

# Fifteen Pages That Shook the Field: *Personal Influence*, Edward Shils, and the Remembered History of Mass Communication Research

By  
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*Personal Influence*'s fifteen-page account of the development of mass communication research has had more influence on the field's historical self-understanding than anything published before or since. According to Elihu Katz and Paul Lazarsfeld's well-written, two-stage narrative, 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 “powerful-to-limited-effects” storyline remains textbook boilerplate and literature review dogma fifty years later. This article traces the emergence of the *Personal Influence* synopsis, with special attention to (1) Lazarsfeld's audience-dependent framing of key media research findings and (2) the surprisingly prominent role of Edward Shils in supplying key elements of the narrative.

*Keywords:* Paul Lazarsfeld; Edward Shils; Elihu Katz; media research; history of social science; disciplinary memory

The bullet model or hypodermic model posits powerful, direct effects of the mass media. . . . Survey studies of social influence conducted in the late 1940s presented a very different model from that of a hypodermic needle in which a multistep flow of media effects was evident. That is, most people receive much of their information and are influenced by media secondhand, through the personal influence of opinion leaders (Katz and Lazarsfeld 1955).

—Joseph Straubhaar and  
Robert LaRose (2006, 403)

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NOTE: Thanks to Peter Simonson for extensive and insightful suggestions.

DOI: 10.1177/0002716206292460

The main thread of American mass communication research holds that the scholars who surrounded 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 prewar 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<sup>1</sup>—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” (pp. 15-17). Both approaches—the fearful *and* the ebullient—described the media message as a “direct and powerful stimulus” (p. 16). Swept up by popular alarm or blinded by utopian rhetoric, both groups 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. Katz and Lazarsfeld conclude their fifteen pages with a review of the new research—much of it generated by Lazarsfeld’s Bureau of Applied Social Research—whose “greater precision” has generated “increasing skepticism about the potency of the mass media” (p. 24).

In one short chapter, the field’s untidy past was neatened. 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.<sup>2</sup> 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 clichés 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.<sup>3</sup> 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.

Katz and Lazarsfeld’s narration is all the more striking for its departure from the many *other* ways that Lazarsfeld, before *Personal Influence*, had presented the same bundle of findings. Over the fifteen years or so that he conducted media research—culminating in *Personal Influence*—Lazarsfeld had 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. He took a

set of findings that were, initially, disappointing in scientific terms and *repackaged* 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 composed of socially concerned liberal intellectuals and another when the audience featured fellow media academics. Lazarsfeld was a masterful *packager*, chameleon-like in his audience adaptability. His last word was *Personal Influence*, and it is the only one we remember.

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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 its 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 effect” storyline *before* Shils’s unwitting intervention, these were scattered and inconsistent, without narrative tightness. As early as 1942, Lazarsfeld and others started to note the difficulty of bringing about attitude change through media persuasion (Lazarsfeld 1942; see also Lazarsfeld and Merton [1943] and Merton and Kendall [1944]). But this observation—repeated often over the next ten years—was not 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.<sup>4</sup> This two-track, schizophrenic framing—a concern, on one hand, for finding out how to make persuasion *work*, and on the other, an effort to draw 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 (Klapper 1949).<sup>5</sup> There is, in Klapper’s summary, a palpable tone of relief that anticipates the upbeat, celebratory cadence of the full-fledged “limited effects” storyline.<sup>6</sup> But there is 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 *Personal Influence's* powerful-to-limited-effects account (Shils 1948, 40-52; Shils 1951). Drawing on his authority as a major exegist of European sociology, Shils attributed the temporary small-group research falloff 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, developed a wrong-headed picture of the transition to modernity (p. 44). 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*: “As 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” (ibid., 47). 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 (ibid., 59).

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Katz and Lazarsfeld, in *Personal Influence*, embraced Shils’s fall-and-rise account with alacrity.<sup>7</sup> 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 these contrasting pictures of modernity. Those with the superceded belief in media potency clung to that image of breakdown and anonymity of “the late 19th century European schools” (p. 17). 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” (p. 16).

