosis—though without Mannheim’s corresponding faith in elite “planning for

54. Shils; “The book may be summarized as a meaningless and unintegrated collection of catch-phrases and slogans, which will enlighten no one and which can be useful only as propaganda to give anxious converts the sense of security which comes with the mastery of certain vital phrases” (“[Book review] The Meaning of Marx: A Symposium [and] Dialectical Materialism” 543).
55. He criticizes one book, for example, as “quite unsuccessful in giving any idea of the rich store of constructive suggestions for sociological thinking and research to be found in the original classics of Marxism” (“[Book review] An Introduction to Dialectical Materialism” 493). It should be noted that his call to mine “Marxist classics” for insight comes well before the widespread sociological popularization of Marx.
56. Shils: “He looked like a very intelligent little rodent, rather adorable to look at but well capable of giving one a nip which would not soon be forgotten” (“Some Academics, Mainly in Chicago” 180).
freedom”—Knight bemoaned the irrationality and self-deception of the common man, that leads him, Knight argued then, into the arms of demagogues. “He thought,” Shils remembers, “that the human race just was not up to living in a free society.” This form of exhausted liberalism, reinforced later by Mannheim, Lasswell, and the broader current of interwar loss of faith in popular reason would remain a cardinal tenet of Shils’s worldview—though like Knight he always rejected the common palliative of elite planning. Shils took, by his own recollection, nearly all of the courses that Knight offered over the next few years. He did not absorb much substantive economics, nor did he adopt Knight’s dispositional skepticism nor his arch-Enlightenment disdain for religion and tradition. But Shils does remark that Knight’s “ruminations about the tenacity of beliefs,” however much Knight himself wanted them banished, helped Shils come to terms with the importance of shared meaning for social order.

As with Park, Shils was clearly impressed by the “querulous and sagacious” Knight’s dogged truth-seeking integrity. And he also claims that “many” conversations with Knight, as well as some of Knight’s writings, intensified his sense of the significance of the “rules of the game” in politics as well as social intercourse more generally. From

57. Ibid 179-81.
58. Knight himself consistently rejected technocratic intervention, and his liberalism was more fundamentally chastened as a result. Shils, much later, credits Knight with some influence in this regard: In a visit to Shils’s graduate student apartment, Knight, Shils and other students were discussing liberal setbacks. Shils remembers exclaiming about the necessity to fight back (nonviolently), to which Knight replied: “But that is just what is wrong; it can’t be done by fighting!” Shils: “I can’t even now convey the implications of this statement as I received it, but it moved my mind a little further toward the idea of the moral consensus around which Robert Park, too, had been circling” (Ibid 182).
59. Knight, Shils claims, was a “a thoroughgoing antinomian who no longer expected the coming of the Millennium” (Ibid).
61. Ibid xxix.
Knight Shils took the idea that the limits set by these unarticulated “rules” make compromise possible in plural societies, an insight that he would develop as part of his distinctive contribution to postwar theories of pluralism, most notably in *The Torment of Secrecy* (1956).\(^6^2\) Shils recalls one of Knight’s sayings—“No society would tolerate indefinitely the twisting of the tails of its sacred cows”—as making a “profound” impression on his thinking about extremist politics in pluralist societies.\(^6^3\)

But it was Knight’s role as a major liaison to Max Weber that made, in all likelihood, the more profound influence.\(^6^4\) Though already familiar with some of Weber’s work, Shils’s real engagement with Weber’s substantive research came through Knight, who often referred to Weber in lecture and who had, in 1927, already translated Weber’s *Wirtschaftsgeschichte* (1923) [*General Economic History*].\(^6^5\) The breakthrough for Shils came when Knight organized, in 1935 or 1936, a Weber seminar devoted to a line-by-line reading of the first three chapters of *Wirtschaft und Gesellschaft* (1922) [*Economy and Society*].\(^6^6\) The original seminar members were Milton Friedman, George Stigler, Allen Wallis, Herbert Goldhamer and Michael Sapir (Edward’s son), though only Goldhamer and Shils stuck it out. Shils recalls being “overpowered” by his daily reading. It was, he

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64. Shils: Knight could “be querulous and as persistent in a discussion as a bulldog; he was sharp and subtle and had an exceptional acumen in detecting contradictions in others’ and in his own positions. While he always complained about the errors of others, he never ceased to confess his own ignorance. He had an exasperated certainty when he referred to what was unlikely to be true, but when it came to the truth of his own assertions he was modest to the point of humility” (“Some Academics, Mainly in Chicago” 179).
wrote, “literally breathtaking. Sometimes, in the midst of reading him, I had to stand up and walk around for a minute or two until my exhilaration died down.” Shils was especially impressed by Weber’s analysis of legitimate authority in the first section of *Wirtschaft und Gesellschaft*, with its typology of charismatic, traditional, and rational-legal forms of authority. These “earth- and epoch-embracing categories” provided, for a number of years, Shils’s understanding of “how the idea of a whole society might be construed”—though his later, “mature” picture of social order depended on substantial tweaking of Weber’s typology.  

Weber, in the end, was the most important influence on Shils’s body of work as a whole, though Shils’s incorporation of Weberian insight was staggered over time and always partial. After the seminar, Goldhamer and Shils, who shared an office before Goldhamer left for Stanford, collaborated on a paper, “Types of Power and Status” (1939), that is a sterile and faithful rendering of the Weberian typology of legitimate authority, applied to status differences. Around this time, Shils claims to have deployed, in an original way, the concept of “charisma” to an analysis of the Nazi party, “before this became the fashion.” (Since he never published any such analysis, he is presumably referring to his (disputed) efforts on behalf of Hans Gerth’s “Nazi Party” paper.)

67. Ibid.

68. “Karl Mannheim” 223; and "Introduction," in Center and Periphery: Essays in Macrosociology xvi.

69. Shils insists on Weber’s central place in his thinking repeatedly. See, for example, "Introduction," in *The Constitution of Society*, where he writes that Weber “should be mentioned for more than any other” thinker (of those who predate Shils) (xxx). He also displayed a bust of Weber in the living room of his Chicago apartment (see Martin Bulmer, "Edward Shils as a Sociologist," *Minerva* 34(1) (1996) 17).


71. "Personal, Primordial, Sacred, and Civil Ties“ 134.

72. See note 15, Chapter One.
Shils’s picture of Max Weber was soon to get filtered through the unique interpretive prism of Talcott Parsons. At some point in 1936, Wirth introduced Shils to a visiting Parsons, who “gave the impression of pacific concentration of mind.” Parsons returned in the summer of 1937 with the “huge” typescript of The Structure of Social Action in hand. Shils attended Parsons’s lectures—and though he later affected nonchalance at this early Parsons encounter, he was unmistakably converted to Parsons’s convergence thesis, his picture of Weber (and Durkheim), and the voluntaristic theory of action itself. He also came to adopt, at least in the years immediately after World War II, Parsons’s high-flying philosophy of social science—his “neo”-less, full-throttled Kantian “analytic realism,” with its deductive zeal for stand-alone theoretic systematicity. In his postwar Parsonsian years, Shils sometimes, like

73. Shils: “The first thing that struck me about Parsons was the look of refinement on his face, which was not common among sociologists. He looked well-bred, and gave the impression of pacific concentration of mind,... looked a little like a genteel easterner, although, like many sociologists, he too came form the Middle West, having been born in Ohio” (“Some Academics, Mainly in Chicago” 191).

74. Shils: Parsons “spoke very slowly in a low, dry monotone, as from a vision. He spoke with some pride and with confident modesty of his intellectual accomplishment in demonstrating, with great meticulousness and rigor, the structure of traditions behind Durkheim, Weber, Pareto, and Alfred Marshall... Until The Structure of Social Action was completed, Talcott Parsons knew exactly where he was going. When it was over he did not stop, but his direction became less clear...” (“Some Academics, Mainly in Chicago” 191). Shils goes on to praise, backhandedly, his late 30s/early 40s applied work on totalitarian movements, stratification, and kinship, which were “excellent essays and sociological in the best sense, but the extent to which they were guided by, or subsumed under, explicit and abstract theoretical ideas was rather slight.”

75. In his 1948 survey of American sociology, Shils chides sociologists for their highly partial knowledge of Durkheim, then suggests Parsons as a corrective! “We may expect that Parson’s [sic] reinterpretation of Durkheim with his necessary and appropriate emphasis on the components of social order will remind American sociologists of what they never should have allowed themselves to forget” (The Present State of American Sociology 43). On Parsons’s highly partial Durkheim, see Charles Camic, "Structure After 50 Years: The Anatomy of a Charter," American Journal of Sociology 95(1) (1989) 60-63; for a detailed analysis of Durkheim’s reception, including Parsons’s role, see Jennifer Platt, "The United States Reception of Durkheim's The Rules of Sociological Method," Sociological Perspectives 38 (1995).

76. See Richard Munch, "Talcott Parsons and the Theory of Action I: The Structure of the Kantian Core," American Journal of Sociology 86(4) (1981), for an excellent treatment of Parsons’s thoroughly Kantian framework. For the roots of Parsons’s “analytical realism” in deductive neoclassical economic theory—his original discipline, and the one that he sought to distinguish sociology from most—see Charles Camic, "An Historical Prologue," American Sociological Review 55(3) (1990). SSA itself is explicit about its analytic realism, and Shils also heard a 1937 keynote lecture of Parsons’s, entitled
Parsons, ascribed this methodological outlook to Weber too, but more often he faulted his erstwhile model for the earth-bound specificity of ideal typical analysis.77

It was probably Parsons who exposed Shils to Pareto’s systematic sociology; he reports reading Trattato di sociologia generale (1916) [The Mind and Society] around this time.78 And it may have been Parsons who familiarized Shils with the Hawthorne experiments of Elton Mayo, T.N. Whitehead and others, which became so important to Shils’s 1951 account of the fall and rise of small group research.79 When Parsons left Chicago after the 1937 summer term, a chance encounter in the late 1930s led to their renewed contact, and probably resulted in Parsons’s postwar invitation to collaborate.

Ernest Burgess, then editor of the American Journal of Sociology, gave Shils a paper on stratification that Parsons had submitted to the journal, in part because Shils was then engaged in research on stratification in black communities as part of Gunnar Myrdal’s monumental An American Dilemma (1944).80 Shils, as he recalled later, wrote “twenty

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78. "Introduction," in Center and Periphery: Essays in Macrosociology xix. For an excellent treatment of what Charles Camic called the “local Pareto vogue” at Harvard in the 1930s, see Barbara S. Heyl, "The Harvard 'Pareto Circle'," Journal of the History of the Behavioral Sciences 4 (1968). Heyl provides evidence that Pareto was useful, for at least some of the circle, as an intellectual alternative to, and debunking of, Marxism.

79. "The Study of the Primary Group."

80. Shils’s paper for the research project, “The Bases of Social Stratification in Negro Society,” was presumably incorporated into the volume’s “Part VIII: Social Stratification.” On Shils’s contribution, see Gunnar Myrdal, An American Dilemma: The Negro Problem and Modern Democracy (New York: Harper & Row, 1962 [1944]) liii, lvi. It is presumably this research (and perhaps also the 1939 “Types of Power and Status” article he wrote with Herbert Goldhamer) that Shils refers to in a 1981 memoir,
foolscap pages of sharply critical comments and suggestions for revision in content, style and organization.”

Not long after, Shils happened to be traveling with Parsons on a train from Detroit to Chicago after a meeting, and they sat for coffee in the dining car. Parsons, to Shils’s surprise and consternation, thanked him “warmly” for Shils’s proposed revisions to his paper, all of which, he added, had been incorporated into the published paper. Shils “pretended that [he] was being mistakenly thanked for someone else’s labors,” but Parsons then revealed that Burgess had sent Shils’s foolscap comments—and that he recognized Shils’s handwriting. Parsons’s paper, published in 1940, stresses, in theoretical terms, the importance of stratification for social order, and also makes the distinction between ascribed and achieved status that would be incorporated into the collaborators’ “pattern variables” scheme in *Toward a General Theory of Action* (1951).

Shils’s experience at the University of Chicago in the first half of the 1930s were not typical. He was nominally a graduate student in the Department of Sociology, but was strikingly detached from what was still, in the early 1930s, the heyday of the much venerated “Chicago School” of sociology. Aside from his brief exposure to Park and his expressed debt to W.I. Thomas—who of course was long gone by 1932—he seems not to

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81. Ibid.
82. Ibid.
83. Ibid 192.
have been affected in any significant way by the Department’s other major figures at the
time, including, of course, Wirth, but also William Ogburn, Ellsworth Faris, Ernest
Burgess, and Herbert Blumer. The department’s “vital gemeinschaftliche ambience”\(^86\) did
not, apparently, envelop the young Shils—who, after all, largely dismissed the
Department’s rich empirical findings in a number of postwar publications.\(^87\) Shils never
incorporated any of the Chicago School’s core research paradigms into his own thinking.
He did not, for example, absorb either of the School’s distinct social psychological lines
of interest, Thomas’s and George Herbert Mead’s\(^88\); nor did he accept Park and Burgess’s
“ecological” approach to the spatial study of cities.\(^89\) Shils was also not caught up, as the
Department was in various ways, with the unreflective, low-grade scientism that had, in
the interwar years, so much influence within the social sciences.\(^90\)

Shils had his own drummer. The University in the early 1930s, he remarked years
later, was a “wonderful place for a young man without social airs and without social
ambitions, poor, rather ignorant, serious and intellectual, launched on a quest without
knowing what he was looking for.”\(^91\) Though he hadn’t published anything, though he
was a student, though he wrote no dissertation, Shils had earned, by the mid-1930s, a

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86. Stephen Turner and Jonathan H. Turner, *The Impossible Science: An Institutional Analysis of
88. Mead, of course, was never a member of the department, but Faris and Blumer both drew on his
approach extensively. See chapter one of Abbott’s history for a masterful treatment of the
historiographical disputes, including the contested place of Mead, surrounding the interwar Chicago
School (*Department & Discipline: Chicago Sociology at One Hundred* 4-33).
90. On this influence, see Robert C. Bannister, *Sociology and Scientism: The American Quest for
Objectivity, 1880-1940* (Chapel Hill: University of North Carolina Press, 1987); Mark C. Smith, *Social
Science in the Crucible: The American Debate over Objectivity and Purpose, 1918-1941* (Durham,
91. “Some Academics, Mainly in Chicago” 179.
kind of local respect based, apparently, on his erudition. Two prominent friends, in particular, seemed to treat Shils with unusual deference—strange-seeming because of their scholarly achievements, contrasted to Shils’s own unpublished student poverty. These friends were Karl Mannheim and Harold D. Lasswell.

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Shils, especially in the years leading up to World War II, encountered, and absorbed, a belief in an irrational populace that, with the rise of fascism, seemed susceptible to illiberal cooptation—in rough parallel with so many other liberals of that decade.\footnote{See, for complementary overviews, Brett Gary, \textit{The Nervous Liberals: Propaganda Anxieties from World War I to the Cold War} (New York: Columbia University Press, 1999); and Edward A. Purcell, \textit{The Crisis of Democratic Theory: Scientific Naturalism & the Problem of Value} (Lexington, KY: University Press of Kentucky, 1973).} Shils, with his reverence for the “Weimar conversation,”\footnote{The phrase is John Gunnell’s. See his brilliant \textit{The Descent of Political Theory: The Genealogy of an American Vocation} (Chicago: University of Chicago Press, 1993).} his deep distress at its violent collapse, and his close ties with many emigrés, was much more open, especially in the late 1930s, to a dissensual picture of society than his later memoirs admit. Shils’s reminiscences of his interwar years tend to gloss over, in particular, the influence of Harold D. Lasswell and the planning-obsessed, post-emigration work of Karl Mannheim\footnote{He claims, in a 1981 reminiscence, to have absorbed nothing of substance from his admittedly deep contact with Lasswell before his departure in 1938 (“Some Academics, Mainly in Chicago” 194); and Mannheim is conspicuously missing from Shils’s long list of influences and intellectual compatriots in his 1982 introduction to \textit{The Constitution of Society} (xxix-xxx). Shils was a prolific self-narrativizer. He was particularly partial to narratives of accidental intellectual discovery, suffused with faux-modest whiggishness. (See "Personal, Primordial, Sacred, and Civil Ties"; “Some Academics, Mainly in Chicago”; “Introduction” in \textit{The Constitution of Society}; “Center and Periphery: An Idea and Its Career, 1935-1987”). His often detailed accounts—he half-apologized in his 1957 “Personal, Primordial, Sacred, Civil Ties” paper for the “rambling tale of my intellectual wanderings” (144)—are extremely useful, but should be treated with some caution. The usual filter of retroactive clarity is compounded, in Shils, with a self-conscious intellectual desire to place himself in certain traditions.}—in part because of his later, wartime reflections on the obstinacy of \textit{Wehrmacht} primary ties and his reflections, in England during and after the war, on the
peculiar stability of English and American political pluralism. Before the war, though, Shils encountered the “problem of the public,” the seeming collapse of laissez-faire capitalism, and the grip of Nazism with worldview-altering distress similar to other interwar liberals. Though his scholarly influences were distinct and particular, Shils was not exempt from that decade’s crisis of liberalism.

Shils’s friendship with University of Chicago political science wunderkind Harold Lasswell is revealing in this respect. His contact with Lasswell, from the mid- to late-thirties, was an important element in his growing—and, in certain respects, temporary—fear of mass irrationality and societal unraveling. Unlike Lasswell and Karl Mannheim—his other companion-in-distress in these years—Shils did not embrace a palliative politics of elite planning, and could not, therefore, point to any obvious protections against illiberal threats (until, of course, his wartime years in Britain). Lasswell’s diagnosis, rooted as it was in the same body of European thought that Shils first encountered as an undergraduate, was an influential component of Shils’s shaken outlook in the second half of the 1930s.

The influence of Lasswell’s grim picture of modern life gets obscured in Shils’s memoirs, with the odd result that Lasswell’s importance, though often highlighted, is never specified. Shils’s neglect here is probably not deliberate omission, but rather a product of his memoirs’ narrower purpose: to disclose the non-obvious intellectual figures and traditions which helped him arrive at his mature thinking. Because the interwar impact of Lasswell (and Mannheim too) on Shils was relatively brief and rather abruptly discarded, they were passed over or, in Mannheim’s case especially, disparaged.
Nevertheless, it is possible, from Shils’s writings and later, scattered statements, to establish his pre-war sympathies for the basic outlook that he would, in the 1950s, label the “theory of mass society.”

Lasswell is prominent, if vaguely so, in Shils’s remembrances. In his 1982 settling of intellectual debts, he includes Lasswell—along with Park and Knight—as one of three “elders” who most influenced him. Shils refers, in a 1975 memoir, to Lasswell’s “extraordinarily lively mind,” and, a few years later, insisted that “no account” of his Chicago years “would be complete” without reference to Lasswell: “… I could not deny that not only did he seem to think that he was the real thing but that in fact he was the real thing.” But when it comes Lasswell’s importance for him, Shils seems at a loss. He does, it’s true, credit Lasswell with stimulating his engagement with psychoanalysis—“what little I still retain in my thoughts.” He also, in passing, attributes his interest in deference to Lasswell, which became such an essential component of Shils’s later understanding of social order. But beyond this—and a “few grains of ideas which grew a little in different settings”—Shils doubts “whether I acquired any fundamental insight from him.” Shils comes off, himself, as a bit perplexed by the hallow prominence of

97. Ibid 194.
100. “Some Academics, Mainly in Chicago” 194.
Lasswell in his intellectual memories.\textsuperscript{101}

Shils’s original attraction to Lasswell, at least, is not nearly so mysterious. As an undergraduate, Shils had read Lasswell’s highly original psychoanalytic study of political personality, published in 1930 as \textit{Psychopathology and Politics}.\textsuperscript{102} Shils wasn’t then interested in psychoanalysis, but he found Lasswell’s wide reading of European theorists impressive: “…any author who knew about Michels, Weber, Moisei Ostrogorski as Lasswell did, was the man for me.”\textsuperscript{103} Shils himself, of course, was an avid consumer—or, in the case of German-language writings, struggled to consume—some of this same literature. When Shils began his studies at Chicago, he was unsurprisingly drawn to Lasswell—the “only teacher,” as he later wrote, “at the University of Chicago who was at home among the famous European figures, such as Weber, Michels, Pareto, Sombart, who seemed to me,” he added, “in my earlier years to be the repertoire of whatever a young person of intellectual seriousness ought to know.”\textsuperscript{104}

Shils remembers, on first hearing Lasswell lecture, being put off by his jargon-ridden affectation of omniscience, “conveyed with an amusingly diabolical archness.”\textsuperscript{105} But if not steeped in the knowledge of all fields, Lasswell was, in fact, “intimate with quite a number of them.”\textsuperscript{106} Shils and Lasswell, by Shils’s account, had a mutually respectful, if somewhat distanced, relationship. Once they became acquainted, in 1935,

\textsuperscript{101} Shils: “I find it hard to estimate the significance which Harold Lasswell had for me” (Ibid).
\textsuperscript{102} On Shils’s undergraduate reading, see Ibid 192.
\textsuperscript{103} Ibid.
\textsuperscript{104} “Introduction” in \textit{Center and Periphery: Essays in Macrosociology} xxvii. Shils called him the “most sophisticated of all the teachers there in his knowledge of the main European literature which interested me, and he was very daring too” (xviii).
\textsuperscript{105} “Some Academics, Mainly in Chicago” 193.
\textsuperscript{106} Ibid.
the pair began to deal with one another as “two sovereign powers—he a larger state, I a smaller one.”107 Though Shils, at the time, was an unpublished graduate student, the two were at first “very wary of each other.”108 After 1936, Shils reports, they became relatively friendly; Shils clearly respected Lasswell’s mind, but claimed that the political scientist’s actual thought was, to his young self, mostly dismissible. In his late-life memoirs, Shils points in particular to Lasswell’s stance of amoral scientism—the posture of “a cold-blooded social-scientific surgeon,” in Shils’s words—alongside his Comtean enthusiasm for social engineering. To Shils, Lasswell’s Hobbesian view of politics as a naked struggle for power, the product of an infelicitous synthesis of Pareto and Freud, was one-sided and morally noxious.109 In the years after Lasswell left the University in 1938, when Hutchins denied him tenure and promotion, Shils claims to have grown increasingly fond of Lasswell, especially after World War II. But even the post-war Lasswell remained foolishly wedded to a “schematic, utopian” belief in social engineering. “I thought that if he had remained in Chicago,” Shils adds with condescension, “I could have helped him to avoid that fate.”110

Shils probably does not misrepresent his reactions to Lasswell, but he does downplay the importance of other aspects of Lasswell’s assessment of modern life, including his view of the “masses” as unstable and irrational. Lasswell, in the words of a student and collaborator, does have a “mystifying intellectual history” that has

107. Ibid 195.
108. Ibid.
109. Shils: “The individual authors who went into Lasswell’s synthesis were all known to me and I liked neither the intellectual result nor the moral overtones of the synthesis” (Ibid 194).
110. Ibid 195
contributed to the misreading of his interwar thinking. Lasswell was painfully awkward—Isaiah Berlin called him a “queer duck”—and a notoriously poor expositor of his own neologism-ridden thought. His “blithely scrambled together technical jargon” was, throughout his career, an impediment to comprehension. His hit-and-run book production, for which he often adopted entirely new intellectual frameworks, meant that earlier work was rendered obsolete by default. His abrupt conversion to the democratic “faith” in the early 1940s, in particular, guaranteed a disconnect between his interwar “elitist amoral phase” and his Cold War “policy sciences” agenda—with the former consigned to virtual mnemonic oblivion. (One striking demonstration of this forgetting is that Wilbur Schramm, the most important figure in the establishment of “communication” as a stand-alone, institutionalized discipline, drafted Lasswell as one of the field’s “four founders” in 1963—treating him, in the process, as a major contributor to the “limited effects” orthodoxy then in place. As it turns out, Lasswell’s interwar writings on propaganda are some of the few scholarly studies on media influence from that era that really do suggest an omnipotent media.)


But it was Lasswell’s interwar thought, of course, that Shils was exposed to. In common with, and drawing upon, some of the currents of thought that Shils later placed under the “mass society” label—most notably Le Bon’s crowd psychology—Lasswell in these years stressed the danger of popular suggestibility, the necessity of elite rule, and the desirability of symbolic manipulation. Almost unique among American social scientists in his deep exposure to Continental elite theorists like Mosca and Michels, his own work was nevertheless a contributor to the wider stream of interwar social scientific loss of faith in a competent public. The American “realist” critique of democratic theory drew, for the most part, on British scholars like Bryce and Graham Wallas but also Le Bon, buttressed by a number of empirical studies that seemed to undercut the traditional requirements for an engaged, informed populace. There is, however, a tendency to overstate the proportion of American social scientists who adopted a position like this, in part because of the outsized prominence of the now-canonic Lippmann-Dewey debate.

Lasswell’s sweeping dissertation, published as Propaganda Technique in the World War in 1927 when he was just 24, can be read as a social scientific counterpart to Lippmann’s second, more chastened book on the subject, The Phantom Public (1925)—only Lasswell doesn’t betray any of Lippmann’s anguished pessimism. Lasswell’s

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115. For an overview of this American current, including Lasswell’s place in it, see Edward Purcell’s classic study (The Crisis of Democratic Theory), as well as Raymond Seidelman and Edward J. Harpham, Disenchanted Realists: Political Science and the American Crisis, 1884-1984 (Albany: State University of New York Press, 1985). Lasswell’s own early realism was the outcome of a 1923 visit to Britain, where he met with Wallas (see Ross, The Origins of American Social Science 403).


117. The Phantom Public, in like manner to Lasswell’s Propaganda Technique, calls for the symbolic “manufacture of consent” alongside technocratic expertise (The Phantom Public (New York: Harcourt, Brace and Company, 1925)). Lippmann’s realism, however, owes more to the specific challenge of
A four-country study of World War I propaganda was not meant as a debunking of nefarious mind-molding. Its purpose was not, as it was for the 1930s Institute of Propaganda Analysis, to build up average citizens’ defenses against manipulative persuasion. Lasswell’s book, instead, was intended as a kind of dispassionate primer for elites—a compendium, as it were, of effective propaganda techniques. Lasswell framed his analysis as a contribution to a dilemma shared by other chastened liberals: If mass democracy is an unavoidable feature of modern life, how then can responsible elite rule be maintained with stability? His answer, in short, is the coexistence of formal democracy alongside guiding propaganda—elite rule with democratic legitimacy:

“Democracy has proclaimed the dictatorship of palaver,” he wrote in a companion essay, “and the technique of dictating to the dictator is named propaganda.” Staving off the violence of the “spasmodic, superficial, and ignorant” many, he added in 1928 is in the “long run interest of society in social harmony.”

Charles Merriam, Lasswell’s mentor and chair of the Chicago department, sent his prodigy off to Europe in 1928 to 1929 to study psychoanalysis. He traveled to Berlin, Vienna, Paris and other cities, was analyzed by Theodor Reik, and studied with Alfred Adler, Karen Horney and others. Back in the United States, Lasswell published

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Psychopathology and Politics (1930) based on his scrutiny of politicians’ and agitators’ psychiatric records. His argument was that political movements “derive their vitality from the displacement of private affects upon public objects”; hatred of one’s father, for example, could become hatred for one’s state. Political crises are often worsened, he added, by the “concurrent reactivation of specific primitive motives.” Though expressed in Freudian terms, Lasswell’s argument here echoes the fear of mass unrest already on display in Propaganda Technique. The citizen is a “poor judge of his own interest,” and the apparent rationality of discursive politics is actually a powder keg of potential violence. Political scientists must apply “preventive mental hygiene” to reduce “the level of strain and maladaptation in society.”

On Merriam’s suggestion, Lasswell spent a semester at Harvard in the early 1930s studying with Elton Mayo (of Hawthorne experiments fame). Mayo, himself a longtime advocate for the technocratic guidance of an unruly mass, deepened Lasswell’s interest in European elite theorists, especially Pareto. He developed the Freudian-Pareto synthesis that Shils refers to in systematic form with his 1935 World Politics and Personal Insecurity, and the same themes—volatile masses, useful manipulation, elite rule, social scientific expertise—are prominent here too: “Government is always

121. Psychopathology and Politics.
122. Quoted in Purcell, The Crisis in Democratic Theory 104.
123. Quoted in Gary, Nervous Liberals 69.
125. On Lasswell’s encounter with Mayo, see Smith, "Harold D. Lasswell and the Lost Opportunity of the Purposive School". For an overview of Mayo’s elitism, see Purcell, The Crisis of Democratic Theory. Mayo: “The world over we are greatly in need of an administrative elite” (The Human Problems of an Industrial Civilization (New York: Macmillan, 1933) 177).
government by the few, whether in the name of the few, the one, or the many.”

Shils’s undergraduate encounter with the European elite theorists and the crowd psychology literature was, at the very least, renewed by his friendship with Lasswell from 1935 on. Like Lasswell, Shils at the time was worried about mass irrationality and the fragility of social order—especially in the context of fascism. Shils’s serious appraisal of psychoanalytic theories of fascist appeal, into the mid-1950s, was the acknowledged result of his friendship with Lasswell. Most importantly, he shared Lasswell’s fear that an unanchored, meaning-deprived mass was susceptible to demagogic appeal—even if he didn’t accept Lasswell’s illiberal solutions. “I accepted that beliefs were a constitutive part of society,” he later wrote of the late 1930s, “and that modern Western societies had undergone a dangerous attrition of belief.” Shils’s anxiety, in the same years, was intensified by his friendships with Karl Mannheim and other refugees from Nazi Germany.

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Shils was an undergraduate when Karl Mannheim published Ideologie und Utopie (1929) [Ideology and Utopia]. “I was dimly aware of the great commotion which it set going in Germany,” he recalled years later. By 1932, Mannheim was still “terra incognita” to Shils—but this would change dramatically, once Shils took up an assistantship under Louis Wirth. Soon after, he became de facto translator of the book, and then sole

127. “Some Academics, Mainly in Chicago” 194.
129. “Karl Mannheim” 221.
translator of Mannheim’s next, *Mensch und Gesellschaft im Zeitalter des Umbaus* (1935) [*Man and Society in an Age of Reconstruction*]. Along the way, Shils was, for a time, an enthusiastic adherent of Mannheim’s sociology of knowledge. And, at least until his World War II service in London, he accepted many of the basic tenets of *Mensch und Gesellschaft*’s bleak prognosis. Though he never embraced Mannheim’s urgent call for wide-scale planning, he was, like Mannheim, deeply shaken by the collapse of Weimar. Shils came to believe, partly under Mannheim’s influence, that modern societies were threatening to unravel, and that their precariousness derived, in large part, from a mass populace that had broken free from its old *Gemeinschaft* sinews. The basic contours of this view—Mannheim’s view—became, years later, the gist of the “mass society theory” label, to the extent that the label ever gets defined. This is not coincidence. Mannheim’s gloomy, dissensual analysis of modernity—which Shils accepts, then turns against harshly during and after the war—becomes the model, for Shils, of other intellectuals’ post-ideological deflation in the 1950s. If Sorel had served as Shils’s archetypical instance of intellectual Prometheanism, Mannheim came to seem like Sorel’s shadow—the purest expression of after-the-fall intellectual dejection. The difference was that Shils had shared Mannheim’s view, and he came to see Mannheim’s shadow everywhere.

As with Lasswell, Shils seems to have downplayed Mannheim’s importance to him in the interwar years. Shils’s own “discovery” of the primary group during the war was a part of this, as was the fact that Mannheim had become the explicit foil to Friedrich Hayek, Karl Popper, and Michael Polanyi during and after the war. In the months after Mannheim’s death in 1947, his wife, Julia, accused Shils of intentionally withholding a
manuscript of Mannheim's that was to be published as part of his posthumous Freedom, Power, and Democratic Planning, and successfully stopped him from publishing what she called “an extremely nasty paper” on Mannheim.  

In other publications over subsequent decades, however, Shils did dismiss, through condescension or outright disparagement, Mannheim's work.  

Mannheim is conspicuously slighted in the memoirs that Shils published over the years. It was only in spring 1995—just months after Shils’s death—that his lovingly drawn portrait of Mannheim appeared in the pages of The American Scholar.

Shils had been prompted, on his first day in Chicago, to read Ideologie und Utopie after Louis Wirth had asked Shils if he knew of Mannheim, who, Wirth revealed, he had met with on a recent trip to Germany (in 1930). An embarrassed Shils was forced to admit that he had not read any Mannheim; in the months to follow, while still a social worker, Shils made his way through the sociology of knowledge treatise. “I was,” he remembered in 1995, “swept off my intellectual underpinnings by its daring grandiose ambition,” despite his hesitancy to accept Mannheim’s solution to the apparent epistemological dilemma—the idea of a synthesis of the partial perspectives of a relatively free-floating intelligentsia. Shils was intrigued, if skeptical, about the notion

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132. “Karl Mannheim.”

133. “Some Academics, Mainly in Chicago” 186.

134. “Karl Mannheim” 222.
of ideas’ socially grounded validity, but his “skepticism,” he admits, “was rather faint in comparison with the impression the essay made on me.” Shils had become, he conceded, “an exhilarated devotee of Mannheim.”

Though Shils’s assistantship under Wirth—ostensibly on the “methodological presuppositions of German sociology”—had become little more than self-guided reading, Wirth announced to Shils, some time in late 1933, that Mannheim had asked Wirth to translate Ideologie und Utopie. Wirth then asked Shils if he would translate the book—and Shils agreed with alacrity. Shils quickly finished, then revised, a draft translation by the fall of 1934. Publication, however, was slowed by a nervous Mannheim eager to ensure a positive American reception, in large part to aid in securing a stable American post to replace his precarious English position. With Wirth’s active encouragement, Mannheim’s strategy was to deliberately modify language to highlight his closeness to American pragmatism—with “spirit” [Geist], for example, rendered as “mind” or “intellect”; “false consciousness” [falsches Bewusstsein] translated as “erroneous knowledge”; and “consciousness” [Bewusstsein] as “mental activity.” One consequence

135. Ibid. In the 1995 account, Shils adds that he had not yet read Weber’s methodological writings, which “would have been a prophylactic against such views” (Ibid).
136. Ibid 223.
137. Ibid 226.
138. For a detailed account of Mannheim’s conscious efforts to manage his American reception, see Kettler and Meja, Karl Mannheim and the Crisis of Liberalism 194-246; Shils reports, in uncharacteristically folksy language, that Mannheim was as “fidgety, for no good reason, as a hen on a nest of woodpeckers” (Ibid).
139. As Kettler and Meja note in great detail, there is a “dramatic contrast” between the English and German versions of I und U (Karl Mannheim and the Crisis of Liberalism 213). A fascinating detail of the translation is that Mannheim’s proposed revisions to the translation, made in England, were rendered with the help of his young assistant, Jean MacDonald—later Jean Floud—who was to become Shils’s colleague and longtime friend, first at LSE and then at Peterhouse College, Cambridge (see Karl Mannheim and the Crisis of Liberalism 183, 213, 290, 295). Kettler and Meja do not refer to Wirth’s active encouragement, but Shils does: “Wirth encouraged [Mannheim], repeatedly referring to Dewey’s Human Nature and Conduct as the American counterpart of Ideologie und Utopie” (“Karl Mannheim” 226).
of the American reframing is that Mannheim’s original German context—the Weimar crisis of mutual distrust—gets obscured and, moreover, the new language of “interests” and “erroneous knowledge” renders Mannheim’s “relationist” route out of the crisis somewhat unintelligible.140

In the end, Ideology and Utopia’s American reception in the late 1930s, so important to Mannheim, was scuttled by the unflattering reviews of other emigrés—most notably Alexander von Schelting and Mannheim’s former student Hans Speier.141 But Mannheim’s own choice for American champion, Louis Wirth, also deserves some of the blame. From the beginning, Wirth seems to have been narrowly concerned to extract from Ideology and Utopia a methodological mandate for American sociology. For Wirth, the issue was a crisis of social scientific value pluralism—and he viewed Mannheim’s sociology of knowledge as a procedure for establishing, through its exposure of the rootedness of all social judgment in interests, a working consensus for a pragmatic, problem-solving discipline.142 This is a far cry from the intractable Weimar babel that was Mannheim’s chief concern. His sweeping philosophical and historical ambitions, indeed, were the very aspects of Ideology and Utopia that Wirth sought to downplay.143 Concern

140. Karl Mannheim and the Crisis of Liberalism 214-16.
141. Ibid 207-10; von Schelting, “[Book review] Ideologie und Utopie,” American Sociological Review 1(4) (1936); and Speier, “[Book review] Ideology and Utopia,” American Journal of Sociology 43(1) (1937). Shils was soon to become friends with both von Schelting and Speier, and the pair’s epistemological critique of Mannheim had an admittedly large effect on Shils (“Karl Mannheim” 225, 231). Von Schelting’s review was rendered more caustic by the editing of ASR’s Howard P. Becker (see, for example, Karl Mannheim and the Crisis of Liberalism 208). Speier’s review in AJS was actually commissioned by Wirth, on Mannheim’s suggestion. Wirth’s close contact with Mannheim nemesis Leopold von Wiese—Wirth helped him secure a visiting professorship in the US in 1934-1935—reflected Wirth’s ambiguous advocacy for Mannheim. Wirth and von Wiese, openly critical of Mannheim, shared an optimistic vision of large-scale empirical sociology (Ibid 202-8).

142. Ibid 216-17.
143. Ibid 213-15
with disciplinary framing was Wirth’s motivation in asking Mannheim to include his 1930 “Wissenssoziologie” article in the English translation—and the grounds, too, for his horror at Mannheim’s insistence on the inclusion of a new introduction, clearly aimed at a non-specialist audience, that unabashedly addresses the wider ethical and epistemological implications of his sociology of knowledge. Wirth’s advocacy for Mannheim in the years after the 1936 English translation was, as a result, lukewarm and circumscribed. His defense of Mannheim against the vociferous attacks of Robert MacIver and Read Bain at a crucial 1937 meeting of the elite Sociological Research Association was qualified and ultimately ineffective. After the meeting and the emigré reviews, Ideology and Utopia was largely excluded from authentic sociological treatment—relegated, as Kettler and Meja observe, to the “uses of a minor ‘classic’ in the discipline’s instructional routines.” So particular was Wirth’s interest in Mannheim’s work that he seems not to have taken the book—or, for that matter, the sociology of knowledge in general—very seriously.

Any appearance to the contrary, on further inspection, turns out to be the effect of Shils’s behind-the-scenes intervention. Shils wasn’t just responsible for the translation; he also heavily revised Wirth’s introductory essay; he planned and drafted nearly all of the lecture notes for Wirth’s class on the sociology of knowledge; and he prepared the detailed memo on Mannheim and his critics that Wirth used, to poor effect, at the SRA

144. The “Wissenssoziologie” article was, in the end, published as the fifth chapter of *I and U*, while Mannheim’s new introduction became the book’s first chapter, following Wirth’s introductory essay.
145. *Karl Mannheim and the Crisis of Liberalism* 227-236.
146. Ibid 199.
147. Ibid 213; and Shils,“Karl Mannheim” 226.
meeting. (Early on, Mannheim seems to have recognized Shils’s central role in the translation effort, as he began sending translation-related letters to Shils directly, usually concerning, in Shils’s memory, “very trivial points or none at all.”148) Like Lasswell, Mannheim granted Shils a surprising degree of deference, in these letters and indeed for the rest of his life.149) Shils, already frustrated at the slow pace of his and Wirth’s revision to Shils’s translation,150 was horrified by Wirth’s draft of the introduction—a draft “so embarrassing” to Shils “by its fatuity and inconsequence” that he gave “serious thought to telling” Wirth to drop his name as co-translator.151 He set out, instead, to overhaul the introduction, and claimed to have “got rid of the most humiliatingly fatuous parts, but not entirely.”152

Based on an interview with Shils, as well as the “unequivocal” documentary record in Wirth’s papers, Kettler and Meja conclude that Shils planned and outlined Wirth’s sociology of knowledge class, and wrote the “bulk” of lecture notes for the course’s first offering in 1935.153 In preparation for Wirth’s showdown with MacIver and Bain at the 1937 SRA event, held in conjunction with the ASS meeting in Atlantic City, Shils drafted a memorandum which, however, Wirth then filtered through his

149. Ibid.
150. Much of the delay, Shils later claimed, was due to Wirth’s insistence to keep his door open to accept his many visitors: “his wisdom and his friendly words were cherished” (Ibid). For all of his frustration, Wirth’s open door permitted Robert Park to pop in frequently, which Shils in another memoir records with delight (“Some Academics, Mainly in Chicago” 190).
151. “Karl Mannheim” 227.
152. Ibid; see also Karl Mannheim and the Crisis of Liberalism 226. When Mannheim received the improved introduction, he wrote Wirth with glowing praise. Shils was embarrassed for Mannheim: “That struck me as a pathetic confession of dependence on someone who could contribute to making his reputation in Anglo-American intellectual life and particularly in America” (“Karl Mannheim” 228).
153. Karl Mannheim and the Crisis of Liberalism 241n.
methodological preoccupations. The memo, which Kettler and Meja have retrieved, offers a much more sophisticated treatment of Mannheim and his critics than those put forward by MacIver, Bain or Wirth at the actual ASR session—whose level of discussion, Kettler and Meja add, was “not high” with both sides combining to “deflate the brilliance of the book.”

Shils’s memo, Kettler and Meja claim, is learned and confident where Wirth’s own comments are tentative and amateurish. In a “curious reversal of normal relations between apprentice and master,” Wirth leans heavily on direct quotations from Mannheim, while Shils relies on his own “elegant and often more precise formulations.”

Shils’s pugnacious and erudite arguments are, many of them, simply ignored by a cautious and selectively motivated Wirth. In the memo, for example, Shils repeatedly stresses MacIver’s epistemological self-contradictions, which Wirth, at the SRA event, opted to leave unaddressed.

Shils’s thorough grasp of Mannheim’s thought owed not just to his careful reading and translation of Ideologie und Utopie, but also to a concurrent immersion in Mannheim’s sociological thought of the 1920s. In those first years in Chicago, he “threw” himself with “unquenchable exhilaration” into studies like “Das konservative Denken”

154. Ibid 226.

155. Ibid 237. Kettler and Meja are clearly more than a little surprised by Shils and his behind-the-scenes prominence—especially in light of Shils’s later dismissals of Mannheim. They refer to him as Wirth’s “brilliant erstwhile assistant” and as “an unusually gifted postgraduate student” (243n, 239).

156. Kettler and Meja report, for example, that Shils provided a reading of Marx, Burke, Savigny and Adam Smith which drew connections to their “sharply-defined political-ethical attitudes” (Shils). Asks Shils: “Are we to assume then that all their errors were due to their ethical motivations and all their correct perceptions were due to their untrammeled free intelligences?” Interestingly, Shils also lays out a critique of his then-friend Hans Speier, Mannheim’s former student who had published, also in 1937, a negative review of I and U, largely on the grounds that Mannheim conflates the distinction between truth and rhetorical effectiveness. Shils accuses Speier, in the memo, of naïve Enlightenment faith—“or else... some purely rationalized personality which makes some men into philosophers”—and points to the counter-evidence in Speier’s own research on eighteenth century intellectuals (238-39).
(1927) [“Conservative Thought”], “Das Problem der Generationen” (1928) [“The Problem of Generations”], and many others. At the time, for Shils, the essays contained “breathtaking observations,” and so his excitement had reached a fever pitch when he received, via the mail in 1935, a copy of *Mensch und Gesellschaft*. Shils, writing in 1975, was rather blasé about the memory: Yes, the book’s ambitious sweep “thrilled me,” he wrote, but its fear of instability came off as exaggerated: “Like other famous figures in the history of modern sociology, he had an eye which was quicker to detect and penetrate into ‘breakdowns’ than into the ‘ongoingness.’” This is the postwar Shils talking—the Shils who had already stumbled upon English civility and small-group solidity—and not the interwar Shils, who was stirred by Mannheim’s warnings, if not his planisme pretensions.

Shils’s 1995 account, in this respect, resonates much better with the other published evidence, including Shils’s own writing up until his London departure. Here’s Shils’s 1995 description: “I received a copy one morning and spent most of the rest of the day and deep into the night reading it in a state of great excitation.” *Mensch und Gesellschaft* was, of course, an entirely different book than *Ideologie*—prompted, not by Weimar’s warring ideologies, but by the Nazi cataclysm. Mannheim composed the German-language book partly in flight, during his brief stopover in the Netherlands, and drafted the rest soon after his 1933 arrival in England, where he remained until his 1947 death.

157. For a list, see “Karl Mannheim” 228.
159. “Karl Mannheim” 228.
For Mannheim, the world really did seem a polar night of icy darkness, and the book’s urgent, plaintive tone reflects his desperation. Mannheim’s argument is clearly developed within the framework of Weber’s great theses concerning modern rationality, but he departs from his Heidelberg precursor in crucial ways. Like Weber, Mannheim is worried about the disenchantment of the world, but far more than Weber he frets over the danger posed by the masses’ roving, unmet need for meaning. More generally, Weber’s account of the tragic self-undermining quality of rationality is, in Mannheim’s hands, not alarming for its “iron cage” unfreedom—but, instead, for the *excess* of freedom that it grants to the newly unimbedded, disoriented masses. Mannheim’s is not a liberalism of stoic pessimism, like Weber’s, but a *chastened* liberalism that, fearing mass instability and demagogic manipulation, calls for a corrective politics of elite planning. If Enlightenment rationality could properly guide various ruling elites in the eighteenth and nineteenth centuries—the age of “individual competition”—the emergence in the twentieth of mass democracy has fostered instabilities that, left unchecked, will yield societal collapse or, worse yet, the opportunistic seizure of power by ruthless tyrannies like the Nazis’. The many technological accomplishments of human reason—including industrial efficiency itself and notably the rationalized instruments of warfare—threaten, however, to slip from human control just as their potential danger swells. To Mannheim, this is no coincidence: The industrial system, by dissolving old communal bonds but also by forcing the factory-floor repression of unmet impulses, helps produce an excitable, leaderless mass that could, at any moment, turn into “machine-wreckers” and “ruthless
Mannheim’s fear of mass instability is rooted in an irrationalist view of humanity, drawn from psychoanalysis and other sources. Mannheim, however, suggests that civilization, rather than exacting the sublimations necessary for stability as Freud argues, instead fails to sublimate—fails, that is, to safely absorb latent irrational impulses in the manner of older societies. In the “well-integrated organic societies” of the past, popular wish fixations were harmlessly channeled into small-scale social milieux. Even once this era of “horde solidarity” gave way to the liberal stage of “individual competition,” popular energies remained partially embedded in organic life. Because popular participation in politics remained, given the era’s “pseudo-democracy,” passive and insubstantial, the elite-led growth of rationality could be maintained. But once “fundamental democratization” had advanced along enough—once the masses, that is, had flooded the stage of politics—the whole social order became hostage to their “sudden emotional eruptions.” What’s worse is that the masses, given their pent-up impulses, are feverishly casting about for meaning and fulfillment. The experience of modern complexity, after the old, reassuring bonds had dissolved, leaves modern man in a “state of terrified helplessness” and with “unharnessed irrationality.” All of this makes for a rickety social order teetering on the brink of collapse and violent chaos. The Hobbesian state of nature is bad enough, but Mannheim is especially terrified by the potential for bad-faith cooption of these mass energies. An “unchained” populace desperately seeking

160. *Man and Society in an Age of Reconstruction: Studies in Modern Social Structure* (New York: Harcourt, Brace & World, 1940) 64. My quotations are from the English translation (Shils’s, as it happens) of the original, German-language portion of *Man and Society.*
new objects of attachment can be deliberately harnessed by unscrupulous demagogues. Hitler and the Nazis, of course, are the shaken Mannheim’s bloody case in point. The fascists’ careful and deliberate manipulation of the new media of mass communication has permitted him to “form human beings according to his own ideas and in this manner multiplies his own human type a million fold.”

The liberal pieties of an older age are, to Mannheim, blind to these new conditions. The only way to prevent social collapse and the worst sort of illiberal dictatorship is through wide-scale, elite planning. Liberalism, he concluded, was merely an interregnum between two forms of planned society. Our choice, he argued, is between chaos or total domination, on the one hand, or a preventative “planning for freedom,” on the other. In opting for the latter, Mannheim proposed a “re-education of man” and a careful top-down coordination, aided in large part by social scientific advice.

Mannheim’s unblushing, Comtean faith in social scientific engineering, in *Man and Society*, is a long way from the clarifying service of the sociologist of knowledge. But then 1933 had displaced Mannheim’s earlier “crisis” of entrenched and warring ideologies. With real war and real trenches, the Weimar cacophony seemed trivial by contrast.