Katz and Lazarsfeld acknowledge their debt to Shils’s “excellent” essay early and often.<sup>8</sup> 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.<sup>9</sup> And with this grounding, Katz and Lazarsfeld had elegantly narrated the history of the field, retroactively drafting Lazarsfeld's body of communication research to 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.

Shils's own personal influence derived from his unusual, border-spanning perch. He had become an empirical social scientist with a penetrating knowledge of social thought in a social scientific milieu in which such knowledge was scarce.<sup>10</sup> He was already, in 1951, 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 social thought, and the other in the world of postwar 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 (first 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 (Shils and Janowitz 1948; Shils 1948, 1951).<sup>11</sup> 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. (In these same debates, Daniel Bell [1956, 1960] and Shils [1957a, 1961, 1962] introduced the complementary "mass society theory" pejorative, which later merged with the powerful-to-limited-effects story, in, for example, Bauer and Bauer [1960]; Bramson [1961]; Gans [1966], and, above all, DeFleur [1966].)<sup>12</sup> 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.

There was nothing smooth or linear about the chain of distinct purposes and contexts that led, eventually, to a standard "disciplinary" history for mass communication research. In this case at least, the memory got formed, altered, adopted, modified, and readopted in disjointed succession. What was for Shils an intellectual coming to terms was for Katz and Lazarsfeld an artful repackaging; 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 2006—this was an accident.

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Shils's 1948 *Wehrmacht* paper, written with Morris Janowitz, was to become one of the most frequently cited in the emerging postwar literature of "communication

research,” both for its 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* (Lazarsfeld, Berelson, and Gaudet 1944)—had suggested that mass-mediated persuasion appeals, on their own, tend not to change behavior, unless supplemented with personal, face-to-face attempts.<sup>13</sup> Even the well-known experimental studies of the “Why We Fight” films (Hovland, Lumsdaine, and Sheffield 1949), conducted by the Army’s Research Branch—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.<sup>14</sup> 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 propaganda.

This context is very often forgotten because the limited effects narrative, when it emerged in the mid-1950s, retroactively reframed the wartime and postwar findings as proof that mass media influence is happily inconsequential. 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. However, international media research, in its front-stage 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.<sup>15</sup>)

Shils’s *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 Shils and Janowitz’s military unit—the Psychological Warfare Division’s 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 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” (Shils and Janowitz 1948, 280). 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 (*ibid.*, 281).

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 were designed with the aim to undermine primary groups, without recourse to such secondary symbols (*ibid.*, 314). 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 (*ibid.*, 314). The “legendary success” of World War I efforts and the “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” (*ibid.*, 314). They were wrong, the authors wrote. Propaganda, in fact, only works 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” (*ibid.*, 314).

The *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 repackaged later as evidence for the thesis that media impact is fortuitously negligible. The other substantive contribution made by the *Wehrmacht* article is its stress on the primary group itself. The explanations that had 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, as in *The People’s Choice*, they were related to socialization and to larger categories of socioeconomic background and group membership on the order of “Catholic” and “urban.”<sup>16</sup> 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, were not 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, prefigured the decisive element of the *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 coauthored *The People's Choice*. In 1946, he returned to the University of Chicago as the dean of its Library School, but he 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* (Berelson, Lazarsfeld, and McPhee 1954), with Berelson as first author.

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" (Berelson 1948). In the paper, Berelson attempted to summarize the extant research findings on the effects of mass media.<sup>17</sup> In line with most writing on this issue during and after the war, Berelson (1948) 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. (P. 167)

The paper, published in 1948 and reprinted in the field's first two, widely influential readers (Schramm 1949; Berelson and Janowitz 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*" (p. 172).<sup>18</sup>

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, 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" (Berelson 1948, 171). 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. (Ibid., 171)

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.<sup>19</sup> 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 Shils for intellectual advice, it seems likely that Berelson's "all-powerful" historical formulation derived from Shils's.<sup>20</sup>

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. (Katz and Lazarsfeld 1955, 16).<sup>21</sup>

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 that correctly perceived the endurance of meaningful small-group ties. 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 postwar 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, 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 (Lazarsfeld 1948c).<sup>22</sup> 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.

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The lecture is full of postwar breathlessness; the field, Lazarsfeld proclaims, is in "transition from social philosophy to empirical sociology" (Lazarsfeld 1948/1990, 14). Some of Lazarsfeld's nomothetic language—the references, for example, to a "system of general laws"—bears the textual imprint of Shils's *Present State* (ibid., 19). 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" (p. 40). As in 1951, Shils described a "rediscovery" of a native sociological tradition rooted in Charles Horton Cooley and a few early Chicago School monographs (p. 40). 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” (p. 41).<sup>23</sup>

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 function of morale, which in turn was the function of small group solidarity” (ibid., 41). Crucially, he also identifies Lazarsfeld’s *The People’s Choice* as another “instance of the unexpected re-emergence of this problem” (ibid., 41). This retroactive designation by Shils is of profound significance, given that *The People’s Choice* itself is silent on the topic of “small” or “primary” groups;<sup>24</sup> 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 (1948/1990) 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. (P. 23)

Lazarsfeld proceeds to illustrate this “notion” with three examples that, though unnamed, are clearly the Hawthorne experiments, the *Wehrmacht* study, and *The People’s Choice*, respectively (p. 23).

In language that closely tracks Shils’s own synopsis of the Hawthorne experiments, Lazarsfeld (1948/1990) refers to “studies . . . on how to increase the productivity of workers.” After glossing, like Shils, researchers’ expectation that external factors like lighting would boost morale, Lazarsfeld observes that “all 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” (p. 23).

The “same importance of the primary groups was found,” Lazarsfeld (1948/1990) continues, “when social scientists investigated why the German 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” (p. 23). 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. (P. 23)

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” (p. 23). 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” (p. 23).

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Shils's artful reframing of *The People's Choice* has, in the Oslo lecture, become Lazarsfeld's reframing.<sup>25</sup> 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*” (Shils 1975, xxv).

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Lazarsfeld, in his and Katz's *Personal Influence* narrative, characterized his own body of media research as the progressive unfolding of a counterintuitive 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 (Gitlin 1978). 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 (Glander 2000; Simpson 1994). 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 (Katz 2001; Simonson and Weimann 2003).

None of them is wrong. Lazarsfeld's published research on mass media was all of these—and more still. Indeed, to read through his 1940s media scholarship is to invite disorientation. This is not on account of the research findings themselves, which were relatively stable over time. 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 (Lazarsfeld, Berelson, and Gaudet 1944).<sup>26</sup> 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.<sup>27</sup>

Thus Lazarsfeld, with Merton as coauthor, 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 antiracist campaigners, in Lazarsfeld's 1947 “Some Remarks on the Role of Mass Media in So-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

(Lazarsfeld 1942). 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, and the framing was highly particularized. In papers directed at broader academic publics—including the 1948 essays “Communication Research and the Social Psychologist” (Lazarsfeld 1948a) and “Mass Communication, Popular Taste and Organized Social Action” (Lazarsfeld and Merton 1948)—the stress on limited effects coexisted, albeit awkwardly, with the propaganda advice. 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.

It is important to stress that nearly all of Lazarsfeld's studies of media impact would, like the original *People's Choice* research, assume a shape called for by Lazarsfeld's interest in the psychology of the decision act. 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, as many others have observed, confined to methodology and decision making, and the Bureau's media effects research program tracked those interests. As Todd Gitlin (1978), Kurt and Gladys Lang (1996, 2006 [this volume]), 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.”

Occasionally, Lazarsfeld conceded that his media campaign studies could only support very qualified conclusions (Lazarsfeld 1948a, 1949). But the inherent limitations of the Bureau-style campaign study did not, however, stop him from making sweeping (and contradictory) claims about media impact elsewhere—and never with more confidence, nor with more lasting impact on the field, than in

the first fifteen pages of *Personal Influence*. There 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. 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.