Though Shils claims to have rejected, even on first reading in 1935, Mannheim’s planning fix, he was clearly impressed by Mannheim’s grim diagnosis of social instability. In his excited state, and without consulting Wirth, Shils quickly wrote

161. Ibid 73.
162. For Mannheim’s critique of planning in *Ideology and Utopia*, see 262-63.
Mannheim to notify him that he was ready to translate *Mensch und Gesellschaft*. They remained, over the next few years, in close correspondence, mostly on matters related to the translation. Shils completed the translation in late 1938, after first adding many bibliographic references in footnotes to Mannheim’s texts. Mannheim, in the acknowledgments to the much-expanded 1940 English edition, wrote:

I have first to give my thanks to Edward Shils (University of Chicago), who has not only prepared the translation of those chapters which coincide with the German book, but who has also supplemented the bibliography which I have been collecting for many years with some kindred items. His very valuable understanding and broad knowledge meant not only responsiveness, but new stimulus.\(^\text{165}\)

Shils joined the College of the University of Chicago as an instructor of social science in 1938, and soon after he extensively overhauled the syllabus for the second-year social science course, which had been given the theme “Freedom and Order” soon before he joined the staff.\(^\text{166}\) The syllabus in place in 1942, when Shils left for London, included a lengthy excerpt from *Man and Society*.

Though he would downplay the temporary agreement later, Shils clearly accepted the basic outline of Mannheim’s bleak assessment—particularly his accent on social unraveling and the dangerous vacuum of anchoring belief. In 1941, Shils participated in a symposium sponsored by the *Journal of Liberal Religion*, which also included Robert K.

\(^{163}\) “Karl Mannheim” 228.


\(^{165}\) *Man and Society in an Age of Reconstruction* “Acknowledgments”.

\(^{166}\) See note 17, Chapter One.
Merton’s famous essay on Mannheim’s sociology of knowledge.\textsuperscript{167} Shils’s remarkable paper, essentially an extended review of \textit{Man and Society}, doesn’t just endorse Mannheim’s diagnosis, but also offers qualified support for the planning regime itself.\textsuperscript{168}

Shils later claimed that his review was an attempt to “put the best face I could” on Mannheim’s book, but the insinuation that he sugar-coated his response to the book isn’t plausible.\textsuperscript{169} For Shils, quite in character, does not pull any punches in the review; with condescending and sometimes nasty language, he is at pains to establish the book’s “numerous” shortcomings.\textsuperscript{170} His criticisms, however, focus almost exclusively on the book’s generality, its failure to draw on “corroborative evidence which is available in many cases,” and its reliance on easily toppled strawmen to justify its methodology.\textsuperscript{171}

Even Mannheim’s planning proposals come under Shils’s fire for vagueness and not for their fundamental implausibility: “Yet despite its generality,” Mannheim’s planning proposal “is a pioneer attempt, and nothing in the recent planning literature is superior to it with respect to the serious problems attacked and the richness of the suggestions.”\textsuperscript{172}

Though Shils’s treatment of Mannheim’s plan for planning falls short of full endorsement, he clearly remains open to the idea of a liberal planning regime.\textsuperscript{173} On

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\textsuperscript{168} Oddly enough, the article title and references within the text incorrectly refer to the book’s title as \textit{Man and Society in an Age of Transformation}. This was, perhaps, the result of an editing error.

\textsuperscript{169} “Karl Mannheim” 230. Shils: “I tried to put the best face I could on my response to it, but evidently not quite successfully because Mannheim wrote to me in a slightly injured but still very friendly tone, saying that he hoped that we would soon be able to discuss our differences face to face.” Shils was then readying himself for his London departure.

\textsuperscript{170} "Irrationality and Planning: A Note on Mannheim's \textit{Man and Society in an Age of Transformation}" 153.

\textsuperscript{171} Ibid.

\textsuperscript{172} Ibid 152.

\textsuperscript{173} Shils: “Observation of the totalitarian regimes has led to the view, widely held, that planning and dictatorship are identical, that planning necessarily involves dictatorial power, and furthermore that the
Mannheim’s anxious diagnosis of mass irrationality—including its thesis of the dissolution of primary groups and community controls—Shils accepts Mannheim’s basic picture without criticism. He nowhere declares himself fully convinced, but his words of praise and his uncritical exposition—as against his openly negative comments on other aspects of the book—strongly implies that he finds Mannheim’s analysis credible.

Shils’s use of the term “mass society” in this 1941 review was one of the earliest in English-language scholarship. The phrase—which Mannheim repeatedly invokes as a shorthand for the mass-ascendant modern age—entered the American sociological lexicon with the publication in 1940 of Man and Society. Indeed, the term was first used in an American sociological journal in 1940, by C. Wright Mills in a review of the book. And the first five “mass society” mentions, from 1940 to 1944, all invoke the term in reference to Mannheim or to other German emigrés. Shils’s later adumbration of what he calls, with intent to dismiss, the “theory of mass society” is in many ways a sketch of Mannheim’s post-emigration diagnosis. It is not merely ironic that Shils had, in these years, adopted the view himself.

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expansion of governmental control over private economic activity must end ultimately not only in the loss of entrepreneurial liberties but of all other significant liberties as well. These are arguments which Mannheim sets himself to refute; and the results, though far from conclusive, are unsurpassed in modern social science and social philosophy” (Ibid 151-52).


175. Mills: “The transition from a ‘democracy of the few to a mass society explains another set of changes’. This ‘mass society’ is one of the least substantiated notions in the book. One wishes Mannheim had characterized it less with words like ‘emotional’ and ‘irrational’ and more with such indices as voting trends” (“[Book review] Man and Society in an Age of Reconstruction,” American Sociological Review 5(6) (1940) 967). There’s an irony in Mills’s somewhat dismissive treatment of the “mass society” term, in that he was, of course, to use the phrase extensively in The Power Elite (Oxford: Oxford University Press, 1956).

176. See Appendix.
In the course of his work for Wirth and Mannheim, Shils became acquainted with a number of intellectual refugees from the Nazi regime—including Hans Speier, Alexander von Schelting, Ernst Fränkel, Hans Gerth, Franz Neumann, and Mannheim himself. These encounters were significant for Shils in a number of fascinating ways. Speier and von Schelting, for example, helped wrest Shils from his embrace of a Mannheim-derived epistemology centered on the social genesis of ideas. As part of his help to a late-emigrating Gerth, to take another example, Shils heavily edited (and may have largely written) Gerth’s well-known paper on the Nazi Party—an expression, certainly, of Shils’s concern with the suggestibility of unanchored masses. His immersion in the emigrés’ often frantic attempts to explain the Weimar collapse and the nature of the Nazi regime—notably including Fränkel’s *The Dual State*, which Shils translated so that Fränkel could establish himself in the Anglo-American world—exposed him to a succession of bleak accounts, set in a despairing and traumatized key. His friendships with the emigrés—the dispersed and shell-shocked remnants of the Weimar intellectual culture that was, in a sense, his first academic passion—seems to have blackened his already dark mood. Shils’s own crisis, put differently, was an amplified and distinctly tangible version of the crisis in American liberalism that had, by the mid-1930s, emerged out of the depression and the distant rumble of fascism.177

Shils came to know Hans Speier first and best, after he wrote to Speier about his unpublished manuscript *Die Angestellte in der deutschen Gesellschaft* [*The Salaried...*](#)

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Employee in German Society]. Soon after, Speier and Shils were brought together in the effort, spearheaded by Speier in late 1934, to convince Mannheim to join him at the New School’s “University in Exile” (which Mannheim declined). In a later memoir, Shils referred to strain between him and Speier, produced by Speier’s polite but dismissive 1937 review of Ideology and Utopia in AJS, but they apparently remained friends up to the 1960s, when Speier became “so deeply enmeshed in the RAND Corporation” that their contact fell off. It was, Shils remembered, his interwar conversations with Speier that led him, Shils, to move beyond Mannheim’s blunt emphasis on social class, to consider instead the independent role of intellectual traditions and institutions in generating knowledge.

Shils also claimed to have become “a close friend” to Alexander von Schelting, the German scholar of Weberian methodology who had, by 1936, established himself at Columbia. It’s not clear how they initially met, only that Shils spent the spring of 1938 in New York, studying with von Schelting and working on his Mensch und Gesellschaft translation. It is possible that Parsons, during his 1937 term at Chicago, had linked Shils with von Schelting; Parsons had written a glowing, six-page review of von Schelting’s 1934 Max Webers Wissenschaftslehre for ASR, and subsequently helped him

179. “Karl Mannheim” 225; and Karl Mannheim and the Crisis of Liberalism 177.
181. Ibid.
182. Ibid.
secure the Columbia post. Shils later credited von Schelting, as well as Parsons and Speier, with revealing *Ideology and Utopia*’s epistemological flaws to him—but here again the question of timing is unresolved. After the war, at least, Shils became a full-fledged Parsonsian analytical realist, with qualified praise for the Weberian position as exemplified by von Schelting. In the foreword to his 1949 translation of Weber’s methodological writings, Shils praises von Schelting’s book, along with Parsons’s SSA, as the “most accurate and elaborate studies of Weber’s methods.”

The help that Shils granted to Hans Gerth and Ernst Fränkel came about, in both cases, after requests from Mannheim. Fränkel, a close friend of Franz Neumann, had remained as a practicing lawyer in Nazi Germany from 1933 to 1938, when he fled to England. With Neumann, Fränkel had been developing, since the late 1920s, a critique of Carl Schmitt’s decisionism and legal formalism more generally. Neumann had left Germany in 1934, and taken up doctoral studies in political science at the London School of Economics, under Harold Laski and Mannheim. Mannheim, on Neumann’s (and Fränkel’s) behalf, contacted Shils sometime in late 1938 with a request for help. Shils agreed to translate Fränkel’s analysis of law under the Nazi regime, *Der Doppelstaat*

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184. Parsons, "[Book review] Max Webers Wissenschaftslehre," *American Sociological Review* 4(1) (1936). For all of his praise, however, Parsons in the review takes von Schelting and Weber to task for their refusal to claim universal law-status for ideal typical analysis. Parsons: “My principal criticism both of Weber and of Schelting, who follows him in this respect, is that they have not completed the process.”
185. “Karl Mannheim” 225.
186. See, for example, "Foreword," in *Max Weber on the Methodology of the Social Sciences* x.
187. Ibid.
189. On the particulars of Neumann’s formal relationship with, respectively, Laski and Mannheim—as well as Neumann’s later vituperative attacks on Mannheim as he became fully integrated into the Frankfurt Institut, see *Karl Mannheim and the Crisis of Liberalism* 196-97.
(1940) [The Dual State (1941)], “in order,” he later wrote, “to help him find his way in the United States.” The widely praised book argues that, under the Nazis, there exists a kind of legal schizophrenia—a “normative state” that incorporates pre-existing code, and which remains operative in the realm of private disputes and business relations, alongside a “prerogative state” of arbitrary Party rule. In an argument that anticipates the four-pillar thesis of Neumann’s 1942 Behemoth, Fränkel insisted that Hitler’s Reich integrates both “rational and irrational activities.”

Mannheim also wrote to Shils on Hans Gerth’s behalf. Gerth, Mannheim’s former student at Heidelberg and a Christian, had remained working as a journalist in Nazi Germany, from 1934 to 1937. After an interview with a Gestapo agent that year, Gerth feared impending arrest and fled to England. It was difficult to locate a post for Gerth, in part due to his late German departure; suspicious first-wave emigrés attacked him as “the Aryan latecomer.” It was Shils, as it turns out, who shepherded Gerth’s article on the Nazi Party through to publication in AJS, which provided Gerth the scholarly legitimacy to secure a relatively stable position at the University of Wisconsin. At Mannheim’s request, Shils helped orient Gerth when he arrived in New York in December 1937, a week before the ASS meeting in Atlantic City. Mannheim had dispatched Gerth to defend his sociology of knowledge at a panel planned for the

conference, organized by Parsons and slated to include his apostate student Hans Speier.194

Gerth spent the next few months teaching German to Harvard sociology graduate students, while studying with emigré political scientist Carl Friedrich.195 Wirth, at both Shils’s and Mannheim's urgings, then secured a temporary post for Gerth at the University of Illinois, and it was from there, in the summer of 1938, that Gerth sent Shils his essay on the Nazi Party.196 According to Shils’s letter in response, he was very impressed by the paper, though he thought it terribly written and poorly organized.197 He also spoke with Ernest Burgess and Wirth—the editor and managing editor of AJS, respectively—to press for a sympathetic reading, and later submitted a written defense of the article to Burgess and Wirth.198 In the meantime, Shils sent Gerth books to review for AJS, to improve his English and enhance his standing in American sociology; and he set about editing and revising the paper itself.199

In his 1938 correspondence, Shils presented himself as merely an editor, but in a 1987 letter to Gerth’s wife, he claimed to have devised the paper’s basic thesis—that Nazi rule depends on a unique fusion of bureaucratic and charismatic sources of

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194. For a detailed account of the high-drama panel session—not to be confused with the concurrent dispute over Mannheim taking place at the Sociological Research Association, involving Wirth, Bain and MacIver—see Karl Mannheim and the Crisis of Liberalism 220-25. Kettler and Meja interpret Parsons’s role in the panel as part of “his move to seize the leadership in interpreting German sociology to Americans” (222). Speier’s attack on his former teacher Mannheim, interestingly enough, does not lean on von Schelting’s neo-Kantian dismissal of Mannheim’s epistemological contradictions—as he did in his AJS review—but instead, in Atlantic City, bases his defense of “truth” with recourse to the natural right arguments of his New School colleague Leo Strauss (222-23).
196. Ibid 413.
197. His letter is cited in Ibid 413-14.
198. Ibid 413.
199. Ibid 414.
authority—and indeed to have written the paper itself using Gerth’s data. The published paper, which appeared in *AJS* in 1940, includes this footnote off the title: “The author is greatly indebted to Mr. Edward Shils, of the University of Chicago, for many valuable suggestions and the complete revision of the article.” In his 1957 paper on “Personal, Primordial, Sacred, and Civil Ties,” Shils appears to allude to his central role in producing the Gerth article’s thesis, referring to his own “abduction of the term ‘charisma’ in an effort to describe the Nazi party in 1938 (before this became the fashion)...” Shils and Gerth later, in the mid-1940s, engaged in a dispute over overlapping Weber translations that both Shils and Gerth (with C. Wright Mills) were preparing for publication—a dispute that ended the men’s friendship. Whether Shils was mere “midwife” to Gerth’s 1940 paper, or something more like a first author, is impossible to determine. But Shils’s clear support for the article’s thesis, regardless, helps supplement other evidence that he was, at the time, worried about the Nazis’ successful exploitation of the German populace’s unmet need for meaning.

Through his contact with the intellectual refugees, Shils was bathed in their acrid, “disaster triumphant” distress. Franz Neumann, in his haunting 1953 commentary,

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200. Ibid 414n. In the same latter, he claimed that Gerth offered him co-authorship, which Shils reports that he refused.
203. See Oakes and Vidich, "Gerth, Mills, and Shils: The Origins of From Max Weber".
204. Gerth: “The National Socialist party in Germany can be adequately described only as a fusion of two types of domination, namely, the charismatic and bureaucratic type... As long as the leader is successful the belief of broad masses of followers in his charisma is not likely to be shattered. They will continue to act in accordance with the slogan: ‘Leader, command; we follow’... the masses who were thereby atomized provided an enormous field for the organizational zeal of the inner circle" ("The Nazi Party: Its Leadership and Composition" 541).
described the psychological trauma of many emigrés: “Being compelled to leave their homeland, they thus suffered the triple fate of a displaced human being with property and family; a displaced scholar; and a displaced homo politicus.” Even among the intellectuals that Shils came to know, there existed an enormous political and scholarly diversity—not everyone experienced Frankfurt-style “hotel grand abyss” estrangement, especially after adjusting to American or English conditions. But there was, from Mannheim to Speier to Fränkel, a sense of despair and disorientation at the violent shattering of German civilization. The Weimar collapse greatly amplified the already widespread cynicism among intellectuals concerning the viability of consensual, democratic politics. If the two students of Weber, Mannheim and Carl Schmitt, could be thought to offer, in the 1920s, competing responses to Weber’s qualified critique, in particular, of democratic rhetoric—one hopeful, the other coldly cynical—the Weimar collapse settled the matter, for many emigrés including Mannheim, in Schmitt’s tragic favor. The Frankfurt scholars, including Neumann, as well as many others, had come to accept Schmitt’s decisionist critique of parliamentary democracy, without of course conceding Schmitt’s normative inferences. Schmitt-derived or otherwise, there was widespread pessimism about the prospects for stable liberal democracy in the emigré

206. On this point, see Ibid.
207. See Charles Maier’s history of the period for an excellent discussion of Weimar-era intellectual cynicism (Recasting Bourgeois Europe: Stabilization in France, Germany and Italy in the Decade after World War I (Princeton: Princeton University Press, 1975)).
208. See Ellen Kennedy, "Carl Schmitt and the Frankfurt School," Telos 71 (1987); and William E. Scheuerman, Between the Norm and the Exception: the Frankfurt School and the Rule of Law (Cambridge, MA: MIT Press, 1994). In many respects, the School’s form of immanent ideology critique—in which critique relies not on one or another independent standpoint but rather on a society’s own ideals and self-descriptions—is heir to Schmitt’s realist assault on democratic principles.
community, especially in the years before the war. Many of the emigrés, including those Shils knew like Mannheim and Fränkel, labored in writing to make sense of the Nazi calamity, and most of these—including Paul Tillich’s, Emil Lederer’s, Erich Fromm’s, Sigmund Neumann’s, to name a few published before Shils’s London departure—are the works of palpably shaken men.

Shils claimed to have read “all” of this exile literature, and he knew some of its authors. Years later, he was to label many of these published analyses—those by Tillich, Mannheim, Lederer and Fromm, most prominently—as the embryonic form of the “theory of mass society.” But before he discovered the primary group—before he read about English religious tolerance—Shils was much more sympathetic to the emigrés’ ashen diagnoses. He was certainly exposed, through this literature, to Schmitt’s critique of democratic politics, which in many ways resonates with the critiques of democracy put forward by Sorel, Mosca, Michels, Pareto and Lasswell, which Shils knew so well.

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209. See, for example, Gunnell, *The Descent of Political Theory: The Genealogy of an American Vocation* 165-74. As Neumann had observed of Weimar, “Democracy never acquired that self-reliant buoyancy that it had in Anglo-Saxon countries” (“The Social Sciences” 14).


211. Shils: “I read all the literature produced by the Germans in exile” (“Karl Mannheim” 225). It’s worth noting that Shils’s acquaintance and future collaborator, Talcott Parsons, was also producing analyses of fascism and anti-Semitism that, in certain respects, resemble the emigrés’ chastened assessments (see, for example, "Some Sociological Aspects of Fascist Movements," *Social Forces* 21 (1942); and "Sociology of Modern Anti-Semitism," in *Jews in a Gentile World: The Problem of anti-Semitism*, eds. Isaacque Graeber and Steuart Henderson Britt (New York: Macmillan, 1942)).

212. For example, Shils stated that, in the 1930s, the “‘theory of mass society’ was beginning to take form. The publicistic admiration for the all-embracing consensus of German society under the National Socialists and of Soviet society was receiving a more intellectual formulation in the writings of Paul Tillich, Emil Lederer, Karl Mannheim, and Erich Fromm” (“Introduction,” in *Center and Periphery: Essays in Macrosociology* xx).

213. Stephen Turner, in his 1999 essay on Shils, notes that Shils’s first published citation to Schmitt comes in 1958 ("The Significance of Shils," *Sociological Theory* 17(2) (1999) 131). Shils was, however,
Like many American intellectuals in the 30s, Shils was forced to make sense of the economic depression of that decade—which, as he later recalled, he “learned from the experience of my own family and our neighbors as well as from my experience as a social worker in New York and Chicago.” But Shils wasn’t, like his fellow intellectuals, primarily oriented to the *American* depression or even to the limited *American* critique of democratic theory. He was, as he put it, “brought up intellectually” in France, Great Britain and Germany; the ascendance of the Nazis and other fascist regimes were “events of almost overwhelming significance” to him. His “exiled friends” were the living embodiments of a collapsed society—the scattered and scarred remnants of a revered intellectual culture. Many of them were victims, too, of the regime’s violent—and ultimately genocidal—anti-Semitism. Shils’s fieldwork among Nazi sympathizers on Chicago’s North Side, in 1941, was almost certainly motivated by his own desire to understand the German catastrophe and its prospects in America.

With the benefit of published hindsight, Shils would repeatedly downplay his onetime attraction to the cheerless theses of Mannheim, Lasswell, and certain emigrés, which he was, anyway, to so abruptly discard in the war years. His many published treatments of Mannheim in the decades after the war, for example, were sometimes bilious, often condescending and almost always silent on his own up-all-night

undoubtedly familiar with Schmitt’s thinking by the end of the 1930s. The three books that Shils translated before the war—Mannheim’s as well as Fränkel’s—all extensively cite Schmitt, as does Gerth’s Nazi Party paper (“The Nazi Party: Its Leadership and Composition”).

215. Ibid xxix.
216. Ibid.
exhilaration.\textsuperscript{218} In a 1974 essay on \textit{Ideology and Utopia}, Shils refers to the book as a “stillborn” and ends the piece with, “It is a great pity that [Mannheim] spent a substantial part of his too short life arguing for a hopelessly wrong position which his own demeanor refuted.”\textsuperscript{219} The essay makes no mention of his earlier excitement nor his role as translator and behind-the-scenes champion. His 1968 entry on Mannheim for the \textit{Encyclopedia of the Social Sciences} is equally dismissive—not just of \textit{Ideology and Utopia},\textsuperscript{220} but of \textit{Man and Society} as well. Shils chastised Mannheim for believing that “modern mass society is afflicted with a degree of dissensus such as no other society had ever suffered,” and ended the article with dismissive half-praise: “The adage which asserts that the mistakes of a distinguished mind are more interesting than the truths of a mediocre one was true of Mannheim. He had in large measure the rare gift of touching on vital and enigmatic things.”\textsuperscript{221}

Shils, in essence, retroactively projected his post-war thinking back onto his interwar years. Writing in 1975, Shils referred to a “common” interwar contrast between the “dilapidated, ramshackle condition of North American and Western European societies” and the fascists’ “solidarity and eagerness for concerted, authoritatively

\textsuperscript{218} The exception is Shils’s posthumously published memoir of Mannheim, which I have drawn upon extensively in these pages (“Karl Mannheim”). This essay’s eulogistic tone, in contrast to the others, makes the piece almost read as penance. But even here he omits himself from the mistaken interwar pessimism: “He greatly overestimated the strength of related tendencies in other western countries. Like many Central European refugees in Western Europe and the United States, he underestimated no less the ramshackle obduracy of such countries as Great Britain, France, the Netherlands, and the United States, in their tenacious devotion to liberal democratic traditions” (229).

\textsuperscript{219} "Ideology and Utopia by Karl Mannheim" 86, 89.

\textsuperscript{220} Shils: “His continuous insistence that the ‘internalist’ (ideological) view is wrong and his failure to recognize how much of it he himself retained led to his failure to perceive the partial autonomy of intellectual traditions and the institutional structure in which autonomous intellectual activity is effectuated” (“Mannheim, Karl,” in \textit{International Encyclopedia of the Social Sciences} 558).

\textsuperscript{221} Ibid 559, 561.
direction,” adding: “I was not sympathetic with this view.”

In 1982, Shils proclaimed that, before the war, he had been “suspicious of those criticisms of modern Western societies which proclaimed the ‘disintegration’ of these societies and which recommended the reestablishment of community—Gemeinschaft.”

It’s true that Shils nowhere suggests a Gemeinschaft restoration, but then neither do Lasswell, Mannheim, or any of the other emigrés. Shils distanced himself from emigré analyses of fascism and modern society in part by erecting a total-belief strawman which, however, the emigrés didn’t espouse—especially not in normative terms, as Shils implies. Writing in 1988, Shils asserted that, in the first years of the war,

The readiness to believe in such falsifications became more acute in the early years of the Second World War when the Germans were advancing in the West and East. It was alleged by many writers who asserted that they did not sympathize with National Socialists… but who explained the German military advance by the combination of profound attachment to the National Socialist ideal on the part of the German soldiers and to the unity of Germany under the National Socialist leadership which not only controlled German society in an unqualifiedly centralized pattern but also inspired the German soldiers.

Shils failed to mention that he too once held a scaled-down version of this thesis, and that, moreover, its espousal was almost never as totalizing as he implies. Shils criticized, in 1975, the notion—held by many “decent persons who had experienced National Socialism and who were alarmed by the danger”—that modern liberal democratic societies were unable to meet humans’ “hunger for solidarity”; this view, Shils added, “made no sense to me.”

Shils even retrospectively detected an “incipient ‘theory of mass society’” in the

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pre-war writings of Tillich, Lederer, Mannheim, Fromm and the Frankfurt scholars: “A consensus was forming. Marxists, refined and vulgar, liberals of faint heart, and other haters, high- and low-brow, of modern society, supported each other. The great tradition of sociology which saw the world in the dichotomy of Gemeinschaft and Gesellschaft was beginning to join forces with these other [emigré] blocks of opinion.”

In the same memoir, Shils insisted that he, by contrast, had in the 1930s noticed the American populace’s ongoing attachment to society, and that he had observed a “renewal of civility” in intellectual circles.

It’s probably true that Shils’s own interwar assessment was often more qualified than the terrified arguments of, say, Mannheim. His views were certainly, in important respects, distinct—he was more averse, for example, to technocratic planning than Mannheim or Lasswell, though not closed to the idea. It’s very probable, though there’s no evidence for this other than his memoirs’ claims, that he was more sensitive to certain constituents of social order, and to democratic stability, than some of the refugee scholars. His fascination with, and fear of, the antinomian intellectual, focused in these years on the Communist and fellow-traveller, no doubt set him apart; but he had not by

226. Ibid xx.

227. He pointed, too, to the great numbers being killed in totalitarian countries at the time, adding: “One could not, unless one was already a determined partisan or a complaisant victim of propaganda, have failed to notice this” (Ibid xxi). The implied contrast was held by virtually no one, and certainly not those (like Shils) who were worried about the potential for social collapse in other Western countries.

228. Here’s a statement of Shils’s somewhat ambivalent, but clearly cautious, view in 1941: “Any government which seeks to be effective must have some way of discovering the preferences of the population whose behavior it seeks to influence. For a democratic government which regards preferences not merely as objects to be manipulated but as a source of guidance in the establishment of the main lines of policy and administration it is especially urgent to possess means of acquiring knowledge of the state of mind of its citizens” (“A Note on Governmental Research on Attitudes and Morale,” American Journal of Sociology 47 (1941) 474). See also "Irrationality and Planning: A Note on Mannheim's Man and Society in an Age of Transformation."

229. Shils, in 1972, remembered watching “these twisters”: “In the 1930s I witnessed with revulsion the rush of the Gadarene intellectuals in the Untied States and Europe into the arms and snares of their
then extended this analysis to fascism nor to the anxious emigrés themselves—as he would in the postwar years.230

Shils’s after-the-fact claims that certain trends—the disintegrative tendencies of Western societies, dangerous deficits in coherent meaning—“made no sense” to him simply isn’t plausible. Though it is indeed difficult to reconstruct his interwar thinking, due to his sealed papers and the paucity of his published writings in those years, it is clear, at least, that he was sympathetic with some of the arguments that he would later relegate to the “mass society theory” label. The few articles that he did write provide some of this evidence. His memoirs, too, let slip mnemonic grains, as it were, through their cracks.

He refers, in his 1938 survey of academic freedom violations, to the danger posed by “serious cleavages in political, economic, and cultural preferences,” in a society “lacking an inclusive consensus.”231 His 1941 review of Man and Society praises and implicitly accepts Mannheim’s arguments about the masses’ irrationality and their unsatisfied needs for meaning.232 In the 1930s, he conceded in a 1975 memoir, he “still accepted much of the conception of modern liberal society to be found in Tönnies, and in Simmel’s Die Grosstädte und das Geistesleben, which I admired so much that I

respective communist parties. I saw and did not like the silliness, dishonesty, and self-deception of some of the best-educated and the most cultivated and many of the half-educated intellectuals of several generations” (“Introduction,” in The Intellectuals and the Powers and Other Essays vii).

230. The one possible exception that Shils refers to is his “Nazi fieldwork” in Chicago in 1941, which he claims to have first hit upon the common features of right and left intellectual Prometheanism—though he didn’t elaborate these thoughts until 1950 and 1954 (“Introduction,” in Center and Periphery: Essays in Macrosociology xxvii). (See Shils, "Introduction," in Reflections on Violence (Glencoe, IL: The Free Press, 1950); and Shils, "Authoritarianism, 'Right and Left'," in Continuities in Social Research II: Studies in the Scope and Method of The Authoritarian Personality', eds. Richard Christie and Maria Jahoda (Glencoe, IL: Free Press, 1954).)

231. "Limitations on the Freedom of Research and Teaching the Social Sciences” 144.

translated it into English.” He admitted, moreover, that he had “accepted, more or less,” the “modern sociological version of the Hobbesian description of the state of nature in its application to western societies” in those years. Later in the same essay, he refers to a view—“to which I was not unresponsive in the 1930s”—that the “urban, liberal-democratic, capitalist society of the past century was in a process of increasing disintegration.” It’s clear, from Shils’s writings and his own remembered beliefs, that he was at least somewhat supportive of the unraveling thesis that he goes on, in future decades, to so vociferously dismiss.

The view that humans are meaning-dependent beings—a Weberian thesis that Shils accepted throughout his life—is, moreover, plainly linked to Shils’s interwar fear that modern society was not throwing up enough. His repeated efforts to distance himself from this argument depends on a caricatural contrast:

I was especially distrustful of the argument that a society had to have a comprehensive, all-answering ‘ideology’ in which all the members of the society shared. At the same time, I thought that some sort of very general legitimatory beliefs were necessary for a society, if the authority which was indispensable to its stability was to be effective and if society was not to become a war of each against all.

In another memoir, he admitted that in the 30s he had “accepted that beliefs were a constitutive part of society and that modern Western societies had undergone a dangerous attrition of belief.” His own contributions to Gerth’s charisma thesis, and his praise for

234. Ibid xxxii.
235. Ibid xxxi. He adds: “Very much of the intellectual tradition in which I grew up put the dissolution of consensus into a very prominent position. As I began to find my own way, the inadequacy, descriptively and theoretical, of this view of modern society became apparent to me. On the other side, many of those writers who discussed consensus in a way which did not entail disintegration, spoke of it in such a schematic and even simplifying way that it was clear that nothing like the consensus they described existed in modern societies” (xi).
237. Ibid x.
Mannheim’s argument that the Nazis had exploited unmet wish-fulfillments, suggest that he, too, worried about the fascist appeal to a belief-hungry populace.239

The effort to rescue Shils’s interwar thought from the enormous condescension of his posterity may be worthwhile for its own sake, but it also helps clarify the otherwise obscure sources for Shils’s postwar “mass society theory” construct. Once Shils had abandoned his dissensual view of modern, and especially Anglo-American, societies; once he had identified, in the 1950s criticism of mass culture, a spent Prometheanism; and once he had recognized intellectual discontent itself as a dangerous source of consensus-erosion—once all of these elements were in place, he turned to the arguments that had, after all, surrounded him before the war, most prominently Mannheim’s claims about “mass society.” To Shils, these emigré analyses of fascism and modern societies seemed the natural antecedents to Dwight Macdonald’s elitism. Based on his own, peculiar intellectual trajectory, he was able, in a manner, to trace their views back to the readings of his youth—back all the way to the French Revolution. Shils’s special sensitivity to the disintegrative thesis—the core tenet that earns one a place in the “mass society theory” ranks—owes to his own, since-abandoned fears. There’s something about Shils’s sensitivity in this respect that’s not unlike the post-apostasy suspicion of Communist infiltration so vividly on display in Cold War, God-thatfailed anti-Stalinism.

This line of argument shouldn’t be taken too far, but once Shils came to identify intellectual crankiness about social disintegration with the threat of real social

239. In just this context—the issue of meaning-dependence and the Nazi provision of coherent belief—Shils indicates that, in his reading of Hans Freyer’s work, he “learned a good deal from despite his Nazi sympathies” (“Karl Mannheim” 225).
disintegration, the failure to recognize the invisible lines of consensus in society suddenly became tantamount to *fraying* them. Society’s loose consensus depends on public belief, Shils came to argue, which in turn depends on the social picture put forward by intellectuals. This was, in the end, the treason of the intellectuals: Their doctrines of breakdown were untrue and deadly. Even the tragic Weimar collapse itself could seem to Shils, in these later years, as a kind of self-immolation: “It seemed such a pity to me that the intellectuals should have been responsible for destroying a society which in so many respects had conferred such benefits on intellectuals.”

CHAPTER THREE
Shils’s Discovery of the Small Group

Shils returned from wartime London in 1945 with a brand-new set of scholarly coordinates. His older preoccupations with intellectual discontent and social order weren’t displaced but rather reframed by his overseas breakthroughs. The two main revisions in his thought—and they were related—had to do, first, with his “discovery” of primary group solidarity in the Wehrmacht and, second, his emergent awareness of the particular social anchor of the English tradition of political civility. The first prompted him to revise his pre-war views on Gesellschaft fragility in favor of a more complicated picture of social order that, put baldly, recognized the persistence of certain Gemeinschaft-like features. English civility was, of course, more proof of this kind of persistence, but here Shils observed a peculiar and precious achievement that was not spread about all societies. He became a vigorous defender of the unseen traditions of English, and in modified form, American “plural” politics—and it was here, especially, that his fixation on intellectual carping was revived. His old recognition of literary dissent came to seem, especially after the war, a threat to the delicate virtues of Anglo-American politics. Shils’s two wartime revelations—the one general, the other particular—are certainly complementary, but they also pulled him in different directions. The broader project of getting at the constituents of social order everywhere had to make room, however, for the specific defense of English and American pluralism. When fellow intellectuals attacked, for example, American popular culture in the 1950s, he roused himself for often polemical counter-parries. One tactic was to assign his interlocutors a
discredited heritage. Like a novelist, he gave them the history he knew—and it was a partial, often misleading lineage that he put to paper.

After the war, Shils seemed ready to establish himself as a leading sociologist. He burst out of his war service in a newly confident sprint, eager to build on an otherwise local reputation for brilliance. His publication pace picked up, and he produced, in the years after the war, a sweeping survey of American sociology, more work on primary groups, Weber translations and commentaries, a second attempt to understand Sorel and his regenerational ethic, and the boldly ambitious theorizing with Parsons. In the weeks just after Hiroshima and Nagasaki, Shils joined with the emigré nuclear physicistLeo Szilard in Chicago to put on an impromptu conference, and to establish a more lasting vehicle for nuclear researchers’ political efforts, the *Bulletin of the Atomic Scientists*.

Through Szilard, Shils met his life-long friend Michael Polanyi in 1946, an advocate (among other things) for the autonomy of science and a critic of Mannheim’s planning regime. While still in wartime London, Shils had already been exposed to the aggressive assault on Mannheim’s planning proposals by Friedrich Hayek and especially Karl Popper—who, from New Zealand, had published a pair of papers in the London School of Economics’s *Economica* that left a strong impression on Shils. In 1946, shortly after Popper joined the faculty, Shils was appointed a half-year reader in sociology at the LSE, became close to Popper, and was further immersed in the ongoing LSE critique of planned order. In this frenetic stretch of his academic career—1946 to 1950—Shils continued his research on primary groups in the military, first on the Soviet army and then on American forces. His late-40s collaboration with Talcott Parsons was intended as
a systematic extension of his small group findings, in terms of their implications for social order more broadly. Shils would, in the early 1950s, pull back from his Parsonsian framework and re-emerge in the second half of the decade with a rich and original account of social order that was to remain his basic picture from then on. But in the heady years of the late 40s, Shils had become a prominent, trans-Atlantic champion of social scientific promise. Like so many others at the time, he had the impression that Minerva’s owl was set to take flight.

The confidence of post-war social science was the product, in large part, of the wide-scale participation of social scientists in the U.S. government’s war effort. Hundreds of social scientists temporarily left their academic posts during the war, to take up direct employment or consultancies for dozens of government and military agencies. Scholars, most of them in Washington, were scattered about an always-evolving acronymic tangle of programs and departments—including, most prominently, the Army’s Research Branch, the Office of Strategic Services, the Office of Wartime Information, and the Library of Congress, but also the Departments of Justice and of Agriculture, the Federal Communications Commission, and many others. Certain research innovations and accomplishments—notably the “American Soldier” studies in the Research Branch—contributed to the postwar social scientific excitement. Perhaps more importantly, the employment overlap of constantly shuffled scholars produced networks of contacts, friendships, and acquaintances that proved, after the war, to be of extraordinary importance to many disciplines and to various lines of scholarship—including, as it turned out, mass communication research itself.
As part of this social scientific mobilization, Shils went to work in the London Mission of the Office of Strategic Services (OSS) shortly after the agency was created in June 1942 out of the short-lived Coordinator of Information office. The OSS was directed from Washington by the legendary William “Wild Bill” Donovan, who went on to reconstitute the OSS in the early Cold War as the Central Intelligence Agency. Shils worked for the OSS’s Research & Analysis (R&A) Branch, which served as a kind of intelligence think tank—never truly influential—that employed an extraordinary range of scholarly talent. Over 900 academics worked for the R&A by the end of the war, including Perry Miller, Arthur Schlesinger, Jr., Felix Gilbert, Paul Baran, Paul Sweezy, Carl Schorske, H. Stuart Hughes, and many others.

It’s not clear how Shils was initially recruited to the Branch, though he may have benefitted from a Parsons referral. (Parsons was friendly with the director of the R&A Branch, Harvard historian William Langer; he also served on the R&A “board of analysts” which, in cooperation with Langer and his predecessor James Baxter III, recruited the early staff through informal networks.)

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1. Barry M. Katz, *Foreign Intelligence: Research and Analysis in the Office of Strategic Services, 1942-1945* (Cambridge, MA: Harvard University Press, 1989) 9. The Coordinator of Information office, created in July 1941 under the direction of the infamous William J. Donovan, was plagued by jurisdictional rivalries and an unclear mandate that included both propaganda and intelligence gathering. On June 13, 1942, the office was split, with Donavan and the intelligence function becoming the OSS, and with the propaganda responsibility moving to the newly created Office of War Information, which also absorbed other agencies including the Office of Facts and Figures in July.


4. Seven R&A historians went on to the presidency of the American Historical Association (Katz, *Foreign Intelligence: Research and Analysis in the Office of Strategic Services, 1942-1945*). As Barry Katz observes, the R&A sheltered "a community of scholars of such eminence and originality as to constitute a phenomenon unique in modern intellectual history."

5. Katz notes that the board was drawn from the “eastern academic mandarinate” (Ibid).
London R&A office, which included Shils, Schlesinger, Sweezy, Miller, and others, thought of itself as a rival to the much larger Washington office. As Felix Gilbert remembered, “We were a rather close-knit group and we considered the Washington office as somewhat unrealistic.”

In late 1943, Shils was assigned to a new and fast-growing Psychological Warfare Division (PWD), created under Eisenhower's Allied command (the Supreme Headquarters Allied Expeditionary Force (SHAEF)). It was here, with the PWD, that Shils conducted his research on German prisoners of war—the research that helped prompt him to overhaul his then-dissensual picture of modern society. Shils and his nominal superior, Henry Dicks, professor of psychiatry at London’s Tavistock Institute, gathered a small staff together for an Intelligence Section within the PWD that, at its peak, employed seven full-time analysts. Among the seven were Daniel Lerner and Morris Janowitz, both of whom had been working as civilians in Harold Lasswell’s content analysis outfit, the War Communications Project housed in the Library of Congress. Both men enlisted as Army privates and were quickly assigned to Shils’s and

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6. Ibid. Shils became close with a young sociologist, Barrington Moore, Jr., who joined the staff.
7. Ibid 80. The Washington R&A office was known around the capital as the “Chairborne Division.” Its Central European section included, famously, Frankfurtscholars Herbert Marcuse, Franz Neumann, and—as a consultant—Otto Kirchheimer. (For an overview of the Frankfurt involvement, see Barry M. Katz, "The Criticism of Arms: The Frankfurt School Goes to War," Journal of Modern History 59(3) (1987).)
8. For an insider’s overview of the PWD, see Daniel Lerner’s breathless postwar memoir (Sykewar: Psychological Warfare against Germany, D-Day to VE-Day (New York: G. W. Stewart, 1949)). Lerner would go on, in the early 50s, to play a prominent role in Lasswell’s elite symbol studies and then, in the mid- to late 50s, to participate in the new, Cold War field of “international communication,” in which clandestine government work was often repackaged for academic publication. On Lerner’s The Passing of Traditional Society, see Rohan Samarajiva, "The Murky Beginnings of the Communication and Development Field: Voice of America and The Passing of Traditional Society," in Rethinking Development Communication, eds. Neville Jayaweera, Sarath Amunugama, and E. Tāi Ariyaratna (Singapore: Asian Mass Communication Research and Information Centre, 1987).
9. Lerner, Sykewar: Psychological Warfare against Germany, D-Day to VE-Day 76.
Dicks’s PWD Intelligence unit. The Section’s charge was to assess captured Wehrmacht soldiers’ motivation to fight—to investigate, for example, the impact of the Allies’ behind-the-lines propaganda, or the rank-and-file soldiers’ commitment to Nazi doctrines. Shils and Dicks drafted a “famous” and certainly elaborate prisoner-of-war questionnaire—the product of “many months of laborious investigation”—that interrogators used to question the soldiers.  

Shils and Dicks (with the “close” collaboration of Janowitz) were focused, especially, on the question of tenacity: Why did German soldiers maintain the fight, and with such disciplined resolution? Their obstinacy seemed to defy all the battlefield indicators that pointed, by 1943, to near-certain Allied victory. If the German troops were resigned to inevitable defeat—Shils and Dicks found that they indeed were—then why didn’t they desert, surrender, or collapse? The mystery was only made more shadowy by the finding that soldiers, for the most part, were not National Socialist faithful, nor were they especially patriotic. Their rather sturdy morale could not be explained by delusions of battlefield optimism nor by fight-to-die belief commitments. 

Dicks, a psychiatrist partial to psychoanalysis, latched onto manliness as an...
explanation for all that soldierly tenacity. He argued, in essence, that battlefield bravery, even especially in the face of certain defeat, was the product of sexual competition among the men. Shils had some initial sympathy for Dicks’s psychoanalytic thesis, and studied Freud’s Massenpsychologie und Ich-Analyse (1921) [Group Psychology and the Analysis of the Ego] in search of further insight. But he came to regard manliness itself as a red herring that obscured the real source of Wehrmacht resolution. The solidarity of the soldiers’ smallest units had “impressed” Shils—their loyalty to one another, their sense of camaraderie, their desire for esteem in their fellows’ eyes. Shils came to regard these small-group attachments as the key fuel that kept the soldiers fighting. The Wehrmacht itself, Shils argued, could be conceived as unit-sized solidarities knit together—as a network of cohesive small groups.

In a number of different memoirs, Shils claimed that the soldiers’ fidelity to one another had prompted him to think about Hermann Schmalenbach’s description of the “Bund” anew. Shils had first encountered Schmalenbach in the course of translating Ernst Fränkel’s Der Doppelstaat around 1940, which prompted him to read Schmalenbach’s never-translated 1922 essay, “Die soziologische Kategorie des Bundes” shortly after. Schmalenbach described the Bund as a kind of Gemeinschaft, though

14. Shils, "Introduction," in Center and Periphery: Essays in Macrosociology xxiii-xxiv. Shils adds that Dicks was “a man of warm and understanding heart, even of enemy prisoners of war” (xxiii).
15. Ibid.
without the bonds of kinship and fixed location that characterized traditional community. According to Shils, Schmalenbach’s modified and modern version of Gemeinschaft made him think again upon Charles Horton Cooley’s notion of the “primary group.”20 In the 1930s, Shils had dismissed as naïve Cooley’s turn-of-the-century expectation that the United States could become a primary group writ large.21 But in light of his German observations and Schmalenbach’s stress on the modern persistence of affective ties, Shils came to view primary-group bonds as an essential component of Wehrmacht resistance. Shils also claimed that the Western Electric Hawthorne studies, especially the book-length reflections by Elton Mayo and T.N. Whitehead that Shils had encountered well before the war, took on suddenly new relevance for their emphasis on small group emotional sustenance in the workplace.22 Shils even found support for the ongoing psychological importance of group esteem and membership in Freud’s Massenpsychologie itself.23

Shils was, of course, also preoccupied with the broader question of social order,

23. Ibid.
and the primary group seemed to help explain how it is that modern societies manage to
hang together. Even if large-scale societies couldn’t take on the properties of small
groups—and Shils remained skeptical of the vision he ascribed to Cooley, that of one big
primary group—the overall stability of these societies depend, nevertheless, on a
substrate of small group loyalty. Though in later accounts Shils admitted that he had had
trouble in these years explaining how primary group ties translate into national-scale
order, he became convinced that these groups mediated—absorbed and redeployed
somehow—the appeals of ideology and national symbolism. He did not dismiss the
importance of National Socialist belief, patriotic pleas, nor the bureaucratic chain of
command; but for most of the troops, these weren’t the salient factors in their will to
fight.  

In Shils’s memoirs, he implied that his primary-group findings in the war
undermined the foolish view that Nazi success owed to its provision of purposeful
belief—without, however, admitting that he himself only recently held a view like this. He
too, of course, had been fearful of the meager allotments of meaning meted out by
modern liberal societies, fearful that this scarcity of belief would lead to social
disintegration or demagogic manipulation. In this sense, the primary-group research
represented a break in Shils’s thinking about social order: No longer did the

25. Shils: “This helped to settle for me the problem raised by the laudators of the totalitarian societies
when they wrote of the ‘faith to live for’ or of the ‘ideology’ which allegedly pervaded those societies”
("Introduction," in Center and Periphery: Essays in Macrosociology xxv). In 1988, Shils claimed that
his findings were “entirely contrary to what had been commonly thought among the willing and
unwilling admirers of the ‘faith’ aroused by totalitarian regimes in contrast with what they criticized or
deployed as the ‘loss of faith’ of the liberal–democratic societies” ("Center and Periphery: An Idea and


*Gemeinschaft*-to-*Gesellschaft* account of modernity, even in its tonally neutral or upbeat versions, seem an adequate description of actual integration. Shils’s initial concern with primary-group mediation would, over the 1950s, become much more complicated and explicit, but it was these wartime findings—filtered through Schmalenbach and Cooley—that led Shils to definitively reject the two-stage disembedding narrative that had, only years before, seemed convincing.

Shils returned to Chicago shortly after V-E Day with Janowitz, whom Shils had convinced to take up graduate study in sociology.26 Over the winter and spring, Shils and Janowitz wrote their classic paper on the PWD findings, “Cohesion and Disintegration in the *Wehrmacht* in World War II,” published in early 1948.27 The paper argues that the “extraordinary tenacity” of the German soldiers, which seemed baffling in light of the “general failure of ideological or secondary appeals,” could be explained however by primary group solidarity.28 The “determined resistance” of the German soldier was motivated, the authors explain, by the “steady satisfaction of certain primary personality demands” met by the smallest units of the *Wehrmacht*.29 Shils and Janowitz argue that basic social-psychological necessities—“organic needs” like affection and esteem—are

27. "Cohesion and Disintegration in the *Wehrmacht* in World War II," *Public Opinion Quarterly* 12(2) (1948). The paper has been reprinted many times. In terms of the actual writing, Shils suggests in one memoir that it was penned in spring 1946, and in another he refers more generally to “1945 and 1946” (see "Introduction," in *Center and Periphery: Essays in Macrosociology* xxiv; and "Introduction," in *The Constitution of Society* xi). Shils noted that, in the course of his and Janowitz’s Chicago-based postwar analysis of the interrogations, he “realized” that Elton Mayo, T.N. Whitehead, and F.J. Rothlisberger had, in their Hawthorne experiments, also “discovered the influence of small, closely knit groups on the conduct of their members in the performance of tasks set them from the outside” ("Personal, Primordial, Sacred, and Civil Ties" 136). In Shils’s important 1951 summary of the “rediscovery” of small group research, Mayo and the others figure prominently (see "The Study of the Primary Group," in *The Policy Sciences: Recent Developments in Scope and Method*, eds. Daniel Lerner and Harold D. Lasswell (Stanford, CA: Stanford University Press, 1951)).
29. Ibid 280.
provided by primary groups in organizations like armies, but also in modern societies more broadly. Here Cooley, Freud, Mayo, Whitehead, and Schmalenbach provide footnoted support for the article’s move to extrapolate beyond the military. Shils and Janowitz, without much elaboration, even suggest that primary group ties are a key element of the wider social order, and that certain pivotal figures within such groups serve as relay-points that connect society-spanning secondary symbols and beliefs to the personal affections of small groups. “It must be recognized,” Shils and Janowitz argue, “that on the moral plane most men are members of the larger society by virtue of identifications which are mediated through the human beings with whom they are in personal relationships.” Primary groups, to Shils and Janowitz, are modernity’s answer to scale and impersonality: These clusters provide otherwise-scarce affective sustenance while at the same time they remain linked up, through member-mediators, to the more expansive bonds of nationalism and belief.

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It wasn’t only Shils’s military research that displaced his interwar pessimism. The stumbled-upon cohesiveness of the Wehrmacht unit did, to be sure, contribute to the abandonment of his late 30s picture of modern society. The affective bonds of the alive-and-well primary group rendered Shils’s older picture moot; modern societies did not

30. Ibid.

31. Under the heading, “The Function of the Primary Group,” Shils and Janowitz note: “Modern social research has shown that the primary group is not merely the chief source of affection and accordingly the major factor in personality formation in infancy and childhood. The primary group continues to be the chief source of social and psychological sustenance through adulthood” After the passage, Cooley, Mayo, Whitehead, Freud and others are cited in a long footnote (283-84). Schmalenbach, in the context of Gemeinschaft persistence, is cited a few pages later.

32. Ibid 314.
look so *Gesellschaft*-like after all, and what’s more they seemed capable of providing, both directly and indirectly, a sense of belonging that could sustain social stability. But Shils’s revised view of society was helped along, too, by his encounter with English intellectual life, during and after the war. This encounter bolstered still further Shils’ revised take on modern social order *in general*. But his London experiences took on a related but distinct importance for his intellectual evolution, in a more *particular* direction. Shils’s English contacts and reading—as a wartime researcher and as a post-war LSE sociologist—exposed him to the Anglo-American tradition of plural politics, which Shils came to defend as a fragile and precious achievement.

The older, more downcast view of modern life that Shils had embraced before the war was challenged, then, on two complementary fronts: Modern societies, for one, *do not* teeter on the edge of chaos after all, and in fact manage to integrate themselves with the help of “leftover” *Gemeinschaft* elements like the primary group. The other new empirical reality for Shils was the *specific* political stability of the United Kingdom. With these two amendments in mind, Shils came to utterly repudiate the diagnosis that he once found frighteningly apt: that of an unchained mass casting about for purpose, within a dangerous and rhetorically deceptive democratic politics.

Shils’s amended picture of modern order was, in large part, an *empirical* revision. Even as his experience with German soldierly solidarity had forced him to think anew about modern disembedding, his exposure to British society and politics led him to reflect on the particular conditions that set Britain apart from its Continental peers. His reflections on the British tradition of civility weren’t elaborated in published form in his
wartime and post-war LSE period (1946-1950), but were instead germinating in observation and close historical reading. (Shils’s first sustained treatment of British and American pluralism came a few years later, in his remarkable, public intellectual denunciation of McCarthyism, *The Torment of Secrecy* (1956).)\(^{33}\) In a 1975 memoir, Shils recalled reading deeply in the literature on religious dissent and toleration in seventeenth-century England, though he never published in the area.\(^{34}\) He singles out John Locke’s *Letters Concerning Toleration* (1689) and Maurice Cowling.\(^{35}\) Shils’s studies of English religious toleration helped to shape his emerging attentiveness to an English tradition of elite and civil governance that had, since the eighteenth century, been adapting successfully to the emerging reality of popular input.\(^{36}\) The unspoken limits on political discussion—the “rules of the game”—that Shils observed in contemporary British politics with such admiration in *Torment* were for Shils the modern expression of a traditional, elite culture of civil politics. The other wartime body of thought that contributed to, as he later put it, “the pluralism that I was discovering,” centered on the prescriptive pluralism of American progressive Mary Parker Follett, the sociologists


Robert MacIver, and the French legal thinker Leon Duguit. The idea that associations and informal groups act as an intermediary source of social stability in all modern societies, of course, was the argument put forward in Shils’s Wehrmacht essay. Follett’s claims, especially, were more normative than empirical, but it’s possible that Shils’s partial extension of his civility analysis from Britain to the United States owed something to these early pluralists.

Stephen Turner makes a highly suggestive case that Shils was also exposed, while in London, to a loose “conversation” that emphasized the ineffability of tradition over the total and rationalist tenets of ideology. Central to the conversation, according to Turner (who himself draws on the recollections of J.P. Mayer), were Michael Oakeshott, T.S. Eliot, and Michael Polanyi, but also—“at a degree removed”—Karl Popper and Friedrich Hayek and “more distantly yet,” Isaiah Berlin, Christopher Dawson and Mayer. Turner argues that the discourse formed as an inchoate but profound defense of liberal democracy against the totalizing claims of fascist and Marxist ideology: “Rationalism, reductivism, and the closure characteristic of ideological systems was the error they sought to avoid; explaining the rise of ideology was for them a problem of explaining a pathology.”

37. "Introduction," in Center and Periphery: Essays in Macrosociology xxiii; and "The Study of the Primary Group" 47. In the 1951 essay, Shils refers specifically to Follett’s The New State (New York: Longmans, Green, 1918) and MacIver’s Community and the Modern State (Oxford: The Clarendon Press, 1926). Harold Laski, who was arguably the most important proponent of a pluralist politics in the late 1910s and early 1920s in his early years, before leaving Harvard for Britain and Marxism, is left out of Shils’s accounts. Laski, of course, was by the late 1940s one of Shils’s LSE colleagues, and an opponent on the left. Shils does, in his mostly dismissive 1994 reflection on Laski, refer in passing that he called himself a pluralist (“The Career of Harold Laski,” New Criterion 12(8) (1994) 24).

38. Turner, ”The Significance of Shils” 131-37. He admits that the conversation wasn’t so much a group as a “current of thought within which there were a large number of personal relationships” (131).

39. Ibid.

40. Ibid.
critique of the ideological cast of mind, as well as his related defense of liberal democracy—which Turner takes to be the central feature of Shils’s thought, a neglected alternative to the criticisms of liberal democracy advanced by Habermas and Foucault.\footnote{Ibid 127.}

Turner’s argument is brilliant and fascinating, but his claims for the “conversation’s” influence on Shils need to be qualified, at least in the absence of additional evidence. Turner’s stress on Oakeshott and Eliot, in particular, is at least chronologically suspect; though it is fair to assume that Shils read Oakeshott and Eliot’s key works from this period—Shils, after all, read everything—there is very little indication that either thinker entered the stream of Shils’s published thought until much later.\footnote{In Turner’s defense, and as he also notes, Shils’s influences were often of a “sleeper” kind—submerged for years only to appear decades later as a central aspect of his thought.} Shils openly acknowledged his debts to Eliot in his 1972 lectures on tradition, but earlier postwar references to Eliot are less flattering.\footnote{Shils, \textit{Tradition} (Chicago: University of Chicago Press, 1981). See, for example, "British Intellectuals in the Mid-Twentieth Century" 135.} Neither, moreover, appeared in his main intellectual self-narratives, nor in his 1982 accounting of intellectual debts. Later in life, it’s clear that Shils admired both Eliot and Oakeshott, though he seems to have never formed a friendship with either.\footnote{See, for example, Gould, "Shils, Edward (1910-1995)," \textit{Government and Opposition} 30(2) (1995). As Shils’s British friend and colleague Julius Gould remembered in reference to Oakeshott (and Elie Kedourie): ‘I know how much he admired both of these contemporaries; but, for whatever reasons, there seems to have been little occasion for personal or convivial familiarity with them” (247).} In a 1994 reflection on Harold Laski, Shils referred to Oakeshott as a “genuine and deep ruminator of original conservative convictions,” but there don’t appear to be any references to Oakeshott in his postwar publications and through the 1950s. It is significant that Oakeshott was appointed to Laski’s old LSE chair the year after Shils’s own tenure there ended.
It seems more likely, moreover, that Shils’s aversion to ideological politics evolved out of his own undergraduate engagement with Sorel and the disgruntled intellectual, though of course his restatement of these old themes—best expressed in his 1950 introduction to Sorel’s *Reflections on Violence*—was probably influenced by the wartime conversation to which Turner refers. The particular channels of influence, however, seem not to have been Eliot nor Oakeshott—certainly not Berlin—but instead emigrés like Popper, Hayek and above all Polanyi. And their influence came, in large part, by way of a thorough debunking of Karl Mannheim and his diagnosis of distress.

The disintegrative picture of modern society that Shils, on observing soldierly camaraderie, had abandoned was, of course, Mannheim’s. His rejection of Mannheim drew significantly upon a direct and explicit intellectual assault on Mannheim by fellow emigrés to England. During the war—even while he maintained regular contact with Mannheim—Shils was exposed to an often vituperative dismissal of Mannheim’s work by Karl Popper and Friedrich Hayek, in the pages of the LSE journal *Economica*. After the war, when both Popper and Shils joined the LSE faculty—Hayek’s affiliation dated to 1931—Shils’s encounter with their critiques was deepened. And in these early postwar

45. Shils seems to have held Isaiah Berlin, however, in permanently low esteem (see Joseph Epstein, "My Friend Edward Shils," *American Scholar* 64(3) (1995) 109: “He didn’t consider Isaiah Berlin great, but merely charming, a man who often wanted courage because he was intellectually hostage to certain Oxford dons”).


47. Here’s Mannheim in *Man and Society* arguing the near-reverse of Shils’s new, happier take on order: “there was at the stage of the democracy of the few an intermediate body between the broad masses and the elites, as represented, for example, by the more or less constant electoral following and the different parties defined by the press…. The parties which in liberal mass democracy strive to attain some importance, turn, for these very reasons, towards these as yet unorganized masses and seek by appealing to emotional, irrational symbols, as these are understood by social psychology, to influence them in the desired direction” (quoted in David Kettler and Volker Meja, *Karl Mannheim and the Crisis of Liberalism* (New Brunswick, NJ: Transaction, 1995) 169).
years, Shils became close friends with yet another emigré Mannheim critic, Michael Polanyi. Combined, these sustained and sophisticated criticisms helped wrest Shils from his interwar, Mannheim-friendly intellectual coordinates.

There is no adequate history of this fascinating emigré dispute, in which Central European transplants to Britain carried on certain pre-emigration debates without, however, disclosing their Continental prehistory.\(^{48}\) Mannheim, Polanyi and Hayek were central figures within distinct but linked intellectual circles in Budapest and Vienna—circles that, in the broadest sense, were preoccupied with the fate, promise and failures of socialism in the early decades of the twentieth century. Budapest, in particular, sheltered a vibrant and complex intellectual world that included Polanyi, his brother Karl, Ervin Szabó, Georg Lukács, Oszkár Jászi and Mannheim. The turn-of-the-century Budapest scene deserves a much more nuanced treatment, but for our purposes it is enough to mention two self-conscious intellectual clusters—the Galilei Circle around the Polanyi brothers and the \textit{Szelmek} [Sprites] around Lukács and Mannheim—both influenced by the modified Sorelian syndicalism of Szabó’s \textit{Szindikalizmus és szociáldemokrácia} (1908) [\textit{Syndicalism and Social Democracy}].\(^{49}\) The Polanyis’ Galilei Circle, formed in


1908, was informed by their cousin Szabó’s critique, but also a commitment to Hungarian national culture.\textsuperscript{50} During the first world war, Lukács, Mannheim and others formed the Sprites, more overtly radical than the Polanyis’ group but also deeply critical of the 2nd International, Engels-Plekhanov current of Marxist materialism.\textsuperscript{51} By the time that the Sprites formed in 1916, Lukács had already undergone one of his intellectual conversions—from a tragedy-of-culture neo-Kantianism to a blend of Hegelian culturalism and Sorelian politics.\textsuperscript{52} This is significant to our story because Mannheim largely adopted Lukács’s Hegelian-syndicalist worldview—indeed, Mannheim’s “Soul and Culture” (1918) essay became the Sprites’ unofficial manifesto.\textsuperscript{53}

There was a great deal of overlap between the two circles, with Mannheim and Lukács as frequent visitors to the Polanyis’ salon and vice versa. Both groups of intellectuals, moreover, published in Oszkár Jászi’s journal *The Twentieth Century*.\textsuperscript{54} But the war and its aftermath, including the short-lived and brutally crushed Hungarian Bela Kun regime, drove the Budapest intellectuals in very different political directions. The Polanyi brothers became Christian social democrats, with Karl especially active in politics. Lukács, by contrast, had joined the Communist Party in December 1918, the Bela Kun government in 1919, and made a final break with his pre- and wartime aesthetic

\textsuperscript{50} “The Great Lie: Markets, Freedom and Knowledge [draft]” 4-5.
\textsuperscript{52} See Ibid 86-97. Through his studies in Heidelberg with Max Weber and Georg Simmel, he had come under the influence of neo-Kantianism and Dilthey’s *Geisteswissenschaften* tradition. But between the 1910 publication of *The Soul and Form* and his 1916 *Theory of the Novel*, Lukács abandoned the tragedy-of-culture view.
\textsuperscript{54} “The Great Lie: Markets, Freedom and Knowledge [draft]” 5.
politics in the landmark *History and Class Consciousness* (1923)—arguably the founding
text of the Western Marxist tradition.  

Mannheim, in response to the Lukács conversion, the war, and the failed
revolutionary regimes, ended up leaving for Heidelberg in 1919 to study with Max
Weber. At Heidelberg, Mannheim attended Weber’s seminars and also worked with
Emil Lederer; Weber and Lederer later sponsored Mannheim’s *Privatdozent* licensing on
the basis of his study of nineteenth-century German conservatism. Though he never
embraced political Marxism, Mannheim was greatly influenced by Lukács’s *HCC*, and
argued for the first time, in 1924, that political and cultural knowledge is socially
grounded. Mannheim enjoyed great professional success in Germany and, after a sped-
up naturalization, was appointed Professor of Sociology at Frankfurt in 1929. That
success, of course, was tragically brief; as a “foreigner” and a Jew, he was ousted from
his chair soon after the Nazi putsch, and fled to England and the LSE by way of the
Netherlands. On arrival in London, he had already written some of what would become

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55. *History and Class Consciousness: Studies in Marxist Dialectics* (Cambridge, MA: MIT Press, 1971 [1923]). *HCC*, of course, remained Hegelian, activist, and openly dismissive of 2nd International-style materialism, but did represent a break from his 1910s Hegelian “revolutionary culturalist” period.

56. Jay reports that Lukács’s sudden conversion came as a great shock to Mannheim (“The Frankfurt School’s Critique of Karl Mannheim and the Sociology of Knowledge” 175-76).


58. Ibid.

59. On the influence of *HCC*’s Hegelian-Marxist synthesis on Mannheim’s early sociology of knowledge, see "The Frankfurt School’s Critique of Karl Mannheim and the Sociology of Knowledge” 177. According to Jay, Mannheim’s debts to Lukács were, in 1924, openly acknowledged, but that by *Ideologie und Utopie* (1929) Lukács was relegated to a single footnote, and openly attacked in “Wissenssoziologie” (1931).

60. *Karl Mannheim and the Crisis of Liberalism* 3.

61. Ibid 145-59. He was invited to LSE by Harold Laski, the Marxist political scientist, as part of an organized effort to rescue eminent scholars persecuted by the Nazis (3). Dahrendorf reports that Laski and the committee nearly recruited Horkheimer’s Frankfurt Institute to the LSE, and that only last-minute objections over their Marxism from Robbins and Hayek prevented the absorption (*LSE: A History of the London School of Economics and Political Science, 1895-1995* 290).
Mensch und Gesellschaft (1935)—and had adopted the chastened, planning-centered liberalism that would become the other Central European emigrés’ intellectual target.

The Polanyi brothers, meanwhile, had after the war both adopted a Tolstoyan, leap-of-faith Christianity. Both were nominally socialist, but Karl was far more invested in radical politics, while Michael focused on a distinguished academic career in chemistry. After a visit to the Soviet Union in 1933, Michael became increasingly hostile to state involvement in the sciences. He emigrated to Britain the same year, taking up a chemistry post at the University of Manchester. Beginning with a caustic 1935 review of J.D. Bernal’s The Social Function of Science, Polanyi conducted an increasingly vigorous campaign against the Soviet-inspired scientific instrumentalism then in vogue among some British scientists. Polanyi had openly declared his liberal commitments in the 1935 Bernal review, which widened a rift with his brother Karl that had already opened over a 1934 dispute between their wives. Michael published a number of additional essays and reviews over the next few years in the same vein, which he collected in 1940 as The Contempt of Freedom. Mannheim, his old Budapest acquaintance and champion of wide-scale planning, had already become a predictable target of Polanyi’s pen.

Hayek’s own antipathy to Mannheim grew out of a linked, but distinct intellectual

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debate in 1920s Vienna, over the viability of socialist “calculation.” The dispute was both academic and political, and pitted an evolving Austro-Marxism against the Austrian current of neoclassical economics. In the 1910s, key figures in the Austro-Marxist cluster, most significantly Karl Renner and Rudolf Hilferding, had analyzed in great depth the trend toward state intervention in the economy. By the late teens, Renner, Hilferding, and fellow Austro-Marxist and Social Democratic Party leader Otto Bauer were proposing a state-managed economy as a key component of their evolutionist program. In a long 1920 paper, Ludwig von Mises challenged the very possibility of such a state-planned economy, on the grounds that pricing and valuation were too complex to be calculated. The “bewildering mass” of daily economic decisions, von Mises argued, renders any attempt at rational planning an act of futility. The debate continued throughout the 1920s; Vienna Circle philosopher Otto Neurath joined on the socialist side, as did Karl Polanyi, who published ripostes to von Mises in 1922 and 1925. Hayek, von Mises’s student, weighed in on the anti-planning side. Though not a participant, Karl Popper—a graduate student for most of the 1920s—was profoundly shaped by the debate.

When Mannheim assumed his post at LSE as a lecturer in sociology in 1933, Hayek was already a full professor of economics there. Hayek had joined the faculty in 1931 on Lionel Robbins’s invitation, and the pair famously spent the decade struggling

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against the Keynesian avalanche at Cambridge.\(^ {69}\) In 1935, Hayek edited a volume on the socialist calculation debate, *Collectivist Economic Planning*, which included Mises’s original 1920 paper.\(^ {70}\) He famously recognized certain weaknesses in the von Mises argument, and set out, over a series of essays published in the LSE’s *Economica*—beginning with “Economics and Knowledge” (1937) and collected as *The Use of Knowledge for Society* (1945)—to critique planned orders in a more systematic fashion.\(^ {71}\) Mannheim was his foil.

Karl Popper, from New Zealand, first published his *The Poverty of Historicism* (1957) as a series of *Economica* essays from 1944 to 1945. Mannheim was *his* foil. And Popper and Hayek both wrote mid-1940s polemics—*The Open Society* (1945) and *The Road to Serfdom* (1944)—with the theme that planning leads to totalitarianism. Mannheim was a significant villain in both books.

Popper, Hayek and Polanyi—whose 1946 *Science, Faith and Society* piled on still more abuse—all used their emigré acquaintance as a kind of intellectual punching bag. Mannheim, after all, was a known and local remnant of certain Central European tendencies that they had come to deplore. It was this bloodbath that Shils waded into.

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Not long after writing what was, on the fundamental points, a positive review of *Man and Society in an Age of Reconstruction*, Shils left for London in the fall of 1942 and went


straight off to visit Mannheim at his small, tidy house on the outskirts of the city.\(^1\) Over the war years, Mannheim was Shils’s frequent host, and the two men spoke often about Mannheim’s troubled LSE tenure and the battering of his many detractors.\(^2\) Shils himself, all the while, was reading these same critics, Popper and Hayek especially, and found their case against Mannheim rather convincing. After Hayek and Popper, and then Polanyi too, Shils had become a different sort of liberal—chastened not so much by the masses nor imminent societal breakdown, but instead by the perils of planisme hubris. From Hayek and Popper he absorbed what became, in some ways, a Cold War commonplace—that well-intentioned attempts to radically redesign the world have disastrous, unintended consequences. Already cognizant of the Weberian warnings against academic prophecy and the ethic of conviction, Shils in the presence of Hayek and Popper’s relentless assault on “scientism” was stripped of any lingering fondness for rule-by-social-science. And if Shils had any leftover sympathy for the view that knowledge is social and historical in character—if he hadn’t, that is, already converted to Parsons’s analytic realist view—then Popper’s elaborate dismembering of “historicism” killed that off too. Indeed, “scientism” and “historicism,” deployed with the same peculiar definitions that Hayek and Popper had given these terms, became for Shils in the postwar years routine pejoratives. Shils even embraced Popper’s falsificationist philosophy of science as an awkward partner to Parsons’s high deductivism. In the case of Polanyi, Shils found a congenial defense of scientific autonomy, along with a rare appreciation for the ongoing importance of intellectual tradition. All of these

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2. Ibid 231-33.
arguments—against philosopher-king confidence, for example, or against a pragmatic view of science—were formulated with Mannheim as the explicit counter-exemplar. Shils’s once-feverish excitement with Mannheim and his thought was, over these years, fully exhausted. By the time Mannheim died in 1947, Shils had apparently drafted a caustic goodbye-to-all-that, which Julia Mannheim successfully quashed. Mannheim would remain, until Shils’s own death, a haunting presence in his intellectual life, to judge by the drastic tonal swings in published commentary over the decades. Shils’s late-life portrait of his one-time intellectual idol, published in 1995, is wistful and emotionally reverent—an implicit expression of regret for the many nasty treatments over the years.

On Shils’s visit to Mannheim in the fall of 1942—his first face-to-face encounter with the author of his two book-length translations—the conversation centered on Mannheim’s American reputation; according to Shils’s later account, Mannheim’s first words were, “What do they think of me in American?” Mannheim had been stung by Alexander von Schelting’s harsh 1937 review of Ideologie und Utopia, and by Hans Speier’s more respectful but dismissive review—all the more painful on account that Mannheim had recommended Speier, his former student, as a sympathetic reviewer to Louis Wirth. According to his memoirs, Shils attempted to allay Mannheim’s fears about

4. “Karl Mannheim.”
5. Ibid 231. On Mannheim’s decade-long anxiety about his American reception—letters indicate that he hoped to emigrate to a prominent post there—see Kettler and Meja’s detailed account (Karl Mannheim and the Crisis of Liberalism 194-246). Mannheim had, in this vein, altered much of the language of the translated Ideology and Utopia to reflect the American pragmatist tradition on the hope that this would aid its American embrace (214-16).
what had been, however, a disastrous American reception. Shils later claimed to have been struck, on this first visit, by Mannheim’s “apprehensive solicitude about the fate of liberal European civilization,” but this reaction was probably layered on to his memory retroactively, as Shils too was anxious at the time.

Shils would dine with the Mannheims about once a month during the war years, by his own account. Shils remembered the dinners with real fondness, as “invariably delicious” and always accompanied by a bottle of fine wine. Mannheim would often invite a glittery assortment of intellectual guests to these dinners, including Arthur Koestler, Arthur T.M. Wilson, and the psychoanalyst Edward Glover. Mannheim and Shils’s private conservations, however, were rarely on substantive sociological topics, but tended to focus instead on Mannheim’s many LSE travails. He was painfully aware, certainly by 1942, that many of his colleagues were hellbent on ridding him from the School. Mannheim’s immense popularity in the classroom was part of the problem; as his former assistant Jean Floud recalled, many LSE faculty “reacted furiously to him … and considered him a charlatan who confused the young.”

Morris Ginsberg, the School’s Professor of Sociology, was especially hostile; only Bronislaw Malinowski and an

7. Ibid.
8. Ibid.
9. Ibid.
10. Ibid 233.
11. In his Mannheim memoir, Shils notes that some of these dinner guests became his friends, and adds—without however specifying names—that “many of them had a tremendous intellectual and professional influence on the subsequent course of my life” (“Karl Mannheim” 233).
increasingly marginal Harold Laski offered any support.\textsuperscript{14}

Mannheim’s problem, in part, was that his unflagging proclamations of crisis failed to resonate among the other faculty; nor were his grandiose calls for social scientific planning well-received.\textsuperscript{15} His reception was damaged, too, by his impolitic criticism of British sociology, published soon after his arrival, which irritated Ginsberg and others for its claim that the field was theoretically barren.\textsuperscript{16} Mannheim also unwittingly assumed the mantle of a British sociological tradition—centered on Patrick Geddes and Victor Branford—that had vigorously pushed social science planning; Ginsberg had been busy trying to reclaim the discipline from this tradition, which, however superficially, resembled Mannheim’s approach.\textsuperscript{17} By 1938, when A.M. Carr-Saunders became the director, the School repeatedly appealed to the Rockefeller Foundation—which partially funded Mannheim’s post—for assistance with his

\textsuperscript{14} On Malinowski, see \textit{Karl Mannheim and the Crisis of Liberalism} 182. In the second half of the 1930s, Laski greatly diminished his role at the School after Lord Beveridge, the School’s director, forced him to curtail his popular radical writings under pressure from alumnæ and trustees (see \textit{LSE: A History of the London School of Economics and Political Science, 1895-1995}, 197). According to Donald MacRae, T.H. Marshall also attempted to mediate between Mannheim and Ginsberg.

\textsuperscript{15} Floud described the general aversion to Mannheim’s “preaching at large the gospel of salvation through sociology” (Floud, “Karl Mannheim” 281).

\textsuperscript{16} In the “British Sociology” paper, Mannheim strongly implied that he could fill the field’s theoretical lacunae. In a July 1933 letter to Wirth announcing his acceptance of the LSE post, Mannheim explained that his choice of LSE over a competing New School offer came down to Laski’s claim that it would be Mannheim’s task, in cooperation with Morris Ginsberg, to “finally and truly to establish sociology in England....” (quoted in Ibid 177).

\textsuperscript{17} For an excellent discussion of this tension, see \textit{Karl Mannheim and the Crisis of Liberalism} 182-85.
relocation.\textsuperscript{18} Mannheim was aware of the attempts to move him along.\textsuperscript{19} It was only in 1945, however, that Mannheim finally left the School to take up a professorship in the sociology of education at the Institute of Education.\textsuperscript{20}

According to Shils, Ginsberg’s hostility was a painful blow to Mannheim’s dignity. He was, according to Shils, sensitive about his status as a lecturer, that he hadn’t assumed a professorship with the attendant privilege to give, for example, the required introductory course.\textsuperscript{21} Shils, with more sympathy than the passage suggests, claimed that Mannheim “wanted to be a Kant”—that student popularity was not enough for him.\textsuperscript{22} His sense of isolation was intensified by the criticisms from Hayek and Popper; Shils later claimed that Popper’s 1945 LSE appointment was “felt to be an injury” to Mannheim, that “Hayek held him in contempt too.”\textsuperscript{23} According to Shils, “Mannheim thought—rightly—that Hayek’s \textit{Road to Serfdom} was directed against him.”\textsuperscript{24}

Mannheim’s close association with “the Moot”—the circle of clerical and lay intellectuals around the Christian thinker J.H. Oldham that included T.S. Eliot, A.D. Lindsay, and many others—also ended in a failure brought on, in part, by Michael

\textsuperscript{18} Kettler and Meja quote a 1938 memo from a Paris officer of the Rockefeller Foundation: “The School is more interested now in developing empirical sociology in England and Mannheim has not the particular qualifications necessary for this new orientation… As indicated by Carr-Saudners, they would certainly carry him for a year or two until he has an opportunity of obtaining another position. They wish, however, to give notice immediately to Mannheim that he cannot expect his position at the school to be indefinitely maintained” (Ibid 188-89).
\textsuperscript{19} In a 1938 letter to Wirth, Mannheim complained that he didn’t get “intellectual support” from his peers and that Ginsberg was attempting to have him moved (quoted in Ibid 182).
\textsuperscript{20} Ibid 252.
\textsuperscript{21} “Karl Mannheim” 226-27.
\textsuperscript{22} Ibid 228.
\textsuperscript{23} Ibid 232. The Popper appointment came, on Hayek’s recommendation, shortly after the publication of \textit{The Open Society} (1945), which included the first “Poverty of Historicism” essay. Shils recalled that Mannheim was wounded, because “Popper clearly had him in mind as their target” (232).
\textsuperscript{24} Ibid.
Polanyi, who Mannheim himself had introduced to the circle. Mannheim attended the second Moot gathering in September 1938, and then all of the group’s subsequent meetings, except its very last: By chance, he died the same weekend in 1947. The Moot, and the establishment English gentility that it represented, was Mannheim’s final answer to the haunting question that he had first posed in Mensch und Gesellschaft: Who will plan the planners? Mannheim’s hopes for elite coordination, after 1938, were invested in the English gentleman and his traditional values; he was impressed, not unlike Shils, by England’s stability and successful integration of democracy with elite rule.

Mannheim presented most of his papers at the Moot before publication, and his calls for the crisis-driven intervention of the traditional elite were met with some sympathy, especially by Eliot. Mannheim’s more aggressive proposals to the group—like his call for a “revolution from above”—alarmed some of the Moot circle, however, as did his not infrequent references to the successful example of fascist regimes.

25. Karl Mannheim and the Crisis of Liberalism 256-58, 266-68.
26. Ibid 268.
27. Man and Society in an Age of Reconstruction: Studies in Modern Social Structure (New York: Harcourt, Brace & World, 1940) 74-75. Kettler and Meja report a late 1938 letter that Mannheim sent to Wirth, in which he excitedly claimed that the group seemed to be getting on board with his “mission” of elite planning (Karl Mannheim and the Crisis of Liberalism 251).
28. Ibid 257-60. In an otherwise caustic treatment of Mannheim, Shils manages to concede that one of his Moot papers, presented in 1943, evinced a growing sensitivity to the importance of religious belief: “The ‘primordial images’ which have directed the life experiences of men though the ages,” Shils goes on to paraphrase, “have vanished, and nothing has taken their place” (“Mannheim, Karl,” in International Encyclopedia of the Social Sciences, ed. David Sills (New York: Macmillan, 1968) 560). He also praised the paper in a 1975 memoir, the “closest he came” to dealing with consensus: “Unfortunately, although I saw him frequently during and after the Second World War, I did not have the problem sufficiently in focus in my mind to be able to draw him into discussion about it. I regret this very much now because I think this would have benefitted his own work and mine” (“Introduction,” in Center and Periphery: Essays in Macrosociology (Chicago: University of Chicago Press, 1975) xvii).
29. Karl Mannheim and the Crisis of Liberalism 250.
30. Kettler and Meja quote from his Moot papers: “We are too lazy to move. Hitler started with six people”; and: “The Germans, Russians and Italians are more advanced than we are in the techniques of managing modern society, but their purposes are wrong and even atavistic. We may look to elite groups in our society, e.g., the Moot, or enlightened Civil Servants, to use these techniques for
Polanyi’s first visit to the Moot in 1944 was, according to a participant, a “ding-dong battle” between the two Hungarian emigrés. In a second meeting attended by Polanyi, the two offered divergent takes on Eliot’s “Clerisy” paper—with Polanyi’s tradition-bound liberalism much better attuned to the Moot’s end-of-war mood.

Mannheim’s influence in the group subsequently waned. Shils later claimed in his memoir that Mannheim had been “very sensitive” to Polanyi’s criticisms. Shils recalled traveling in a taxi with Polanyi the day after Mannheim’s death, when Shils informed him of Mannheim’s passing. Polanyi made no reply. In a rare censure, Shils later wrote that Polanyi “was probably thinking of one of his philosophical conundrums and there was no clear category in his mind for Mannheim.”

Shils himself may have contributed to Mannheim’s sense of isolation. He remembered that Mannheim “was daunted by me,” and attributed the elder scholar’s meekness to Shils’s critique of his planning proposals. Shils had read the manuscript of what would, in 1943, be published as *Diagnosis of Our Times*. According to Shils, he criticized its vagueness and optimism, which Mannheim took “extremely amiably, at least in my presence.” Mannheim protested that the manuscript had already been delivered to the publisher, but that in the future, in Shils’s paraphrase, “he would heed different ends” (Ibid 261).

31. Ibid 256.
32. Ibid 267. As Kettler and Meja argue, Mannheim’s growing disappointment with the Moot “coincided with the rising influence of Polanyi” (266).
33. “Karl Mannheim” 234.
34. Ibid.
[Shils’s criticisms]; he made it seem as if he were a schoolboy and I was his teacher.”

Mannheim, Shils continued, used to “repeatedly seek my recommendations on the sociological literature he should read. He seems to have thought that I knew a great deal, much more than I in fact did.”

Mannheim in his English years was saturated in criticism, much of it harsh. The muted response to his cries of crisis, the widespread rejection of his planning proposals, were met by Mannheim with wounded but dogged seclusion. When Shils arrived at Mannheim’s home soon after hearing of his death in January 1947, he was embraced by Julia who declared, “Ginsberg killed him!” Mannheim’s emigré experience—more than half his scholarly life—was a tragic, drawn-out denouement to a brief but brilliant career, brought on by world-historical nightmares that were, for him, all too real. Shils himself seems to have come around to this view, eventually. At the conclusion of his 1995 memoir, Shils referred to the emigration as an “unmitigated catastrophe for Mannheim”:

Despite [T.S.] Eliot’s profound appreciation of his intellectual and moral qualities, and other strong indications that he was not entirely alone, nothing assuaged Mannheim’s anxiety about his status and his future. That was a great pity. He was a man of great intellectual and moral merit. He was a good person, capable of receiving and expressing affection, but he needed more than that. He had a genuine drive for truth and delight in intellectual activity. He said many things which are still worthy of mediation.

But a younger Shils, perhaps ambitious and certainly drained of any intellectual respect for Mannheim, reacted differently. According to Julia’s letters, Shils in 1948 showed her a draft of an article on Mannheim that he was set to publish; in a letter, she called the article “blind violence to somebody to whom you owe so much.” In another letter to her

37. Ibid.
38. Ibid 233.
39. Ibid 235.
40. The letter is date April 1948; quoted in Karl Mannheim and the Crisis of Liberalism 245-46.
sister later that year, she claimed that she had thwarted Shils’s plans to “publish an extremely nasty paper on Karl”—and it’s true that Shils’s first extended treatment of Mannheim did not appear until 1968.\textsuperscript{41} In the same letter to her sister, she claimed that Shils had refused to return the full manuscript of Mannheim’s uncompleted “Essentials of Democratic Planning,” leaving the “book … a torso.”\textsuperscript{42} (Presumably the draft was eventually returned: \textit{Freedom, Power, and Democratic Planning} was published in 1950, without reference to any missing portion.)\textsuperscript{43} Mannheim had lost all appeal to Shils by this time, and the sustained criticism of Popper, Hayek and Polanyi played no small role in the intellectual parting.

From 1942 to 1944, Hayek published his three “Scientism and the Study of Society” essays in \textit{Economica}.\textsuperscript{44} Drawing on his basic claims about the limits of social knowledge—claims first developed in his revised “socialist calculation” critique, “Economics and Knowledge” (1937)\textsuperscript{45}—Hayek labels “scientistic” the belief that total comprehension of the social world is possible, and that such knowledge can be used to engineer solutions to social problems. Hayek pointed to L.T. Hobhouse, Joseph Needham, and Mannheim as archetypical adherents to scientism.\textsuperscript{46} All three, he claimed, held the absurd and elitist tenet that some “super-mind”—usually their own—can

\textsuperscript{41} Quoted in Ibid 246; and Shils, "Mannheim, Karl," in \textit{International Encyclopedia of the Social Sciences}.

\textsuperscript{42} Quoted in Karl Mannheim and the Crisis of Liberalism 246.

\textsuperscript{43} \textit{Freedom, Power, and Democratic Planning} (New York: Oxford University Press, 1950).

\textsuperscript{44} "Scientism and the Study of Society I," \textit{Economica} 9(35) (1942); "Scientism and the Study of Society II," \textit{Economica} 10(37) (1943); and "Scientism and the Study of Society III," \textit{Economica} 11(41) (1944). The three papers were collected, with other writings, in \textit{The Counter-Revolution of Science} (Glencoe, IL: Free Press, 1952).


\textsuperscript{46} See, for example, "Scientism and the Study of Society III" 31.
understand the impossibly complex tangle of human life. Mannheim is singled out for
special abuse here, and Hayek goes so far as to attributes Mannheim’s scientism to his
sociology of knowledge which, he adds, is a “derivative of the ‘materialist interpretation
of history.’”47 When Hayek turns to the other element of scientism—social engineering—he attributes to Mannheim the “doctrine of technical supremacy” and notes that “[o]nce
again one of the best illustrations of this tendency is provided by K. Mannheim.”48 And in
his 1944 *Road to Serfdom*, Hayek writes of Mannheim’s *Man and Society*: “The
difference cannot be better illustrated than by the extreme position taken in a widely
acclaimed book on whose program of so-called planning for freedom we shall have to
comment yet more than once.”49

Popper’s three-part “The Poverty of Historicism” series, also published in LSE’s
*Economica* from 1944 to 1945, is in many respects a brief against Mannheim. His quarrel
is with “historicist” thinkers, though Popper’s definition of “historicism”—this is true too
of his and Hayek’s “scientism”—hardly resembles the term’s conventional meaning.
“Historicism” normally refers to the view that knowledge of human life has an
irreducibly historical character—the idea, often associated with Dilthey and the German
Historical School, that each age or national culture or event must be interpreted in its own
terms. Popper means something that is nearly the reverse: *His* “historicism” label applies

47. Ibid 31-32. Hayek’s reference here to Mannheim’s sociology of knowledge is, to say the least, a
stretch, since *Ideology and Utopia*’s recourse to social genesis undermines claims to total knowledge,
as the book’s many critics were at pains to emphasize. It’s true that Mannheim imagined a synthesis of
partial truths that, in some versions, is cobbled together by a relatively free-floating intelligentsia, but
he was always careful to employ the “relatively” qualifier. And it’s only true in a very indirect sense
that Mannheim’s sociology of knowledge is Marxist; Hayek’s reference here is misleading.
48. Ibid 34.
49. Ibid.
to any doctrine that claims to have uncovered laws of development, laws that explain the pattern of human history. Though conceding the logical contradiction, Popper claimed that historicists have typically also argued that social science should discover these laws and then use them to guide society. Comte, Hegel and Marx are his historical exemplars, but he devotes the most space and vitriol to Mannheim. In his second essay, for example, eighteen footnotes and well over half of the in-text quotations are Mannheim’s. He justifies this otherwise curious fixation with the claim that Mannheim’s *Man and Society* is a near-perfect stand-in for the whole deplorable doctrine: “This book is the most elaborate exposition of a holistic and historicist programme known to me and therefore singled out here for its criticism.” Popper contrasts Mannheim’s technocratic zeal with the proper “piecemeal” approach to social reform. The “utopian engineer,” he writes (with Mannheim interspliced), “aims at remodeling the ‘whole of society’ in accordance with a definite plan or blueprint; it aims at ‘seizing the key positions’ and at extending ‘the power of the State… until the State becomes nearly identical with society.’” (In a footnote to this last passage, Popper adds that the formula “is nearly identical with one by C. Schmitt.”) In Mannheim, he finds

50. Mannheim, especially in his sociology of knowledge phase, was arguably a historicist in the conventional sense, but certainly not in Popper’s terms. Popper probably featured Mannheim so centrally because he fit the second half of his erstwhile definition, in terms of social engineering. It’s also possible that Popper focused on Mannheim at least in part for Hayek’s benefit. Popper certainly knew, at least from publication, that Hayek often targeted Mannheim. The second “Historicism” essay, moreover, contains a number of almost obsequious references to Hayek especially, but also to Ginsberg, the Webbs, Tawney and Malinowski. Popper was appointed, of course, to the LSE shortly after the publication of these essays, and Dahrendorf reports that Hayek, upon reading Popper’s recent work, was instrumental in arranging the appointment (*LSE: A History of the London School of Economics and Political Science, 1895-1995* 422-23).

52. Ibid.
53. Ibid.
historicism and “Utopian dreams” in an “unholy alliance” that is all too common for historicist thinkers. Though Popper links the historicist mindset to the “totalitarian intuition” here, he greatly extends the argument that mandarin technocracies bring on tyranny in The Open Society and its Enemies (1945).

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Shils’s turn against Mannheim, even if the break was not explicitly noted until years later, was already obvious in his postwar publications. Some of this implicit notice got signaled through newly acknowledged debts to Popper, Polanyi and Hayek. None of the three contributed to Shils’s eventual, multifaceted account of social order, except—indirectly—Polanyi through his stress on intellectual tradition. Hayek’s claims for “spontaneous order” on analogy to the price system, and Popper’s similarly effortless notion of social self-regulation, could not, for Shils, carry the explanatory burden. But all three influenced Shils in important ways, with Popper and Polanyi leaving the more profound debts. All three helped turned Shils, for example, decisively against any

54. Ibid.
56. In a 1960 essay that attempted to explain the absence of a vibrant sociological tradition in Britain, Shils was quite dismissive of his one-time idol: “Karl Mannheim quickened the pulse of British undergraduate and foreign students for a time but he found little intellectual hospitality among his co-equal except for a few educationalists, journalists and literary men; he left no mark on British sociology” (166). He also disingenuously dismisses Man and Society: “It was a grandiose disquisition on epochal trends and the enthusiasm which it called forth among students sent very few of them into the field. (I do not recall that there was one native Briton among the few who did a little field-work under his sponsorship)” (“On the Eve: A Prospect in Retrospect,” in Essays on the History of British Sociological Research, ed. Martin Bulmer (Cambridge: Cambridge University Press, 1985 [1960]) 170.)
57. Stephen Turner suggests that Shils may have been influenced by Popper’s 1949 reflection on tradition and reason, which is certainly plausible though I found no proof of the influence (“The Significance of Shils,” Sociological Theory 17(2) (1999) 131). See Popper, “Towards a Rational Theory of Tradition,” The Rationalist Annual 66 (1949).
version of social scientific technocracy.\textsuperscript{59} Shils maintained close friendships with both Popper and Polanyi, though separately since the two emigrés had an early falling out.\textsuperscript{60} Shils was only acquainted with Hayek, though he helped to arrange Hayek’s 1950 appointment to Chicago’s Committee on Social Thought—which Shils had joined, on Hutchins’s invitation, in 1947. Like Polanyi, Shils was never an unqualified supporter of laissez-faire economics in Hayek’s mold, though he respected the kind of classical liberalism endorsed by Hayek (and also by his old mentor Frank Knight and future friend and Chicago economist Harry Johnson). But in the early postwar years Shils did indicate his general agreement with his LSE colleague’s knowledge-based critique of socialist planning. In a glowing 1948 review essay of Parsons and A.M. Henderson’s Weber translation, \textit{The Theory of Social and Economic Organization} (1947)—published in \textit{Economica}—Shils presented Weber as, in essence, a liberal critic of socialism.\textsuperscript{61} Weber’s political writings, according to Shils’s summary, were “strenuous efforts, written with remarkable passion and eloquence, to point the tactical path for a stable democratic, liberal (i.e., non-socialist)

\textsuperscript{59} As I discuss below, however, Shils’s often strident aversion to “scientism” exists in tension with some published remarks on social science policy, which are must more ambiguous and even tortured in the social scientist’s role (see “Social Science and Social Policy,” \textit{Philosophy of Science} 16(3) (1949)).

\textsuperscript{60} Popper, Polanyi and Hayek were all participants in the liberal Mont Pelerin Society, which Hayek founded in 1947, though Polanyi left early. Polanyi’s differences with Hayek were, in part, over the former’s adherence to Keynesian state intervention in the economy. On Polanyi and Popper’s relationship, see Philip Mirowski, “Economics, Science and Knowledge: Polanyi vs. Hayek,” \textit{Tradition & Discovery} 25(1) (1998).

\textsuperscript{61} Shils, “Some Remarks on ‘The Theory of Social and Economic Organization’,” \textit{Economica} (1948). The Parsons-Henderson translation comprised the first, systematic part of \textit{Wirtschaft und Gesellschaft} (Weber, \textit{The Theory of Social and Economic Organization} (New York: Oxford University Press, 1947)). Shils’s portrayal of Weber as a liberal critic of socialism is, if nothing else, partial; Weber was certainly a liberal of an especially anguished kind. And he did, of course, point to certain impossibilities in socialist doctrine—but also, it must be added, to severe limits to the potential of democracy in the sense that Shils invokes here. And he was no cheerleader for capitalism. Shils is here engaged in an act of Cold War Weber tweaking.
order in Germany.”  

In a footnote, he observed that Weber’s place in the “history of the discussion of the economic theory of Socialism” has already been pointed out by Hayek, who, Shils added, has “argued against the possibility of formally rational calculation under Socialism.”  

Here, Shils cites approvingly the same argument that Hayek deployed against Mannheim and other “scientistic” thinkers. Shils also begins to use the “scientism” pejorative in its peculiar Hayekian sense; in the foreword to his 1949 Weber translation, for example, Shils observes that some social scientists embrace the “‘scientistic’ attitude” that the right ends of policy can be determined by social science research.  

The Popperian echoes in Shils’s work are louder still. Popper’s stress on the dangers of Mannheim-style planning, his use of “historicism,” and especially his falsificationist philosophy of science all contributed to Shils’s postwar intellectual evolution. Popper and Shils both began their LSE tenures in the fall of 1946—Popper was recalled to London from New Zealand, where he had emigrated after the Anschluss in 1938. (Popper later referred to The Poverty of Historicism (1957 [1944-1945]) and The Open Society and its Enemies (1945) as “my war effort,” and the two works prompted Hayek and Robbins to arrange in late 1945 for a Readership in Logic and Scientific Method for the Austrian philosopher.)  

Popper and Shils both became profound influences on the remarkable cluster of sociology graduate students who would, in the

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63. Ibid 38n.
1950s and 1960s, populate the sociology departments of Britain’s new provincial universities. In his postwar LSE years, Shils often cited Popper’s account of historicism to critique German sociology in general, and, by implication, Mannheim in particular. In Shils’s magisterial history of American sociology for the British field, The Present State of American Sociology (1948), for example, he relegated Mannheim to a single footnote. “The best discussions of some of the methodological assumptions of German sociology,” he wrote in the footnote, are Popper’s “The Poverty of Historicism” and Parsons’s The Structure of Social Action—both of which, of course, are dismissive of Mannheim’s work. As a kind of citational afterthought, he then referred to a 1934 Mannheim essay, as “a brief survey of the main topics covered in Germany sociology.” In the history, Shils invokes “historicism” in Popper’s sense of the term, though with traces of the more typical meaning; he claims that German sociology grew out of “the background of the Romantic conception of das Volk, out of historicism, with its stress on wholeness and gradual development, and out of a preoccupation with the problems of the State.” By 1961, Shils had fully blended the two meanings of historicism, in a scathing critique of C. Wright Mills’s The Sociological Imagination (1959). Shils criticizes Mills’s position that social laws are valid only for a particular epoch. “The informed reader,” he continues, “will soon recognize the identity of Professor Mills’ historicism with that of Karl


67. Shils, The Present State of American Sociology (Glencoe, IL: Free Press, 1948) 4n. In the next footnote, he implicitly slights Mannheim again, claiming that there are no surveys of French sociology comparable to those on German sociology offered by Popper and Parsons.

68. Ibid 5.
Mannheim as he expounded it in *Man and Society* … The foundations of Professor Mills’ position are clearly delineated and emphatically criticized in Professor Popper’s *The Poverty of Historicism*, 1957.**69** Popper’s “historicist” label, and its application to Mannheim in particular, remained a clear if somewhat confused element of Shils’s intellectual arsenal.

Popper’s philosophy of science, expounded first in German in 1934 and later translated into English, also helped shape Shils’s postwar assessment of social science and its promise.**70** But until Shils’s quiet break from Parsons in the early 1950s, Popperian falsification coexisted in Shils’s thought somewhat awkwardly with Parsons’s position. Popper and Parsons aren’t exactly incompatible—they both reject empirical *verification* as the ultimate truth standard—but Parsons’s claims for the analytic reality of his categorical schemas are a great deal more muscular than Popper’s much more tentative reliance on deductions that are, moreover, subject to empirical refutation; truth for Popper is a real but forever deferred counterfactual ideal that is at best approached through vigorous error elimination. But Shils, in these years, mixes the two positions anyway, with frequent references, for example, to the need for “general theory” of internally consistent propositions, on the one hand, and also a rigorous program of hypothetico-deductive testing, on the other.**71** One explanation may be that both Parsons and Popper

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71. *The Present State of American Sociology* and "Social Science and Social Policy," for example, are filled with both kinds of exhortations. Shils: “Because problems are dimly ‘felt,’ because they are neither related to a general theory of behavior on the one side nor rigorously connected with the categories and indices to be chosen for observations on the other, the results of the research can very seldom become part of the cumulative movement of truth which constitutes the growth of scientific
were engaged in critiques of two competing social science methodologies—Weber’s ideal typical analysis and Mannheim’s sociology of knowledge—that Shils had come to reject. In his 1949 Weber foreword, for example, Shils cites both Parsons and Popper as providing “useful analyses of some of Weber’s methodological problems.”