Over Lazarsfeld's media-research career, the contrasts in framing were dramatic, especially in evaluative terms. It is possible, however, 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 audiences' preexisting 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 overreaction that 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.<sup>28</sup>

If, employing the first frame, Lazarsfeld counseled the propagandist in her art, in the second frame, he reassured 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 is portrayed as a *strategic complement* to media-based propaganda campaigns. In the limited effects frame, by contrast, face-to-face persuasion and self-selection are re-deployed as *obstacles* to successful media appeals.

If this is Jekyll and Hyde, it is in keeping with Lazarsfeld's penchant for rhetorical expediency. His legendary resourcefulness revealed itself in any number of *other* ways—in his contract-to-scholarship alchemy, in his reanalytic 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, 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—reveals that 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. The book, unlike some of his other published reflections, was addressed to his scholarly peers, more of whom than ever were sociologists. The fall and rise of the small group account that Shils supplied was in many respects a perfect sense-making device. Some of *The People's Choice* findings that 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. And 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 place the narrative in terms of scientific progress. And “the *Gemeinschaft* after all” framing provided a legitimating link to the discourse of learned social theory.

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*In the few years after Personal Influence, the  
“power-to-limited-effects” narrative was solidly  
established as the remembered history  
of mass communication research.*

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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. They 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<sup>29</sup>—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.<sup>30</sup> 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-functional "gratifications" studies—those by Herta Herzog (1940, 1944a, 1944b), Rudolf Arnheim (1944), and Berelson (1949) especially—were named predecessors to the "phenomenalist" functionalism called for by the Bureau's Charles Wright, Joseph Klapper, and, under the later "uses and gratifications" label, Elihu Katz. (The late 1950s, early 1960s call by Wright [1959, 1960] and Klapper [1963] 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 that, especially from the 1970s on, has gained a large interdisciplinary audience. Each of these "survivors," for some of the reasons cited above, were included in early "readers"—which played an especially crucial role in establishing their mnemonic resilience.<sup>31</sup>

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 (1946) brilliant Kate Smith war bond study, has barely limped along in semiobscurity.<sup>32</sup> The "limited effects" narrative, of course, cannot account for all of this forgetting; there are many other factors 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 Elihu Katz and Paul Lazarsfeld set out to establish a sense-making basis for mass communication research, they might have borrowed from any number of available storylines that surfaced over the decade that *Personal Influence* gestated.<sup>33</sup> Or they might have framed the Decatur findings using any one of the several illustrative scaffolds that Lazarsfeld 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 one of Lazarsfeld's fundamental academic stimuli. 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 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 field of *public opinion research*. The cluster public opinion research 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 extradisciplinary and interdisciplinary, *Gemeinschaft*-like in its personal networks; tethered to foundations, 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.<sup>34</sup> 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.<sup>35</sup> Lazarsfeld's own deep immersion in media research almost perfectly 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 that had *preceded* his own. This ignorance, of course, was a fundamental precondition 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 an analytic contrast that, in the Lazarsfeld-Shils case at least, helps to clarify the mnemonic transfer that took place—the 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.) 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. There were many 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 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.

Lazarsfeld’s context-dependent credit-seeking helps explain not just his research program in mass communication but also his and Katz’s particular *Personal Influence* shaping 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, as we have seen, on the main *audience* that any particular study was intended for. He used, in particular, book-length studies like *The People’s Choice* 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.<sup>36</sup> 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 aid. It provided a ready-made narrative contrast that helped, moreover, to reframe 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 would go on to supply 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*.<sup>37</sup>

The story has such staying power *because* it is a great story, containing dramatic clarity, the frisson of breakthrough, vivid (and violent) metaphors like “magic bullets” and “hypodermic needles”—the elements, in short, that make for riveting narrative. It has also proven *plastic*, flexible enough to bend without losing its form. And it has 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 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.