When Shils emerged from his post-Parsons hibernation in the mid-1950s with his important “Personal, Primordial, Civil, and Sacred Ties” essay (1957), he had dissolved the Parsons-Popper amalgam in favor of Popper:

> The earlier view of a steady progress from particular facts to general theories has now been replaced by the more sophisticated image of a hypothesis, derived from a general theory, being tested by a systematic scrutiny of particular facts: then the theory is either disconfirmed by the facts and is replaced by one more adequate to them or the hypotheses and corresponding theory are confirmed and the problem is settled.

Popper’s influence on Shils was an important one—ranging as it did from the dogmas of planning to his picture of social science—but it was Michael Polanyi, more than Hayek and even Popper, who became for Shils in these post-war years an intellectual authority. All three emigré scholars, however, helped pull Shils apart from Mannheim and his dissensual and distressed assessment of modernity. The Mannheim diagnosis, now discarded, would become the defining model for the bundle of views that Shils later dismissed as the “theory of mass society.”

Shils met Polanyi in 1946, through the Chicago atomic scientist Leo Szilard.

Polanyi was to have a profound impact on Shils’s postwar intellectual evolution—and not

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72. "Foreword" x. In the footnote, Shils also includes von Schelting’s 1934 study as, together with SSA, the “most accurate and elaborate studies of Weber’s method.” But Shils, here and in other essays from the period, and of course Parsons himself praise von Schelting’s clear exposition of Weber’s methods, but differ with Weber and von Schelting on their viability.

73. "Personal, Primordial, Sacred, and Civil Ties," British Journal of Sociology 8(2) (1957) 144.

just through his critique of Mannheim. Polanyi’s absolutist claim for the autonomy of the scientific community became the framework for Shils’s own thinking about science policy, his main supplement to Weber’s “Science as a Vocation.”75 The idea of a scientific community devoted to truth-seeking—Polanyi’s animating ideal—became for Shils too a fundamental article of belief and a standard with which to critique reckless intellectuals.76 Polanyi’s counter-intuitive insistence that tradition is an essential element of scientific achievement framed Shils’s thinking not just on intellectual tradition, but on tradition and social order more broadly.77 Shils differed with Polanyi on some important points—he never openly adopted Polanyi’s “personal knowledge” epistemology, he found Polanyi’s conception of “dynamic order” insufficient to explain social consensus, and he carved out a narrow but significant role for social science in the wider public realm which contravened Polanyi’s principle of autonomy. The two scholars shared, regardless of all that, a common sense of the academic ethic that, for Shils, helped motivate his assiduous pursuit of social-scientific deviants.

It was just after the war that Shils met Szilard, through “some old Weimar friends.”78 With president Robert Hutchins’s permission, the University of Chicago had been chosen as the wartime location for a major piece of the Manhattan Project, which involved building a radioactive pile under the University’s squash courts. Code-named

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76. Shils even referred to the community of scientists as the “prototype of the free society” (quoted in Shattock, "Edward Shils, the Intellectuals and Minerva," Minerva 34(1) (1996) 1).
the Metallurgical Project, the subterranean pile that the nuclear physicists built resulted in
the first self-sustaining nuclear reaction to release atomic energy.\textsuperscript{79} Szilard and a few of
the other scientists had attempted to intervene, unsuccessfully, to prevent the Japanese
bombings in 1945. Shils himself was horrified by Hiroshima and Nagasaki, and hurriedly
devised a scheme for the international control of atomic energy.\textsuperscript{80} Szilard heard about
Shils’s plan, approached him, and the two then appealed to Hutchins for funds to put on
an immediate conference on the topic.\textsuperscript{81} Hutchins not only supplied ten thousand dollars
but also chaired each session at the conference, which was rushed out in the last week of
September 1945.\textsuperscript{82} At the conference, Hutchins declared Shils’s scheme to be utopian, but
he nevertheless agreed to fund an Office of Enquiry into the Social Aspects of Atomic
Energy, which Shils vice-directed.\textsuperscript{83} Working with Szilard and other scientists, Shils
helped found and edit the \textit{Bulletin of the Atomic Scientists} and was active in the
scientists’ campaign for international control; though the campaign failed, the scientists’
pressure was instrumental in getting the Atomic Energy Commission established outside
the military.\textsuperscript{84} As Shils remembered in 1972, he “attended numerous meetings, long

\textsuperscript{79} Mary Ann Dzuback, \textit{Robert M. Hutchins: Portrait of an Educator} (Chicago: University of Chicago

\textsuperscript{80} Shils, "Robert Maynard Hutchins," in \textit{Remembering the University of Chicago: Teachers, Scientists,

\textsuperscript{81} Ibid 190. Shils notes that the anthropologist Robert Redfield was their intermediary.

\textsuperscript{82} Dzuback, \textit{Robert M. Hutchins: Portrait of an Educator} 216-17; and Shils, "Robert Maynard Hutchins”
190.

\textsuperscript{83} Alvin M. Weinberg, "Edward Shils and the 'Governmentalisation' of Science," \textit{Minerva} 34 (1996)
39-40. It is fascinating to note that Shils reported that part of his scheme was the “internationalization
of scientific research” and the close collaboration of Soviet and Western scientists ("Robert Maynard
Hutchins” 190-91). Just a few years later, support for Soviet-American intellectual exchange was
anathema to Cold War liberals like Shils.

\textsuperscript{84} Dzuback, \textit{Robert M. Hutchins: Portrait of an Educator} 217; Martin Bulmer, "Edward Shils as a
\textit{Minerva} 37(4) (1999) 393; and Weinberg, "Edward Shils and the 'Governmentalisation' of Science”
39-40.
'briefings,' went barnstorming and wrote frequently in the *Bulletin* through the campaign for international control and civilian control…”85 Before 1950, Shils published eleven articles in the *Bulletin*, and separately produced a pamphlet for Britain’s National Peace Council, *The Atomic Bomb in World Politics* (1948).86 There’s something perfectly ironic about Shils’s peace activism, cranky conservative that he became, but he was greatly affected by the scientists’ campaign. He discovered in the nuclear physicists a living community of responsible intellectuals, committed to scientific truth but also motivated by a “remarkable sense of civic responsibility and … goodwill toward mankind.”87 Shils later reflected that he “was able to witness a major effort by a group of intellectuals to influence governmental policy, not to block it or to frustrate it or to disparage it but to influence it.”88 Shils’s experience with scientific community in-the-flesh rendered concrete Polanyi’s more abstract conception, and would serve as a real-world yardstick with which to measure other intellectuals.

Shils met Polanyi shortly after his arrival in London to take up the LSE sociology post. He was immediately struck by Polanyi’s charisma: “The noble elevation of his bearing, his eloquence of speech, his compassion and his clarity of conviction were of a piece with his devotion to the discovery and possession of truth and his conception of it as one of the first obligations of a good society. They made profound impression on

85. "Introduction," in *The Intellectuals and the Powers and Other Essays* x.
88. Ibid viii.
me." Shils must have been disappointed when, a few months later, he traveled to Polanyi’s University of Manchester to deliver a lecture on intellectuals and politics. It was, he later admitted, his first public statement on intellectuals, and it was not well received by the audience—“and least of all by Michael Polanyi,” who “roundly denounced” the paper. Despite the public censure, Shils was clearly taken by the Hungarian chemist. Just a few months later, he wrote a breathless profile of Polanyi and his movement for the *Bulletin* and its American readers. He glossed Polanyi’s 1930s written denunciations of the Soviet-inspired instrumentalism of J.D. Bernal, J.G. Crowther and others, adding that their point of view “was definitely preponderant among British scientists during most of the 1930s and there was practically no opposition until 1940,” when Polanyi began to form his Society for Freedom in Science. Shils closed the piece with an empurpled tribute:

> What began fifteen years ago as a major attack on the freedom of science in the English speaking world has led by counteraction to an increasingly systematic analysis of the nature and conditions of freedom in science such as the present century has not yet seen. It recalls in its clarity of mind and in its moral fervor, John Stuart Mill and though lacking the grandeur of that great spokesman for human freedom, it has the compensating merit—for scientists—of coming form scientists who know what freedom in the laboratory really means.

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91. Ibid. Shils notes that it gave him “special pleasure” when Polanyi in 1960 delivered a lecture at Cambridge which revealed that he had accepted most of the tenets of the 1947 paper.


93. Ibid 80-81. The group was officially formed in May 1941 (David Kettler and Volker Meja, *Karl Mannheim and the Crisis of Liberalism* (New Brunswick, NJ: Transaction, 1995) 266). Shils presented Polanyi as more of a scientific systematizer than he actually was: “Pure science consists of a systematic body of valid propositions. Prof. Polanyi emphasizes the systematic and cumulative character of these propositions. It is in their relevance to the systematic structure that single facts acquire interest, however momentous these facts themselves may be for the practical ends of men” ("A Critique of Planning in Science: The Society for Freedom in Science" 80-81).

94. Ibid 82.
In 1950, Shils succeeded in convincing Hutchins to offer Polanyi a Committee on Social Thought position, which he accepted. The State Department, however, refused him a visa under the Cold War-inspired McCarran Act, apparently because of a speech that Polanyi had given in the late 1930s to a group that turned out to be a Communist front. Shils later called the McCarran Act “insane” and referred to the “egregious wrong-headedness” of the Polanyi visa denial. After J. Robert Oppenheimer had his security clearance revoked at the height of the McCarthy hysteria in 1953, Shils edited a double issue of the Bulletin on visa and security policy; he called the Oppenheimer incident a “heavy-handed, stupid act of injustice.” The Polanyi and Oppenheimer cases almost certainly inspired Shils to write, in 1954, the Torment of Secrecy, which he dedicated to Polanyi (and Hutchins).

Shils spent a semester at the University of Manchester in 1953, presumably on Polanyi’s invitation. There Polanyi introduced Shils to the Congress of Cultural Freedom, which had been organized (with secret CIA funding) two years earlier. Shils helped Polanyi, Raymond Aron, and Sidney Hook plan the Congress’s now-famous 1955 “Future of Freedom” meeting in Milan; Shils wrote a conference report for the

97. Ibid 191-92; and Weinberg, "Edward Shils and the 'Governmentalisation' of Science” 41.
Congress’s *Encounter*, “The End of Ideology?”, that was arguably the first salvo in the eponymous melee.\(^{102}\) He was to take on an increasingly active role in the affairs of the Congress in the late 50s and early 60s.\(^{103}\) When Polanyi’s Congress-sponsored *Science and Freedom* ceased publication, Shils proposed a successor journal, *Minerva*, which he founded in 1962. In a 1976 memorial tribute, Shils claimed that Polanyi and Weber had been *Minerva*’s twin inspirations.\(^{104}\)

Polanyi is best-known for his commitment-based philosophy of science, laid out in *Personal Knowledge* (1957).\(^{105}\) To Polanyi, the universalism of science cannot in the last instance be secured on any firm philosophical foundation, nor can its truth claims be anchored negatively in an academic culture of doubt. Instead, Polanyi stressed the inescapably tacit, inarticulable dimension of science in its real practice, in which the effort to transcend subjectivity is itself an inherited act of faith.\(^{106}\) The point isn’t, as with Popper or even Pierce-derived pragmatist philosophies of science, to subject all scientific assumptions to relentless skepticism. For Popper’s “shifting sand” epistemology of doubt Polanyi would substitute a “shirt of flame” commitment that depends, moreover, on its

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102. Shils, "The End of Ideology?," *Encounter* (1955). The last chapter of Raymond Aron’s *The Opium of the Intellectuals* (1955) was also entitled “The End of Ideology.”


106. Polanyi expressed this leap of faith eloquently to Mannheim. Mannheim, in a discussion after the first Moot meeting that Polanyi attended, had challenged Polanyi on his religious commitments. In a subsequent letter, Polanyi wrote: “No life can be without some conviction and the necessity to embrace one as is irresistible to the normal intelligence as it is to our normal moral instincts. So there is no way out. We must choose—and usually we have chosen already by implication. That is, we must choose in such a fashion that what we instinctively love in life, what we spontaneously admire, what we irresistibly aspire to, should make sense in the light of our convictions” (quoted in Kettler and Meja, *Karl Mannheim and the Crisis of Liberalism* 257).
unarticulated and traditional *exemption* from rational scrutiny. This seems to have been too much for Shils, for he rarely invokes Polanyi’s “personal knowledge” in his reflections on social scientific truth. He does occasionally appeal to Polanyi’s view but seems to tweak the “personal knowledge” concept’s meaning in such a way that it comes off as a nurturing *supplement* to the pursuit of knowledge. Shils comes closest to Polanyi in his invocation of a quasi-religious commitment to academic truth in his 1979 Jefferson lectures and in essays like “The Academic Ethic” (1984), but even here the faith component is called on to *motivate* academic pursuit rather than to support its truth content. Polanyi is an obvious influence, but so is Weber’s damn-it-all plea to seize one’s daemon.

Polanyi’s description of the social structure of science was, for Shils, much more important; *his* Polanyi was the author of *Science, Faith, and Society* (1946). The continuity and evolution, in actual scientific practice, of intellectual *traditions* became a central tenet of Shils’s thinking and writing. Following Polanyi, Shils recognized the

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108. For example, in his tonally surprising embrace of C. Wright Mills’s term, the “sociological imagination,” his definition draws on Polanyi: “It is the unformulated categorical apparatus, which Professor Michael Polanyi has called ‘personal knowledge.’ It is the precipitate of intelligent curiosity, the assimilation of the results of systematic inquiry, long study, experience of the world, and ordered reflection” (“Professor Mills on the Calling of Sociology” 616).


110. In his memorial tribute, Shils explicitly linked the two: “In a different but no less eloquent way he expressed the same view of science as Max Weber’s lecture on *Wissenschaft als Beruf*. Michael Polanyi’s conviction about the primary value of science was nearly identical with Max Weber’s, although far more serene and less anguished” (“A Great Citizen of the Republic of Science: Michael Polanyi, 1892-1976” 3).

significance of human-built institutions in securing that transmission, and he admitted too
the vital role that intellectual authority and deference play in the formation and
persistence of scientific community. In this respect, Polanyi’s account of self-
sustaining, internally coherent “dynamic orders” like the literary sphere, the tradition of
common law, and science was crucial for Shils; the account served, in effect, as a more
cheerful rendering of Weber’s differentiation of value spheres. Polanyi’s idea of the
“dynamic order,” indeed, became the grounds on which Shils insisted on the partial
autonomy, even in practice, of academic communities. To Shils, the “academic seclusion
of science” (Polanyi’s phrase) was both a fact and a norm; and this autonomy was the
product of actually existing scholarly communities that treat intellectual
accomplishments, more or less consistently, as their chief justification.

Shils was forced, however, to confront Polanyi’s account of intellectual autonomy
with the extraordinary social scientific involvement in the war effort, including Shils’s
own. Not only did the wartime service seem a not insignificant support for the Allied
victory; it also produced—once time and resources were granted for post-war
reflection—a tangible and lasting contribution to scientific truth. Shils was very much
caught up in the general postwar excitement over advances in empirical social science

112. Shils, "A Critique of Planning in Science: The Society for Freedom in Science” 80-82; and Shils, "A
chastised Mannheim for neglecting the institutional structures of intellectual activity, “which make
possible the continuity of intellectual traditions” (“Mannheim, Karl” (1968) 558).


114. Ibid; and "A Great Citizen of the Republic of Science: Michael Polanyi, 1892-1976" 2. Mannheim, again,
gets criticized by Shils for downplaying this empirical reality: “His continuous insistence that
the ‘internalist’ (ideological) view is wrong and his failure to recognize how much of it he himself
retained led to his failure to perceive the partial autonomy of intellectual traditions and the institutional
structure in which autonomous intellectual activity is effectuated” (“Mannheim, Karl” 558).
methods that war research like *The American Soldier* volumes put on vivid display. Shils’s prose, like that of so many others at the time, was feverish with comparisons to natural science, and suggested that social scientists were on the cusp of general, systematic knowledge. (Perhaps it’s impish to point out that his postwar stance was “scientistic” in the more conventional sense of the term.) The fact that many of the advances were incubated in government service—hadn’t Polanyi warned passionately against the seductions of instrumental science?—prompted Shils to embark on a complicated, even tortured, series of reflections on science policy, exemplified by his 1949 “Social Science and Social Policy.”¹¹⁵

Shils’s essay was written in the context of a Cold War-influenced debate on government funding for basic and applied research in the social sciences; the National Science Foundation had been founded in 1947 without, however, including social science.¹¹⁶ The essay proposes a qualified, wary embrace of government funding and even directly applied work; the tension between his wartime experience and his Weberian/Polanyi instincts is palpable in nearly every paragraph. In character, Shils sets up the essay with an historical account of scholar-government partnerships that serves, in effect, to exonerate the recent entanglements as a mere recovery of much older practices. He invokes a good portion of the social theoretical pantheon, from Plato to Mill, to argue that most great thinkers were involved in “policy” problems.¹¹⁷ The classical political

¹¹⁵. “Social Science and Social Policy.” On Shils’s growing interest in science policy after the war, see “Introduction,” in *The Intellectuals and the Powers and Other Essays* x-xi.
¹¹⁷. In addition to Plato and Mill, he names Aristotle, Smith, Bentham, James Mill, Ricardo, Hobbes, Locke, Burke, Mach and Hegel. “The politician’s problems, reduced to fundamentals, were their
philosopher’s concern with order and the prince’s exercise of power was extended by modern liberalism, Shils argues, to “the maintenance of liberty in a framework of order.” But this congenial alliance was broken up, Shils continues, in the nineteenth century as the social sciences became de-politicized. The split was made partly on the grounds of professional autonomy, but also—and here Shils’s old undergraduate concern surfaced again—because of a “pronounced alienation of intellectuals … from the existing holders of power.” The gap between the academic and the political widened still further in the twentieth century, in part because a legitimation-hungry political science tried to carry the value-free mantle of natural science.

To Shils’s evident relief, the wartime experience brought the government and scholar back together. Previous government service, such as it was, had involved merely “inventory” work, but the distractions and demands of war subdued government officials’ own typical claim to interpretive authority. During war, “social scientists pass from the role of mere hewers of the wood of facts to that of counsellors.” It’s not just that the ear

problems” (“Social Science and Social Policy” 219).

118. Ibid 219.
119. Shils implies that there were gains for autonomy, though he doesn’t necessarily imply that they were worth it; social science, he writes, was eager “to rid itself of any political traces in content and especially in its conception of its calling” (Ibid 220).
120. “This contemptuous and fearful alienation from the holders of power and the marks of important decisions helped psychologically to disqualify social scientists from the realistic study of the exercise of power and the responsible choice of problems of investigation in the light of their relevance to the making of decisions” (Ibid 222).
121. Shils seems to have his interwar Chicago colleagues Merriam, Gosnell, and Lasswell in mind here. “A peculiarly apolitical political science grew up… a morally directionless and scientifically sterile descriptive discipline” (Ibid 220).
122. Ibid 225. Shils is closer here to Lasswell’s postwar “policy science” position than his later condescending treatment of his one-time colleague suggests. In a 1981 memoir, Shils notes that after WWII he came to feel warmly toward Lasswell. “Unfortunately, by this time he was confirmed in a schematic, utopian belief in the feasibility of social engineering. I thought that if he had remained in Chicago, I could have helped him to avoid that fate” ("Some Academics, Mainly in Chicago," American Scholar (1981) 195).
of the prince is gained; the World War II service, in particular, was valuable for the advances in research method that it spurred—he names public opinion polling, sampling and interviewing procedures, sociometric methods, refinements in content analysis technique. If anything, the wartime research failed to increase “systematic knowledge of human behavior” at the pace that research methods improved. This deficit of general theoretical knowledge must, he insists repeatedly, be addressed explicitly, but he also observes that, if properly approached, government-funded research may be a profitable means of extracting such knowledge. Good applied research, after all, generates general hypotheses, which in turn has “more chance of giving rise to the demand for researches to test them than if they are produced by a few isolated and scattered investigators.” The resource and manpower needs of large-scale, “scientifically adequate” research, moreover, require government, commercial, or foundation monies.

Shils concedes that there are many risks, some of which he takes more seriously than others. In a telling contrast to his 1938 paper on academic freedom, Shils downplays the potential for a values clash between social scientists and their government sponsors. Conflicts will arise, he argues, only for those social scientists who hold “firm moral convictions about the ultimate ends of social policy”—and these conflicts would emerge, anyway, at any institution, even a university. The moral position of the government

123. “There can be no doubt about the great advantages for social science of involvement in practical affairs. The rich development of research techniques which has been the most heartening development in the empirical social science in the past quarter of a century, has, to a very large extent, been stimulated by the interest of power-exercising bodies” ("Social Science and Social Policy" 227-28).
124. Ibid 229.
125. Ibid.
126. Ibid 230. The reference, of course, is to “Science as a Vocation.”
scientist, in a “very fundamental respect,” is not so different from university-based social scientists. He does admit that all the government work will probably lead to refined skills of human manipulation, but he claims that this risk may be held in check by strict respect for both the ends and procedures of representative democracy.

Shils claims that, therefore, “nearly all” of the objections to government work have to do with its potential impact on science. He worries that social scientists will end up as mere “botanists.” He frets, too, about the distortions brought on by lopsided funding; some topics with practical importance will come into a “great inheritance” of funds, while other less timely topics—like the “identification with the symbols of a larger community”—will be left unstudied. Social scientific overconfidence might lead to disappointed policy-makers, moreover, who may thereafter refuse the “counsel” of social scientists. And then there is the risk that social research will become bureaucratized, which is especially dangerous for a science “which is still in a formative state.”

The main threat, however, is that general theory might continue to atrophy under the weight of technique refinement. There’s nothing inevitable about this outcome—Shils

127. Ibid.
128. “... there is no reason why social scientific knowledge should not become self-knowledge, the self-knowledge of individuals and the self-knowledge of the community. If it does become self-knowledge and modification of behavior becomes self-modification then its ethical status is unquestionable” (Ibid 238). In his 1948 history of American sociology, Shils registers the same fear, referring to the “danger of becoming, as psychology has for the most part, a collection of scientifically valid recipes for the manipulation of behavior” (The Present State of American Sociology 64).
129. "Social Science and Social Policy” 239.
130. Ibid 233.
131. In a passage that’s very interesting in light of Hayek, Popper, and Polanyi, Shils argues that such counsel, “though seldom scientific, still is occasionally wise and hence worthy of serious consideration” (Ibid 235).
132. The fear is that bureaucratized research will choke competition and freedom of inquiry, and also force many of the best social scientists to waste time doing administrative work. Perhaps more likely, he adds, is that mediocre scientists will take on these administrative roles, and therefore take control of the field’s purse-strings and direction (Ibid 235-36).
hopes that applied research will be designed to test general hypotheses—but he urges that universities remain a central research incubator. Universities are more sensitive to the “longer view,” and they bear a peerless responsibility to pass along the “great tradition which runs from Aristotle to classical economic theory.”¹³³ Some combination of university-based and applied research is desirable, Shils argues. Invoking Weber on value-relevance, he insists that we cannot, anyway, avoid invoking values in the selection of research problems. But this inescapable fact is “completely compatible with the utmost rigor in research procedures, the freedom from political partisanship in the exertion of the investigation and the maximum degree of scientific systematization.”¹³⁴ Since we cannot avoid an evaluative point of departure, we should self-consciously select one carefully. Shils nominates, not surprisingly, the problem of order.¹³⁵ Government-funded research may develop “fruitfully,” but social scientists must convince governments and foundations of the need for “basic research on problems relating to order.”¹³⁶

Shils had moved away from this rather sanguine assessment of social science policy by the late 1950s. In “The Intellectuals and the Powers” (1958), for example, he remains insistent that social scientists and other intellectuals maintain an affinity with the “central” powers like government and business—but he no longer places any scientific

¹³³. Ibid 242.
¹³⁴. Ibid 239.
¹³⁵. He mentions “equality” as another option, but claims that it is “of less practical and ethical value,” and less central than order, which is “entailed in every policy” both as a particular end and as a pre-condition (Ibid 240).
¹³⁶. Ibid 240-41. Shils makes the same point, though more obliquely, in his 1948 history of American sociology: “It does mean that a scientific sociology will not develop unless it is motivated by that broad curiosity about human nature and its vicissitudes in the universe which grows on the rational level from a sense of responsibility to a great civilization, a concern for the clarification and improvement of its moral rules and the practical desire to perceive the conditions of their realization or maintenance or improvement” (The Present State of American Sociology 64).
promise in technocratic projects. Instead of convincing government to fund studies of social order, Shils by the late 1950s is exhorting social scientists to project an image of society that helps maintain that order. In “The Calling of Sociology,” he explicitly elevates the “illumination of opinion” over two undesirable alternatives: radical social critique and government technical aid. Many intellectuals’ failure to fulfill this “public opinion” role earned them, of course, Shils’s ire and often enough the “mass society theorist” label.

It must also be admitted that Shils himself, during and after the period when “Social Science and Social Policy” was written, arguably violated his own confident prediction that government-sponsored research would hew to democratic principles. He and many other prominent social scientists worked on behalf of the Cold War national security state on projects that abused the “principles,” at least, of “representative democracy.” By the spring of 1950, soon after the essay was published, Shils was in Germany with Henry Dicks, his wartime collaborator, working on a long-classified, still-unpublished RAND Corporation study on The Soviet Army. Later, Shils was an active participant in the CIA-funded Congress for Cultural Freedom, even though he was

138. "The Calling of Sociology," in The Calling of Sociology and Other Essays on the Pursuit of Learning (Chicago: University of Chicago Press, 1980 [1961]). Shils says something very similar in his attempt to distance, mildly, his own view of scientific autonomy from Polanyi’s: “I myself would perhaps lay greater weight than Michael Polanyi on the obligations which scientists and scholars and scientific and academic institutions have for the well-being of their societies, through the training of the best minds for the professions at once practical and intellectual, which serve the justice, freedom, and well-being of societies” (“A Great Citizen of the Republic of Science: Michael Polanyi, 1892-1976” 4).
reportedly aware of the funding after 1955.\textsuperscript{140} He was also involved, though in a complicated way, with some of the secret, government-funded “modernization” research that, in the late 1950s and early 1960s, was part of an American effort to influence newly independent, non-aligned Third World nations.\textsuperscript{141} There’s no doubt that Shils was a principled defender of liberal democracy, but he nevertheless violated his own standards for ethical research by relying on a means-blind morality that he otherwise justly condemned.\textsuperscript{142} Shils’s willingness to secretly aid the Cold War effort is an expression, I think, of his core tension between a commitment to liberal order, on the one hand, and devotion to open and truthful scientific inquiry, on the other. In some ways, the history of mass communication research, in Shils’s narration, fell victim to this tension too.

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Shils’s skill at institutional and intellectual border-balancing was honed during his four-year tenure as reader of sociology at the London School of Economics.\textsuperscript{143} It is a too-perfect irony that Shils filled the very same post that Mannheim had resigned just a year

\begin{itemize}
  \item \textsuperscript{140} Frances Stonor Saunders, \textit{The Cultural Cold War: The CIA and the World of Arts and Letters} (New York: New Press, 1999) 394-95.
  \item \textsuperscript{141} On Shils’s “modernization theory” involvements, see Nils Gilman, \textit{Mandarins of the Future: Modernization Theory in Cold War America} (Baltimore: Johns Hopkins University Press, 2003); Shils’s role is documented throughout Gilman’s excellent history.
  \item \textsuperscript{142} For a revealing example of a major scholarly work that concealed its own government funding and propagandistic purpose, see Rohan Samarajiva’s excellent study of Daniel Lerner’s \textit{The Passing of Traditional Society} ("The Murky Beginnings of the Communication and Development Field: Voice of America and The Passing of Traditional Society," in \textit{Rethinking Development Communication}, eds. Neville Jayaweera, Sarath Amunugama, and E. Tāi Ariyaratna (Singapore: Asian Mass Communication Research and Information Centre, 1987)). Samarajiva artfully contrasts the study’s misleading secrecy with the ethical standards proposed by Lerner’s MIT colleague, Ithiel de Sola Pool, in his "The Necessity for Social Scientists Doing Research for Governments," \textit{Background} 10(2) (1966).
  \item \textsuperscript{143} Shils once referred to the LSE as “the other half of my Anglo-American life” ("Introduction," in \textit{The Intellectuals and the Powers and Other Essays} ix).
\end{itemize}
earlier. By timing, will, and deliberate acts of intellectual translation, Shils was to have a profound impact on the then-emerging field of British sociology. For an assortment of reasons—Oxbridge disdain, the prominence of social anthropology, the miscarriages of L.T. Hobhouse and Patrick Geddes—sociology in Britain had never taken hold. Its emergence as a discipline, in effect, dates to the late 1940s and early 1950s, when the nation’s first wave of sociology faculty were nearly all trained at LSE. Of this remarkable cohort of students—including J.A. Banks, Basil Bernstein, Norman Dennis, Ralf Dahrendorf, and John Westergaard—a dozen went on to newly created professorships at the various provincial universities that had cropped up after the war.

Shils was a formative influence on the students—in most accounts, one of just two or three that stood out. His impact owed to his intellectual authority, and his intellectual authority, in turn, derived from his steely erudition. He was, in effect, an ambassador not just for American sociology but also for the European tradition. In Halsey’s accounts, Shils and David Glass are portrayed as the two figures whose impressions were left

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144. See Shils, “Karl Mannheim” 232.


147. A.H. Halsey provides a detailed account of the student cluster, of which he was a part ("Provincials and Professionals: The British Post-War Sociologists").

148. Most often T.H. Marshall and/or David Glass are mentioned.
deeper:

Both oddly enough were indifferent lecturers, but endowed with a compelling charisma … Glass offered a method, Shils a theory … [Shils] not only presented classical European sociology to them but did so in an American voice which simply assumed that undergraduates would become graduate students and subsequently professionals. His blend of tutorial ferocity and Olympian erudition challenged their still half-formed ambition to fearful effort. His Current State of American Sociology (1948) conveyed the idea that a subject of great difficulty and worth was at once both dignified in its European antiquity and accessible in its American modernity. Sociological research was a living practice as well as a hallowed tradition.149

In a similar vein, Donald MacRae recalled that Shils imbued his students “with a belief in their endeavours and the desirability of sustained empirical inquiry… [T]o all his students sociology became not only a discipline, but a commitment, and from him many learned a quirky tolerance and a resigned moralism.”150 A number of accounts refer, in vivid language, to Shils’s imperious bearing.151 Noel Annan, who would later recruit Shils to King’s College, Cambridge, remembered Shils as a “master of theoretical sociology” who was not “afraid to bludgeon with his erudition those who did not realise sociology was a subject that rested on a great European intellectual tradition.”152


150. MacRae, "The Basis of Social Cohesion," in Man and the Social Sciences, ed. W.A. Robson (London: Allen and Unwin, 1972) 53; quoted in Bulmer, “The Development of Sociology and of Empirical Social Research in Britain” 22. MacRae was a fellow lecturer to Shils. In Dahrendorf’s memory, Shils was a “widely read, thoughtful theorist as well as conversationalist” whose presence even after he left in 1950 “continued to be felt and often seen” (LSE 377).

151. Here’s Jean Floud, his LSE colleague (and the former assistant to Mannheim, who, as Jean MacDonald, helped Mannheim tweak Ideology and Utopia to reflect American pragmatism): “I confess that I judged him harshly as an abrasive and unhelpful colleague. He did not teach even-handedly. He took up students he thought bright and worthwhile, then unceremoniously dropped them—dumped them, in fact, into the lap of junior colleagues. He would breeze into my room, fling a file on the desk and say, with a jovial air which did nothing to mitigate the offense, ‘Here, you’re a good teacher, see what you can do with this one’… His great learning, prodigious memory and breadth and seriousness of his professional interests were, of course, there for the looking; less obvious were his virtues of character—his warmth, humanity and capacity for sympathy and compassion… a most grateful intellectual beneficiary and affectionate friend” (Edward Shils (1910-1995),” Minerva 34 (1996) 85). And Mark Benney (who would later join the Social Science staff of the College of the University of Chicago), remembers Shils as a “short, glowing, voluble man … introduced as Professor Shils. There was no escaping the fact that Shils was Jewish—he imposed it by sprinkling his talk with vivid but obscure Yiddish terms, by telling complicated Rabbi stories one after another, and even by a certain Talmudic obsessiveness in discussion…..he was hilariously scathing about what passed for sociology at that august institution [referring to LSE]. ….he moved compulsively around the room as he talked or listened, examining every book on the shelves…” (quoted in Bulmer, “Edward Shils as a Sociologist” 9).

152. Annan, Our Age: Portrait of a Generation 256n, 255. Annan refers to Shils was one of “two greats” at
Shils’s version of American sociology stressed its empiricism and the new quantitative methods. On the European side, he seems to have taught Weber and Durkheim through a Parsonsian prism.\footnote{153} Shils’s assigned *The Structure of Social Action*, whose “817 arid pages” (Pitrim Sorokin) were treated with awe and some suspicion. (Two LSE graduate students, Ralf Dahrendorf and David Lockwood, published well-known critiques of Parsons in the 1950s.)\footnote{154}

Shils managed to have a significant hand, too, in one of Britain’s earliest and most successful research outfits, Michael Young’s Institute of Community Studies. Shils encouraged Young and Peter Willmott to found the Institute, provided his own money during a financial crunch, and arranged a crucial Ford Foundation grant.\footnote{155} (Willmott claims that the Ford grant was won “thanks largely to the intervention of Shils”; he almost certainly appealed to his friend and Lazarsfeld associate Bernard Berelson, then director of Ford’s Behavioral Sciences Division.\footnote{156} He also seems to have been an LSE, the “only place to go after the war.” The other was Glass.

153. Albrow: “To a large extent, in fact, for students at the LSE in the immediate years after the end of the war, theoretical sociology came not so much from Ginsberg, who was seen as remote, or Mannheim, but from Edward Shils, who offered an American interpretation of the European tradition and became the presenter of Parsons’s ideas” (“Sociology in the United Kingdom after the Second World War” 200). Halsey: “‘Classical sociology’ as developed on the continent by Weber, Durkheim, and Pareto was imported into LSE for the most part by Edward Shils in the form of Parsons’ *The Structure of Social Action*” (“Provincials and Professionals: The British Post-War Sociologists” 153). Shils also assigned Weber’s two “Vocation” essays, which apparently softened the students’ politics, most of whom settled into a Laborite and ethical socialism. Halsey: “They all read Max Weber’s two essays on Science and Politics as vocations and chose the former for themselves while in no way abandoning their political enthusiasm” (155).


155. Willmott, "The Institute of Community Studies" 146-47.

156. Ibid. Berelson owed Shils: His 1952 presidential address to the American Association of Opinion Researchers, and the last, theoretical chapter of his and Lazarsfeld’s 1954 *Voting* study were written with Shils’s significant help (see the acknowledgments: "Democratic Theory and Public Opinion," *Public Opinion Quarterly* 16(3) (1952); and *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: University of Chicago Press, 1954)).
important intellectual influence on Young and Willmott:

An eminent theorist himself, his catholic approach found room for new work in the British 'empirical tradition' of Booth and Rowntree. We had, as newcomers to his subject, been excited by an article by Shils in *Pilot Papers* (1947) on American sociology.157 and Young had attended his famous seminars at the London School of Economics in 1948 and 1949. We benefited from his wisdom and erudition in a series of tutorial sessions that he generously gave us at Bethnal Green during the Institute’s first year.158

Shils’s stature within post-war British sociology was certainly elevated, and it drew on both his classroom presence as well as his survey of the American field, published in 1947. The audience for the essay, however, wasn’t merely British; a lengthened version was soon published in book form in the U.S. by the Free Press. *The Present State of American Sociology* was one of the first books issued by the house that would, over the 50s, become the single most important publisher of sociologists. (It almost goes without saying that Shils was friendly with Free Press founder Jeremiah Kaplan, and helped guide the Press’s ambitious classics translation and reissuance effort.)159

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*The Present State of American Sociology* is a heady and brash book—almost a manifesto.160 When Shils wrote the text, he was still relatively unknown and unpublished, a lecturer without a Ph.D. or tenure. Shils’s breadth of knowledge, cutting prose-style, and often-severe assessments must have startled its disciplinary audience, as surely did the book’s sharply critical diagnosis. Shils’s argument is that American sociology remains in its pre-scientific infancy, despite some rich observations and promising

157. Willmott refers here to the first version of Shils’s history of American sociology, which was published in the British journal ("The Present Situation in American Sociology," *Pilot Papers* 2(2) (1947)).
158. Willmott, "The Institute of Community Studies" 146.
159. Bulmer, "Edward Shils as a Sociologist" 11.
160. The boldness of Shils’s survey bears comparison to the 1943 “social pathologists” article by C. Wright Mills—Shils’s later nemesis. Shils and Mills have many other fascinating parallels.
research techniques. With nomothetic zeal, Shils exhorts his fellow sociologists to get on with the real science—general theory at a high level of abstraction. The unstated model is Parsons.

Sociology’s “present state” is one of “backwardness”—it remains in the “foothills of science,” and not yet at its heights. Too many researchers, Shils complains, are content with mere “botanizing,” gripped as they are by a “descriptive passion.” Sociological knowledge has been indefinitely stalled by the neglect of testable hypotheses, which in turn must be “fitted” into general propositions if real knowledge growth is to occur. The problem is made worse by the “cultural poverty” of most practicing sociologists, who pass over through ignorance many of the most compelling topics—including law, political systems, science, and revolution.

Shils concedes early American sociology its strengths, particularly its energy and attentiveness to empirical richness. But in practice at least the exhaustive descriptions were left as they were, without rigorous analysis. Here Shils’s own University of Chicago, the leading department in the interwar years, comes under criticism. The entire interwar generation—Shils names Thomas and Park at Chicago, as well as Cooley and Ross—failed to articulate its hypotheses, so that their problems were “lost from sight while the subject matter with which they illustrated their hypotheses continued to be pursued in discrete and undisciplined form.” In the 1920s, the community study, and

162. Ibid 2.
163. Ibid.
164. Ibid 6-7.
the parallel field of urban sociology, flourished with “vivid, energetic curiosity about the rich and mysterious texture of metropolitan life” under Park’s brilliant guidance. But Park, like so many other modern sociologists, overemphasized the “loosening and disruption of communal bonds,” and he failed to develop his “rich, aphoristic insights” into systematic theory. Once Park left, his original vision “vanished” and the department settled into the “repetition of disconnected investigations.”

Shils comes off as more excited about the emergence and refinement of research techniques like polling and content analysis. He devotes a number of pages to what he labels “Communications Analysis and Public Opinion.” Polling and academic survey research are praised not for the results that they yield—“the questions will probably have to be asked anew”—but instead for the steady improvement in technique that they have stimulated. Merton’s “focused interview” technique, and Lazarsfeld’s panel method are all extolled, as are the substantive works in which they are applied, Mass Persuasion and The People’s Choice. But Shils, in a by-now familiar refrain, criticizes the public opinion field for its failure to generate results that might be used to establish “universal propositions.” Shils points, likewise, to the promise of Lasswell’s quantitative content analysis.

165. Ibid 9-10.
166. Ibid 10.
167. Ibid 11. It is worth noting that Shils officially left Chicago’s sociology department for the Committee on Social Thought in 1947.
168. Ibid 35.
169. The People’s Choice is given the most attention by far, and Shils clearly recognizes the study’s importance to small group research: “The most valuable synthesis of the various methods of public opinion analysis has been produced by Paul Lazarsfeld… a very carefully organized study … [with] ingeniously contrived indices, which should provide a model for research workers in the next years … The study concludes with hypotheses seeking to explain the effectiveness of the small group as over against the mass media and stresses the need for more research on the problem” (Ibid 41).
170. Ibid 35.
analysis method—though the technique “has been of relatively little scientific value…”

Shils’s offers a surprisingly upbeat assessment of psychoanalysis—surprising because of his harsh appraisals shortly after. He even praises Theodor Adorno’s unpublished *Authoritarian Personality* study which he will, in 1954, tear apart. Adorno and his colleagues, Shils writes, “have succeeded in isolating the set of personality and attitudinal characteristics which make for receptivity to anti-Semitic ideas.” There’s actually an interesting mystery here: The preface to Bruno Bettelheim and Morris Janowitz’s *Dynamics of Prejudice* (1950)—like *The Authoritarian Personality* (1951), sponsored and commissioned by the American Jewish Committee under Max Horkheimer’s editorship—reveals that Shils “joined the project as co-director [with Bettelheim] shortly after its inception,” and credits him with the research design; “Unfortunately, other duties took him to London …” before the findings were evaluated. Shils was almost certainly off to his LSE post, but the London departure doesn’t exactly explain his removal from, or abandonment of, the study.

Only Parsons’s Social Relations initiative and John Dollard’s Yale Institute of Human Sciences are truly exempted from prose-abuse. The two groups have moved the field from an “atheoretical prehistory into a more sophisticated stage, closer to

171. Ibid 38.
173. *The Present State of American Sociology* 29. Two pages later he refers to the new attention being paid to the internal structure of the family, which, Shils observes, parallels the “revival of interest” in the small group. Then: “In this shift of interest, the large collaborate work of the International Institute of Social Research, *Autoritat und Familie* (edited by Max Horkheimer) has had an important influence, although the growing acceptance of psychoanalysis also helped to bring about the change” (31).
The Parsons cluster gets most of Shils’s attention—which he calls an “important opportunity to synthesize technical virtuosity, analytic rigor, historical breadth, psychological imagination and a value-directed sense of relevance in the selection of research problems.” Oddly enough, he proceeds to describe his own collaborative project with Parsons—though without naming it or himself—as the topic that “American sociologists” are “increasingly … defining as their task”: the study of the integration and disintegration of groups structures, which sociologists “aim” to build into a “system of laws.”

The book concludes with a plea to rescue the field’s “aimless and enthusiastic botanizing” with “generalized sociological knowledge on a high level of abstraction,” in part by a selecting social order as an orienting problem. Shils must have become embarrassed by the book—perhaps for its Parsonsian paeans to general propositions or its on-the-cusp bravado—as he refused to have it reissued.

176. Ibid.
177. Ibid.
178. Ibid 64. Shils: “It does mean that a scientific sociology will not develop unless it is motivated by that broad curiosity about human nature and its vicissitudes in the universe which grows on the rational level from a sense of responsibility to a great civilization, a concern for the clarification and improvement of its moral rules and the practical desire to perceive the conditions of their realization or maintenance or improvement” (64).
CHAPTER FOUR
Paul Lazarsfeld, Accidental Media Researcher

It is not enough, of course, to state that Edward Shils supplied crucial narration for a body of media research findings, and leave it there. The small-group research account that Shils provided, after all, could have languished in dust-gathering stacks—in yellow-papered anonymity—suffering the same fate bared by the overwhelming preponderance of all published research.¹

To understand the appeal of Shils’s narrative, it is important to make a distinction between the raw findings of any given research and the framing within which these are placed. This contrast is especially artificial, since “raw findings” do not meaningfully exist independent of some attempt at sense-making in language. But the distinction nevertheless helps to illuminate a significant feature of the Shils-Lazarsfeld case. For historically specific reasons, the interwar public opinion cluster, as well as postwar empirical sociology, tended to be meticulous and attentive to research design and process, but rather loose and instrumental with contextual framing. The packaging of any given study—the attempt to explain wider significance—was by default an important but late-stage afterthought. If anything, it became acceptable, and the typical practice, to invoke research predecessors without much care for faithful renderings.² There were many

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¹. As David Hull pointed out—and as many others have confirmed—“nearly all work in science is ignored,” and the research that receives attention is itself typically “transformed and misrepresented” (Science as Process (Chicago: University of Chicago Press, 1988) 395).

². Though the conditions here were historically specific, many of them remain, of course, prominent in academic sociology and communication research today. On the use and abuse of the literature review form and broader social-scientific citational practices, see, for example, Michele Lamont, "How to Become a Dominant French Philosopher: The Case of Jacques Derrida," American Journal of Sociology 93 (1987); Charles Bazerman, Shaping Written Knowledge (Madison: University of Wisconsin Press, 1988); Michael H. MacRoberts and Barbara R. MacRoberts, "The Negational
explanations for this, but one of them was the basic lack of historical knowledge, which conditioned both the initial thinness of the research histories and their widespread acceptance. The field, for reasons I discuss below, was at the same time in a state of relative reputational flux. Narrated claims to originality, for the reputation-savvy, were crucial instruments for earning prominence, even especially because such claims were, in historical terms, rather poorly “policed.” For specific reasons, research framing was a vital reputation-building instrument that was, moreover, relatively flexible. The conditions, in short, were felicitous for mnemonic intervention.

It was within the interwar public opinion cluster—and then, after the war, within sociology at large—that Paul Lazarsfeld’s drive for scholarly reputation played itself out. Here was the context that gave shape to Lazarsfeld’s roaming and indiscriminate voraciousness. Lazarsfeld had no real interest in voting behavior, soap sales, or media effects per se. Most of his substantive research topics were the random-seeming progeny of intellectual, institutional and funding opportunities that Lazarsfeld then exploited in his restless search for distinction—which, after all, was anchored only by rather flexible interests in decision-making and methodology.

A word is in order here about the general significance of reputation-seeking, and of fields’ organizational attributes, as explanations for academic memory-making—or, for that matter, for the sociology of academic life in all its aspects. My view is that they

do not have general significance. I favor a case-by-case explanatory agnosticism that lets, as it were, the empirical evidence “assert” itself first—after which one or more kinds of explanation might turn out to “fit” well. There is simply too much complexity and diversity within the modern academic arena, across fields and over time, for any unitary scheme to bear much explanatory burden—without, at least, inflicting major violence on the empirical reality which it purports to explain. The analytic monisms of Andrew Abbott, Randall Collins, Pierre Bourdieu, and Robert Whitley are, in this respect, valiant and magisterial failures—though each may be profitably employed in more particular contexts. It’s true that all attempts to understand even small-scale academic developments, if they are not to whither away into utter ideographic irrelevance, get it “wrong” in significant ways—sifting out only a minuscule measure of the vast booming, buzzing reality, and filtering that tiny yield through an assortment of cognitive, social, academic, and other prisms. But it seems to me that we can, nevertheless, distinguish between more or less evidence-sensitive explanatory efforts, and that we benefit most, in truth terms, by training our attention on what is, inescapably, rich and particular.

Among sociologists of intellectual knowledge, Pierre Bourdieu is probably most closely identified with the reputation-seeking thesis, though this sort of argument is fairly common. His many-faceted expansion of the “capital” concept is an enormously fruitful analytic tool, and it yields explanatory dividends across many aspects of social life—especially because it does not restrict the pursuit of distinction to conscious strategy. But what Bourdieu calls the “law of the quest for distinction” is not, in fact, law-like; it simply cannot capture the whole panoply of human life, especially over time and across
cultures. When applied to modern academic and intellectual life, the notion of a rivalry over scarce “credit” resources does indeed have a great deal of explanatory fitness. It is often possible, moreover, to extract real insight by thinking about academic life in terms of partially autonomous “fields” with their own reputational hierarchies and complex relations—often through heterodox, margin-dwellers—within a wider “intellectual field” that is itself related in analogous ways to the “media” field, for example, or the broader “field of power.” This is all very good to think with, and often truly helpful in particular contexts. As I have indicated already, Bourdieu’s style of approach works very well in making sense of Paul Lazarsfeld’s career. But it is still important to keep in mind that academic norms of originality, for example, are not a universal constant but instead the product of a specific historical tradition and more or less prominent even within particular national academic cultures.

In some large sense, to take the other example close at hand, Edward Shils’s intellectual trajectory could be forced into Bourdieu’s scheme, in terms of his lifelong quest to understand social order. But on the “ground” the distinction thesis just doesn’t help much to understand the choices that he made within the conditions that he faced. He hardly jostled over credit in the usual ways—quite the reverse, in fact—and the particularities of his “field” memberships renders the “orthodox-heterodox” formulation especially inappropriate. For Shils, it proved much more revealing to treat his career as an intellectual historian might—with special attention to his intellectual and political

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influences, with some due measure of consideration paid to the social, world-historical, and institutional contexts that surrounded him. It really does depend.4

The same willed agnosticism is appropriate to the related issue of any given field’s organizational and resource features. These kinds of factors are often of vital importance, and they have been, by and large, neglected by historians and sociologists of science, and especially by historians of social science disciplines.5 Richard Whitley’s brilliant analytic mapping of relationships between various intellectual and organizational conditions was, in this respect, a much-needed corrective.6 But for all of its complexity and genuine insight, his typological mania does not yield descriptions of fields-as-they-are—especially in intellectual terms—all that well.7 Whitley as a selectively deployed analytic aid is better than Whitley the totalizing schemer—good for specific formal distinctions, for example, and helpful too as a reminder to keep an attentive eye out for links between a field’s organization and its intellectual life. The public opinion cluster here, for example, does not fit as a “fragmented adhocracy,” a “conceptually integrated bureaucracy,” nor any of the other five field types that he identifies. But some of his analytic tools are very helpful indeed.8

4. The tragic but necessary tension between credit- and truth-seeking that Guy Oakes and Arthur Vidich posit in their recent Hans Gerth-C. Wright Mills case study seems to be an advance on an exclusive distinction-quest focus—provided that the relationship between credit- and truth-seeking is recognized for its historically particular configurations (Collaboration, Reputation, and Ethics in American Academic Life: Hans H. Gerth and C. Wright Mills (Urbana: University of Illinois Press, 1999)).

5. The neglect of institutional factors—and a shifting set of external influences—in the historiography of communication studies is especially glaring because the discipline, in its institutionalized form, has been so fundamentally shaped in these ways. One exception is John Durham Peters, *Institutional Sources of Intellectual Poverty in Communication Research,* Communication Research 13(4) (1986).


7. Lazarsfeld, however, would have been proud of his sixteen-fold table.

In the case of Shils—and in many instances of intellectual “transfer”—it seems more fruitful to leave Whitley behind altogether to consider, instead, the disconnects between a field’s organizational character and some of its intellectual contents. Here Andrew Abbott provides a useful contrast, which he thinks tends to hold generally, between an “extraordinarily stable disciplinary social organization” and an “extraordinarily fluid disciplinary cultural system.”

Though it is more or less true in various circumstances, the idea of intellectual fluidity—especially on the edges of existing disciplines—does seem to explain some of the changes that occur in various fields’ intellectual “repertoires.” In this respect, the role of intellectual (and mnemonic) ambassadors like Shils, who perch between organized fields and even across national and chronological boundaries, takes on new and special importance.

Some of the existing literature on Lazarsfeld and the Bureau of Applied Social Research, often labeled “critical,” looks to the Bureau’s funding sources and proprietary research contracts as determinants of its media research findings. A historiographical current of the 70s and 80s, exemplified by Todd Gitlin’s famous 1978 “Dominant Paradigm” essay in *Theory and Society*, attributed the Lazarsfeld circle’s limited effects conclusions to its dependence on market research and especially media firm sponsorship, on the theory that Lazarsfeld and his Bureau colleagues were telling the media barons what they wanted to hear, that mass media exposure is harmless.

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an astonishing, long-forgotten or -classified funding effort on behalf of the State Department, the military, and the CIA for Cold War-related Third World propaganda research, which began (using the personnel and infrastructural remnants of the World War II anti-Nazi efforts) in the early 1950s and accelerated rapidly in the late 50s and well into the 60s.\footnote{11}

Both currents—especially the second—are important for understanding post-war mass communication research. But both need to be qualified, brought into dialogue with one another, and opened up to more complex narratives. The earlier, Gitlin-style reliance on commercial funding simply cannot perform the heavy-lifting that the critics ask it to do—despite the fact that Lazarsfeld did indeed package some of his findings in a media-friendly way, especially if the executives were the intended audience. And certain Bureau-linked figures, most prominently Frank Stanton of CBS and Joseph Klapper, who by the late 1950s had also joined CBS, were indeed carrying the industry’s water in a more-or-less shameless way. But Lazarsfeld, with his genuine scientific interests and aspirations, was much more complicated; one of the ironies is that Gitlin and the others essentially accepted Lazarsfeld’s own \textit{Personal Influence} self-description, and the powerful-effects contrast too, so that their account rested on rickety and misleading foundations from the start.

The more recent current is much less dependent on the domestic limited-effects

narrative, in part because the focus has really been on *international* communication research where—as Herbert Schiller pointed out long ago\(^\text{12}\)—scholars had typically *celebrated* media influence for its modernizing benefits. The revelations of extensive, secret propaganda work, most notably in Christopher Simpson’s *Science of Coercion* (1994), read alongside the era’s published studies, clearly demonstrates that much of the public work had been *re-packaged* as benevolent modernization theory. This second-wave “critical” history is a significant advance on the credulous and Whiggish textbook narratives that it should displace, and studies like Simpson’s have introduced, for the first time, more sophisticated and rigorous methods into the study of the field’s history. Yet the work that has been published so far suffers from some serious limits. One of these is its overcommitment to an otherwise laudable resource-based sociology of knowledge.

One of the drawbacks of such an approach is that the complex and distinct motives of key researchers gets neglected; it is important, for example, to distinguish between zealous Cold Warriors like Wilbur Schramm or Daniel Lerner and apolitical funding opportunists like Lazarsfeld.

Neither of these two “critical” currents adequately captures, moreover, the conditions that led to the adoption of the “limited-effects” narrative—though they both illuminate important slivers of the story. The Bureau media research program conducted only a single *type* of study—on short-term persuasion campaigns—in part because Lazarsfeld was interested in decision-making, but largely because the Bureau’s clients, the wartime government and various commercial firms, wanted only this kind of

information. Lazarsfeld maneuvered adroitly within these confines—designing the research in such a way that it would yield academic results of interest. But the fact remains that the Bureau’s research findings were almost exclusively centered on short-term campaign effects. This was sometimes admitted to be a crucial limitation, with appropriately qualified inferences. But when Lazarsfeld, with Katz’s help, set out to solidify his media research reputation in *Personal Influence*, he nevertheless drew sweeping conclusions about media effects in general. The limitations imposed by the funders, then, were plainly important, but only to the extent that Lazarsfeld’s rendered the studies’ findings in such a way that they could support his artful re-framing. He was, in this respect, too resourceful.

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Paul Lazarsfeld (1901-1976) came of intellectual age in Vienna between the wars, within the same milieu—though on the socialist side—from which Friedrich Hayek emerged.¹³ The intellectual life of Vienna in the 1920s was extraordinarily fecund, and the young

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¹³. The historical literature on Lazarsfeld is vast, though there is relatively little on his Vienna years. See, however, the recollections of his then and future collaborator (and first wife), Marie Jahoda (“PFL: Hedgehog or Fox?,” in *Qualitative and Quantitative Social Research: Papers in Honor of Paul F. Lazarsfeld*, eds. Robert K. Merton, James S. Coleman, and Peter H. Rossi (New York: Free Press, 1979)); and the reminiscences of his close friend and intellectual partner Hans Zeisel (“The Vienna Years,” in *Qualitative and Quantitative Social Research: Papers in Honor of Paul F. Lazarsfeld*). Some of Lazarsfeld’s early American writings, in which he was making a case to potential clients for his distinct approach to market research, provide detail on his Vienna years, especially the studies of his research institute; see, for example, Lazarsfeld, “The Psychological Aspect of Market Research,” *Harvard Business Review* 13 (1934). Lazarsfeld’s 1969 memoir also contains some fascinating reflections on his Austrian youth (“An Episode in the History of Social Research,” in *The Intellectual Migration: Europe and America, 1930–1960*, eds. Donald Fleming and Bernard Bailyn (Cambridge, MA: Harvard University Press, 1969)). The co-editor of the *Intellectual Migration* volume was Harvard historian Bernard Bailyn, Lazarsfeld’s son-in-law who later provided some personality insight in a short memoir (“Recollections of PFL,” in *Qualitative and Quantitative Social Research: Papers in Honor of Paul Lazarsfeld*). The Freedom Forum’s Media Studies Center commissioned an extremely helpful guide, with an emphasis on media-relevant materials, to Lazarsfeld’s collection of papers at Columbia (Ann K. Pasanella, *The Mind Traveller: A Guide to Paul F. Lazarsfeld’s Communication Research Papers* (New York: Freedom Forum, 1994)). Columbia’s Butler Library also contains an extensive oral history interview. The many other published histories of Lazarsfeld all draw on these same few sources.
Lazarsfeld immersed himself in its socialist and philosophical currents with enthusiasm. His socialist and academic interests would commingle, over the decade, without much friction—indeed, with some surprising mutual benefit. When Lazarsfeld decided to remain indefinitely in the United States, however, he rather abruptly dropped his radical commitments. He had cast his lot, by then, with science.

But for the 1920s, at least, the socialist movement—and especially its youth wing—was the major fulcrum for Lazarsfeld. The Austrian Social Democratic Party was evolutionist and democratic, the intellectual home of the Austro-Marxist thinkers, and a major political and social force in interwar Austria. Lazarsfeld formed the Party’s major youth group, the Verein Sozialistischer Mittelschule, started its newspaper, and organized some of its winter retreats and summer camps. Some of the Party’s key intellectual figures, including the physicist Friedrich Adler, the philosopher Otto Neurath, and Party leader and Austro-Marxist Otto Bauer, played pivotal roles in Lazarsfeld’s intellectual evolution.

It was Adler who prompted Lazarsfeld to pursue a Ph.D in applied mathematics. Writing from prison in 1917 or 1918, in response to a letter from the sixteen-year-old Lazarsfeld, Adler urged the young student to learn as much math as possible. As

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14. Zeisel: “But of all the influences on our lives one was paramount—that of the socialist party of the day...” (“The Vienna Years” 12); see also Jahoda, “PFL: Hedgehog or Fox?” 4; and "An Episode in the History of Social Research" 270-72.

15. Zeisel, "The Vienna Years" 12; Jahoda, "PFL: Hedgehog or Fox?" 4; and Bailyn, "Recollections of PFL." 16-17. Zeisel records that Lazarsfeld, moreover, took an active and founding interest in the youth group’s theatrical productions. “At its first performance Paul appeared in a self-written take off of the monologue in Offenbach’s Beautiful Helene” (12).

16. The details are drawn from a 1972 letter Lazarsfeld wrote to Lewis Feuer of the University of Toronto, quoted in the Media Studies Center guide to his papers (Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 31).
Lazarsfeld would later recall in his 1969 memoir, the applied math Ph.D. was the “almost accidental” result of Adler’s intervention. The happenstance influence foreshadowed an intellectual career that would, along these lines, get shaped by Lazarsfeld’s adaptive response to a series of constraints and opportunities.

Otto Neurath, the polymath and future Vienna Circle figure, was one of the lecturers invited to address the socialist youth group’s winter recesses—the one that Lazarsfeld, and his close friend Hans Zeisel, “remembered with particular fondness.”

When Neurath later published his Lebensgestaltung und Klassenkampf (1928)—with its claims for the unity of science—Lazarsfeld and Zeisel formed a debating club, with Rudolf Carnap and Oskar Morgenstern as its official co-presidents. The seminar, one of Lazarsfeld’s early organizational ventures, included leading young thinkers like Heinz Hartmann and Erich Voegelin. (Lazarsfeld never became a logical positivist, but he did for many years conduct a joint seminar on the philosophy of social science at Columbia with Ernest Nagel.)

Socialist party leader Otto Bauer intervened decisively in what would become Lazarsfeld’s most important Austrian work of social research, Marienthal (1933), co-

17. "An Episode in the History of Social Research" 272. See also Zeisel: “In Paul’s case this meant also the very personal influence of the physicist Friedriech Adler, the socialist antiwar hero of the day, who was largely responsible for Paul’s early interest in mathematics” ("The Vienna Years” 10).
18. Zeisel, "The Vienna Years” 11. Zeisel’s memory claim is made here on his and Lazarsfeld’s behalf. Adds Zeisel: Neurath was a “giant of a man in more than one sense, the last polyhistor I knew” (12).
19. Carnap looms large in Zeisel’s memory. Of the Vienna Circle figures, “Carnap was the great event. His introductory lecture into philosophy was an unforgettable intellectual adventure. Students from all over the university were packing his lecture hall, having their first encounter with critical analytical philosophy…” (Ibid 11).
20. It’s not clear, from Zeisel’s account, if Carnap and Morgenstern were real participants or more like honorary figureheads (Ibid 13).
written with Jahoda and Zeisel. Inspired by his reading of the Lynds’ *Middletown* (1929), Lazarsfeld hoped to conduct a community study centered on working-class leisure practices; Austrian trade unions had just won a shortened work day. Lazarsfeld and Zeisel discussed the study with Bauer, who reacted furiously to the planned focus on leisure. Zeisel recalled Bauer’s response: With ten percent unemployment, *that* was the leisure time to study—the social and psychological effects, that is, of persistent unemployment. Lazarsfeld and the others used an array of qualitative and quantitative measures, including depth interviews and historical data, to study the depression-ravaged industrial town. His methodological agility and eclecticism was on vivid display in this union-sponsored study, but many of its innovations were devised in the course of other, mostly small-scale market-research contract work he conducted before and after *Marienthal*.

Lazarsfeld’s market research program was the centerpiece of the fledgling applied psychology institute that he founded, around 1927, as an adaptive response to anti-Semitic barriers to a conventional academic career. He had received his doctoral degree in applied mathematics in 1925, at the University of Vienna, and the same year began

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24. Zeisel, "The Vienna Years” 11-12; Jahoda, "PFL: Hedgehog or Fox?” 5; and Paul F. Lazarsfeld, "An Episode in the History of Social Research” 272.

25. Zeisel, "The Vienna Years” 13.

26. There is some uncertainty as to the founding date of Lazarsfeld’s institute. Zeisel later claimed 1925, as did David Sills in his biographical sketch—probably on Zeisel’s authority (David Sills, "Paul F. Lazarsfeld," in *International Encyclopedia of the Social Sciences* (18) (New York: Free Press, 1979)). Lazarsfeld, in his memoir and elsewhere, claimed 1927, and this seems more plausible given that 1925 was the year that his Ph.D. was awarded, and the year too that he began his assistantship with the Buhlers (Lazarsfeld, "An Episode in the History of Social Research” 274). See the discussion in Jean M. Converse, *Survey Research in the United States: Roots and Emergence, 1890-1960* (Berkeley: University of California Press, 1987) 133n.
teaching at a gymnasium while also working for a professor of psychology, Charlotte Buhler. Buhler and her husband, Karl, had recently joined the University to head up its Psychological Institute. The Buhlers were formative influences on Lazarsfeld, and they clearly recognized his scholarly talents. But even after Lazarsfeld was selected as their assistant in 1931—following many years of research projects and doctoral thesis supervision under their auspices—it was plain to Lazarsfeld and the Buhlers that his academic prospects were severely limited. As he later recalled in an oral history interview, he and the Buhlers both knew that, as a Jews, he could never become a dozent. As a kind of reparation, the Buhlers actively supported Lazarsfeld’s applied research institute and later enthusiastically endorsed his candidacy for the 1933 Rockefeller fellowship that brought him to the United States.

Though Lazarsfeld had already been exposed to the wider current of psychoanalysis, his identity as a psychologist—the only disciplinary label he ever wore with real comfort—grew out of his work with the Buhlers. In the case of both psychologists, it was less the substantive thrust of their research than its methodological implications that captured Lazarsfeld’s interest. Charlotte Buhler’s research centered on childhood development and life phases; under her supervision, he wrote a short book on

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28. On Lazarsfeld’s dissertation supervisions, see Zeisel, “The Vienna Years” 14.
30. Zeisel registers the small world of professional and intellectual Vienna: “We knew Alfred Adler well, although he probably knew us better. Before he turned to curing souls, he was a pediatrician; Paul and I had been his patients” (“The Vienna Years” 11).
the psychology of youth employment, *Jugend und Beruf* (1931) [Youth and Occupation]. Lazarsfeld claimed, as early as 1934, that it was Charlotte Buhler’s childhood research that taught him the “value of interpretation.” Karl Buhler was best-known for his psychology of language, and here again it wasn’t the linguistic analysis itself but his approach that enticed Lazarsfeld. Karl Buhler’s emphasis on the value of introspection was a spur, for Lazarsfeld, for his interpretive analyses of subjective interview data—central to his Vienna market research but also his entire career. In his first English-language publication, a 1934 overview of his “psychological” approach to market research for the *Harvard Business Review*, Lazarsfeld referred to the Buhlers as “the two psychologists to whom I owe most of the stimulation which led to the approach maintained in this paper.”

Lazarsfeld himself traced his earliest interest in social research methods to a chance bookstore encounter: “I remember my excitement when, around 1925, I saw in a bookstore window a scatter-diagram form a German monograph on correlation analysis. It was just like the way musicians describe hearing their first Beethoven chord. I see myself staring, thinking what an unbelievably good idea that diagram was.” He and Zeisel consumed British and social scientific studies with “ardor,” including *Middletown*

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33. Lazarsfeld: “It was Charlotte Buhler’s compilation of a larger range of data toward a vision of phases in childhood, adolescence, and adult life, which taught me the value of interpretation” (“The Psychological Aspect of Market Research” 69n).
34. Lazarsfeld: “It was Karl Buhler’s theory of language, with its new ways of using introspection of the analysis of a total field of human activity, which taught me the possibilities involved in the structural analysis of special groups of action” (Ibid).
35. Ibid.
36. Lazarsfeld, ”An Episode in the History of Social Research.”
and Charles Booth’s surveys of London. Market research was also, for Lazarsfeld, a trans-Atlantic import; he learned about the phenomenon through a student, unnamed, who had been conducting interviews for an American firm. Lazarsfeld and Zeisel carefully studied one of the early German-language applied statistics textbooks, and Lazarsfeld himself, soon after, published his own text, _Statistisches Praktikum für Psychologen und Lehrer_ (1929) [Statistical Methods for Psychologists and Teachers].

Lazarsfeld’s successful establishment of an applied psychology institute—the Österreichische Wirtschaftspsychologische Forschungsstelle—was an early act of entrepreneurial pluck. Drawing on renowned networking skills and realpolitik savviness, Lazarsfeld assembled a board of prominent government, business and academic figures. Though the institute was not officially linked to the University, it was in effect an appendage of the Buhlers’ Psychological Institute. Karl Buhler agreed to serve as board president, and proved to be a decisive source of credibility for the unknown Lazarsfeld.

Most of the Bureau of Applied Social Research’s characteristics that distinguished it from its competitors and imitators—methodological innovation, high analytic intelligence, the valued hunch, the quantitative-qualitative mix, directorial omnipotence, the scholarly re-analysis of client data, even the chaos and financial precariousness—all of these were already in place at the Forschungsstelle. Zeisel later recalled the

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37. Zeisel, “The Vienna Years” 11.
38. Converse, _Survey Research in the United States: Roots and Emergence, 1890-1960_ 133.
39. Lazarsfeld, _Statistisches Praktikum für Psychologen und Lehrer_ (Jena: G. Fischer, 1929); see Zeisel, “The Vienna Years” 12.
40. Converse, _Survey Research in the United States: Roots and Emergence, 1890-1960_ 133.
41. Lazarsfeld: “And it was the respect with which [Karl Buhler’s] name is met that procured me the opportunity to gather my concrete material during the last years” (“The Psychological Aspect of Market Research” 69n).
improvisational energy that drove the institute:

All of its studies were brilliant, late, and cost more money than we could bill our client. Salaries were paid from what was left over. The only thing we ever paid for on time was the coffee our researchers needed when they worked on their reports in a lonely corner of a coffeehouse.42

In many ways, the young staff’s “unabated enthusiasm” (Zeisel) resembled the heady atmosphere of the Bureau in its late 40s heyday. Lazarsfeld’s prodigious organizational energies and his social dexterity ensured the institute’s early survival, but it was only with demonstrable commercial success that the Forschungsstelle could attract a steady flow of market research contracts. The “ice broke,” as Zeisel recalled, when a prominent industrialist wrote to Lazarsfeld with the news that his sales had shot up 27 percent after implementing an institute study’s recommendations.43 An indiscriminate parade of products—butter, coffee, milk, coat hangers, rayon wool—passed through Lazarsfeld’s analytic clutches.44

Lazarsfeld’s approach to market research was highly original, and his emphasis on interpreting consumer motive, in particular, would provoke both admiration and controversy when he brought the techniques to the United States in the 30s. The idea was that consumer decision-making is often unconscious and sometimes irrational—all of which, however, can be gleaned from consumers’ motivational self-descriptions with the aid of careful questioning and analysis. His 1934 Harvard Business Review article was a kind of manifesto for this approach, complete with illustrative examples from the Vienna studies.45 He makes the case for interpretation, for instance, by citing an institute study on

42. Zeisel, “The Vienna Years” 13.
43. Ibid 14.
44. See Lazarsfeld, "The Psychological Aspect of Market Research"; and Zeisel, “The Vienna Years” 14.
45. Lazarsfeld, "The Psychological Aspect of Market Research."
men’s clothing; in a series of highly inferential steps—he actually uses the “reason analysis” label—the Forschungsstelle team used unstructured interview data with customers to surmise that the consumer’s desire for indicators of quality gets displaced, in their absence, onto the clerk and the store’s reputation. The recommendation: to literally display the production process in store-hung posters. In another study, of a laundry service, Lazarsfeld found that tightfisted Viennese women use such services only during emotional crises; he recommended that his client solicit customers who have had a recent family death: “This was not of any spiritual comfort to the family, but it proved to be extremely successful for him, and it is certainly the right way to use a psychological interpretation of market data by deducing a concrete action from it.”

Lazarsfeld’s life-long fascination with the decision act was clearly stirred by his purchase analyses. His devotion to methodological improvisation was, moreover, reinforced in the course of his Vienna market research studies. And the leveraging of client demands for scholarly ends—the Bureau’s modus operandi—was already standard practice at the Forschungsstelle. He even used the potential for scientific payoff in his pitch to the Harvard Business School readership, though not without more old-fashioned appeals: “…we got results which were interesting for the theoretical psychologists and worth the money to the business man.”

46. Ibid 54.
47. Lazarsfeld: “It seems to us that one of the outstanding contributions of the psychology to the problem of market survey is the careful, general study of the structure of the purchase in order to prepare us to find in a special study what could possibly be characteristic of the investigated commodity” (Ibid 65).
48. Zeisel: “But there was never any doubt that the one was only the means toward the other” (“The Vienna Years” 14).
49. Lazarsfeld, “The Psychological Aspect of Market Research” 71. Lazarsfeld concludes the 1934 article with: “There is no better way for the psychologist to approach these important questions than to participate in market surveys. There is no better support for the practitioner than to have the
In the flurry of Vienna market studies, one stood out for its later, serendipitous import: The institute conducted one of the first European radio listening surveys—which would, in 1937, catch the attention of Hadley Cantril and Frank Stanton, then casting about for someone to direct their newly funded radio research project. At the time, however, it was just another study: “… I had done some radio research in Austria—a listener survey,” Lazarsfeld recalled in an oral history interview, “but to me there was little difference between research on buying chocolates and research on radio listening.”

By the time the 32-year-old Lazarsfeld left for America in 1933, for what was intended to be a one-year traveling fellowship, he had already adapted resourcefully to a series of challenging conditions. His Vienna experience was, however, uniquely formative. In the course of his circumstantial maneuvers, he had developed a pair of intellectual interests, a research model, and a set of adaptive skills. Over the nearly five-decade span of his American career, Lazarsfeld’s basic orientation to his adopted scholarly fields would remain unchanged. This is not to say that his was a linear and stable career—nothing like this could be said for a scholar who ranged from soap to latent variable analysis. But its patchwork character, its sheer topical diversity, does look, from another angle, much steadier. Methodology and decision psychology—these are his great topics in 1934 and in 1974. His colleague and ex-wife Marie Jahoda asserted, in Isaiah Berlin’s contrast, that Lazarsfeld was a fox masquerading as a hedgehog. In fact

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51. Jahoda, "PFL: Hedgehog or Fox?"
the reverse is true: He was an unusually disciplined hedgehog who, with apologies to
Berlin, had the adaptive traits of a chameleon. It was the environment—in institutional,
funding, and intellectual terms—that was unstable. Lazarsfeld, to be sure, reacted to
environmental change adeptly, without however sacrificing his core interests. It only
looked that way.

Lazarsfeld’s quick and successful integration into American academic life seems
remarkable, compared to the often-wrenching experience of other Nazi refugees. Many
emigrés eventually made “successful” adjustments, to be sure, but most faced an
excruciating transition period. Some could never quite adapt to their wartime haven, and
opted to return to Central Europe in the years after the war. The contrast between
Lazarsfeld and Frankfurt scholar Theodor Adorno, though somewhat hackneyed, is
revealing in this respect. (Adorno worked under, and clashed with, Lazarsfeld at the
Office of Radio Research in the late 30s.) It is striking to read Lazarsfeld’s memoir
alongside Adorno’s bewildered and alienated reminiscences, published in the same 1969
volume: “I consider myself European through and through, considered myself as such
from the first to the last day abroad, and never denied it.” Lazarsfeld, in a memo to
Cantril and Stanton soon after Adorno’s arrival, used his own relative Americanization to
measure Adorno against: “He looks exactly as you would imagine a very absent-minded
German professor, and he behaves so foreign that I feel like a member of the Mayflower
Society.”

52. Adorno, "Scientific Experiences of a European Scholar in America," in The Intellectual Migration: 
Europe and America, 1930-1960, eds. Donald Fleming and Bernard Bailyn (Cambridge, MA: Harvard

Now it’s true that Lazarsfeld himself never shook off the feeling that he was an outsider—he was, as he stated on more than one occasion, self-conscious of his “foreign” appearance, his accent, and his Jewish background. And it’s also true that he arrived in the United States already better matched to its empirical, ahistorical academic culture than most of the other refugees. But he nevertheless approached the American landscape with a more malleable countenance than was worn by his fellow emigrés. One index of his rather supple mind-set—determined, as he was, to build a scientific career in these strange new surroundings—was his abrupt and decisive abandonment of socialist politics. Many of the biographical treatments of Lazarsfeld admit to stupefaction at his sudden indifference to socialism and indeed to politics itself. There is also a line of interpretation that, in its effort to defend Lazarsfeld from Gitlin-like accusations of corporate obeisance, stretches his Austrian socialism across the Atlantic—pulls at that exculpatory leftism until it is made to cover his entire American career. This is simply


54. "An Episode in the History of Social Research"; Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 9; and Platt, A History of Sociological Research Methods in America 224-25. And Lazarsfeld’s American reception was, no doubt, not without its seen and unseen anti-Semitic traces. His Bureau student, Peter Rossi, remembers being present when the University of Washington sociologist George Lundberg told Lazarsfeld that he was a good fellow, although he was Jewish (Platt, A History of Sociological Research Methods in America 224).

55. Franz Neumann, writing in 1953 in an essay centered on the experience of those, like himself, who found an intellectual home in the United States, but who were, nonetheless, disoriented on arrival: “The German exile, bred in the veneration of theory and history, and contempt for empiricism and pragmatism, entered a diametrically opposed intellectual climate: optimistic, empirically oriented, a-historical, but also self-righteous” ("The Social Sciences," in The Cultural Migration, ed. W. Rex Crawford (Philadelphia: University of Pennsylvania, 1953) 19). Though not self-righteous, Lazarsfeld was already optimistic, empirically oriented, and a-historical on arrival.

not plausible, and Lazarsfeld himself, in late-life oral history interviews, conceded the he had lost his political appetites soon after landing in America.57

His socialist commitments, in Austria, were not half-hearted nor were they merely channeled to activist ends. Marienthal, after all, had been framed in terms of the left’s key empirical question—would working-class deprivation, as predicted, provoke revolutionary protest or, instead, resigned apathy? (Apathy, Lazarsfeld and his colleagues concluded. As he later quipped to co-author Jahoda, an approaching revolution requires economists; a successful revolution, engineers; and a lost revolution—social psychologists.)58 The American Lazarsfeld would keep his distance from politics, certainly of the radical kind, and never again conduct large-scale studies centered on “leftist” questions. When research programs, like his studies of media effect, forced him to confront controversial political issues, he was always measured and diplomatic—and would slide along a centrist-to-liberal continuum depending on the real or imagined audience. When later asked about his political molting, he proposed—though conceding his own puzzlement—that his socialist energies had perhaps been sublimated into research work, with his voting studies in particular as substitutes for political participation.59 It seems more likely, however, that Lazarsfeld recognized from the start that the socialist movement in America was not only anemic—wrecked, as Sombart had written, on the reef of roast beef and apple pie—but also a very real obstacle to a successful academic career. In Austria, he could scrutinize soap sales and run socialist

57. See the summary in Converse, Survey Research in the United States: Roots and Emergence, 1890-1960 134n.
58. Jahoda, "PFL: Hedgehog or Fox?" 5.
youth camps, but not in the United States. Lazarsfeld chose science—and scientific prominence—and was willing to sacrifice politics to those ends. He later admitted that he had, on arrival, consciously altered his writing style, on Charlotte Buhler’s recommendation, to downplay ardor in favor of detachment.  

It is easy to over-extend this argument along the lines of a classic American literary trope—the story of a cynical and crafty confidence man driven by a relentless and amoral ambition to make it, through the deceptive means of fabrication-cum-self-invention. It is also a trope in the literature of anti-Semitic slander. As applied to Lazarsfeld—and to most cases of scientific reputation-seeking—this kind of account is woefully inadequate. The main problem is that the self-invention storyline very poorly captures the specific dynamics of academic life—which, after all, place the burden of collective knowledge gain on the quest for distinction itself. The pattern of any given field’s recognition is asked to serve as a filtration surrogate, to separate scholarly gains and promise from the many more failures and dead ends. Particular advances for knowledge are only spread about and remembered—recognized, that is, for what they are—through the flagging that reputation entails. And the rewards of scientific credit supply a motivation—at the individual but also departmental and disciplinary levels—that should complement the academic culture’s parallel valuation of intrinsic truth-seeking. The messy and clotted project of collaborative truth-making, in short, would be inconceivable without distinction-seeking and the assessment-shorthands of earned recognition. In one respect, the wheels of academic truth are meant to turn in analogous...
ways to the free market’s, where private pecuniary gain sought on a large scale is said to produce emergent, society-wide benefits.

If the sociology of academic knowledge has established anything, it is that recognition and credit are not always meted out in strictly Darwinian terms. Reputation turns on a number of contingent factors other than, say, the judgment of competent peers. All that this means, however, is that we should not assume beforehand that credit or reputation (contemporary or remembered) works in the way that the model suggests that it should. Sometimes the reputation system “performs” better than it does in other contexts—and the variations derive, for the most part, from the specific conditions that make up particular fields.

Lazarsfeld’s American drive for scientific distinction was first made within a field, public opinion research, that was in many respects organized around a set of methods. His genuine intellectual interest in technical research innovation was, in the terms discussed above, in line with the field’s focus and collective competence. His early reputation as a formidable methodologist was based on careful assessments of his highly creative contributions. This is not to say that the attention Lazarsfeld paid to self-promotion wasn’t also important; his credit-seeking, after all, was crucial to his methods’ diffusion to the rest of the field, with fruitful consequences. In the realm of methods, reputation and knowledge gain were profitably linked.

But the field was not nearly so well-equipped to assess other, more substantive claims to originality. Public opinion research, in the mid-1930s and beyond, was a very strange organizational beast. There was no disciplinary structure—save a single journal,
Public Opinion Quarterly and, after 1948, the American Association for Public Opinion Research—to hold the field together; instead it cohered around the methods themselves and a set of overlapping personal relationships among psychologists, sociologists, pollsters, market researchers, and foundation officials clustered in the Northeast. The field’s resource base was supplied by many of the same foundation and market research figures. Some of the field’s features—especially its topical and theoretical aimlessness, and its a-historical scientism—meant that it was poorly equipped to judge novelty claims outside the methodological arena, particularly if the claims were asserted by way of historical contrast.

Lazarsfeld was fortunate to arrive in the United States just when the public opinion cluster took shape, in the mid-1930s. With the well-publicized 1936 “birth” of modern polling techniques, commercial polling firms, which often doubled as market research shops, formed an alliance with social scientists eager to exploit the academic potential of these techniques. Many of the psychologists came out of a young tradition of attitude measurement, whose insights were then mingled with the pollsters’ and market researchers’. The Rockefeller Foundation, which had turned away from basic social science in the early 30s, quickly recognized the applied relevance of the new methods.

Lazarsfeld—by way of psychological and statistical training as well as market research experience—was almost perfectly positioned to make his mark on the emerging field. His assiduous cultivation of contacts and professional friendships and—somewhat later—his organizational talents were important factors in his rise to prominence, but his reputation ultimately ascended on the strength of genuinely important contributions. He
earned his high reputation within the field, which is to say that credit and achievement were working, as they should, in tandem.

Lazarsfeld’s social acumen was not, in other words, a substitution for academic merit, but really more of an enabling complement. His trademark research style, which he described as “improvisation guided by available material and personal interests and contacts,” meant that scholarly gains were dependent on entrepreneurial agility. It may be possible to push this slightly further: His intellectual fecundity seemed to feed off of his madhat organizational scrambling.

It is useful here to invoke, then collapse, a contrast employed by Gary Oakes and Arthur Vidich in their recent study of Hans Gerth, C. Wright Mills and their 1946 Weber collaboration. In Oakes and Vidich’s interpretation, Gerth supplied the “intellectual capital”—German-language skills and a deep knowledge of Weber’s thought—while Mills provided “social capital, “the interpersonal know-how, networking skills, and publishing knowledge that proved crucial to the volume’s success. Lazarsfeld, to put the matter bluntly, had both sorts of capital in abundance, and he blended them with real acuity. This was how an Austrian applied math Ph.D who specialized in psychological market research could become the president of the American Sociological Association—all the while remaining, fundamentally, a psychologist. He was not Dale Carnegie with glasses, but instead a genuine scholarly talent with a complementary set of social skills.

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62. Platt’s observations on this question are in the same vein: “There is more than one reference in the files to reservations about Lazarsfeld’s personal style, his possible charlatanism and excessive salesmanship—but these are overcome because he is also seen, surely rightly, as outstandingly dynamic and entrepreneurial as well as intellectually stimulating” (A History of Sociological Research Methods in America 168n).
These social skills are the stuff of legend, but less attention has been paid to his specific gift for mnemonic caretaking. He was quite conscious of the vagaries of reputational survival, and took steps to ensure that his contributions would not be forgotten. In the search to explain why one scholar gets remembered over his peers, it turns out that the presence, or not, of a left-behind “school” is crucial—and even a single, assiduous, mnemonic champion can be enough to keep a reputational flame lit.

Lazarsfeld was fortunate, or careful enough, to have both, but he was also a proactive tender to his own legacy, especially late in his career. When Lazarsfeld wrote an introduction to his friend and colleague Samuel Stouffer’s collected papers, he gently reprimanded Stouffer for not understanding the sociology of academic labeling:

> When you go through Stouffer's papers, you find at every point an interesting new contribution, but it is never tagged… While Sam and I worked on various similar matters, most of the time in complete agreement, he did it and I added a slogan to it… he didn’t recognise, so to say, the important things he did, and therefore impeded in a way his role in the history of sociology.  

Lazarsfeld was acutely aware of his own role in the history of social research, and he devoted considerable time and energy to its care. In a 1969 memo, for example, he was at

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63. See, for example, Lamont, "How to Become a Dominant French Philosopher: The Case of Jacques Derrida." See also Gladys and Kurt Lang’s fascinating study of the reputation of etchers (Etched in Memory: The Building and Survival of Artistic Reputation (Chapel Hill: University of North Carolina Press, 1990)). Incidentally, the Langs are mass communication researchers who made important contributions to the field from the 50s on but whose reputation suffered for its failure to fit into the “powerful-to-limited-effects” narrative except, fallaciously, as an exemplar of the discredited “powerful” camp. Even a recent, otherwise fair treatment repeats the classificatory mistake: Elihu Katz and Daniel Dayan, "The Audience is a Crowd, the Crowd is a Public: Latter-Day Thoughts on Lang and Lang's 'MacArthur Day in Chicago'," in Canonic Texts in Media Research: Are There Any? Should There Be? How About These?, eds. Elihu Katz, John Durham Peters, Tamar Liebes, and Avril Orloff (Cambridge, UK: Polity, 2003).

64. Quoted in Platt, A History of Sociological Research Methods in America 32. Platt, in an excellent chapter, attempts to explain the reputational disparity between Stouffer and Lazarsfeld with this and other factors. Platt conducted a citation study of the two, over 1940-1960, and found that “Lazarsfeld received more citations from authors with whom he had some kind of personal contact—most of them, moreover, not accountable for by the citing and the cited work coming form the same field; his colleagues have left innumerable memorials to him, as well as imitating the institution he created; he had his festschrift, though his continuing activity made it so late that it became a memorial volume and there have been further publications since his death” (259). Needless to say, Lazarsfeld’s long-time intellectual partner, Robert K. Merton, was an especially astute labeler.
pains to narrate the history of “public opinion and consumer research” in a manner that stressed the centrality of market research—as exemplified by the 1937 Techniques of Marketing Research, to which he contributed four chapters. He devoted much of his late-career energies to histories of social research methods, which were unsurprisingly attentive to his own central place.

Lazarsfeld, as he departed from the field of media research in the late 40s and early 50s, set out in much the same way to seal his reputation as an innovative pioneer. It is significant, however, that Lazarsfeld’s later efforts on behalf of his methodological legacy were corrected and qualified almost immediately, whereas his self-aggrandizing media storyline remains, in 2005, the core of many textbook accounts. This is true even though the exaggerations in his social research writings were less pronounced than those found in Personal Influence. The explanation lies with the public opinion cluster, and the specific circumstances in which the field came to receive—from Rockefeller, media firms, and then the federal government—the mantle of media research. Public opinion scholars, including Lazarsfeld, were mostly ignorant of previous media scholarship, which was, moreover, scattered about in interdisciplinary pockets. Nor were they especially curious about their predecessors. Lazarsfeld himself, as he emphatically and repeatedly declared over his career, had no interest in media effects per se—but instead

65. Lazarsfeld: “Public opinion and consumer research is a field in which the collaboration between experts within and outside the university has always been very important.... The story begins around the 1930s. Sampling theory became known, especially through English influences; and attitude measurement was put on the map due to Thurstone. But commercial research agencies disseminated the ideas widely through public opinion polls and market research agencies. It is still worthwhile to look at a publication which the first MRA published with McGraw in 1937 called The Techniques of Marketing Research,...” (quoted in Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld’s Communication Research Papers 23-24).

66. See, for example, Lazarsfeld, Qualitative Analysis: Historical and Critical Essays (Boston: Allyn and Bacon, 1972).
stumbled into the field as a result of funding opportunities. When he and Katz came to narrate the history of media scholarship in 1955, the public opinion cluster—and the world of empirical sociology it had become, to some extent, embedded in—had no competence or historical knowledge to assess the storyline. There was nothing duplicitous about Lazarsfeld’s narration, even with all of its mistaken assumptions. Lazarsfeld probably did not know better, nor was he invested enough—except in reputational terms—to find out. The rest of the field, moreover, could not and did not monitor its basic claims. There was nothing, in short, to stop the public intellectual defenders of American mass culture to wield their pens in its name. Nor were there any red flags standing that might have warned the newly institutionalized discipline of communication in its search for a usable, teachable past.

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It was, somehow fittingly, the union-sponsored *Marienthal* study that secured Lazarsfeld his Rockefeller Traveling Fellowship. This one-year fellowship became his means to permanent escape when, in 1934, the Austrian fascists seized power.\(^{67}\) Karl Buhler had arranged for Lazarsfeld to report the *Marienthal* findings at the International Congress of Psychology in Hamburg, and his presentation caught the attention of Rockefeller Foundations officials.\(^{68}\) They urged him to apply, which however Lazarsfeld in the months to follow neglected to do; he had apparently forgotten about the opportunity. It


\(^{68}\) Ibid; see also Glander, *Origins of Mass Communications Research during the American Cold War: Educational Effects and Contemporary Implications* 113.
was only after a Rockefeller follow-up in 1932—when he was queried about his silence—that he hastily completed the paperwork.\textsuperscript{69} The Foundation, for all its eagerness to secure Lazarsfeld’s visit, apparently judged his socialist politics and Jewish background to be liabilities; according to his papers, the Foundation weighed these “handicaps” in its 1934 decision to grant Lazarsfeld an extension on his fellowship.\textsuperscript{70}

In his initial application, Lazarsfeld pointed to two objectives for his American sojourn: to study applied psychology organizations, and to learn the survey techniques and other methods used in “market analysis, salesman training and advertising.”\textsuperscript{71} Lazarsfeld spent the year traveling all over the United States, building up a network of contacts in the social psychology and market research fields. Some of his earliest meetings were with American scholars he had met at the International Congress, including the Columbia sociologist Robert Lynd—whose \textit{Middletown} (written with Helen Lynd) had partly shaped the \textit{Marienthal} study. Lynd, at the time, was studying unemployment in Montclair, New Jersey, and Lazarsfeld volunteered to help.\textsuperscript{72} More than any other, the connection with Lynd would prove essential to the succession of increasingly prominent posts that Lazarsfeld was to secure in his first decade in America.

Lazarsfeld leveraged his prodigious energies into a series of cross-country trips and volunteer research projects. Always identifying himself as a psychologist, he visited

\textsuperscript{69} Lazarsfeld, "An Episode in the History of Social Research."

\textsuperscript{70} The evidence in the Lazarsfeld papers are discussed and cited in Glander, \textit{Origins of Mass Communications Research during the American Cold War} 113-14.

\textsuperscript{71} The application is quoted in Pasanella, \textit{The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers} 10. Interestingly, Lazarsfeld also announced his intention to spend a “certain amount of time in Chicago”: “The investigations of Prof. Thomas and Prof. Burgess in social psychology have been the model for a series of surveys which I myself have carried out through our Vienna Research Bureau of Economic Psychology.”

Harvard, the University of Chicago, Ohio State, the University of Rochester, the University of North Carolina, Cornell, and the University of Pittsburgh over the course of the year. At Harvard, Lazarsfeld gave a talk to Gordon Allport’s seminar; Allport would become a key figure in the public opinion cluster. At UNC, he sought out the quantitative sociologist Stuart Chapin and William McDougall, the pioneering social psychologist. In Rochester, he visited with the marketing psychologist Luther Fry, and at Cornell he met John Jenkins, the applied psychologist.

His extended stays in New York and in Pittsburgh were, however, the most important for his future academic career. In addition to Lynd, Lazarsfeld met with the NYU psychologist Rensis Likert—who, after a stint of applied research with a life insurance institute, would in 1939 head up the Department of Agriculture’s important Division of Program Surveys. Likert, too, would become a central figure in the soon-to-emerge cluster of public opinion research. But it was Lazarsfeld’s volunteer work for the Psychological Corporation—the newly formed applied psychology vehicle—that connected him with the tight-linked circle of New York-based market researchers.

73. On his psychologist self-identification, see Ibid 10. On his dizzying travels, see Ibid 10-11; Glander, Origins of Mass Communications Research during the American Cold War 113-14; Converse, Survey Research in the United States 134; and his own memoir ("An Episode in the History of Social Research").


75. See Wilbur Schramm, The Beginnings of Communication Study in America: A Personal Memoir 49; and Converse, Survey Research in the United States 134.

76. Ibid.


Through the Corporation, Lazarsfeld met Arthur Kornhauser, George Gallup, Percival White, and many other leading commercial analysts; the American Marketing Society soon commissioned him to write four chapters of a planned methods survey, published in 1937 as *The Technique of Marketing Research*.  

Lazarsfeld’s two- or three-month stay in Pittsburgh was even more serendipitous. Lazarsfeld traveled to Pittsburgh to meet David Craig, who directed the University of Pittsburgh’s Retail Research Institute. The day before his arrival, the Institute had been asked by a department store to study customer aversion to products with “rayon” on their labels. “On the following day,” stated the report that Craig and Lazarsfeld eventually issued, “by accident the two authors of this paper happened to meet”:

> One was an Austrian with a method, seeking an opportunity. The other was an American with a problem, seeking a solution that appeared attainable through the Austrian’s method. The result was a happy collaboration, a three-month cooperative arrangement involving a thousand organized interviews and innumerable experimental calculations, summarized briefly herewith.  

In typical Lazarsfeldian form, the report boasted about the authors’ resourcefulness at turning a “trivial question” into one of “potential academic interest.”

Judging by their correspondence, the Craig-Lazarsfeld relationship was a mercurial one, but the collaboration led, in 1934, to an offer of a full-time research post at the Institute. By this time, Lazarsfeld had already applied, and received, an extension on

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80. Converse reports a two-month stay, but a University of Pittsburgh report (see below) claims three (*Survey Research in the United States* 134).
82. Ibid 51.
his Rockefeller fellowship.\textsuperscript{84} (After the attempted coup in July 1934, Lazarsfeld’s parents were briefly imprisoned for hiding a well-known (and unnamed) socialist from the police.)\textsuperscript{85} He decided, over the course of this second year, as the Austrian climate steadily worsened, to emigrate permanently to the United States.\textsuperscript{86} In 1935, while back in Austria to settle his affairs and secure a proper immigration visa, his University of Pittsburgh offer was summarily withdrawn.\textsuperscript{87}

It was Robert Lynd who rescued Lazarsfeld. The two had remained in close contact since their 1933 meeting. In a series of surprisingly warm and chatty letters sent from Muncie—where the Lynds were preparing their \textit{Middletown} update\textsuperscript{88}—Lynd kept careful tabs on Lazarsfeld’s job prospects. One letter, sent just before the job fell through in June 1935, asked whether the Pittsburgh post was secured. “I devoutly hope so as I hate the thought of losing you from my thinking. I’ve missed you in past 2 mo’s.”\textsuperscript{89} When informed that the offer had been withdrawn, Lynd wrote back that the news came “like a... because you come to me from a mixed-up, strange, foreign would composed of an unfettered imagination and a rather precise control of it that you exercise only when you feel like it... [Y]our sophistication in the world of ideas combines with a wholly charming elaphantine innocence in the conventions which I have always known so thoroughly” (11).

\begin{itemize}
\item \textsuperscript{84} In his renewal application, Lazarsfeld wrote: “I am staring now on a survey of American literature on motivation... reviewing the theoretical basis of the application of psychology in market and social research and compiling the studies—which are mostly without contact with one another—and by this way evaluating the different methods” (quoted in Ibid 11). He is probably referring to his ongoing work for the American Marketing Association methods volume.
\item \textsuperscript{86} Pasanella, \textit{The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers} 11.
\item \textsuperscript{87} Ibid.
\item \textsuperscript{89} The letters are quoted extensively in Pasanella, \textit{The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers} 11. In the same letter, Lynd gushed: “I have five assistants and we’re having a grand time. Muncie is receiving us royally, bless their generous hearts!”
\end{itemize}
bombshell to me.” He set out to find Lazarsfeld a post, and initially speculated about market research work, which, he cautioned, would not be very stimulating. Soon after, Lynd located a research project, funded by the National Youth Administration and centered at the University of Newark, that was looking for a director. Lazarsfeld left Austria immediately to take up the post—and just a few months later had convinced the University’s ambitious president to permit him to convert the project into a permanent organization, the Newark Research Center. He had, through ingenuity and persistence, managed to recreate the Forschungsstelle.

The Lynd-Lazarsfeld relationship, which evolved over many decades at Columbia, is fascinating and deserves more study. Lynd’s initial help with the Newark job was followed up repeatedly: a key recommendation helped Lazarsfeld obtain the directorship, in 1937, of the new Office of Radio Research (ORR); Lynd’s crucial intervention two years later enabled Lazarsfeld to move the ORR to Columbia University and secured him a temporary post in its sociology department; and, when a full-time position opened in 1941, it was Lynd who championed Lazarsfeld against Robert MacIver’s preferred choice, Robert K. Merton. In a now legendary compromise, Lynd and MacIver arranged to appoint both scholars—which led, of course, to a lifelong partnership between Merton and Lazarsfeld that fueled Columbia’s postwar sociological

90. Ibid.
91. Lynd, in the same August letter: “While advertising agency research is lousy and jobs are scarce, I should think there is a real likelihood that you could wangle something… But you would be doing market research and make no mistakes about that—with only overtones of ‘motivation.’”
92. In an undated letter, Lynd: “Now here’s something not to be Viennese about but to write at once: In Newark Friday afternoon, I unearthed a job that’s crazy for you: the social agencies want someone to locate and appraise all available social data on Newark” (quoted in Ibid 11-12).
93. Ibid.
juggernaut. Some time after the 1941 appointment, perhaps over a period of years, the Lynd-Lazarsfeld relationship soured. Lynd, in 1951, wrote a long, reproachful letter to Lazarsfeld, accusing him of disloyalty. In a later oral history interview, Lazarsfeld claimed that Lynd “always was, without knowing it, an anti-Semite. There is not the slightest doubt.” It seems likely that differences over politics played some role in the split: The leftist Lynd’s roots were in Protestant reformism; his Knowledge for What? (1939) was a plea to use social science-based planning to bring about a better society. He championed Lazarsfeld for just this reason—because of his applied focus—and was probably disappointed by Lazarsfeld’s studied, corporate-friendly apoliticism.

The Research Center, loosely affiliated with the University and housed in an


95. Lynd: “I place emphasis on departmental loyalties, on reciprocal mutualities; and you, on personal loyalties... doing things for other people so they will reciprocate... I, with my Protestant moralism, see society as made up of persons in mutual interaction” (quoted in Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 5.) In a letter written after her husband’s death, Helen Lynd suggested that Lazarsfeld drove the men apart: “Paul’s attitude toward my husband was very complex. I think that what I have called 'shame' does enter into his feeling of indebtedness, but there are two other things that are almost independent. One is his strong sense of hierarchy. I may have mentioned to you that he said many times that in meeting a new person his first thought always was: ‘Is he above me’ or ‘Am I above him.’ Another is his extreme reserve about showing his feelings in any direct way” (quoted in Ibid 30).

96. Quoted in Glander, Origins of Mass Communications Research during the American Cold War 114.


abandoned brewery nearby, had much the same character that distinguished Lazarsfeld’s past and future organizational endeavors: a young, energetic and enthusiastic staff, supported by commercial research contacts.99 Like most of Lazarsfeld’s other ventures, the Newark Center eked out a precarious, contract-by-contract existence. As Rose Goldsen, one of Lazarsfeld’s Newark staffers, recalled, the Center was often edged onto the “cliffhanging precipice of finance.”100 Lazarsfeld conceived the center as a “consulting service to social and business agencies in the city,” though with the usual intent to squeeze academic relevance from the work.101 In addition to the federal research, which centered on unemployment, the Center did contract work for Eastman Kodak (on home movie cameras), DuPont (on the “relative pleasantness” of various fabrics), and the Milk Research Council (on “why young people dislike milk”).102 Lazarsfeld’s institute also conducted a magazine readership study for an unknown sponsor, which may have enhanced his media research credentials when, in 1937, he was offered the directorship of the new Princeton Office of Radio Research.103


100. Neurath, "Paul F. Lazarsfeld and the Institutionalization of Empirical Social Research."

101. From an undated letter to the University of Newark’s president, quoted in Glander and archived in Lazarsfeld’s Columbia papers (Origins of Mass Communications Research during the American Cold War 115).

102. Glander’s quotations and summary are from the undated latter to the University’s president (Ibid).

103. Ibid
During the Newark Center years, 1935 and 1936, Lazarsfeld continued work on his American Marketing Society chapters, and entered broader methodological debates on interview technique just as the public opinion field began to cohere. In his memoir, Lazarsfeld recalled that around this time he came to realize that he could be a “connecting cog” between certain “speculative” and “empirical” currents in American social science: “A European ‘positivist’ was a curiosity welcomed by men aware of the subtler trends in the American social sciences.”

A number of market psychologists, including the Psychological Corporation’s Henry C. Link, had been dismissive of attempts to glean consumer motivation from self-reports, in line with the prevailing behavioral orthodoxy. Lazarsfeld’s introspective motive analysis, honed in Vienna, was a direct challenge to the black-box view. In early 1935, he published his classic, manifesto-like article, “The Art of Asking Why in Marketing Research.” Lazarsfeld’s arguments for “reason analysis” and for the indispensability of the unscripted, reactive depth interview were widely influential, and prefigured the “focused interview” that Merton was to codify a decade later.

104. "An Episode in the History of Social Research." Lazarsfeld, though he uses the term here (in scare quotes), was not really a positivist—not in the Comtean sense, nor in terms of the Vienna Circle’s philosophy of science. The term is a relentlessly abused one that is normally used now as a pejorative meaning something like “scientistic,” or as a loose contrast to “critical.”


106. Lazarsfeld, "The Art of Asking Why in Marketing Research," National Marketing Review 1 (1935). Lazarsfeld later credited this paper as the first time he laid out his individualist, decision-study “accounting scheme” that he applied to mass communication research, which distinguishes between a) the individual’s attributes; b) the attributes of the product (or program content); and c) external influences on actions. See Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 6.

excellent history of survey research, Lazarsfeld was a “prominent participant in the market research community and was already making a visible mark on its practice…” Lazarsfeld had, through will, careful maneuvering, and a measure of luck, established a niche for himself in applied psychology. It was nearly all luck, however, that brought him into the media research arena.

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CHAPTER FIVE
Lazarsfeld’s Personal Influence

It was the presidential election of 1936 that played improbable midwife to the new interdisciplinary field of public opinion research.¹ That year, three pollsters—all former (and future) market researchers—engaged in a dramatic face-off with the sovereign of straw-vote journalism, the Literary Digest poll.² George Gallup, Elmo Roper, and Archibald Crossley, using relatively new sampling procedures, correctly predicted Roosevelt’s victory—besting the Literary Digest’s two million straw votes. The David-and-Goliath triumph of the pollsters was a publicity-stoked symbolic event, and would take on truly mythic import for the emerging polling industry and its market research parent. The pollsters’ upstart success, however, was not merely a hyped-up, front-page news frenzy—though it was this—but also reverberated among a scattered group of social scientists, many of whom were already interested in interview and questionnaire techniques. The 1936 Literary Digest thrashing had very real organizational consequences: The public opinion cluster—and its long-lasting, if unstable, alliance between social scientists and commercial analysts—formed in its


². Converse provides the details (Survey Research in the United States 87-131).
immediate wake. And the 1936 showdown would long remain a mnemonic benchmark for this peculiar, quasi-academic field—even if the public victory was occasionally downplayed by some of the reputation-sensitive social scientists in its midst.

In the aftermath of their triumph, Crossley, Roper and especially Gallup mounted a many-faceted promotional campaign to establish the new polls’ scientific and democratic significance. Gallup, for example, repeatedly represented the polls as democracy’s answer to the vexations of modern scale and distance. With populist bluster, Gallup portrayed the polls as a popular megaphone—the “surprisingly sound” collective judgment of the “little people of the country.”

The pollsters were also eager to assume the trappings of scientific legitimacy, and widely touted their new “science of polling” as a crucial tool for the objective picture of society that social scientists were fast-forming. (Without conceding too much to the pollsters themselves, many of their soon-to-be social scientific allies claimed much the same.) Gallup, the most publicity-savvy of the three, published tirelessly with this theme, in the popular press as well as in the new social science journal, *Public Opinion Quarterly*, that had emerged in the aftermath of his *Literary Digest* success in 1937. His 1938 pamphlet, “The New Science of Public Opinion Measurement,” was typical of his exhortational campaign. He self-consciously named his polling organization “the American Institute of Public Opinion,” which he established across the street from Princeton University. The pollsters, and their market research brethren, were to

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4. See Converse *Survey Research in the United States* for a thorough description of the pollsters’ post-*Literary Digest* promotional efforts.
unremittingly seek out a scholarly mantle, for both commercial and stand-alone reputational reasons—and their hunger for scientific recognition rendered them willing partners to university-based social scientists. They approached the academics, to put it crudely, with a dowry, in the form of raw data, financial resources, and survey machinery.

The public opinion field’s bundle of methods, and its academic-commercial coalition, had roots in two overlapping but distinguishable traditions—one primarily psychological, the other centered on market research. Their competing genealogical claims would play out over the decades to come, as the market researchers, in particular, struggled to assert their primacy against the social scientists’ reputation-sensitive stress on academic legatees.5 (It should be noted that a few key social scientists, notably Hadley Cantril, Samuel Stouffer, and Lazarsfeld himself, were careful to give market research its due, though in a still-qualified way that highlighted the scientists’ improvements. Their peace-keeping efforts were crucial to the alliance’s durability.) It is impossible to neatly disentangle the academic and commercial claims, in part because their had been, long before 1936, so much intermingling between applied psychologists and market researchers. But it is possible to locate a primarily academic current, centered on social psychological efforts to measure attitude, distinct from though contributory to the market-research community’s own evolving methods.

Attitude measurement emerged in the 1920s, primarily as a quantitative strand of the nascent psychology-based social psychological field.6 (The concept of “attitude” is often credited to W.I. Thomas and *The Polish Peasant* (1920)—and was indeed one source for the psychologists’ emergent conception. But the sociological currents of social psychology then coalescing at the University of Chicago were mainly outside the effort to quantify attitude—though the Park student Emory Bogardus’s artful synthesis of attitude scaling and the Parkian “social distance” concept, in the mid-1920s, was a conspicuous exception.) Floyd Allport, author of the then-leading psychological textbook *Social Psychology* (1924), developed the attitude-measurement scale which was then adapted by University of Chicago’s L.L. Thurstone in 1928 to include equal-appearing intervals.7 Rensis Likert, in his 1932 dissertation, modified Thurstone’s elegant but demanding technique to require fewer scale items and questions.8 The Hawthorne studies’ employee-attitude measurements in the late 20s and early 30s, and the sprawling, survey-based monographs produced by Charles Merriam’s Chicago political science department in the 20s and 30s—especially Harold Gosnell’s—were also significant contributors to a loosely structured academic tradition of attitude measurement.9

6. James Good provides a wonderful history of the historiography of social psychology, with its “dual heritage” origin myth formed in the 1930s and reinforced thereafter—which, however, accurately reflected the independence trajectories that the disciplinary versions of social psychology had, by the 1920s, embarked upon ("Disciplining Social Psychology: A Case Study of Boundary Relations in the History of Human Sciences," *Journal of the History of the Behavioral Sciences* 36(4) (2000)); Ian Lubek’s treatment in the same issue is rich and complementary to Good’s ("Understanding and Using the History of Social Psychology," *Journal of the History of the Behavioral Sciences* 36(4) (2000)).


Market research, with roots as early as the mid-1890s, was fundamentally the creation of applied psychologists and the new marketing and advertising professions in the 1910s. A number of journals, research institutes, and trade associations were formed in that decade. Newspapers, magazines, and then in the 1920s radio networks, sponsored a series of studies into circulation, listenership and advertising effectiveness—largely to persuade each medium’s potential advertisers. Consumer-oriented product and marketing research also flourished in the 1920s and into the Depression years. The applied psychology consortium, the Psychological Corporation, added a Market Surveys Division in the early 1930s, which conducted a flurry of sample-based surveys using students as interviewers. By the early 30s, market researchers were using a wide variety of techniques, including scaling, unstructured interviews, and sampling procedures. By the mid-1930s, of course, Lazarsfeld himself was an important innovator in the field.

Despite these antecedents, it was the migration of sampling techniques to national issues and elections, in 1936, that triggered the formation of the relatively coherent cluster of “public opinion” researchers. But crucial too were the handful of entrepreneurial social scientists—notably Cantril, Stouffer, Likert and Lazarsfeld—who recognized and exploited the polls’ and polling organizations’ potential to transform

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empirical social science. Just months after the election “upset,” the embryonic field gained a publishing vehicle in the *Public Opinion Quarterly*, which was to long serve as the field’s formal crossroads.\(^1\) Founded by the Princeton political scientist Harwood Childs with Cantril’s help, its editorial board and its first-volume roll of authors included leading figures from the worlds of polling and market research, empirical sociology, psychology, and, to a lesser extent, political science—the figures, many already linked, who would soon form the small world of public opinion research. Among the contributors and board members: Floyd Allport, Harold Lasswell, Bruce Lannes Smith, Roper, Crossley, Gosnell, Gallup, Harry Field, Clyde Hart, Daniel Katz, as well as Lazarsfeld, Cantril, Stouffer, and Likert. Floyd Allport led off the first issue with a rousing charter, “Toward a Science of Public Opinion,” that surveys “points of common agreement in the work of various scholars which may prove useful in guiding us past blind alleys and setting us upon the proper road.”\(^12\)

The way that the field defined “public opinion,” in terms of a measurable aggregate of individual attitudes, was a striking departure from the various conceptions that had, before the polls, been dominant. The loosely structured and cross-national tradition of public opinion thought that emerged at the end of the nineteenth century and


\(^12\) Floyd H. Allport, "Toward a Science of Public Opinion," *Public Opinion Quarterly* 1(1) (1937) 13. The issue’s editorial foreword was no less dramatic (and invoked the “mass” term, moreover): “Always the opinions of relatively small publics have been a prime force in political life, but now, for the first time in history, we are confronted nearly everywhere by mass opinion as the final determinant of political, and economic, action. Today public opinion operates in quite new dimensions and with new intensities; its surging impact upon events becomes the characteristic of the current age—and its ruin or salvation” ("Foreword," *Public Opinion Quarterly* 1(1) (1937) 3).
flourished after the war was an important symbolic contrast for the new scientists of public opinion—but not much else, as its more historical and theoretic character was grounds for neglect rather than minable insight. Figures as diverse as James Bryce, Gabriel Tarde, A. Lawrence Lowell, Walter Lippmann, John Dewey and Graham Wallas—all of them concerned with the fate of democracy in modern times, with more or less doubt—were swept away by the new empirical evangelists who, moreover, claimed the “public opinion” mantle. In an even more direct way, the upstart empirical field defined itself against Chicago sociology and its “collective behavior” tradition—with its processual, multi-layered account of the emergence of the “public” and public opinion from “crowd” and “mass” formations. The tradition had its roots in Robert Park’s dissertation, Masse und Publikum (1904) [The Crowd and the Public], which

13. William Albig, an important but often dissenting figure within the cluster, bemoaned the “disproportionate influence of the methodological dogmatism” and “an often spurious objectivity and cultivated illiteracy” (“Two Decades of Opinion Study: 1936-1956,” Public Opinion Quarterly 21(1) (1957) 17).

14. Berelson’s 1956 state-of-the-field essay—written while still director of Ford’s Behavioral Sciences Division—is a somewhat startling, muscular defense of the older thinkers’ consignment to their mnemonic dustbin. Berelson celebrates their supercession as one of seven stages on the way to full “behavioral science” status. With unblushing Whiggism—and using “scholarship” as a pejorative—he writes that “[t]wenty years ago the study of public opinion was part of scholarship; today it is part of science.” His catalog of the field’s progress is somewhat jarring to our rather less confident ears, and reads something like a parody: There have been, he writes, “revolutionary change in the field of public opinion studies: the field has become technical and quantitative, theoretical, segmentalized, and particularized, specialized and institutionalized, ‘modernized’ and ‘group-ized’—in short, as a characteristic behavioral science, Americanized” (“The Study of Public Opinion”). Berelson’s behavioral sciences zeal was to crescendo with his 1964 Human Behavior: An Inventory of Scientific Findings, an attempt, as the dust jacket states, to “summarize what social scientists believe they have substantiated about the way human beings behave”—with 1045 findings (Berelson and Gary A. Steiner, Human Behavior: An Inventory of Scientific Findings (New York: Harcourt Brace, 1964)). (For a history of the “behavioral sciences” term, see Peter R. Senn, “What is Behavioral Science? Notes toward a History,” Journal of the History of the Behavioral Sciences 2 (1966).) Lazarsfeld, in a well-known paper the next year, takes Berelson gently to task in calling for a reconciliation between the “aspiring new science and the classical tradition,” who had in the mid-1930s “confronted each other like petulant antagonists.” Continues Lazarsfeld: “Progress in the clarity of formations and the respect for evidence is often accompanied, at least temporarily, by an insensitivity to the broader visions and the more general concerns characteristic of an older tradition... We’ve made good progress. We should not be deterred by the classicists’ sometimes outmoded style of reasoning. The essence of progress, it has been said, consists in leaving the ashes and taking the flames from the altars of one’s forebears” (Paul F. Lazarsfeld, "Public Opinion and the Classical Tradition," Public Opinion Quarterly 21(1) (1957) 40, 49). There’s more than a little of John Milton’s “he who put out the people’s eyes reproaches them of their blindness.”
distinguished between the modern “crowd” and “public” phenomena.\(^\text{15}\) (It is an almost perfect irony that a shriveled-up caricature of the Chicago “collective behavior” tradition, in the example of Herbert Blumer, supplied much of the straw that was later used by Katz and Lazarsfeld to construct the “powerful effects” contrast. Blumer, by the late 40s and early 50s, had become the most prominent and vocal critic of public opinion research.)

The widespread sense that the new public opinion researchers had replaced the earlier currents at Chicago and elsewhere was, in certain respects, confirmed by Rockefeller funding patterns. In the 1920s, the Laura Spelman Rockefeller Memorial had provided massive, unrestricted block grants for social science research, and a substantial portion of the $112 million that was disbursed went to the University of Chicago and its social science departments. The LSRM merged with the Rockefeller Foundation in 1929, and by 1932 the Foundation had officially turned away from social science block awards to applied, project-oriented grant-making. The Hawthorne researchers’ Industrial Hazards Project, with its practical focus, was an early benefactor of the new applied orientation—as was, a few years later, Lazarsfeld’s Rockefeller-funded Office of Radio Research.\(^\text{16}\)

\(^{15}\) Robert E. Park, The Crowd and the Public, and Other Essays (Chicago: University of Chicago Press, 1972); and, for example, Herbert Blumer, "Collective Behavior," in An Outline of the Principles of Sociology, eds. Robert E. Park and Edward B. Reuter (New York: Barnes & Noble, 1939). There was an interesting contact between Lazarsfeld and the Chicago sociologists, which deserves follow up study. As Pasanella records, Lazarsfeld received a congratulatory letter from University of Chicago anthropologist (Park son-in-law and Shils acquaintance) Robert Redfield in January 1936, after leading a seminar there in late December 1935. Redfield complimented Lazarsfeld on his “excellent job in the seminar last night” and passed along the thanks of Lloyd Warner, Herbert Blumer and Louis Wirth, “who were personally much stimulated by your work” (quoted in Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers (New York: Freedom Forum, 1994)) 11).

\(^{16}\) There is a sprawling literature on the Rockefeller social science grant-making in the interwar years. The treatments by the Bulmers, on the one hand, and William Buxton and Stephen Turner, on the other, are revealing side-by-side reads (Martin Bulmer and J. Bulmer, "Philanthropy and Social Science in the 1920s: Beardsley Ruml and the Laura Spelman Rockefeller Memorial, 1922-1929," Minerva 19 (1981); and William Buxton and Stephen Turner, "From Education to Expertise: Sociology as a 'Profession,'" in Sociology and its Publics: The Forms and Fates of Disciplinary Organization, eds. Terence C. Halliday and Morris Janowitz (Chicago: University of Chicago Press,
The Rockefeller funds certainly contributed to the emergence of the public opinion cluster.

The field, as it formed, had a number of unusual features. The first and most obvious distinguishing trait was its intellectual fixation on method and technique, to the effective exclusion of any overriding topical concerns. Comprising mostly of psychologists, sociologists, and market researchers, the cluster also had no stable disciplinary base, and depended for many years on the POQ for any institutionalized coherence. A regional web of interpersonal friendships and contacts was, at least until the founding of AAPOR in 1948, the field’s main adhesive. The self-conscious and central inclusion of the commercial analysts within that web was itself somewhat strange, given the often-aggressive border maintenance that so many other academic fields conduct.

17. The methods are those of “survey research,” though the term is anachronistic and was first applied in its field-defining sense only after the war. (Indeed, Lazarsfeld claimed credit for the term.) Included under the “survey” umbrella were various interview techniques, a mix of structured and non-structured types, usually but not always using sampling and scaling procedures, and almost always including standard SES and other demographic variables. In market research and media-based surveys, quantitative content analysis—as developed by Lasswell but refined by many others including Berelson—was a typical supplement. Very often the quantifiable studies were analyzed using the body of statistical procedures developed in the 1920s, notably by Ronald Fisher—though Lazarsfeld relied on his own, not widely adopted, “latent structural analysis” approach. The point is that they shared a body of methods united by non-experimental survey techniques that of a generally quantitative character. There was a definite belief, moreover, among the researchers that the methods constituted a coherent bundle.

18. One index of the overlapping personal networks was the fact that the POQ was not peer-reviewed until after the war, and most of its articles were commissioned by its editors. As W. Phillips Davison remarked, in his history of the journal, “The group of those conducting opinion surveys and working on problems relating to public opinion was small, and interpersonal networks were strong” (“A Story of the POQ's Fifty-Year Odyssey” S8). Many others have detailed the plentiful and cross-cutting friendships, which only grew tighter in the lead-up to, and during, the war. Turner and Turner provide the most complete account of the field’s overlapping membership, and its many connections, in the 30s, during the war, and after, with foundations and government officials. One of the results, as Turner and Turner observe, of the tight network was that the methodological polemic, so common to the late 20s and 1930s, gave way to an unofficial non-aggression pact that took hold especially in post-war sociology and helped to smooth the peaceful and even sometimes symbiotic relations between theorists like Parsons and empiricists like Stouffer. (In the case of Merton and Lazarsfeld, of course, the accommodation was more than gentlemanly, though even here Lazarsfeld maintained a skeptical attitude to even Merton-style functional analysis and social theorizing more broadly (The Impossible Science: An Institutional Analysis of American Sociology (Newbury Park, CA: Sage Publications, 1990) 113).
especially against commercial contaminants.\footnote{On the self-conscious inclusion of the commercial analysts, as well as the often-flaring tensions between the “two cultures,” see Converse, \textit{Survey Research in the United States} 106-20. Later reviewing \textit{POQ}’s first volume, Harry Alpert records that one of the journal’s four official sections was “reported the activities of specialists in the art of opinion-management: advertisers, public relations counsellors and fund raisers, and it was even suggested that a major purpose of the \textit{POQ} was ‘to bring the student and measure of public opinion into the closest intellectual cooperation with those who manipulate it, the professional public relations counsellors’” ("Public Opinion Quarterly Volume I: A Review” 186). There is a large literature on academic border-maintenance. The Merton student Thomas Gieryn, who helped to refine the concept in a number of publications, provides a helpful overview in "Boundaries of Science," in \textit{Handbook of Science and Technology Studies}, ed. Sheila Jasanoff (Thousand Oaks, CA: Sage Publications, 1995).} The public opinion field is, to use a favorite Lazarsfeld term, a “deviant case” in a related sense too: Its researchers addressed themselves to a diverse set of audiences, many of them outside the academic arena. Fellow scholars within the cluster, of course, were one important audience, as were the psychological and, increasingly, sociological disciplinary publics. But other major audiences included the clients and funders in the business and foundation worlds, along with the news media and the lay public, through opinion polling itself. Some of this audience diversity was a predictable by-product of the cluster’s atypical resource base: Far less reliant on undergraduate student enrollments—or any university source for that matter—the researchers subsisted instead on corporate contracts, in-kind aid from market researchers and pollsters, the Rockefeller grants and, once the war began, federal government largesse. All of these field characteristics, once mixed into the everyday practice of academic life, presented the distinction-seeking social scientist with complex, unstable grounds for recognition. The reputational wobbliness, however, was an opportunity for the savvy credit-entrepreneur—who, like Lazarsfeld, took the field’s tremulations as a sign of exploitable pliancy.

Robert Whitley’s four-way scheme, designed to distinguish academic fields
according to their intellectual and organizational features, is a helpful if inelegant tool to make sense of the public opinion cluster.\textsuperscript{20} This is not to say, however, that the opinion researchers fit into any of his neatly labeled boxes. The analytic contrasts that he uses to construct his elaborate, sixteen-fold classificatory table are nevertheless revealing when applied to the public opinion cluster. Whitley distinguishes between two different axes along which any particular academic field might be placed: first, “task uncertainty”—the extent to which research activities have predictable outcomes—and “mutual dependence,” the extent to which researchers depend on one another for reputation and access to resources. His claim is that a field like post-1945 physics has high mutual dependence and low task uncertainty; that is, physicists tend to rely on each other’s results and techniques, depend on their in-field colleagues for distinction, and have a relatively coherent set of research priorities. On the opposite ends of both axes, a field like sociology has low mutual dependence and high task uncertainty; in other words, reputations and credible research claims can be made without a tight reliance on colleagues’ opinions or the existing research, and the intellectual and methodological agendas of the field are plural and loosely coordinated.\textsuperscript{21}

\textsuperscript{20} Whitley, \textit{The Intellectual and Social Organization of the Sciences} (Oxford: Clarendon Press, 1984). Whitley’s effort to think about the \textit{differences} between scientific fields—as against the much more common focus, especially in the history and sociology of natural science, on \textit{general features}—deserves much attention and empirical follow up. His basic thesis is, no doubt, sound: “The comparative analysis of scientific fields as reputational systems of organizing and controlling research requires some means of summarizing their most significant differences which are connected to different sorts of intellectual structures and to different sorts of environmental circumstances” (83).

\textsuperscript{21} Here’s Whitley on fields like sociology and management studies, which he labels (on the basis of a more elaborate conceptual differentiation that I do not elaborate here) a “fragmented adhocracy,” where “research is rather personal, idiosyncratic, and only weakly co-ordinated across the research sites. Individual dependence upon any single reputational organization is limited and so scientists do not have to produce specific contributions which fit in to those of others in a clear and relatively unambiguous manner. Rather, they tend to make relatively diffuse contributions to broad and fluid goals which are highly contingent upon local exigencies and environmental pressures. Typically, these fields are open to the general ‘educated public’ and have some difficulties in excluding ‘amateurs’ from competent contributions and from affecting competence standards. The political system is
Whitley further divides both axes to get at more particular field traits, and here his approach begins to bear fruit when applied to public opinion research. He asserts that *technical* task uncertainty can be separated out from *strategic* task uncertainty; and that *functional* dependence and *strategic* dependence are distinguishable too. The public opinion research field, in these terms, has *high* strategic task uncertainty but comparatively *low* technical task uncertainty; the field, likewise, has a great deal of *functional* dependence but only a small measure of *strategic* dependence.

Whitley’s jargon, here as elsewhere uncommonly cumbersome, deserves some plain-English clarification. By *technical task uncertainty*, Whitley means, roughly, the extent to which research techniques are accepted across a field; he also points to the belief, or not, that the techniques will yield consistently usable results. Here the public opinion cluster seems to register at the low end of the Whitley continuum: Though evolving to be sure, the opinion researchers’ bundle of survey techniques were relatively stable, widely employed with predictably reliable results, and the object of much clarifying, field-wide scrutiny. But on the *strategic* side, the opinion cluster was far less unified or coherent—except in the loose sense that the methods themselves came to be a stand-alone agenda. By *strategic task uncertainty*, Whitley refers to any given field’s intellectual priorities—whether or not, that is, the field has a relatively agreed-upon topical focus and direction. The opinion researchers were—and remain today, it must be therefore pluralistic and fluid with dominant coalitions being formed by temporary and unstable controllers of resources and charismatic reputational leaders. Reputations are also fairly fluid as standards alter and are open to a variety of interpretations."

22. Whitley’s prose-style is far more impenetrable and willfully abstract than even the admittedly abstract subject matter calls for.
said, in their “survey research” incarnations—in a state of topical chaos, whose rudderless course was driven mainly by the often-episodic whims of its funders.

Along these same lines, the opinion cluster splits between the functional and strategic axes of “mutual dependence.” What Whitley means by “functional” here is the relative degree of reliance that researchers have on others within the field for raw results, techniques, and resources. “Strategic” dependence, by contrast, refers to the importance of one’s in-field colleagues for validating research accomplishments and reputations. On the functional side, the opinion researchers were really quite dependent on one another—for technical innovations, for data to be re-analyzed, and for the expensive survey machinery itself. This was, moreover, a structured dependence, with the academics supplying methodological improvement, by and large, and the commercial analysts providing basic results, some funding, and the large-scale survey apparatus. This exchange was often formalized, as with Cantril’s links to the Gallup organization, and on a case-by-case basis, with Lazarsfeld’s use of CBS data, the Roper interview teams, and the quasi-academic services of the National Opinion Research Center. Though structured in this sense, the exchanges took place by way of the dense, interlocking personal relationships that so distinguished the field from more diffused disciplines.

In strategic terms, however, public opinion researchers were much less dependent on one another for reputational success. Peer evaluations within the cluster were, of course, important, but they were by no means the exclusive source of esteem or recognition. The diverse funding constituencies—an assortment of corporate clients, the Rockefeller Foundation, and, especially after 1941, the federal government—became
default audiences, that were, to be sure, more or less integrated into the field itself but nevertheless broader than the cluster of distinction-seeking academics and commercial analysts. The public relevance of the research—in terms of issue and election polling but also, significantly, the extensive media-related work—ensured that news outlets and the educated lay public were important audiences as well. Even within the scholarly frame of reference, the established disciplines of psychology and sociology had large disciplinary publics crucial for validation but not, however, part of the cluster itself. The point is that recognition could be earned and redeployed within and across a number of audiences outside the field.

Whitley’s scheme, as applied to public opinion research, helps, then, to make sense of its distinctive features. It is important, first, to recognize that the “non-strategic” properties of the field—its fixation on method, its interpersonal network, and its resource entanglements—held it together. Its substantive intellectual side, by contrast, was not asked to carry out much of the adhesive work.

This basic discrepancy had a number of reputational implications. The field’s relative technical coherence, on the one hand, provided an outlet for disciplinary credit-seeking, in that the cluster’s technically astute members could (and did) act as methodological ambassadors to their “home” disciplines. The field’s parallel stress on technical innovation opened up a space for reputational gain within the cluster that, however, was also portable and often transferred to the established disciplines. In a similar manner, the field’s functional interdependence allowed reputations to bubble up and diffuse through friendships, fueled in part by the mutual debts produced by all those
technical and resource exchanges.

Most important of all, however, was the relative incoherence of the field in Whitley’s “strategic” terms. The fact that the field had little topical focus nor any monopoly on the attribution of credit meant that its reputational pattern was in flux—dependent, that is, on a number of outside audiences that were, moreover, responsive to various substantive claims otherwise unimportant to the field’s internal, method-based rewards. The field, to put the point differently, provided some validation, along with the technical skills and resources, to go about credit-seeking beyond the field. For the entrepreneurial scholar, the field’s reputational openness—the relative weakness of its internally defined hierarchy—was an opportunity to accumulate distinction within and beyond in a mutually reinforcing cycle.

The public opinion cluster was, in a crude sense, a one-stop filling station, whereby funds, methodological status, and technical wherewithal could be earned and then deployed to accumulate more credit within non-academic or disciplinary publics for methods and substantive work, then doubled-back to the field—which, in turn, enhanced one’s internal standing, leading to more resources and still more opportunities to earn distinction. What I mean is that the field, with all of its resource, credit, and technical significance, was an extraordinary fertile but ultimately quite flexible base to seek reputation from, which could be returned to, in a virtuous resource-and-credit circle. This is exactly what Lazarsfeld accomplished.

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Our understanding of this interwar adoption of mass communication has been greatly
improved recently by three scholars—William Buxton, J. Michael Sproule, and Brett Gary—whose rich, archival studies, however, do not seem to be in conversation with one another. From all three accounts, the Rockefeller Foundation’s John Marshall, in particular, emerges as a key protagonist—who certainly had been the most neglected major figure in the historiography of mass communication research. But even before the Foundation took an active interest in radio in the early 1930s, the newspaper, magazine, radio and advertising industries had long conducted and sponsored audience research.

Newspaper and magazine readership studies trace their roots to the end of the nineteenth century; such audience inquiries were often demanded by advertisers, and both industries, by the mid-1920s, had formed medium-wide tracking surveys to meet their

own needs as well as advertisers’. Very soon after commercial radio emerged in the early 1920s, the nascent industry sponsored listenership studies and experimented with various audience-measurement schemes. The decade’s inter-medium competition to attract advertisers led to a fast-growing market research specialty in advertising effectiveness—a niche field which continued to grow into the 1930s as Depression-era advertisers demanded cost-effective placement. Industry-sponsored audience research, much of it proprietary, would remain an important funding stream for the public opinion field, once it coalesced after 1936. Lazarsfeld’s Rockefeller-backed Office of Radio Research, for example, received many additional funds and data from the Columbia Broadcasting System, through ORR associate director Frank Stanton—director of research and later president of CBS.

That Rockefeller investment in radio research, as Buxton details, was an outgrowth of the Foundation’s involvement in the public debate over educational radio—the so-called “radio wars” of 1927 to 1934, when federal communications policy was in flux. The debate—its factions and the ultimate outcome—set the initial parameters for


26. For a short history of this early, industry-sponsored research, see Converse, Survey Research in the United States 90-92.


the Foundation’s subsequent radio research programs, which culminated in its sponsorship of the crucial, interdisciplinary “Communications Seminar” from 1939 until the U.S. entry into the war.

After the anarchic and interference-plagued airwaves of the 1920s had been brought under initial federal control with the 1927 Radio Act, a vigorous public debate broke out. Educational broadcasters, in particular, worried that the Act’s new technical standards would drive out non-commercial stations. There was, as Buxton describes in great detail, a critical split in the educational camp—between a moderate group backed by John D. Rockefeller, Jr. and the Carnegie Corporation, and a less conciliatory group funded by a small Payne Fund grant. The Rockefeller-backed group urged cooperation between the networks and reformers, and proposed to develop high-quality educational programs that commercial broadcasters would find attractive. The Payne-backed group set out, instead, to lobby Congress and build public support for a fixed-percentage spectrum set-aside. The 1934 Communication Act that emerged from the legislative and public battles was, of course, a victory for the commercial broadcasters, which included no mandated set-aside. But the Act did gesture toward the “public interest,” and the new Federal Communications Commission called a meeting to reconcile differences between the educators and the broadcasters, which ended in further acrimony.

The Rockefeller Foundation’s interest in the study of radio, as Buxton documents, emerged from the meeting and its bitter aftermath. The Foundation’s Humanities

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30. Ibid 154-55. The Rockefeller-backed group was the National Advisory Council on Radio in Education (NACRE); the Payne-backed group was the National Committee on Education by Radio (NCER).
31. Ibid 155-56.
Division (HD) essentially took over the government’s under-funded effort to achieve a reconciliation.\textsuperscript{32} John Marshall, the HD assistant director, played the key role in this and other communication-related Rockefeller initiatives.\textsuperscript{33} After the post-Act meeting, he interviewed the main players in the debate, including broadcasters. As his diaries reveal, he came to share the moderate group’s belief that commercial and educational goals were ultimately reconcilable.\textsuperscript{34} Accepting commercial radio as a given—indeed, largely adopting the Rockefeller-backed moderate group’s stance—Marshall sought to convince the networks to voluntarily embrace some educational programming.\textsuperscript{35} The Foundation, in this vein, funded fellowship appointments at NBC and CBS to train public broadcasters.\textsuperscript{36} But Marshall, by 1936, came to believe that only objective audience data would persuade the networks that some educational programming is in their interests.

It was Hadley Cantril, the ambitious, 30-year-old psychologist, who convinced Marshall that the new polling techniques could yield valuable data on audience interests and motivations.\textsuperscript{37} Cantril, then at Columbia’s Teachers College, had recently published

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\item[32.] “In effect, the Rockefeller Foundation assumed a task that neither broadcasters, educators, nor state officials were willing or able to undertake” (Ibid 153).
\item[33.] For background on Marshall, a literary medievalist and former scholar of English at Harvard, see Gary, "Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words.” It is only a slight exaggeration that, as Buxton argues, Marshall “almost single-handedly gave coherence and direction to the assorted Rockefeller projects related to the relatively new media of mass communication” ("The Political Economy of Communications Research" 156). His crucial contributions were not so much intellectual but financial and organizational. As Gary observes, his interests in radio were “largely derivative and synthetic, and not especially original. His importance should be measured by his role as an administrative catalyst and agent for scholars, and not for the questions he asked or the problems he framed” ("Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words” 130).
\item[34.] "The Political Economy of Communications Research” 158.
\item[35.] Buxton: “While the Act set the framework for the incorporation of educational interests into the commercial broadcasting system, it did not provide the resources, programs, and expertise through which this reconciliation between educators and broadcasters could take place… the role of charting the path of cooperation between educational and broadcasting interests fell to the Humanities Program of the Rockefeller Foundation” (Ibid 153).
\item[36.] Ibid 160.
\item[37.] Based on interviews, Converse provides this description of Cantril: “He was unusually skilled and
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The Psychology of Radio (1935) with his Harvard mentor Gordon Allport. As Marshall later recalled in an oral history interview, the “historic moment” came when he read Allport and Cantril’s book—based mostly in experimental laboratory studies, but including some survey data—which in its conclusion urged that more research be conducted on listeners.

When Marshall interviewed him—it was after the election—Cantril invoked the polling procedures and proposed to Marshall that he, Cantril, conduct laboratory and sampling-based research into listener tastes. (After the 1936 Literary Digest upset, Cantril had became enamored with the new sampling methods. Cantril had been serving on a committee, formed in early 1936 by the FCC to mediate between educators and broadcasters, which Marshall was closely following. Impressed, Marshall urged Cantril to submit a request for funding; his initial proposal called for ongoing research into “what listeners find of interest in radio programs and … why these interests exist”—research, he argued, that would not be trusted if it emanated from either the broadcasters or the educators.

A series of complicated maneuvers followed—which Buxton documents—that resulted in Cantril’s revised proposal, this time with CBS’s Stanton as a partner, for a

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41. Converse, Survey Research in the United States 125.
42. Buxton, "The Political Economy of Communications Research" 161.
43. Ibid 160.
radio research bureau, which Marshall openly supported. In his statement to the Rockefeller trustees, Marshall framed the research center as the key to bringing the broadcasters around to educational programming: “If the present project succeeds, as I expect it will, in demonstrating the feasibility and significance of studying the actual and potential public service of radio to its total audience, it will set a style which the broadcasters cannot afford to disregard.” In early 1937, the trustees approved a grant of $67 thousand over two years to fund a “Princeton Radio Research Project,” whose charter explicitly forbade research that questioned the commercial basis of the broadcasting. (As Theodor Adorno later remarked, “I cannot say that I strictly obeyed the charter.”) It was Marshall who arranged that the Project be located at Princeton, and he seems to have played the crucial role in securing a post for Cantril in the University’s psychology department. At around the same time, Cantril became one of the founding editors of the Princeton-based Public Opinion Quarterly—which the Foundation also funded.

44. Ibid 164. From the Cantril and Stanton proposal: “If radio in the United States is to serve the best interest of the people, it is essential that an objective analysis be made of what these interests are and how the unique psychological and social characteristics of radio may be devoted to them” (164). And: “During the past decade commercial broadcasting has done a considerable amount of systematic research and has accumulated much practical experience which needs only slight modification to be useful for more general purposes. Unless we know who listens to an individual program, how it is received, and what accounts for its acceptance, no insight into the social effects and potentialities of radio is possible... For this reason one of the groups of activities planned by the Princeton Radio Research Project is the adaption and extension of the commercial sort of radio research toward more general use” (quoted in Glander, Origins of Mass Communications Research during the American Cold War 116).

46. Ibid 167.
When the Radio Project grant was made, Stanton was designated director, with Cantril as associate director. But Stanton, as he later recalled, was so “completely involved in what [he] was doing at CBS” that he declined to leave the network.\(^{50}\) Cantril, too, was unwilling to assume the directorship, and the pair went looking for an appropriate candidate by asking around in psychological and sociological circles. Lynd, here again, was the crucial Lazarsfeld champion: He suggested to Cantril and Stanton the Austrian emigré and noted his previous radio listenership studies.\(^{51}\) Though the Foundation was initially reluctant—concerned, as Marshall recalled later, that Lazarsfeld’s interests were too broad—the appointment was made. And the Princeton Radio Project, in effect, migrated to Newark.\(^{52}\) The Rockefeller Foundation’s expressed desire to get educational programming on commercial radio had, through this circuitous route, issued in the Office of Radio Research.

As Buxton, Sproule and especially Gary document in great detail, the

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51. Ibid 12.

52. From Stanton’s oral history interview: “In 1936, Hadley [Cantril] and I blocked out a rather ambitious program... and we submitted it to the Rockefeller Foundation... And quite out of the blue, one day we got word that they had made a grant to us... I was so completely involved in what I was doing at CBS that I didn’t want to leave... So when the money was granted, neither Hadley nor I wanted to take on the job of running the project. So then we looked at the people we knew in psychology or sociology and ran across some publications that Lazarsfeld had published from Vienna in the field of radio research... [W]e both said, ‘Gee, if we could get Paul Lazarsfeld, he would bring a fresh point of view’ (quoted in Ibid 13). From Marshall’s oral history interview: “… Hadley rang me and said Frank had decided he couldn’t serve as head because of CBS. He asked if he could appoint Lazarsfeld... It seemed to us at the Foundation that Paul’s range of work was too broad. He said that he would try out the lines of work that seemed most productive... I felt Paul’s methodological interests lent a double focus. He and I had an oral agreement that he would accept outside studies only when they involved his methodological interests. He agreed that was just, and we proceeded that way. Lazarsfeld’s work led to a great deal in the Foundation. It brought the Foundation into polling as an instrument of research. There was some disquiet about that on the part of certain trustees. They were afraid polls would influence more than measure” (quoted in Ibid 13). And from a letter from Cantril to Lazarsfeld in August 1937: “I wanted to direct a rather great variety of studies so I was sure that from year to year my methodological experience could increase. I think your project would do splendidly. Radio is a topic around which actually any kind of research methods can be tried out... and applied” (quoted in Ibid 14).
Foundation’s next venture into media research was motivated on entirely different grounds, with arguably more lasting consequences for the “communication” field. With the rapid Nazi conquest of continental Europe as explicit backdrop, Marshall organized a “Communications Seminar” that was, in effect, a self-conscious precursor to the government propaganda campaigns of the war—convened, indeed, in the knowledge that explicit government efforts, at that time, were not politically feasible.53 One of the outcomes of the Seminar was the consolidation of the “communications” label itself, which was put forward as a deliberate alternative to the “propaganda analysis” tradition—whose Progressive emphasis on propaganda inoculation was, in the new context of intervention and defense against the Nazis, part of the problem.54 Many of the Seminar participants—including Lazarsfeld, Cantril, and Lasswell—would go on to leading roles in the government’s wartime propaganda activities. As it evolved, the Seminar came to define the study of mass communications in largely quantitative terms, and identified the question of media effects as its driving problem. The field was conceived, in the Seminar, as an emerging scientific field, on the one hand, but also as a crucial instrument of effective propaganda design.55

53. Gary: “With war breaking out in Europe, Rockefeller officers and the founding fathers of communication research were galvanized by the recognition that the Roosevelt Administration, hamstrung politically, could not adequately prepare for war on the propaganda front. Isolationist sentiment and bad memories from World War I limited the administration’s ability to influence domestic public opinion or control foreign and domestic antidemocratic propaganda. The Rockefeller Foundation, whose university-, museum-, and library-based projects had more room to experiment with potentially controversial activities, took up the slack. With Marshall and the Foundation providing funding and serving midwife roles, Rockefeller-funded research laid the groundwork for a wide range of national security projects that were eventually absorbed by the state” ("Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words” 125).

54. The best treatments of the Seminar are in "Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words”; Sproule, "Propaganda Studies in American Social Science: The Rise and Fall of the Critical Paradigm”; and Buxton, "The Political Economy of Communications Research."

55. Gary’s broader argument, laid out in most detail in his 1999 book The Nervous Liberals, is framed in terms of two major debates among post-World War I liberals. He describes, in impressive detail, the
The Seminar—a truly fascinating episode in the sociology of knowledge—was initially conceived by Marshall in terms of his interest in media study as a potential bridge-building between educators and commercial broadcasters. In late 1938, he proposed, to the Rockefeller trustees, a series of conferences centered on the prospects for the public’s media-led education.\textsuperscript{56} In August 1939, just before the outbreak of war, the Foundation agreed to fund the series, which, in the proposal’s language, was designed to “develop a disciplined approach to the study of mass communication, through such media as radio, motion pictures, and print.” One of its explicit charges was to identify a “general body of theory about mass communications in American culture.”\textsuperscript{57}

Before the first conference—the meetings only later came to be called the “Communications Seminar” or, less often, the “Communications Group”—the Nazi debate, from the early 1920s on, between chastened realists like Walter Lippmann and progressive populists as to the competence of the public in terms of democratic theory and practice. Gary argues that, with this debate in the background, a second major conflict came to the fore as the rise of fascism, World War II and the possible U.S. entry came to the fore: a debate that pitted traditional liberal concerns for civil liberties against emergency-context national security concerns. Many liberals, even those who had in the 20s opposed Lippmann’s view, came to help in the building of what Gary rather generously calls a “propaganda prophylaxis”—a set of state-driven defenses against fascist propaganda that involved propping up U.S. domestic morale and countering fascist propaganda at home and abroad. Even if they didn’t buy Lippmann’s chastened liberalism in full, many otherwise progressive liberals, citing the emergency context, adopted a wartime view of governance that closely paralleled Lippmann’s “manufacture of consent” model—a group of expert elites that determines the best interest of the country and goes about winning consent for this predetermined policy course. Gary makes it quite clear that this “propaganda prophylaxis” and the wartime service of communication scholars was an honorable, good faith effort—retroactively justifiable given the uniquely “just war” context. He allows, though, for a kind of unintended consequence of well-intentioned action: This good faith, emergency effort became routinized, through inertia and the subsequent cold war context, into the national security state, whose propaganda activities are less defensible. Gary’s broader narrative, grounded as it is in the dilemmas of liberalism, is very helpful in understanding some of the Communications Seminar developments, but it fails in this sense to account for the differences among the Seminar participants, on the one hand, and the stand-alone importance of some of the social scientists’—Lazarsfeld included—primary interest in academic distinction-seeking and genuine methodological interests in the tools that were to emerge as central in the study of mass communication. Gary does a much better job, in his micro-level discussions of the Seminar, in separating these sometimes complementary motives out (The Nervous Liberals: Propaganda Anxieties from World War I to the Cold War).

\textsuperscript{56} See Gary, "Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words” 131 for a summary of the proposal.

\textsuperscript{57} Quoted in Ibid 132.
conquest of Central Europe was already underway. The Seminar’s work over the following two years was forged by two competing, but ultimately merged, agendas: To map out the scientific study of mass communication, but also to design an extra-governmental plan for combatting Nazi propaganda and mobilizing war support. Most of the Seminar members, at least by 1940, had come to adopt a robustly scientistic view of communication research that was, significantly, also conceived as a major weapon in the world struggle.  

J. Michael Sproule has aptly labeled this seemingly schizophrenic scientific instrumentalism as an “ideology of service and science” — though it must be added that this coupling was not uncommon in interwar social science. Before this rough consensus was formed, however, the Seminar’s debates split along two axes that were not, moreover, clearly parallel to one another. Some of the participants—notably Lynd and literary theorist I.A. Richards—were resistant to the others’ stress on quantitative techniques. Along another axis, Seminar members disagreed about whether media research should be used for propaganda design—and here Lynd sided with the majority view, against Donald Slesinger, Charles Siepmann, and Lyman Bryson. In the end, the group’s momentum and the developments in Europe brought most of the

58. The Seminar’s non-Foundation participants were Lazarsfeld; Harold Lasswell; Robert Lynd; Hadley Cantril; Geoffrey Gorer, an Oxford-trained anthropologist; Lyman Bryson, an adult education specialist; Donald Slesinger, former dean of the Social Sciences at Chicago and director of the Rockefeller-funded American Film Center; I.A. Richards, the prominent Canadian literary theorist; Douglas Waples of the University of Chicago’s library school (and mentor to Bernard Berelson, who began his academic career there); Charles Siepmann, a communication analysis for the BBC; and Lloyd Free, the once and future Cantril collaborator who would, in 1940, take over the editorship of the Public Opinion Quarterly and after the war participate centrally in the Cold War propaganda efforts (Ibid 133).


members together in recommending a quantitatively-oriented science of propaganda design.

The group first convened just after the early-September invasion of Poland and the late-August Nazi-Soviet Non-Agression Pact. With the European crisis casting the darkest of shadows over the meeting, Marshall told the participants that, “in view of the world situation,” they should “consider what research studies might be undertaken at once… studies that would be of immediate significance.” Marshall had already assured Rockefeller officials that the group’s discussion would be “purely informal and that they would receive no public attention.” At the meeting, he warned the gathered participants against crediting the Foundation for “any materials that grow out of the discussions.” The effort to develop a “general body of theory” was, from the start, shrouded in secrecy.

The group’s first two meetings, according to Lloyd Free’s minutes, were largely concerned with the role that the members’ communication expertise might play in the inevitable state-managed “emergency psychology” campaign to come. In these early meetings, Lynd argued strenuously for active, expert-driven opinion management for the duration of the emergency—and suggested that, after the war, some form of expert planning could be kept on. Donald Slesinger, director of the Rockefeller-funded
American Film Center, agreed with Lynd (and most participants) about the poorly equipped citizenry, but in Progressive fashion urged that social science be used instead to create “more democratic and intelligence citizens in the face of widespread propaganda.”

The Seminar’s first summary paper, written by Marshall, reflected the group’s early ambivalence. The paper, “The Job to be Done—Now,” argued for an immediate focus on research method refinement, which would, only then, be useful to policy makers. This early statement, significantly, fretted openly about the potential threats of government propaganda and censorship. Lasswell produced two follow-up, “working” papers which reflected his newfound democratic commitments that were, here and in his postwar “policy sciences” movement, wedded to both objectivism and expert service. Lasswell’s earlier work on propaganda—notably his 1927 dissertation and his 1935 overview of the field—had been far less sanguine about democratic practice and had, indeed, considered its viability in only formal terms, otherwise dependent on elite-driven symbolic manipulation. (One of the many ironies of the remembered history of mass

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66. Quoted in Ibid 134. Gary also quotes from a letter that Lynd sent to Marshall for his “private eye,” where he characterized Slesinger as a “bright fellow, but... an everlasting promoter... I don’t trust his ability to appraise a problem in which he has a potential stake without doing so in the mood of ‘What’s in it for me?'” (134).

67. Ibid.

68. Lasswell, as Gary documents, openly suggested a Rockefeller-funded research campaign with both scientific and policy objectives. Here’s Gary’s summary: “Central direction should be given by ‘specialists in the study of communications,’ who would advise ‘those who have money to support research’ with respect to three primary purposes: (a) ‘to stimulate and guide research in the field of communications’; (b) to maintain the reputation of objectivity for those involved in this research, especially on ‘controversial public questions’; and (c) to push this research toward ‘the pursuit of truth about important social problems’” (Ibid 135).

communication research was that Lasswell would, in the early 1960s, be seamlessly woven into the “limited effects” narrative in key histories by Wilbur Schramm.) In the second working paper, Lasswell framed the Seminar’s project in terms of effective persuasion:

There is at present little or no knowledge of the degree to which people or groups of people in the United States… are susceptible to persuasion. There is, further, little or no knowledge of what types of persuasion… these groups are susceptible to.

Slesinger dissented from the propagandistic implications of Lasswell’s proposals, as he had with Lynd. “We have,” he stated to the group, “thought in terms of fighting dictatorship-by-force through the establishment of dictatorship-through-manipulation.”

But, with the notable exceptions of Lynd and I.A. Richards, the Seminar members all agreed that the wartime emergency provided an extraordinary opportunity to refine research technique and test empirical propositions. The group assigned its members with specific responsibilities for various quantitative techniques—Lasswell and Gorer, for example, were to study content analysis; Lazarsfeld and Waples opted for panel interviews; and Cantril was charged to study polling methods. Lynd and I.A. Richards chafed at the others’ quantitative inclinations. Lynd was probably intimidated by his colleagues’ methodological arcana—and he certainly thought that the group’s

71. Quoted in Gary, "Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words" 136.
72. Quoted in Gary. The full statement that Gary quotes is: “We have used the language of students and practitioners of propaganda, and have tacitly accepted their objectives… We have been willing, without thought, to sacrifice both truth and human individuality in order to bring about given mass responses to war stimuli. We have thought in terms of fighting dictatorship-by-force through the establishment of dictatorship-through-manipulation” (Ibid 135-36).
73. As I observe below, it is likely that Lazarsfeld’s innovative panel method—in which a fixed “panel” of subjects is interviewed repeatedly over the course of time—was devised during this Seminar period.
74. See Gary’s summary (Ibid 136).
75. As Lynd wrote to Marshall in April 1940: “I have felt uneasy as hell this winter in my role as
“scientific” focus was too far removed from direct social policy guidance. Richards perceived, probably correctly, that his qualitative approach to content analysis—rooted in his theory of literary semantics—was not taken seriously by other Seminar members. He resigned from the Seminar in early 1940.

By late spring of that year, after European conditions had greatly deteriorated, the group’s willingness to engage in propaganda design—to defend against Nazi propaganda and to help mobilize support for the war—became far less qualified. In a May 1940 meeting, Marshall declared:

In a period of emergency such as I believe we now face, the manipulation of public opinion to meet emergency needs has to be taken for granted. In such a period, those in control must shape public opinion to support courses of action which the emergency necessitates... No one, I think, can blame them for that impulse.

The Seminar issued its first group report in July, “Research in Mass Communications,” which laid out the famous “who says what in which channel to whom with what effect” formulation—which, however, has long been credited solely to Lasswell, probably because this initial, group formulation was kept secret.

committee member for you. I am interested in the field of communication and regard it as very important. But the doggone thing just isn’t something in which I have either central interest or special competence” (quoted in Ibid 137).

76. As Gary documents, Lynd viewed his colleagues’ quantitative mania as mere description of the status quo. Here’s Lynd: “Both Lasswell and Gorer are largely content to describe what happens, with the implicit assumption that it will somehow be useful. I think this is a needlessly cumbersome and wasteful way for research to proceed” (quoted in Ibid).

77. See Ibid.

78. Quoted in Ibid 139. As Gary records, Marshall was at pains to distinguish between the emergency context and long-term persuasion: “There is always a danger that if emergency mechanisms are not recognized as such that they become permanent” (Ibid).

79. Gary quotes the report: “In brief, that job is to learn what mass communications do in our society... what they do became a question of what effects do mass communications as a whole, or any single communication, have. What effects they have likewise inescapably involved discovering to whom was it said. And that analysis, to be complete and properly illuminating, required answers to a fourth and final question—who said it and with what intention. In brief, then, the job of research in mass communications is to determine who, and with what intentions, said what, to whom, and with what effects” (Ibid 138).

80. The Lasswell essay in which the now-clichéd research formula appears is “The Structure and Function of Communication in Society,” in The Communication of Ideas, ed. L. Bryson (New York: Harper, 1948). As Gary observes: “Normally attributed solely to Lasswell, the paradigm was the product of
war-related opinion management was unambiguous:

We believe … that for leadership to secure that consent will require unprecedented knowledge of the public mind and of the means by which leadership can secure consent… We believe… that we have available today methods of research which can reliably inform us about the public mind and how it is being, or can be, influenced in relation to public affairs.  

Lazarsfeld registered this shift in his own 1940 edited collection, Radio and the Printed Page. Citing the world crisis, he wrote that the “role of radio as a tool of propaganda has receded to the background because not what to do but how to do it has become the problem of the day.”

The early ambivalence and qualifications were missing from the July report, and some of the Seminar members—Lyman Bryson and Charles Siepmann in particular—openly decried its “fascistic” implications. In response to Bryson, Siepmann, and Rockefeller officer Joseph Willitz’s complaints, the group’s final report, issued in October, was far less brazen in its language and recommendations. Titled “Needed Research in Communication,” the document called for “two-way communication” between the government and the people; without it, the report warned, “democracy is

81. Quoted in Ibid 139.
83. Gary observes that Siepmann was horrified not just by the contents, but also because some of his own language in letters to Marshall had been incorporated, without permission, into the body of the report. At the meeting, Siepmann dissented, on somewhat pragmatic grounds, from the report’s call to propaganda: “Public opinion and vested interests are violently opposed to such a development which would be labeled as fascist or authoritarian.” Bryson was more blunt: The report, he stated, “looks to me like something that Herr Goebbels could put out with complete sincerity… I believe that the assertion of the democratic principle should be made without any … qualification.” A Rockefeller Foundation officer, Joseph Willits, also attacked the document in the same terms: “The preoccupation in this memorandum does not seem to me to be with the critical examination and improvement of what is to be decided upon and put over by government, but with the ‘putting over’ of what has already been decided… Our government may be forced to go in as completely for propaganda as has Mr. Goebbels, especially if it goes to war. But the techniques it needs to employ to make its propaganda effective ought not to be described as a vitalizing of democratic procedure. It too much resembles the methods by which democracy has been destroyed” (quoted in "Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words” 140).
The report concedes that the new mass communication research might be used for propagandistic ends, but asserts that such “authoritarian” cooptation can and should be avoided:

The same research could, to be sure, be used to turn communication into powerful propaganda, if those responsible chose to do so. That, it seems, is a risk which must be taken with all research which has the possibility of application for authoritarian ends. If, however, those responsible, whoever they may be, wish to avoid authoritarian controls, the contributions which research can make can be the means of strengthening one of the essential phases of the democratic process. That process hitherto has been endangered in times of increased tension more by ignorance than by intention.85

The document’s disclaimers, however, are more than a little disingenuous, as the Foundation was already building an elaborate network of propaganda-related research projects, in lieu of a government-directed campaign. At the Seminar’s September meeting—just a month before the report was issued—Marshall reported that the Foundation’s projects would engage “the three-fold task of maintaining civilian morale at home, of maintaining good relations with friendly countries, and of waging propaganda warfare with countries hostile to us.”86

Even as the report was distributed to a number of scholars, university presidents, foundation officers, publishers and government officials,87 Marshall and Lasswell were approaching government officials and, in Gary’s words, “quietly made it known that Foundation monies might be available to facilitate government-needed communication

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85. Quoted in Gary, “Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words” 142.

86. Quoted in Ibid.

87. Including, as Gary reports, Robert Hutchins, Louis Wirth, Henry Luce, Talcott Parsons, William O. Douglas and Archibald MacLeish (Ibid 142).
research.\textsuperscript{88} From early 1940 until the U.S. entry into the war in December 1941, the Foundation served, in essence, as an unofficial arm of the state when the Roosevelt administration—hampered by a public culture still wary of propaganda, and a somewhat isolationist Congress—could not feasibly do so itself. Marshall was quite candid about this in his proposals to the Rockefeller trustees: “Whether or not this is something for the foundation to consider, I do not know… But the early neglect of this type of study [by others] may mean that it is the only agency as yet ready to recognize its importance and to provide the necessary funds…. for the type of work which later may be needed in national defense.”\textsuperscript{89} Fellow Rockefeller officer Stacey May, who was at the time also working with the Office of Emergency Management, wrote to Marshall to warn him that the “last war left the country suspicious of propaganda” and that, as a result, the government would be “slow to develop ‘morale’ activities for fear of being accused of propagandizing.”\textsuperscript{90}

In his response, as Gary documents, Marshall agreed and observed that even “communications research” was plagued by propaganda fears. Despite the “growing recognition of the need for such research,” he continued, any Roosevelt-led efforts “would not be looked on favorably by Congress.”\textsuperscript{91} By the end of 1940, the Foundation had set up and funded an elaborate bundle of propaganda-related projects; even those research initiatives, like the Office of Radio Research, that were originally conceived

\textsuperscript{88.} Ibid.  
\textsuperscript{89.} Quoted in Ibid 143.  
\textsuperscript{90.} May continues: “Everyone seems to believe the situation delicate in the extreme. Certainly the isolationist group would be quick to pounce on any known move in the executive branch that could be ticketed with the label ‘Propaganda’” (quoted in Ibid).  
\textsuperscript{91.} Quoted in Ibid.
with other purposes were, by this time, brought into the propaganda fold. Among the Rockefeller-backed projects by 1940: Cantril’s Princeton Public Opinion Research Project (which the Foundation had funded after Cantril’s split with Lazarsfeld); the Princeton Shortwave Listening Center (which included *POQ* ex-editor Harwood Childs); the Graduate Library Reading Project at Chicago (under Waples’s directorship, with the notable participation of Berelson); the Film Library of the Museum of Modern Art (which included Siegfried Kracauer’s studies of Nazi film propaganda); the Library of Congress Radio Project; the Totalitarian Communications Project at the New School (under Ernst Kris and Hans Speier); and the Lasswell’s content analysis operation, the Experimental Division for the Study of Wartime Communications at the Library of Congress.\(^92\)

The Communications Seminar set the agenda for, and helped to mobilize, the extraordinary Rockefeller campaign to build up a wartime propaganda apparatus when the government itself could not. The Seminar’s intellectual agenda for “communications research”—a term, as Sproule shows, that was self-consciously selected as a fresh alternative to the Progressive “propaganda analysis” label—was, in part, shaped by the world crisis and the felt need to understand, and master, persuasion technique. Many of the scholars’ preferences for particular, and often quantitative, methods predated the Seminar; but the selection of so many figures central to the public opinion research cluster, along with the consensus-building of the Seminar process itself, surely helped to establish the opinion-related techniques at the center of the wartime and post-war mass

\(^92\). This list is nearly a direct reproduction of Gary’s extremely thorough summary (Ibid 125).
communication research agenda.

For much of 1941, the Rockefeller initiatives were awkwardly coordinated with the fledgling Roosevelt-administration efforts, which were underfunded and poorly planned for the same reasons of public wariness and Congressional oversight. But once the U.S. entered the war in December, the Rockefeller projects were incorporated into rapidly expanding wartime bureaucracies. By the summer of 1942, most of the major government propaganda-related initiatives were in place, and staffed to an astonishing degree by the main figures in the public opinion cluster. The government effort spanned dozens of agencies and employed thousands of social scientists; the mind-numbing maze of programs and offices is only briefly touched on here.

Rensis Likert’s Department of Agriculture Division of Program Surveys, became, essentially, a survey organization for-hire. The group did contract work for a number of federal agencies, most notably the Treasury Department for its war bond studies. Early in the war, Likert’s Program Surveys group served as one of two official survey divisions within the domestic propaganda agency, the Office of War Information. The other OWI


94. See Dorwin P. Cartwright, "Some Principles of Mass Persuasion: Selected Findings of Research on the Sale of United States War Bonds," *Human Relations* 2 (1949); see also Converse, *Survey Research in the United States* 174, 225. Many among the Kurt Lewin “group dynamics” MIT-based (Iowa State-based before that) research team worked for Likert’s Agriculture organization—and many migrated with Likert to the University of Michigan, where Likert’s Survey Research Center and the Center for Group Dynamics shared an institutional umbrella, the Institute for Social Research (see Ibid 356-78).

95. For an overview of the OWI, see Allan M. Winkler, *The Politics of Propaganda: The Office of War
group—market researcher Elmo Wilson’s Polls Division—was engaged in a turf battle with the Likert group that played itself partly over structured-vs.-unstructured interview technique preferences. At the end of the war, the Agriculture team conducted most of the data-gathering for the Strategic Bombing Survey under Eisenhower’s Supreme Allied Command.

The Office of Strategic Services, the CIA precursor, contained a Research & Analysis Branch with large Washington and London operations. The Psychological Warfare Division, also under the Eisenhower Command, employed dozens of public opinion researchers. Lazarsfeld, Robert Merton, and some of the other Bureau staff acted as roving consultants; Lazarsfeld and Merton, for example, did work for the OWI, the Research Branch, and Likert’s organization. Lasswell’s Library of Congress content analysis program thrived throughout the war, staffed by a number of young scholars who would, after the war, emerge as important figures in mass communication research. Lasswell’s team worked closely with the Justice Department, which used its content analysis findings as admissible evidence in its comprehensive campaign to prosecute

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96. As Converse details, Lazarsfeld was asked to mediate and produced a diplomatic, middle-ground report that gave credence to both camps. Wilson’s group ultimately “won,” but its operation was ended not long afterward when, in late 1943, Congress slashed the OWI budget (see Converse, *Survey Research in the United States* 195-200).


99. See Lerner, *Sykewar: Psychological Warfare against Germany, D-Day to VE-Day*.

home-grown enemy propagandists. Most famously, Samuel Stouffer directed the Army’s Research Branch, which conducted hundreds of studies into all aspects of the soldierly experience—including, under the psychologist Carl Hovland, tests of the “Why We Fight” morale films’ effectiveness. At the State Department, the fledgling Voice of America office employed a number of opinion researchers, as did the Federal Communications Commission. A number of the cited authors describe the many reading groups and informal friendships that formed among the legions of social scientists.

A vastly disproportionate number of the wartime social scientists were drawn from the opinion research cluster. They emerged from the war with expanded ranks—many learned the survey techniques during their service—thicker personal networks, and a large measure of war-proven legitimacy. The opinion researchers’ impact was most pronounced in sociology, where Stouffer’s *American Soldier* volumes were treated as important contributions—even by critics. The Bureau’s wartime and postwar publications, some of it media-related but much of it focused on method, were widely

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105. Daniel Lerner conducted a fascinating diffusion study of the volumes themselves, for Lazarsfeld’s Free Press “Continuities in Social Research” collection on the *American Soldier* ("The American Soldier and the Public," in *Continuities in Social Research: Studies in the Scope and Method of "The American Soldier",* eds. Robert K. Merton and Paul F. Lazarsfeld (Glencoe, Ill: Free Press, 1950)). He traces, for example, the elaborate formal and informal marketing that contributed to the book’s enormously successful reception.
touted—and came to be personified by Lazarsfeld and, in a more complex way, by Merton. Shils’s 1948 *Present State* treatment, indeed, was an index for the general sense of excitement that greeted, at least for a time, the new survey techniques within postwar sociology. Lazarsfeld’s media research, and the other opinion researchers’ communication studies, were quite closely identified with the survey methods themselves. Lazarsfeld’s—and the broader survey community’s—media-related research, however, fell off at the end of the 1940s, when government and industry funding became scarcer. Those monies quickly reappeared in the early 50s and rapidly grew over the decade as social scientists at the Bureau and other research centers were recruited into a multi-pronged Cold War campaign for Third World hearts and minds. Lazarsfeld, however, had by then already returned to his more genuine methodological interests.

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When Lazarsfeld was offered the directorship of the new Princeton Radio Research Project in 1937, he made sure that the new post would complement his existing interests and competencies. He negotiated to continue his market research contract work, for example, and he also succeeded in establishing survey research techniques as the core of the Project’s methodological approach. In effect, he integrated the Project into his Newark Research Center, and based the Project’s activities out of its Newark office. These arrangements helped to bring on an early falling out with Cantril, one of the

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Project’s two associate directors and its originator. But Lazarsfeld maintained a very
successful, and symbiotic, relationship with CBS’s Frank Stanton, the Project’s other
associate director.\footnote{There is some confusion about the timing of various name changes. The organization’s initial title, according to the charter, was the “Princeton Radio Research Project,” and it was officially housed in Princeton’s Woodrow Wilson School for Public and International Affairs. Most of the participants, however, retroactively employ the “Office of Radio Research” moniker. The “Office of Radio Research” name, however, was only formally adopted once Lazarsfeld secured a two-year Rockefeller extension in 1939. (See Converse, \textit{Survey Research in the United States: Roots and Emergence, 1890-1960} 149-50.)}

In accepting the directorship, Lazarsfeld made it clear that he viewed the Project
as a convenient vehicle with which to pursue his ongoing methodological inquiries. In a
letter to Cantril, in which he informally accepted the offer, Lazarsfeld referred to his
reasons for founding the Newark Research Center:

\begin{quote}
I wanted to direct a rather great variety of studies so I was sure that from year to year my
methodological experience could increase. I think your project would do splendidly.
Radio is a topic around which actually any kind of research methods can be tried out…
and applied.\footnote{Quoted in Pasanella, \textit{The Mind Traveller: A Guide to Paul F. Lazarsfeld’s Communication Research Papers} 14.}
\end{quote}

He conditioned his acceptance of the Rockefeller funds with the proviso that he be
allowed to maintain his non-radio contract work under the Newark Center’s auspices. As
the Foundation’s John Marshall later recalled, he and Lazarsfeld struck an informal
agreement that he would only accept contract research which advanced his
methodological interests.\footnote{Marshall, in a 1977 interview: “I felt Paul’s methodological interests lent a double focus. He and I had an oral agreement that he would accept outside studies only when they involved his methodological interests. He agreed that was just, and we proceeded that way” (quoted in \textit{Ibid} 13.)} (Of course, Lazarsfeld could cast almost any client project in
methodologically interesting terms.)

The original Cantril-Stanton proposal had called for a mix of experimental and
survey-based approaches to the study of radio, but Lazarsfeld managed to have the
Project’s official charter altered to reflect a more exclusive focus on polling and ratings data.  

(Cantril’s roots were in experimental psychology, and his 1935 collaboration with Gordon Allport, *The Psychology of Radio*, was largely—though not completely—based on laboratory experiments.) As Lazarsfeld recalled in a 1974 speech, he had chafed at the experimental approach—in part because there was such a already rich trove of unanalyzed data waiting to be tapped:

But [the experimental method] was an area in which I neither had experience nor great belief that was the right way to go about it. I finally convinced the foundation that at least at the beginning, a more fruitful way would be to utilize the tremendous amount of program ratings which were available in the files of the commercial networks... in getting these ratings, a large amount of information on each respondent was collected.... but never analyzed because they were not needed for competitive purposes. My idea was to get hold of this material and analyze it as we did in the Vienna study.

Though not without misgivings, the Rockefeller trustees approved Lazarsfeld’s altered plan for the Project. In Newark, the original Cantril-Stanton plan was nicknamed the “Old Testament”—with the new plan, of course, as the “New Testament.”

Lazarsfeld also succeeded in establishing the Project’s de facto operations at his Newark Center. In conjunction with the appointment, Lazarsfeld had been granted a formal research post within Princeton’s Woodrow Wilson School of International and


113. In his oral history interview, Marshall recalled: “Lazarsfeld’s work led to a great deal in the Foundation. It brought the Foundation into polling as an instrument of research. There was some disquiet about that on the part of certain trustees. They were afraid polls would influence more than measure” (quoted in Ibid 13). In a December letter to Lazarsfeld, Stanton alludes to “skeptics,” presumably within the Foundation: “As soon as possible, now that you have the project definitely outlined, I would suggest that you prepare a concise release over your name which lists the specific fields of interest... This can be given to interested ones as well as the skeptics” (quoted in Ibid 14).

114. See Ibid 12.
Public Affairs—where the Project was officially housed.115 But the research activities, staff, and Lazarsfeld himself largely operated from Newark.116 There Lazarsfeld continued to conduct market research, though he presumably did not need the funds. More likely, the studies—including, for example, “Should Bloomingdale’s Maintain its Restaurant?” and “Explanatory Study of the Psychology of the Refrigerator Purchaser”—were harnessed to Lazarsfeld’s longstanding interests in interview methods and decision-making psychology.117

All of Lazarsfeld’s rather assertive changes to the original Project irritated Cantril, especially the experimental undercutting.118 The rift was made worse by Lazarsfeld’s erratic management style and the Project’s chaotic financial state.119 (The early Radio Project, like nearly all of Lazarsfeld’s past and future organizational efforts, was a highly improvisational affair, with missed deadlines, hasty report-writing, and sloppy accounting.)120

116. Lazarsfeld, "An Episode in the History of Social Research”; Delia, "Communication Research: A History” 52. Ann Pasanella remarks in passing that he spent most of his time at Princeton, but offers documentary evidence to the contrary. She quotes a September 1938 one-sentence memo from Stanton: “Who answers the phone at the new office, and have you ever considered changing the name since it is no longer the Newark Research Center?” (The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 15).
117. The study titles, and many others, are quoted in Glander, who provides rich detail from archival sources (Origins of Mass Communications Research during the American Cold War 115-16).
119. Citing interviews with ORR researcher Marjorie Fisk, Converse relates that the two men had “grand battles” over money, and that Cantril was very disturbed by the Office’s financial chaos (Ibid 150). See also Lazarsfeld’s own account: "An Episode in the History of Social Research.” As Converse notes, Cantril completely skipped over the ORR affair in his 1967 autobiography. (See Cantril, The Human Dimension: Experiences in Policy Research (New Brunswick, NJ: Rutgers University Press, 1967).)
Lazarsfeld, for his part, resented Cantril’s attempts at oversight. But the dispute seems to have reached a fever pitch with jockeying over credit for the Radio Project’s “War of the Worlds” studies. Orson Welles’s 1938 parody news bulletin of alien invaders caused panicking among listeners, and the whole episode was famously analyzed in Cantril’s *The Invasion from Mars* (1940). The history of this bestselling book deserves further study as, on the available evidence, it is difficult to disentangle—or even fully comprehend—the disputants’ charges. It seems likely that Lazarsfeld originally proposed what he and Cantril, in their correspondence, called the “Mass Hysteria Study.” The Lazarsfeld archives at Columbia contain a November 1938 memo from Lazarsfeld, informing Cantril and Stanton that the Foundation had approved special funding for the “Mass Hysteria Study.” (The Welles broadcast was October 30.) In late December, Cantril published a preview of the study for the *Princeton Alumni Weekly*, which apparently played up his own central role. Though Lazarsfeld’s letter of complaint does not survive, it is clear from Cantril’s January response that Lazarsfeld had reacted angrily to the *Weekly* summary. Cantril took obvious umbrage at Lazarsfeld’s accusation:

> I am glad you expressed yourself on the release, but I must say that the reaction seems a bit infantile. Perhaps we should have directors’ uniforms with differential insignia. It is hard to imagine people like Frank, Gallup, Allport, Katz, Stouffer would maintain petty

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122. See, for example, Cantril’s comments below as well as *The Mind Traveller: A Guide to Paul F. Lazarsfeld’s Communication Research Papers* 15. The Welles’s idea certainly fits into Lazarsfeld’s well-known talent for identifying and acting upon what he called “firehouse research”—last-minute, event-driven studies that are rapidly designed and carried out to take advantage of a fortuitous, research-relevant occurrence. Merton’s *Mass Persuasion* is a famous product of such “firehouse” research.

123. Described and quoted in Ibid. The preface of the published book includes: “Since the budget of the Princeton Radio Project was obviously unable to anticipate this particular study, the investigation was made possible by a special grant from the General Education Board” (*The Invasion from Mars: A Study in the Psychology of Panic* xiv).

jealousies, and I should like to think that you, too, would have sufficient perspective not
to let such trivia bother you… In the official university release I clearly indicated that the
whole project was under your direction.125

The lecture-by-comparison, no doubt, offended Lazarsfeld, and he later asserted that his
“justified complaint” against Cantril was that “he forced me to make him co-author of the
_Invasion from Mars_ while he had practically nothing to do with it.”126 The odd thing is
that the study, when it was eventually published as _The Invasion from Mars_, did not
credit Lazarsfeld at all. Cantril is the book’s sole author, though there is a “with the
assistance of” credit for Project staff member Hazel Gaudet and the then-unaffiliated
Herta Herzog. It seems likely that Cantril authored the book, and that Lazarsfeld’s
reference to “co-author” refers, instead, to “co-director.” Presumably, Cantril agreed to
write up (or insisted on writing up) the results, thus earning the book’s sole authorship.
The _Invasion_ preface thanks Stanton for advice, for careful reading of the manuscript,
and for commissioning and supervising two “special” CBS surveys; and Gaudet for
actually administering the investigation.127

The plot thickens still further, as the preface notes that Herzog “made an
independent survey of the panic before this study was undertaken,” and credits her for
other help.128 Cantril goes on to declare that his “greatest indebtedness” is to Lazarsfeld—
and cites extensive substantive and methodological assistance. With perhaps a slight trace
of pique, Cantril adds: “Because of his insistence, the study has been revised many times,

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127. On Gaudet: “She not only made most of the tabulations based on the interviews, but many of the ideas
reflected in the tabulations and the text were contained in her detailed memoranda to the writer. From
first to last she was indispensable in the progress of the research” (Cantril, _The Invasion from Mars: A
Study in the Psychology of Panic_ xv).
128. On Herzog: “On the basis of her experience and insight, we were able to prepare the interview
schedule used here. She made the initial study of the checks attempted by the listeners and analyzed
the case studies reported in Chapter VIII” (Ibid).
each revision bringing out new information hidden in the statistics and the case studies."  

The Invasion’s popular style, and its arresting topic, made for rapid sales, and the book was later issued as a mass-market paperback. Like his Allport collaboration, the book uses a mix of experimental and survey-based methods, and Lazarsfeld’s imprint on the latter is unmistakable.

Just months before publication, Cantril resigned his post at the Radio Project in 1939. Soon after, in an apparently coordinated move, the Foundation permitted Lazarsfeld to move the Project’s affiliation to Columbia, and also agreed to fund a new Cantril-directed polling organization, the Princeton Office of Public Opinion Research (OPOR). Cantril’s OPOR, established in 1940, was closely linked to Gallup’s Princeton-based company, which carried out its actual fieldwork. Significantly, Cantril’s organization—which shuttered in 1955—never carried out any radio-related research. Though Cantril himself avoided writing about the conflict, it was widely known among his colleagues in the opinion research field that he considered Lazarsfeld his arch-rival, and he made frequent disparaging comparisons between the OPOR and Lazarsfeld’s ORR/Bureau. Lazarsfeld, in his memoirs and elsewhere, was more public about the dispute, though he did concede, late in life, that Cantril had inspired Herzog’s original Professor Quiz gratifications study, printed in the 1940 ORR collection Radio and the

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129. The passage before the one quoted reads: Lazarsfeld “has not only given the writer innumerable suggestions for analysis and interpretation, but he has, with his rigorous and ingenious methodological help, provided the writer an invaluable intellectual experience” (Ibid xv-xvi).

130. E.g., the supplemental use of unstructured interviews, which are used for illustration but also to construct a Lazarsfeld-style index. See especially 131-134.

131. See Converse’s summary of Cantril-related interviews (Survey Research in the United States: Roots and Emergence, 1890-1960 150).
Lazarsfeld’s relations with Stanton were never marked by rancor of this kind, though the intellectual partnership implied by Stanton’s co-editorship of the three *Radio Research* volumes did not, in fact, exist. Their relationship, instead, rested on a mutually beneficent exchange—Stanton supplied CBS funds and data, while Lazarsfeld wrote and edited publishable radio research. Stanton’s Ohio State dissertation in psychology was a highly innovative scheme to measure broadcast ratings; soon after his degree was awarded, in 1935, he joined the Columbia Broadcasting System as its sole researcher. It’s not clear from the published evidence, but it may have been the Rockefeller Foundation’s John Marshall who introduced Stanton to Cantril in the early stages of the Princeton Project planning. Regardless, Stanton’s CBS commitments prevented him from playing a hands-on role in the ORR, and he became something more like a data donor and paymaster. During his ten years as research director at CBS, he rapidly built up a research department that had, by 1946, swelled to over 100 employees. In that year, CBS founder William Paley asked him to take over the

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132. In a 1975 letter to Pasanella: “But only now do I remember that he was the one who suggested the first studies of the type ‘Why Do People Listen to Professor Quiz?’ This whole approach is now well-known under the name of ‘Gratification Studies,’ especially stressed by Elihu Katz” (quoted in Pasanella, *The Mind Traveller: A Guide to Paul F. Lazarsfeld’s Communication Research Papers* 30).


134. As David Sills—a later Lazarsfeld student, Bureau researcher and longtime Lazarsfeld chronicler—observes, Stanton wrote few if any reports; unsurprisingly, he was oriented almost exclusively to CBS and his business colleagues, and only distantly connected—through Lazarsfeld—to the university community (“Stanton, Lazarsfeld, and Merton—Pioneers in Communication Research” 106).  

135. For this and other biographical information on Stanton, see Ibid.

136. In Sills’s words, Stanton’s CBS contributed the “lion’s share” of the ORR’s data and non-Rockefeller funds (Ibid 108).

137. Ibid 106.
network’s presidency—a position he held for the next twenty-five years. Needless to say, Stanton did not have much time to pore over page proofs.

But in the Project’s earliest years, Stanton and Lazarsfeld did occasionally work together. Lazarsfeld helped Stanton improve a device—later named the “Lazarsfeld-Stanton Program Analyzer”—that permitted researchers to track audience reactions to radio programs in real time. (The design permitted the subject-audience to register “likes” and “dislikes” at any given moment, which were then aggregated and plotted against the run-time.) When the first Radio Research volume was in preparation, in late 1940 and early 1941, Stanton seems to have worked closely with Lazarsfeld in the after-hours. Though Stanton eventually resigned his associate directorship, he remained on the ORR/Bureau board for many more years and continued to supply funds and research data.

Lazarsfeld did not, of course, consider the Stanton relationship a worrisome conflict of interest—despite Stanton’s prominence in the radio industry. Indeed,

138. Ibid.
140. In the fall of 1939, the Project, by then renamed the Office of Radio Research, had moved to an office on Union Square. In Stanton’s oral history interview, he recalled, “Paul and I were down there at Union Square every night and sometimes late at night…” (quoted in Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 15). In Lazarsfeld’s recollection, the Stanton help comes off as less substantive. Referring to the post-1939 Office, Lazarsfeld recalled in a 1972 letter: “When we began our work, he was still in the Research Department of CBS and helped us greatly through advice and subsidiary funds…” (Ibid 28).
141. See, for example, Converse, Survey Research in the United States: Roots and Emergence, 1890-1960. Glander quotes from a later Lazarsfeld letter: “During the first ten years it [the Bureau] was helped in every conceivable way by Dr. Frank Stanton. He was first my collaborator and then a member of the Board until the time that it was decided that the governing board should consist of members only. But even now Dr. Stanton in his capacity as President of the Columbia Broadcasting System is still in many ways supporting the Bureau’s activities” (Origins of Mass Communications Research during the American Cold War 116). According to Stanton, he and Lazarsfeld continued to meet on Sundays, even after he resigned his ORR post (The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers 15).
Lazarsfeld regarded the Stanton collaboration as a *model* for successful social research. The idea, which he frequently generalized, was that an industry under study ought to provide research monies in exchange for much-needed analysis—and the scholar can use the funds and access to advance his own scientific questions. As we have seen, Lazarsfeld was especially skilled at leveraging commercial inquiries to scholarly ends—and remained, throughout his life, an impassioned advocate for this “applied research” model.142 He even, on a number of occasions, attributed the low quality of television research to the industry’s unwillingness to fund studies. In a 1973 letter, for example, he asserted that the “reason” there is “much less” known about TV “today” than there was about radio “forty years ago” is “simple”: “[T]he industry today is so successful and so without complication that they do not need research, and the foundations have lost interest.”143 It is not surprising that one of the reasons that he withdrew from the media research field in the late 1940s was that the networks had by then moved nearly all their research in-house.144

There is no doubt, however, that the CBS connection limited the scope of

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142. See, for example, Lazarsfeld et al., *An Introduction to Applied Sociology* (New York: Elsevier, 1975). In his memoirs, he recalls that he had often been irritated by the “antibusiness” attitudes of his students (“An Episode in the History of Social Research”). See also Katz and Lazarsfeld, *Personal Influence: The Part Played by People in the Flow of Mass Communications* (Glencoe, IL: Free Press, 1955) 6-7.

143. Here is the full passage from the 1973 letter to W. Philip Davison, then at the Columbia Journalism School: “The old mass communication research has really remained rather stagnant… I think that we know much less about TV today than we knew about radio forty years ago. The reason is simple: the industry today is so successful and so without complication that they do not need research, and the foundations have lost interest” (quoted in Pasanella, *The Mind Traveller: A Guide to Paul F. Lazarsfeld's Communication Research Papers* 29). In a 1966 speech, he made a similar point: “Unfortunately, the coming of TV has not enlarged the scope of mass communication research but rather narrowed it down. TV was such an immediate spectacular commercial success that it did not look for the help of research the way radio did … [thus] a very important source of funds dried up” (quoted in Ibid 22).

144. David Morrison, "Personal communication" (2005). Morrison is writing a paper on Lazarsfeld's stillborn early 1950s television research, which the Ford Foundation initially sponsored but later withdrew.
potential Radio Project research—and probably limited, too, the kind and degree of criticism that he could level at the radio industry. The initial Rockefeller grant, of course, had explicitly placed scholarship that questioned the commercial basis of the industry off-limits. But Lazarsfeld and the Project (and its successors) conducted almost no production-side research into radio—even of a kind that posed no direct challenge to the industry’s commercial character.145 When later asked about the paucity of industry-related research on radio, he expressed puzzlement at the lacuna. In an oral history interview, he claimed that,

I never understood the importance of studying the inside of the communications industry. I even refused papers which were offered for our books because I found them too descriptive. It’s hard for me to understand, but I was really completely blind to it.146

He protests too much: He wrote quite knowingly about ownership issues in 1941—in an article, tellingly, that appeared in the Frankfurt School’s obscure (and only briefly English-language) journal.147 It is probably true that one of the reasons for Lazarsfeld’s neglect is that production-side research—into the industry’s economic structure, or into the program design process—did not mesh nearly so well with his methodological and decision-act interests. But it is also likely that he perceived that this kind of study might

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145. Lazarsfeld did, however, conduct research into an ownership issue, at least once. He was hired by radio station-owning newspapers. Lazarsfeld: “Just before the war, the FCC opened hearings on the question of whether newspapers should be permitted to own radio stations. We received funds to set up punched cards providing, for every station, data on ownership and on the way news program were handled. These funds were provided by a committee including all radio stations owned by newspapers. This was, at the time, an important source of income for us, but I made it a condition of our work that the FCC would have complete access to our data” (“An Episode in the History of Social Research”). It is fascinating to note that only the audience findings from this study, and not the ownership research, were later published—in the sixth chapter of Lazarsfeld, Radio and the Printed Page.

146. Quoted in Pasanella, The Mind Traveller: A Guide to Paul F. Lazarsfeld’s Communication Research Papers 30. In a 1975 letter to a Swiss journalism scholar, he wrote: “I am sure you are aware that my work is only in the field of mass media research; I never had anything to do with the production side of the industry. As a matter of fact, it has been very little studied and how this deficiency would be amended is also an interesting topic” (quoted in Ibid 30).

endanger his ongoing access to funds and data. In a 1941 speech to a joint meeting of the
National Association of Broadcasters and and the American Association of Newspaper
Executives, he admitted to the worry and, implicitly, to the resulting chilling effect:

Those of us social scientists who are especially interested in communication research depend upon
the industry for much of our data. Actually most publishers and broadcasters have been very
generous and cooperation in this recent period during which communications research has
developed as a kind of joint enterprise between industries and universities. But we academic
people always have a certain sense of tightrope walking: at what point will they shut us off from
the indispensable sources of funds and data? Lazarsfeld walked this tightrope many times over the 1940s, and when his audience was
the educated public—arguably the industry’s most feared source of criticism—he tended
to downplay any potential negative effects of radio. This is not to say that he never
criticized the industry—he did, albeit mildly, on a number of occasions—but almost
always to academic audiences. It is fair to assume, given all the evidence, that
Lazarsfeld’s dependence on CBS, in particular, cast a limiting shadow over the potential
scope of study as well as the degree of permissible criticism.

The ORR’s topical anarchy in its earliest years, brought on in part by Lazarsfeld’s
ongoing commitment to client-driven, non-radio contract work, began to worry
Rockefeller Foundation officers, including Marshall. As Lazarsfeld remarked in his
memoir, for all the Project’s data riches and its staff’s enthusiasm, by the fall of 1938 the
“image of the office was not good.” No central theme was “visible,” and Lazarsfeld and
the rest of the office began “hearing rumors that important people questioned whether we
knew where we were going.” The Rockefeller grant was coming up for renewal the

148. Quoted in Glander, Origins of Mass Communications Research during the American Cold War 117.
149. As Lazarsfeld admitted in his memoirs, his “all-eggs-in-one-basket policy,” in which he labeled all the
Office’s research—radio-related or not—under the “radio research” umbrella, contributed to his
problems with the Foundation (“An Episode in the History of Social Research”).
150. “An Episode in the History of Social Research”. It was late fall, of course, when Lazarsfeld and
next year, and Lazarsfeld realized that he had to demonstrate results in some published form. From its mountain of data, Project-collected as well as scavenged—Lazarsfeld was a legendary data bricoleur—he decided to organize a volume around a comparison of radio with printed media. (This seems to have been largely prompted by funder-driven data contingencies: He had access, of course, to voluminous CBS research, but also newspaper and magazine readership surveys.) Early plans for the book—which was published as *Radio and the Printed Page* in 1940—eased the Foundation’s worries, though Marshall assigned Lloyd Free, the Seminar secretary and opinion researcher close to Cantril, to move along the book’s editing.  

The Foundation approved a three-year extension on the original grant in late 1939.

As part of the grant extension, the Foundation agreed to break off the Project’s affiliation with Princeton University and gave the entity a new, free-standing name—the Office of Radio Research. As it happened, the University of Newark had recently asked Lazarsfeld to vacate its offices there, pleading space constraints. Once the Rockefeller grant was renewed, Lazarsfeld moved the newly-minted Office to Union Square in New York City in the fall of 1939. Lazarsfeld apparently had problems persuading Columbia to take on the new affiliation, though Stanton, Lyman Bryson (the Seminar member and Teacher’s College professor), and—most crucially—Lynd intervened to convince the

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Cantril had their initial falling out over the Orson Welles study. From his memoir or the other published accounts, however, it is not clear whether Cantril had complained to Marshall about the Office’s lack of focus.


152. Ibid 149.

University. Lynd not only vouched for Lazarsfeld, but secured for him a courtesy appointment in the University’s sociology department—which was not only crucial for the ORR affiliation, but was also Lazarsfeld’s accidental admission to the sociologist’s ranks.

Radio and the Printed Page (1940) reflects the exigent conditions under which it was assembled. The volume is a series of discrete studies which—despite valiant efforts to forge connections—betray their original independence.

The title and introduction promise a comparison between print media and radio, but the book’s 354 dense pages honor that pledge hardly ever. It’s true that vast caches of reader- and listener-data—ORR-collected and otherwise scavenged—get probed and tabulated early and often. But these data are woven into the book’s narrative rather indiscriminately, and only occasionally related to one another. The volume’s thematic

154. Here’s Stanton’s oral history interview on his and Bryson’s intervention: “The Princeton Project was way behind the times. Paul was a flamboyant character, and they [Columbia officials] didn’t quite understand him…. Columbia couldn’t quite make up its mind. Was he kosher? I helped as did Lyman Bryson…” (quoted in Ibid 15). In a 1972 Lazarsfeld letter to the dean of the Columbia Journalism School: “Dr. Stanton was instrumental in getting the grant to Cantril. When we began our work, he was still in the Research Department of CBS and helped us greatly through advice and subsidiary funds…” (quoted in Ibid 28).

155. The Columbia ties were made official in the spring of 1940, with Lynd as chair of a new governing board (Converse, Survey Research in the United States: Roots and Emergence, 1890-1960 268). The ORR affiliation may have been won by the University’s interest in educational broadcasting, shared of course with the Rockefeller Foundation. Citing a letter from the Columbia provost to Lynd, Converse remarks that, “There was, apparently, a mutual interest on the part of the Rockefeller Foundation and the Columbia administration in the development of educational radio” (Ibid). It was Lynd, too, who later arranged for the ORR to occupy office space at Columbus Circle that had been part of the University’s medical school. As Converse records, the space came complete with a surgical amphitheater and rising and descending floors (Ibid 268-69).

156. Many of its chapters had been, or would be, published as stand-alone journal articles. At least two appear, for example, in the pair of special issues of the Journal of Applied Psychology that Lazarsfeld guest edited in 1939 and 1940. (So many of the special issues’ articles were written by Lazarsfeld that he resorted to a pseudonym—Elias Smith—to disguise his authorship of a few of the studies; see Lazarsfeld, “An Episode in the History of Social Research.”) Most of the Lazarsfeld-authored studies in the two special issues were not radio-related.
anarchy was a product, no doubt, of its frantic, deadline-driven compilation. In his memoir, Lazarsfeld wrote that it was a “grueling task to assemble the manuscript in a short period of time; we worked day and night literally, in relays, to accomplish it.”

The book’s meditations on listener pleasure stand out to the 21st century reader, because they so clearly prefigure the Bureau’s strand of “gratifications” research that would, over the postwar decades, cohere into a distinct tradition. Herta Herzog—soon-to-be Bureau associate and future wife to Lazarsfeld—had interviewed listeners of the *Professor Quiz* program, and her findings and reflections about the show’s appeals are threaded into two of the book’s chapters. But the topic of listener gratification is in every sense peripheral to the book’s argument.

And there *is* an argument in *Radio and the Printed Page*, submerged and fragile though it is. Educational broadcasting is the book’s only real leitmotif, and here Lazarsfeld manages to develop a distinct position that, however, skillfully splits the difference between the Rockefeller Foundation’s original stance and the commercial broadcasters’ deflected laissez-faire. Recall that Marshall, with Cantril’s guidance, had conceived the Radio Research Project with the early 1930s debates over educational broadcasting very much in mind. The initial Rockefeller idea was that reliable, independent data on audience preferences would reveal a genuine market for educational programming; the corresponding hope was that the Project’s research yield would

convince broadcasters to cooperate with the educationalists, if only on self-interested grounds.

Without explicitly dismissing Marshall’s early, conciliatory vision, Lazarsfeld interpreted the ORR’s listener data against this optimistic grain. According to the surveys, the audience for educational programming tended to be the already educated; the less-learned listeners—the educationalists’ target audience—were opting instead for the medium’s lightest, least edifying fare. There is, Lazarsfeld sighed, no vast pool of under-informed listeners eager to have their minds stocked with broadcast knowledge. We must, he concluded, rid ourselves of the unrealistic “mousetrap fallacy”—the idea that strong programs will automatically attract large and receptive audiences.159

Marshall’s idea had been that the data would prompt commercial broadcasters to embrace the educationalists. Not only did Lazarsfeld plunge that vision in a cold, statistical bath; he also explicitly shifted the public interest burden off of the radio networks and onto the educationalists. Much of the book is taken up with a plea that educational broadcasters engage in “audience building”: If the problem is the audience and its disinterest—and not, say, the paucity of programming itself—then it is the educational advocates’ responsibility to wake the slumbering listener. An aggressive “audience-building” campaign, Lazarsfeld counseled, will finally win over the networks: “Nothing will do more to remind the radio industry of its obligation to give public service than a public enlightened to the fact that this is its due.”160

Educationalists would do well, Lazarsfeld continued, to emulate the industry’s

159. Lazarsfeld, Radio and the Printed Page.
160. Ibid.
carefully honed appeal to genuine audience pleasures. Here, it turns out, is the rationale for the book’s *Professor Quiz* analysis: The show’s multiple audience gratifications point the way to a more listener-friendly model of educational programming:

If the people on the lower cultural levels do not want to listen to serious broadcasts, why should we try to inveigle them into doing so? If they prefer to get education of a sort from listening to ‘Professor Quiz’ rather than to a round-table discussion, why shouldn’t they?  

Educational broadcasters should accept the centrality of audience appeal—should “exploit,” that is, popular programs like *Professor Quiz* for their genuine (if limited) educational content, and should, moreover, design programming with listener preferences for entertainment in mind.  

Lazarsfeld proceeds to argue that organizing an audience *on the ground*, community by community, is more important, anyway, than the programs themselves. Educational advocates should recast themselves in a local mold, and focus less on changing the radio dial. The educationalist’s role would be to “canalize” the “many suitable radio programs” for his community, with local tie-ins and guided discussion.  

If the educator had imagined himself, before, as the provider of broadcast “powder” which “drove the bullet forward,” he would do better, instead, as the local “trigger” which releases the “ever-present energy” of radio programs. Here, Lazarsfeld argues that the failures of mass mediation could be partly salvaged through face-to-face contact.

161. Ibid 94.

162. “If young people feel that they can learn something by listening to ‘Gangbusters,’ if adults like to follow ‘psychological’ programs, no intelligent educator will merely frown upon them and let it go at that. Teachers should explicate these programs, so far as possible, in their classroom work. Psychologists should endeavor to raise program standards, because if they do not the opportunity afforded by these broadcasts for the education of large audiences will be missed” (Ibid).

163. Ibid. The word “canalize” is itself significant, as Lazarsfeld—first in 1942, and then in his classic 1948 collaboration with Merton—would describe “canalization” as one of three rarely-met conditions under which mass communication could be persuasive.
The book’s argument—that the audience, and not the networks, are to blame for poor educational payoffs, and that it’s the educators’ responsibility to build up an engaged populace, in part by giving listeners more of what they want—had been previewed in Lazarsfeld’s 1939 application for the Rockefeller grant extension. In that document, Lazarsfeld reflects on the Project’s first two, “frankly experimental” years, with direct reference to the Foundation’s interest in educational radio. “The work to date,” Lazarsfeld wrote, “has demonstrated the weak pulling power of such programs.” Lazarsfeld points to two “central factors” in accounting for the disappointing tune-out—the same factors that he highlights in Radio and the Printed Page. First, the element of “self-selection” in listener choice; the intended audience—the uneducated—consistently opt for lighter fare. This “tendency to listen only to those things one is ‘for’ in advance,” Lazarsfeld observes, is a “crucial handicap.” The other major “failure” is the educationalists’; they have not adapted to, and learned from, rival commercial programs, which model the “adequate” format and “level of psychological appeal” necessary to win audiences:

One can dismiss outright the tricks that hold and build audiences for meretricious, pseudo-educational programs like ‘Professor Quiz.’ Or one can study these and learn why homo Americus responds to them.

It is not the industry’s fault that the educational promise of radio has gone unfulfilled, Lazarsfeld insists here; the networks, indeed, have a great deal to teach the educators about over-the-air learning.

Radio and the Printed Page, Lazarsfeld’s first book-length treatment of the new educational promise of radio.

medium, is revealing in a number of overlapping ways. There is, first, Lazarsfeld’s trademark resourcefulness, expressed even in the doggedly gathered data stockpile itself. The book’s argument is rather frayed twine—here a chapter, gone the next—but Lazarsfeld’s focus on educational broadcasting does bind the otherwise unwieldy book. In typical fashion, he manages to re-frame a set of potentially disappointing findings—on audience disinterest, in this case—in a novel, coherent direction.

The book also establishes a pattern that Lazarsfeld will adhere to in his many other radio-related publications over the 1940s. Radio and the Printed Page is a careful book, tailored to its main audiences, the Foundation and the industry itself (through Stanton). Lazarsfeld’s “audience building” thesis attends not only to the Foundation’s reformist liberalism but also to the industry’s desire to deflect criticism. The fact is that Rockefeller and CBS made the book—and the ORR itself—possible in the first place; and the Lazarsfeld shop’s survival quite literally depended on both constituents’ ongoing good will. The book managed, with this dependency in mind, to thread the argumentative needle between educational advocacy and network prerogative.

The pattern that Lazarsfeld set with this book wasn’t that he would, from this point on, only publish industry- and foundation-friendly research, and nothing else. No, the pattern that he set—the pattern that he followed over the decade to come—was to package, and re-package, his findings to appeal to an always shifting roster of audiences. The important readers of Radio and the Printed Page were Stanton and the Foundation’s officers, but this would change; Lazarsfeld’s many future radio studies would be pitched to their respective audiences.
In this respect, two of the book’s findings are especially relevant. Recall that the observed self-selection of radio listeners was the main empirical basis for Lazarsfeld’s “audience building” argument. This “self-selection” finding—expanded over time to include perceptual filtering—would get featured prominently in nearly every Bureau communication study in the years to follow. The idea of selectivity—that media audiences seek out (and absorb) only those messages and outlets with which they already agree—would prove fundamental to The People’s Choice (1944), Personal Influence (1955), and most of the published radio research in-between.

Significantly, Radio and the Printed Page raised the other major theme of Bureau media research—the importance of interpersonal influence. In its “go local” prescription to educators, the book insists that the educational benefits of radio can be partly salvaged by on-the-ground audience organizing. Citing advertising and political practices, Lazarsfeld observes that his advice—for a face-to-face supplement to otherwise ineffective educational radio—is in line, he writes, with the “ideas of propaganda which have proved successful in other fields.”

This pair of themes—selectivity and interpersonal influence—threads its way through a decade’s-worth of Bureau-sponsored media scholarship. The Bureau’s core bundle of interpretive findings—that direct media persuasion is often ineffective, relative to face-to-face influence which, however, can be used to supplement direct appeals—is already on embryonic display in Radio and the Printed Page.

What’s significant about all this is that these findings, from 1940 to 1955, got

165. Lazarsfeld, Radio and the Printed Page.
deployed *differently* from one publication to another. The same bundle of empirical conclusions, in other words, was *packaged* in strikingly diverse form over time. The interpretive oscillations were not random, nor did they trend in any consistent direction. As with *Radio and the Printed Page*, it was, again and again, the assumed *audience* that ultimately shaped the packaging on offer.

Over Lazarsfeld’s media-research career, the contrasts in framing were dramatic, especially in evaluative terms. Indeed, it is possible to isolate two dominant frames, with the caveat that not all of Lazarsfeld’s media-research studies over the 1940s cleanly fit either. The striking thing about the two claims is that they are, in evaluative terms, diametric opposites of one another. In much of the published media research, the findings are framed, first, in terms of *effective propaganda design*. The limits of direct media persuasion, in this frame, are presented as a challenge for persuaders that, however, can be got around through messages that appeal to audience’s pre-existing interests and through supplementary face-to-face persuasion. In this first frame, then, the selectivity and interpersonal influence findings are packaged as advice to the would-be propagandist. The overriding message: Persuasion is complicated, so here’s how to navigate around the obstacles.

The other prominent frame—less often deployed than the first, but (with *Personal Influence*) granted the definitive last word—takes the same basic findings, but treats them as evidence, instead, that the impact of media is *happily negligible*. Selectivity and interpersonal influence, in this second frame, are treated as reassuring buffers between man and media. Public fretting about movie-made children is an over-reaction which
neglects—to quote the *Personal Influence* subtitle—the “part played by people.” Here, the message is a populist one—that media persuasion is very often ineffective.

If, employing the first frame, Lazarsfeld counseled the propagandist in her art, he reassured, in the second frame, the targets of propaganda that they need not worry. In both frames, direct media appeals are described as relatively ineffective *on their own*. But in the *effective propaganda* frame, face-to-face influence and predisposition-sensitive appeals are portrayed as strategic *complements* to media-based propaganda campaigns.

Interpersonal persuasion and self-selection, in the *limited effects* frame, are conceptually re-arranged—maneuvered, that is, to *block* media appeals.

If this is Jekyll and Hyde, there’s nothing too strange about the Lazarsfeld case. He was exceptionally skilled at kneading data into an audience-pleasing shape. It is possible, indeed, to plot his use of one or another frame against his intended audience. Over the decade, that audience would constantly shift about—government propagandists, anti-prejudice campaigns, market researchers, the educated public, and (especially at the end of the decade) fellow sociologists. It was Lazarsfeld’s singular talent—to borrow the media-industry cliché—to give the audience what it wants.

When the audience was would-be persuaders, the message centered on propaganda design. Thus Lazarsfeld, with Merton as co-author, presented wartime communicators with a sort of technical manual in their 1943 “Studies in Radio and Film Propaganda.” A similar bundle of research-derived tips was offered to anti-racist campaigners, in Lazarsfeld’s 1947 “Some Remarks on the Role of Mass Media in So-

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called Tolerance Propaganda.” The stress, in these papers, was on how to harness face-to-face and predispositional factors to the act of persuasion.

When the audience was more public—citizens’ groups, industry critics, the industry itself, and the academics of the public opinion field—the message shifted to emphasize the counter-intuitive innocuousness of the mass media. Lazarsfeld’s eve-of-war address at a Rockefeller-sponsored University of Chicago conference in 1941, for example, downplayed the threat of media string-pulling, with reference to the same principles of self-selection and personal influence. At the other end of the decade, Lazarsfeld’s preface to Bureau student Joseph Klapper’s 1949 *Effects of Mass Communication*—a state-of-the-knowledge review commissioned by the SSRC’s Public Library Inquiry—is upbeat and reassuring, like the monograph itself. The emphasis, in these more public essays, was on the mass media’s limited effects.

When the audience was more exclusively academic and, in particular, sociological, the Bureau’s media research findings were placed in more complicated frames. In certain cases—as with Lazarsfeld’s famous 1941 essay for the Frankfurt School’s *Studies in Philosophy and Social Science*—the audience was very select indeed,

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170. Lazarsfeld came to accept the “sociologist” label at around the time these essays were written, in 1948.
and the framing was highly particularized. In papers directed at broader academic publics—including the 1948 essays “Communication Research and the Social Psychologist” and “Mass Communication, Popular Taste, and Organized Social Action” (co-written with Merton)—the limited effects and effective propaganda frames coexisted, albeit awkwardly. These papers tended to be lively, fecund, and more judgmental than his other work—and, indeed, it is on their bases that various Lazarsfeld defenders have built a case for his status as a critical scholar manqué. Methodological ambitions, the limits of existing data, the need for long-term measures of media impact—all these themes were touched upon in the essays written for fellow social scientists.

Radio and the Printed Page contained, in embryonic form, the findings that would, over the next decade and a half, get poured into a rotating set of interpretive casts. But Radio was ultimately concerned with attracting an audience—building a constituency—for educational broadcasting. The book was Lazarsfeld’s cobbled-together answer to the Rockefeller Foundation’s original question.

But Lazarsfeld's own questions, about research design and decision-making, were never foregrounded in the motley assemblage of data that, in Radio, was bashed into barely coherent shape. His questions would, finally, get properly posed in the ambitious

171. Lazarsfeld, "Remarks on Administrative and Critical Communications Research."
study that was eventually published as *The People’s Choice* in 1944.173 In 1940—the same year that *Radio* appeared and just half a year after the ORR affiliated with Columbia—Lazarsfeld’s Office conducted a months-long, field-based study of media persuasion in Erie County, Ohio, over that year’s presidential campaign season. (As Peter Rossi describes in his detailed post-mortem on *The People’s Choice, Voting*, and the early voting studies of the University of Michigan’s Survey Research Center, Lazarsfeld first envisioned a study of consumer preferences and advertising exposure, but could not secure the necessary funds. In a series of interviews, Lazarsfeld told Rossi that he had, at some point before the 1940 election, “hit upon the idea of studying the impact of a presidential campaign upon a panel of voters, who would be interviewed repeatedly during the course of the campaign.”)174

In line with *Marienthal, Middletown*, and the community studies tradition, Lazarsfeld had selected a single city, Sandusky in Erie County, as a living laboratory for his inquiry into real-time media persuasion. Unlike *Marienthal* and *Middletown*, however, the Erie study eschewed rich, place-specific observation in favor of the individualist, survey-based tool-kit of the opinion research field.175 And Lazarsfeld

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174. The statement is Rossi’s, but he is summarizing the Lazarsfeld interview. See "Four Landmarks in Voting Research," in *American Voting Behavior*, eds. Eugene Burdick and Arthur J. Brodbeck (Glencoe, IL: Free Press, 1959). The election study was more palatable to the Rockefeller Foundation—less tainted by commercialism—than the initial consumer/advertising conception, and the Foundation agreed to support the study financially after the focus shifted to the election.

175. *Marienthal, Middletown* and the others drew on survey data, to be sure, but Lazarsfeld himself admits, in the preface to the second edition of *People’s Choice*, that one of the study’s failings was its neglect of local context (Lazarsfeld et al., "Preface to the Second Edition," in *The People’s Choice: How the Voter Makes up his Mind in a Presidential Campaign*). Onetime Bureau student James Coleman, in his later treatment of Lazarsfeld’s legacy, stressed the individualist focus of the Erie and other Bureau studies: “Even when a community was the setting, the analysis was not of the community but of individual decision-making in a social context. This direction of work Paul Lazarsfeld so impressed upon the discipline of sociology that we all do it now, not just those in a ‘Lazarsfeld tradition’ or a
designed the study with his pair of intellectual interests very much in mind. Indeed, the Erie research plan put his enthusiasm for methodological innovation in the service of his ongoing inquiry into the psychology of the decision act. For the Erie fieldwork, Lazarsfeld invented and deployed a survey technique that could track individual opinion change over time. The repeated survey had long been part of the opinion researcher’s arsenal, of course, but Lazarsfeld’s panel method enabled researchers to monitor a constant group of Erie County residents over the entire campaign. Six hundred voters were interviewed every month during the seven-month lead-up to the election. The panel technique was meant to expose the mind of the individual decision-maker to the researchers’ scrutiny. An electoral campaign, to Lazarsfeld’s reasoning, was an ideal backdrop for an inquiry into the decision act: a structured campaign of influence that culminates in a scheduled choice.¹⁷⁶

The Erie study was designed to measure, in particular, the impact of media appeals on citizens’ presidential votes—though the published book downplayed the original design.¹⁷⁷ To this end, Lazarsfeld and the research team conducted an exhaustive content analysis of the election barrage—in print and over the airwaves—for the length of the campaign. Panel members were extensively surveyed, too, about their media

¹⁷⁶. Rossi’s interviews confirmed that Lazarsfeld conceived the study in terms of his longstanding interest in decision-making (Rossi, “Four Landmarks in Voting Research” 314).

¹⁷⁷. As late as 1942, in passing references to the Erie study, Lazarsfeld was still describing the research as centered on media influence. In a 1942 article on the newspaper industry’s changing relations with new media, Lazarsfeld relates, in a footnote, that the “Erie County inquiry, incidentally, was done to study the role of newspapers and other media of communication during a presidential campaign” (“The Daily Newspaper and its Competitors,” Annals of the American Academy of Political and Social Science 219 (1942) 35).
consumption habits. The idea was to connect the media persuasion to the decision—to measure, that is, the expected influence of the media campaigns on panelists’ voting.

In the end—and to Lazarsfeld’s surprise and, indeed, horror—the fieldwork uncovered little evidence of direct media persuasion. Only 54—“few indeed”178—of the 600 panelists, for example, appeared to have changed their voting preferences as a result of the candidates’ media campaigns. The findings were a grave disappointment, and for a year they paralyzed Lazarsfeld and his co-authors.179 Lazarsfeld had labored to find funds for the study, and he and his collaborators had planned the project meticulously—down to the elaborate content analysis and detailed media-related questionnaires—only to discover, after-the-fact, that the mass media failed to bring about conversions.

*The People’s Choice*, when it was eventually published four years later, was framed as a “voting” or “election” study, and generally gets remembered this way too.180 But the fact that *The People’s Choice* was put forward as an election study—and not, as planned, as a report on media influence—had everything to do with with the disappointing findings. *The People’s Choice* is, as a result, a study of media persuasion that molted, due to hypothesis-denying data, into a book about voting. As published, the book took as its argument the claim that—contrary to conventional wisdom and democratic theory—voters’ preferences are for the most part *already formed* before a


179. Based on his interviews, Rossi reports that Lazarsfeld set aside the data for a full year after conducting the preliminary analysis, on account of his disappointment at the lack of media persuasion findings (Rossi, "Four Landmarks in Voting Research” 316).

180. In the book’s introduction, Lazarsfeld and his co-authors are unambiguous: “This is a report on modern American political behavior—specifically on the formation of votes during a presidential campaign.” The researchers set out to “discover how and why people decided to vote as they did” (*The People’s Choice* 1).
campaign begins, and that these preferences are rather inflexibly grounded in voters’ social backgrounds.\textsuperscript{181}

In the course of their data analysis, Lazarsfeld and the others had realized that voters rarely switch their candidate allegiances. The other surprise was that a few demographic characteristics—socio-economic background, religion, and rural-vs.-urban residence—could predict fairly accurately any given individual’s eventual vote. On the basis of these findings, Lazarsfeld constructed an “Index of Political Predisposition” (IPP) which, they claimed, largely explained voting outcomes. The authors explicitly presented their counter-intuitive finding for social determination as a real-world rebuke to the “rational independent voter” that undergirds democratic theory.\textsuperscript{182}

This thesis, of course, was an artful adaptation to a set of unexpected findings, and it is on these grounds that Rossi calls the study “largely inductive.”\textsuperscript{183} Indeed, the inclusion of the key demographic variables was accidental and fortuitous; these data were only collected as a matter of standard survey practice, and not because these data were expected to occupy an important place in the research analysis.\textsuperscript{184} So neatly had Lazarsfeld and his co-authors re-framed the inquiry—from successful media persuasion to the social character of voting—that the study’s original purpose was nearly impossible to discern in the published report.\textsuperscript{185}

\textsuperscript{181} “There is a familiar adage in American folklore to the effect that a person is only what he thinks he is, an adage which reflects the typically American notion of unlimited opportunity, the tendency toward self-betterment, etc. Now we find that the reverse of the adage is true: a person thinks, politically, as he is, socially. Social characteristics determine political preference” (Ibid 27).

\textsuperscript{182} Ibid 316.

\textsuperscript{183} Rossi, “Four Landmarks in Voting Research” 319.

\textsuperscript{184} Ibid 318.

\textsuperscript{185} Rossi: “So few of the initial expectations of the study were borne out in the findings that it would be difficult for the reader to reconstruct the study’s original aims from the report alone. The analysis as
In addition to the book’s central voting thesis, *The People’s Choice* draws attention to its methodological innovations. In characteristic Lazarsfeldian move, the introduction includes a capsule history of survey research methods. The tools of opinion research, the authors argue, have advanced considerably in previous years, rendering “much more precise the study of certain determinants of vote.” But hitherto existing methods have been unable to follow the “vagaries of the individual voter along the path” to his vote. “This study, designed to yield such answers, used the so-called panel technique as the next step forward in opinion research: *repeated interviewing of the same people.*” The narrated claim to methodological novelty is even more prominent in Lazarsfeld’s preface to the book’s second edition.

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The bundle of “selectivity” findings that would, in Klapper’s *The Effects of Mass Media* (1949) and *Personal Influence*, prove so crucial to the “limited effects” conclusion were, in *The People’s Choice*, already deployed as the principal explanation for the fixity of vote preferences and the relative ineffectiveness of media persuasion. Drawing on Kurt Lewin’s experimental social psychology, Lazarsfeld and his coauthors refer repeatedly to the comforts of consistency and group conformity. The Erie subjects tended to select only those media which confirmed already-held beliefs; and when the voters were exposed to contradictory messages they tended to discount, filter out, or re-interpret the content in finally presented is organized along lines very different from what the original emphasis in the design would seem to indicate” (316).

187. Ibid.
188. Lazarsfeld, "Preface to the Second Edition".
such a way that their original beliefs were, paradoxically, re-affirmed. The fact that people “select their exposure along the line of their political predispositions,” the authors write, is only a “special case of a more general law which pervades the whole of field of communications research.”

The desire for group acceptance, in particular, accounted for the “bottleneck of conversion” that the researchers discovered. It is important to note, however, that the “group” category was not framed by Lazarsfeld and the others in terms of primary groupings, but instead in relation to broad demographic clusterings like “Catholic” and “poor.” This is important because, in Personal Influence and elsewhere, the vogue for small groups was read, retroactively, back onto the Erie study—even though The People’s Choice makes only passing and speculative reference to small group conformity per se.

Though the media question was, for the most part, downplayed in the book, Lazarsfeld, Berelson and Gaudet point to a pair of media effects that, in contrast to conversion, do coexist profitably with the principles of selectivity and group consistency. There is, the authors argue in the book’s middle chapters, a media-induced “activation effect,” as well as a closely related “reinforcement effect.” It is “naïve,” they write, to

189. The People’s Choice 76. Lazarsfeld’s claim that people attempt to avoid any “uncomfortable state of mental indecision or inconsistency” is explicitly indebted to Lewin’s “Field Theory.” Lewin’s student Leon Festinger would famously summarize these and related social psychological principles in his 1957 The Theory of Cognitive Dissonance.

190. Lazarsfeld and his co-authors do, in the second to last chapter (“The Political Homogeneity of Social Groups”), speculate that real social networks are likely to correlate with demographic overlaps. They suggest, tentatively, that these actual social groupings are the source of in-group conformity pressures—though of course their variables-based demographic data do not support this conclusion. “If a person’s vote intention is to a great degree a symbol of the social group to which he or she belongs, then we should not be surprised that people iron out inconsistencies in their thinking in such a way as to conform to the group with which they live form day to day” (Ibid 148).
expect much direct conversion from media persuasion, but presidential campaigns nevertheless serve to activate partisans and reinforce their already held beliefs. “Latent predispositions” are awakened by media exhortations; the “continuing flow of partisan arguments” helps prevents defections to the other party’s candidates.

The idea of (mere) reinforcement, in particular, would prove vital to the “limited effects” conclusions of Klapper’s 1949 and 1960 book-length literature reviews and *Personal Influence* itself, but here the reinforcing is framed, instead, as a bona fide effect. The observed pattern of activation and reinforcement, indeed, is recruited to the book’s thesis that the democratic ideal falters in practice. In *The People’s Choice*, the paucity of true converters is deployed to support the book’s central claim:

> The real doubters—the open-minded voters who make a sincere attempt to weigh the issues and the candidates dispassionately for the good of the country as a whole—exist mainly in deferential campaign propaganda, in textbooks on civics, in the movies, and the minds of some political idealists. In real life, they are few indeed.

The fact that the same bundle of evidence can be so easily put forward as reassuring evidence of media non-interference in the democratic process—the claim, in *Personal Influence*, that the American political system is not ridden with top-down manipulation but instead richly endowed with face-to-face discussion—is a startling testament to interpretive plasticity.

There is another, though on reflection unsurprising, irony. In the memory of mass communication research, *The People’s Choice*’s unsupported and passing speculation about the greater efficacy of face-to-face influence, and the corollary suggestions concerning “opinion leaders” and “the two-step flow of communication,” are far better remembered than the book’s other, more fundamental claims. The book concludes with
these ideas ("The Nature of Personal Influence"), but can only suggest them as stimulants
to further research, since the original study design only inadvertently captured data that
was, even so, merely tantalizing and not conclusive. (Indeed, Rossi reports that the key
survey question that led to the “opinion leader” formulation—“Have you tried to
convince anyone of your political ideas recently?”—was initially included as part of a
series of queries designed to track “extroversion”!)\(^{191}\)

The claims of this last, conjectural chapter would, famously, go on to shape the
research design of the Decatur/Personal Influence study—and, as related there, emerge as
the building blocks of the “limited effects” construal. The chapter’s interlinked
postulates, however, derived from the authors’ reflections on three variably supported
findings: that personal persuasion had more self-reported influence on voting preferences
than the mass-mediated sort for most voters; that the voters who reported influencing
others, however, cited the formal media as more effective sources of influence; and that
these “opinion leaders” also tended to consume much more media than others. One
possible implication, according to the authors, was a model of media influence that could
account for the selectivity and personal influence findings. The now-famous idea—the
“two-step flow of communication”—is that media messages are consumed, first, by
opinion leaders, who then pass the messages along to others. The evidence suggests, the
authors write, that “ideas often flow \textit{from} radio and print \textit{to} the opinion leader and \textit{from}
them to the less active sections of the population.”\(^{192}\)

As in later, more detailed renderings of the idea, the role of the opinion leader is

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\(^{191}\). "Four Landmarks in Voting Research" 318.
\(^{192}\). \textit{The People’s Choice} 151.
described here with ambiguity: Does he or she merely (and faithfully) relay the media message, or does the leader filter the content before passing along his or her modified version? This turns out to be a crucial question, and one that remains elusively treated in *Personal Influence*. The normative implications of the model largely hinge on which of the two inflections gets highlighted: If these opinion leaders merely relay an otherwise intact over-the-airwaves message, it is possible to read the finding as a worrisome, top-down propaganda system that is all the more effective given the message’s disguised source—or else, in a similar but normatively reversed reading, as a sophisticated blueprint for the would-be propagandist. C. Wright Mills, in his multi-year struggle with Lazarsfeld over the interpretation of the Decatur findings, reportedly opted for the downcast interpretation. Lazarsfeld, in many occasions throughout the 1940s, presented the finding instead as an effective strategy for would-be persuaders.

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Lazarsfeld was not interested in media impact *per se*, but because he could secure funds and because a media study was, in the end, an excellent way to get at his particular “decision act” questions. He could secure funds, ironically, due to the reputational momentum he had already built up in his accidental radio specialty. His Vienna era indifference to the distinction between buying chocolate and listening to radio was, in 1940, still very much in place; either might offer the researcher a window, after all, on

193. Accounts of the fascinating Lazarsfeld-Mills encounter are, for the most part, thin—in part because Mills’s 300-plus page write-up of the Decatur data apparently does not survive. John Summers, in a recent paper, provided evidence from the Bureau archives, and from Mills’s and Lazarsfeld’s letters, that the scholars’ warring ideological readings of the Decatur data was a significant (though not exclusive) source for their eventual parting of ways. See Summers (2005), “The Decatur Study,” paper presented at Politics, Social Networks, and the History of Mass Communications Research conference, October 22, Columbia University.
the motives and influences behind individual choice. There was no underlying intellectual reason that radio and newspapers went under the research microscope. As the just-published director of the Office of Radio Research, it was, however, easier to win financial backing for a study centered on media persuasion than chocolate sales.

Nearly all of the future ORR and Bureau studies of media impact would, like the Erie research, take on the shape called for by Lazarsfeld’s decision-act interest. It was a peculiar shape: Bureau media inquiries were designed, in most cases, to test short-term persuasion campaigns’ effects on individuals' attitude or behavior. Given the initially crude psychological model that Lazarsfeld worked with, effects were conceived in terms of change—in terms, that is, of conversion. Set up in this way, the studies were almost guaranteed to produce “disappointing” findings. The failure to find media “effects” in Erie County and in the other Bureau studies was, in other words, a predictable result of narrow research design—which itself derived from a very particular question that, moreover, got posed with little concern for the broader issue of media impact on society. Lazarsfeld’s curiosity was, instead, confined to methods and decision-making, and the Bureau’s media effects research program tracked those interests. As Todd Gitlin, Kurt and Gladys Lang, and others have argued, the findings that would get summarized under the “limited effects” banner were, from the start, methodologically determined: The quantitative study of short-term, media-induced attitude or behavior change was, time and again, to produce only minimal evidence for conversion. This, in itself, proved to be an important finding indeed, especially for would-be propagandists. But the Bureau model, first deployed in Erie County, could hardly produce results adequate to the wider
and much more complex question of, for example, *long-term* media “influence.”

The inherent limitations of the Bureau-style campaign study did not, however, stop Lazarsfeld from making the broadest sort of claims for media impotence *in general*. Lazarsfeld, to be sure, framed his media effects findings in *many* ways over the decade after *The People’s Choice* appeared—and sometimes he even conceded that his media campaign studies could only support very qualified conclusions. But he also, in other instances, yielded up much more sweeping statements—and never with more confidence, nor with more lasting impact on the field, than in the first fifteen pages of *Personal Influence*. In *Personal Influence* and elsewhere, the finding that direct, broadcasted appeals only infrequently bring about observable *change* (on their own) was boldly redeployed to support a much farther-reaching assertion: that media have only limited effects. This was, of course, an unwarranted extrapolation from a truly narrow body of evidence, but the extrapolation was nevertheless made. Lazarsfeld’s truth-seeking commitments were, so to speak, otherwise occupied; in the case of media influence, his entrepreneurial cunning was left to steer his findings into various interpretive molds. The result was a decade-long framing drift that culminated in *Personal Influence*’s immodest history narration.

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Shils’s and Janowitz’s 1948 *Wehrmacht* paper was to become one of the most frequently cited in the emerging postwar literature of “communication research,” both for its

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substantive findings but also for its clear framing. Shils and Janowitz’s stress on the relative insulation of primary groups from generalized propaganda confirmed, with new and different evidence, an emerging research truism. A diverse body of wartime survey research on propaganda effectiveness—including studies on war bond publicity conducted for the Department of the Treasury, and Lazarsfeld’s own 1940 election study *The People’s Choice* (1944)—had suggested that mass mediated persuasion appeals, on their own, tend not to change behavior, unless supplemented with personal, face-to-face attempts.\textsuperscript{195} Even the Research Branch experimental studies of the Army’s “Why We Fight” films—which provided more evidence of mass mediated persuasion—had nevertheless pointed to a series of qualifications and conditions that made for more or less effectiveness.\textsuperscript{196} The many social scientists who had mobilized for propaganda service came away from their wartime experience with a basic consensus: Persuasion, especially the mass-mediated kind, is not a simple affair, but instead only works under certain conditions that should be heeded in future propaganda work.

Social scientists’ conclusions about the complexity of mass persuasion were put forward, that is, in the unambiguous context of an ongoing project to design effective


\textsuperscript{196} The studies were conducted by the Army’s Research Branch, under the direction of the young psychologist Carl Hovland, who was to continue his experimental studies of media influence at Yale until his death in 1961. The wartime studies were collected and re-analyzed as part of the Carnegie-funded American Soldier series; Hovland’s mass communication research was summarized in volume three (Hovland et al., *Experiments on Mass Communication* (Princeton: Princeton University Press, 1949)).
propaganda. This context is very often forgotten because the limited effects narrative, when it emerged in the mid-1950s, retroactively re-framed the wartime and post-war findings as proof that mass media influence is happily negligible. For the wartime scientists, however, there were no sighs of relief at the failures of propaganda—and, indeed, these partial failures prompted the researchers to search for the particular conditions under which propaganda would work. (As it happened, the search for effective propaganda would remain a core research concern for mass media scholars until the mid-1960s. From the mid-1950s on, in the context of the Cold War shift to the campaign for Third World allegiance, domestic media research summaries stressed minimal impact, while international media research, in its frontstage published work, tended to point to positive media impact on economic development, while its backstage unpublished reports remained focused on the search for effective propaganda strategy.)

The Wehrmacht article was, in line with the other wartime research, fundamentally concerned with workable propaganda design. This should hardly be surprising, since the major mission of the PWD Intelligence Section had been to assess the effectiveness of Allied propaganda. But the article’s unambiguous concern with propagandizing well still comes off as startling, mainly because the term has since recaptured its appalling connotations. As the article’s brief editorial foreword in Public


198. That the paper was clearly framed as a lesson for future propaganda efforts does not mean that Shils wasn’t also interested in the scientific understanding of social order more broadly.
Opinion Quarterly states, “This study thus provides an example of the sociological and psychological analysis which the propagandist must make if he is to obtain maximal response to his communications.” 199 From this angle, the Shils and Janowitz paper is an attempt to explain the largely indifferent reaction of German soldiers to Allied propaganda, especially when appeals were made to systems of belief or general symbols. 200 With this failure in mind, Shils and Janowitz report that on the basis of their unit’s early findings, a series of air-dropped leaflets was designed with the aim to undermine primary groups, without recourse to such secondary symbols. 201 Propaganda tailored to address these small-group bonds, the authors stress, is more effective than broader appeals.

Shils and Janowitz, in short, framed their findings in much the same way as other social scientists emerging from the war—as technical lessons for a complicated task. But the authors also narrated their conclusions, and in a way that prefigured the “powerful-to-limited-effects” storyline that would become, years later, the standard history of mass communication research. Shils and Janowitz, that is, contrasted their sober and realist findings with a naïve belief in media omnipotence that, they claim, had been widely held, even recently: “At the beginning of the second world war, many publicists and specialists in propaganda attributed almost supreme importance to psychological warfare operations,” they wrote. 202 The “legendary success” of World War I efforts and the

199. “Cohesion and Disintegration in the Wehrmacht in World War II” 280.
200. Ibid 281.
201. Ibid 314.
202. Ibid.
“tremendous expansion” of advertising and public relations in the interwar years “had convinced many people that human behavior could be extensively manipulated by mass communications.”\textsuperscript{203} They were wrong, the authors wrote. Propaganda, in fact, works only under specific conditions, and even then its impact is narrower than once thought. “The erroneous views concerning the omnipotence of propaganda,” they wrote, “must be given up and their place must be taken by much more differentiated views as to the possibilities of certain kinds of propaganda under different sets of conditions.”\textsuperscript{204}

The \textit{Wehrmacht} paper, in light of the later, mid-1950s emergence of a full-fledged “powerful-to-limited-effects” narrative, was important in a number of overlapping ways. It provided, first, one of the empirical anchors for the notion that face-to-face contact trumps more general, impersonal appeals like those issued by mass media. Though this was treated in the paper itself, and by early commentators, as a lesson for propaganda design—take advantage of personal networks, the paper instructs; tailor propaganda to address small-group solidarity—the article’s finding of relative small-group imperviousness would be \textit{repackaged} later as evidence for the thesis that media impact is fortuitously negligible. The other substantive contribution made by the \textit{Wehrmacht} article is its stress on the primary group itself. The explanations that had \textit{already} been put forward for the failures of direct propaganda tended to stress social psychological verities about cognitive consistency and selective perception; to the extent that these filtering tendencies were placed in a social context at all, they were related to socialization and to larger categories of socio-economic background and group membership on the order of

\textsuperscript{203} Ibid.\textsuperscript{204} Ibid.
“Catholic” and “urban.” Very little attention had been paid, before Shils and Janowitz’s article, to the importance of small-group ties as an ongoing buffer between persuaders and their targets.

But the paper’s more crucial contributions, arguably, weren’t empirical so much as historical and imagistic. The article’s scholarly narrative, that a mistaken belief in media omnipotence has given way to a more qualified, scientific finding, became the decisive element of Elihu Katz and Paul Lazarsfeld’s 1955 *Personal Influence* emplotment of mass communication research. Shils’s narrative influence, in this respect, came through his friend and intellectual dependent Bernard Berelson, but also directly through Lazarsfeld himself.

Berelson was an ambitious operator who managed to transform a lowly post in the University of Chicago’s library school in the late 1930s into the directorship of the Ford Foundation’s Behavioral Sciences unit by the early 1950s. He had, in 1940, joined Lazarsfeld’s Office of Radio Research at Columbia, where he co-authored the landmark election study *The People’s Choice* (1944). In 1946, he returned to the University of Chicago as the Dean of its Library School, but remained affiliated with Lazarsfeld’s research organization, by then renamed the Bureau of Applied Social Research. He directed the second major Bureau election study, of the 1948 presidential race as it played out in Elmira, New York, published in 1954 as *Voting*, with Berelson as first author.206

205. See, for example: “There is a familiar adage in America folklore to the effect that a person is only what he thinks he is, an adage which reflects the typically America notion of unlimited opportunity, the tendency toward self-betterment, etc. Now we find that the reverse of the adage is true; a person thinks, politically, as he is, socially. Social characteristics determine political preference” (*The People’s Choice* 27).

The Shils-Berelson relationship was an important one for many different reasons. Here, it is enough to point to Shils’s, and the Wehrmacht article’s, influence on a crucial paper that Berelson wrote later the same year, “Communications as Public Opinion.” In the paper, Berelson attempted to summarize the extant research findings on the effects of mass media. In line with most writing on this issue during and after the war, Berelson explicitly framed his contribution in terms of the search for effective propaganda—and, in this instance, with code-worded allusions to the heating-up Cold War:

Of the importance of this topic it is hardly necessary to speak. If the defenses of peace and prosperity, not to mention other desirable political conditions, are to be constructed in men’s minds, then the critical position of communications and public opinion for that defense is evident.

The paper, published in 1948 and reprinted in the field’s first two, widely influential readers in 1949 and 1950, became known and often-cited for its pithy statement of qualified effect: “Some kinds of communication on some kinds of issues, brought to the attention of some kinds of people under some kinds of conditions, have some kinds of effects.”

Like the Shils and Janowitz paper, Berelson’s article contrasts the field’s mature, measured conclusions with a naïve interwar belief in media potency: “To speak roughly,

207. "Communications and Public Opinion," in Communications in Modern Society, ed. Wilbur Schramm (Urbana, IL: University of Illinois Press, 1948). Berelson’s paper was delivered at a 1948 conference organized by Wilbur Schramm, who would become the key figure in the institutionalization of “communication” as a quasi-discipline. The conference talks were published in a collection edited by Schramm, who, as Dean of the College of Communication at the University of Illinois, directed the University of Illinois Press.

208. Ibid 167.

209. Ibid 172. The two readers, one edited by Wilbur Schramm, and the other edited by Berelson himself along with Shils’s co-author Janowitz, were especially important given the complete absence of mass communication textbooks at the time (Mass Communications: A Book of Readings, ed. Wilbur L. Schramm (Urbana: University of Illinois Press, 1949); and Public Opinion and Communication, eds. Berelson and Morris Janowitz (Glencoe, IL: Free Press, 1950)). Berelson’s paper was included in the two additional editions of Schramm’s reader, 1960 and 1972; and included, too, in the three additional editions of Berelson and Janowitz’s reader, 1953, 1966, and 1981.
in the 1920’s propaganda was considered all-powerful—‘it got us into the war’—and thus
communication was thought to determine public opinion practically by itself.”210 In a
manner similar to Shils and Janowitz, but with a stronger claim to represent an emerging
scientific consensus, Berelson continues,

> In the 1930s the Roosevelt campaigns ‘proved’ that the newspaper had lost its influence
and that a ‘golden voice’ on the radio could sway men in almost any direction. Now, in
the 1940’s, a body of empirical research is accumulating which provides some refined
knowledge about the effect of communication on the public and promises to provide a
good deal more in the next years.211

In Berelson’s account, a mistaken and questionably scientific interwar conviction that
mass media are all-powerful has been displaced by a more sophisticated understanding of
the particular conditions that make for effective influence.

Berelson consulted both the Wehrmacht article and Shils himself as he drafted his
paper, though it is impossible to demonstrate that Berelson’s statements of qualified
effect, or his claim for the interwar belief in propaganda potency, derive exclusively from
Shils or Shils’s writings. The idea that persuasion only works under certain conditions,
after all, was a rather widely held tenet among the war-service social scientists. But
Berelson’s narrative contrast to the interwar belief in media potency does seem closely
linked to Shils’s similar, earlier statement. The Wehrmacht article was, after all, the first
published source to narrate the history in this two-stage manner; and Berelson’s was the
second. Neither version offers any evidence, citational or otherwise, for its historical
claims. Given this Shils-Berelson sequence; given the linguistic and substantive overlap
between the two narratives; given Berelson’s admitted consultation with Shils and the
Shils-Janowitz article; and given Berelson’s ongoing and demonstrable dependence on

211. Ibid.
Shils for intellectual advice—it seems likely that Berelson’s “all-powerful” historical formulation derived from Shils’s.²¹²

Berelson’s few lines of history ended up as the only cited source for the repeated claim, in Katz and Lazarsfeld’s *Personal Influence*, that interwar research had fixated on media omnipotence. Needless to say, this was a crucial borrowing: Katz and Lazarsfeld’s characterization of interwar media research is a paraphrase, with credit, of the passage in Berelson’s 1948 paper:

First the newspaper, and later the radio, were feared as powerful weapons able to rubber-stamp ideas upon the minds of defenseless readers and listeners. In the 1920’s, it was widely held that the newspapers and their propaganda ‘got us into the war,’ while in the 1930’s, many saw in the Roosevelt campaign ‘proof’ that a ‘golden voice’ on the radio could sway men in any direction.²¹³

Recall that Berelson’s nearly identical claims offered no source or citation—nor did, in keeping with Shils’s usual practice, the account in the *Wehrmacht* paper. The interwar “powerful effects” portrayal, for all its longevity and mnemonic traction, was built atop a solid-seeming but really quite hallow foundation.

Shils would later provide Katz and Lazarsfeld, in his 1951 account of the “rediscovery” of the primary group, with another foundation stone that was, like the “powerful effects” characterization, quite porous. This was the idea of an underlying contrast between two pictures of society, one that mistakenly stressed anonymous and isolated masses, superseded by another which correctly perceived the endurance of

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²¹² A piece of possible counter-evidence is that Shils, in his statement, refers especially to the social scientists in the early days of the war—those who would go on to design the war’s psychological warfare efforts—while Berelson’s vague, passive formulation could imply a scholarly or more public set of beliefs, or both. Shils does, however, refer to the social scientists’ early-war belief as an extension of the wider post-World War I faith in propaganda potency in such a way that Berelson’s statement might be read as a sloppy restatement.

²¹³ *Personal Influence* 16. The footnote, which is appended to the passage above, is to the 1950 reprint of Berelson’s 1948 article (in *Public Opinion and Communication* 51).
meaningful small-group ties. This was, in a sense, Shils’s own intellectual trajectory, but in the 1951 piece he generalized the account. Katz and Lazarsfeld, in *Personal Influence*, would go on to fuse Shils’s image-of-society contrast with the powerful-to-limited-effects progression, by ascribing the anonymous-masses picture to the interwar media analysts, and the small-group-vitalism to post-war social scientists like themselves. Shils’s *Wehrmacht* paper already sets up this contrast in social imagery, albeit only in passing and without the clear narrative signposts that appear in his 1951 article.

Even before the straightforward 1951 narration, however, Lazarsfeld recognized in the *Wehrmacht* paper—and in Shils’s *Present State of American Sociology* (1948)—a framework with which to make sense of the Bureau’s findings, in *The People’s Choice* and elsewhere, that interpersonal influences appear more effective than mass mediated appeals. The Shils and Janowitz article made enough of an impression to feature prominently in a little-known 1948 lecture, “What is Sociology?,” which Lazarsfeld delivered in Oslo during a semester-long visit.\(^{214}\) His remarks on sociology, in that postwar moment when he had finally warmed to the disciplinary label, are in themselves unremarkable. But the Oslo lecture is a crucial record of Shils’s early and pivotal role in Lazarsfeld’s embrace of the small group as an interpretive frame.

The lecture is full of postwar breathlessness; the field, Lazarsfeld proclaims, is in “transition from social philosophy to empirical sociology.”\(^ {215}\) Some of Lazarsfeld’s nomothetic language—the references, for example, to a “system of general laws”—bears

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\(^{214}\) The lecture was published in Norway in pamphlet form (*What is Sociology?* (Oslo: Skrivemaskinstua Universitets Studentkontor, 1948)). It was later reprinted in a Norwegian sociology journal ("What is Sociology?," *Sosiologi i dag* 20(4) (1990)). My page references are to the 1990 reprint.

\(^{215}\) Ibid 14.
the textual imprint of Shils’s *Present State*. But it is the Oslo lecture’s focus on primary groups—the *Wehrmacht* findings are brought together with the Hawthorne studies and Lazarsfeld’s own *People’s Choice*—that is unmistakably indebted to Shils.

Shils, in *The Present State*, had included a short version of the small-group-reemergence thesis that would, in the 1951 “Study of the Primary Group,” take center stage. Under the heading “The Small Group,” Shils had, in 1948, identified a “new focus of interest on the small group in American empirical sociology.” As in 1951, Shils described a “rediscovery” of a native sociological tradition rooted in Charles Horton Cooley and a few early Chicago School monographs. With the same narrative arc that he would deploy in 1951, Shils described a regrettable abandonment of the “primary group” theme in the interwar years: “... the whole trend of American sociology, despite its fruitful point of origin, moved away from a preoccupation with this subject, as in the development of urban sociology and community studies, spatial patterns and the larger neighborhood and urban community came to the foreground of attention.”

Shils, as evidence for the resurgence, points to the Hawthorne studies of Mayo and Whitehead, and their accidental discovery that “productivity was to a large extent the

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216. Ibid 19.
218. Ibid.
219. Ibid 41. Shils continues: “This original and indeed central problem of sociology has come once more into the very center of attention, not through deliberate, conscious pursuit of the phenomena, but as an incident in the failure of other methods of study to provide answers for hypotheses regarding variables which were originally thought to have little connection with small (primary) group membership. In none of the following instances has the rediscovery of the problem been stimulated by direct descent from its earlier formulation in the work of Cooley—indeed the history of the study of primary groups in American sociology is a supreme instance of the discontinuities of the development of this discipline: a problem is stressed by one who is an acknowledged founder of the discipline, the problem is left unstudied, then, some years later, it is taken up with enthusiasm as if no one had ever thought of it before” (42).
function of morale, which in turn was the function of small group solidarity.”  

Crucially, he also identifies Lazarsfeld’s *The People’s Choice* as another “instance of the unexpected re-emergence of this problem.”

This retroactive drafting by Shils is of profound significance, given that *The People’s Choice* itself is silent on the topic of “small” or “primary” groups; the book’s claims for voter intransigence, after all, are grounded in, and explained through, category-wide demographic variables like “rural” and “Protestant.”

The Oslo lecture is plainly dependent on Shils’s *Present State* and the *Wehrmacht* study, though neither is explicitly credited. “Sociologists,” Lazarsfeld observes in the lecture, “have developed the notion of a primary group”:

> By this they want to indicate that most people are not affected in their daily lives by the larger community in which they live. They are so to say embedded in very small groups, their families and a few friends and coworkers. The affection they can get from these small primary groups, the influence which is exercised by these few people, and the reactions which help them adjust to this little world of their own, is what explains a great part of their behavior.

Lazarsfeld proceeds to illustrate this “notion” with three examples which, though unnamed, are clearly the Hawthorne experiments, the *Wehrmacht* study, and *The People’s Choice*, respectively.

In language that closely tracks Shils’s own synopsis of the Hawthorne experiments, Lazarsfeld refers to “studies … on how to increase the productivity of workers.” After glossing, like Shils, researchers’ expectation that external

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220. Ibid.
221. Ibid.
222. Indeed, the phrases “small group” and “primary group” do not appear in the text.
224. Ibid.
factors like lighting would boost morale, Lazarsfeld observes that “[a]ll these technical factors are of some importance but much more important turned out to be the personal relations the workers had with each other and with their foreman.”

The “same importance of the primary groups was found,” Lazarsfeld continues, “when social scientists investigated why the Germany army in the last war fought so well at a time when no one could doubt any more even in Germany that the Germans had to loose [sic] the war.” With clear reference to Shils’s and Janowitz’s *Wehrmacht* article, Lazarsfeld adds:

> The soldiers were not much affected by the general political situation of Germany, and not even by general military events in which the whole army was involved. As long as the small primary groups, of which the German army was carefully built up, kept intact, the moral [sic] of the German soldiers was not destroyed.

Lazarsfeld’s own *People’s Choice*—though again without direct reference—rounds out the social scientific triptych. “Sociologists,” he observes, “turned toward making empirical studies of politics and actual behavior of voters,” and found that the parties have a “great stability.” In an unannounced departure from the analysis in *The People’s Choice* itself, Lazarsfeld claims here that political opinion is “something which develops in small primary groups”: “A person is surrounded by other people who look at public affairs just as he does himself.”

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225. Ibid.
226. Ibid.
227. Ibid.
228. Ibid.
229. Ibid.
Shils’s artful reframing of *The People’s Choice* has, in the Oslo lecture, become Lazarsfeld’s reframing. And the *Personal Influence* storyline to come, in which barriers to propaganda get repackaged as safe-for-democracy proof that media impact is *thankfully* minimal, has plain roots in Lazarsfeld’s 1948 embrace of Shils’s primary group arguments. Shils himself recognized Lazarsfeld’s linked borrowings in 1948 and 1955: “I was heartened … by the invocation by Professor Paul Lazarsfeld in a lecture which he delivered in Oslo in 1948 very shortly after our paper appeared,” he wrote in a 1975 memoir, “and then a few years later by his adoption of this hypothesis in his and Professor Elihu Katz’s *Personal Influence.*”

The empirical findings of the *Wehrmacht* paper were, no doubt, important in themselves. But it was Shils’s confident *framing*, in terms of social theoretic imagery and the history of research there and in his 1951 account, that gave early form to the storyline that would emerge as the standard history of mass communication research years later.

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In the years to follow the publication of Personal Influence, Shils provided a second, complementary contribution to the remembered history of mass communication research. He played the fundamental role in naming, defining, and propagating the “theory of mass society,” an ostensible body of thought united by the failure to recognize the relative thickness of fellow feeling, custom, and the voluntary association. This was, of course, the same charge leveled at 19th century sociology already in his 1951 account of the small group, but he had, by the late 1950s, expanded his indictment. In essence, he took the term “mass society”—which, thanks largely to emigré scholars, had entered the sociological and public intellectual lexicon by the early 1940s—and bundled any and all adherents to the downcast Gesellschaft picture under the “mass society theory” refrain. It was a big crooked canopy that he erected: French post-Revolution conservative thought, German Romanticism and its sociological descendants, the French social psychology of the crowd, the Arnold-Leavis culture and society tradition, certain twentieth-century aristocratic laments over encroaching masses, and anti-fascist emigré intellectuals desperate to uncover the conditions that gave rise to Nazism. The other thing he did was to explicitly, and repeatedly, link this adherence to the “mass society” view with the

1. Daniel Bell’s own role in the formation of the “mass society theory” was important and complementary.
critique of mass culture put forward by many anti-Stalinist and liberal intellectuals in the 1950s. The two complaints, the one about an atomized population and the other an indictment of its culture, were to Shils cut from the same cloth and mutually reinforcing. Both stances exemplify the reckless, corrosive worldview of the Cassandra intellectual—the principal target of Shils’s sustained scorn and enmity throughout his scholarly life.

Both interventions—the early, more focused narrative contrast between Gesellschaft and Gemeinschaft persistence, as well as the later “mass society theory” construal—had a profound impact on the way that the history of mass media research got remembered. This is true despite the tendentiousness of both accounts. As intellectual history, Shils’s construal of nineteenth century European sociology and his “mass society” ascription are both caricatural.

Shils had a much firmer grasp of the intellectual currents in question. The “mass society theory” label was for him, though, a polemical bludgeon to whack at contemporary disputants—erected in straw because the house of Marx had already, or so it seemed then, crumbled. He was attacking, under the guise of history, those who dissented from the mid-1950s commonplace that America (and other Western liberal societies) were fundamentally plural and healthy. He did this by assigning opponents like C. Wright Mills a disreputable heritage. Mills had famously branded the upbeat prognosis of Daniel Bell, Seymour Martin Lipset and other mid-century intellectuals as “the American Celebration.” Though Shils came to a like-minded pluralist view through his own, distinct intellectual trajectory, and indeed because of his distinct coordinates, he was an especially bilious and vehement polemicist against the gloomy picture put
forward by critics like Mills.

Shils’s neatly labeled, synthetic histories provided to communication research a readymade scaffolding for the story of its own past. The “mass society” construct, linked as it was to the critique of mass culture, provided the perfect historical foil for the emerging field. In a sense, the wartime and postwar “discovery” of the obstinate audience was a body of evidence looking for a narrative context—which it found in the image of an interwar faith in media potency. Postwar communication research, caught up in (and contributory to) the celebratory mood of 1950s social science, leaned very heavily indeed upon the “mass society” construal to make sense of its past and present.

By the time the “limited effects” storyline had fully hardened into standard textbook boilerplate—by the mid-1960s—the idea of a coherent body of thought called the “mass society theory” had already passed into authorless doxa. The conditions of its creation were long forgotten, and it became the foundation for the capsule history in nearly every mass communication textbook to be published—and there were many—in the decades to come. Like “limited effects,” it became a casually referenced slogan, propagated in a rapidly expanding 1960s and 1970s field that had, after all, little else to bind it together. The field’s resolutely quantitative and present-minded orientation in these years—itself a product, in part, of a history that so unblushingly chronicled the triumph of measured empiricism over adolescent conjecture—helped secure the uncritical diffusion of the label, until it had earned a self-validating ubiquity.

One of the ironies of the remembered history of mass communication research is that, when a critical rejoinder to the “dominant paradigm” finally emerged in the 1970s,
the rebels largely adopted the standard history of the field, “limited effects,” “mass society” and all. So too with the nascent 1970s British sociology and cultural studies of media, which constructed its identity against the “American effects tradition.”

3. The key diffusion figure here was James Halloran of the University of Leicester, whose 1964 *Effects of Mass Communication* and other writings in the late 1960s and early 1970s framed the American sociology of media for the two major traditions of 1970s British media research, political economy and cultural studies (*The Effects of Mass Communication* (Leicester: Leicester University Press, 1964)).
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