## Notes

1. The question of *Personal Influence* authorship is a complicated one. The original data were collected in 1945 and proceeded, over the next decade, to pass through the hands of a number of Bureau associates and students. Notoriously, C. Wright Mills was one of the first. Accounts of the fascinating Lazarsfeld-Mills encounter are somewhat thin—in part because Mills's three-hundred-plus-page write-up of the Decatur data apparently does not survive. John Summers (2006 [volume]), surveys extensive evidence from the Bureau archives, and from Mills's and Lazarsfeld's letters, to piece together the most exhaustive picture yet. But Mills was not the only Bureau figure to attempt an analysis of the Decatur data. David Gleicher, Peter Rossi, and Leo Srole all wrote extensive analyses, some of which appeared, in modified form, in part II of *Personal Influence* (see Katz and Lazarsfeld 1955, xiii; Katz 2005, xvii). It is, however, undisputed that Elihu Katz, then a young graduate student, drafted part I of *Personal Influence* (including the crucial “Images of the Mass Communications Process”). Katz had prepared an earlier version of what became part I as a 1953 Bureau report, commissioned by the Implementation Committee on Television (Katz 1953). (Peter Simonson brought this report to my attention.) In a series of personal communications in 2005 and 2006, Katz has acknowledged that Lazarsfeld must have known of Shils's small group account, but Katz expressed doubt that Lazarsfeld explicitly urged him to bring small group and mass communication research into mutual dialogue. Based on evidence presented here—including clear proof that Lazarsfeld, by 1948, was already reframing *The People's Choice* in small group terms, with Shils as his guide—I have elected to treat the crucial *Personal Influence* historical narrative as jointly authored.

2. The story of the “powerful-to-limited-effects” narrative, as it became firmly established in the field's memory, is complex and deserves much greater study. Here I can only point to a few crucial early adoptions.

Elihu Katz himself repeated the story in a number of follow-up publications, including Katz (1957, 1960) and Katz and Foulkes (1962). Lazarsfeld himself reaffirmed the narrative in, for example, Lazarsfeld and Menzel (1963). Bureau student Charles Wright, in his 1959 textbook (one of the first), devoted a full chapter to the “limited effects” storyline (p. 50). Joseph Klapper, another Bureau student, is also an important figure here; his influential 1960 literature review is a book-length defense of the “limited effects” finding (p. 5). Leon Bramson (1961) (as had Bauer and Bauer [1960] at around the same time) merged the “limited effects” story with explicit reference to the now-stock “mass society theory” (pp. 96, 100). The crucial adoption of the narrative, however, was DeFleur (1966); this early textbook was deeply influential, and is frequently *the* cited source for the “limited effects” storyline (pp. 101, 121).

3. Though Katz and Lazarsfeld do not refer to “magic bullets” or “hypodermic needles” in *Personal Influence* itself, the “hypodermic” image *does* appear in a 1953 Bureau report written by Katz—a report, moreover, that formed the basis for part I of *Personal Influence*. On the 1953 Katz report, see note 1. Lazarsfeld’s onetime colleague at the Bureau, Bernard Berelson, also used the “hypodermic” term in his 1954 *Voting* study (Berelson, Lazarsfeld, and McPhee 1954, 233). Lazarsfeld, of course, is the study’s second author. Deb Lubken, whose (2005) unpublished paper on the adoption of the “hypodermic needle” is masterful, brought my attention to the *Voting* passage with the “hypodermic” reference. Berelson, the student of library information turned Lazarsfeld associate turned Ford Foundation social science rainmaker, is a fascinating but neglected figure in the history of social science research

4. Two classics of media research from the immediate postwar years—Merton’s *Mass Persuasion* (1946) and Lazarsfeld and Merton’s “Mass Communication, Popular Taste and Organized Social Action” (1948)—display this tension in acute form.

5. In a fascinating foreword, however, Lazarsfeld (1949) 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 preexisting beliefs and norms is the main, and salutary, effect of mass media (Klapper 1960). His 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.

6. Klapper (1949, I-1, I-23, I-29): “the research of several months suggests that allegations of public taste being thus lowered are far more prevalent among casual observers than among the more formal social scientists. The latter, who are not numerous, treat the subject with extreme caution. . . . It seems no exaggeration to say that by and large people like what they read or hear because they read or listen to what they know they will like. . . . A host of related objective studies and careful conjectures indicate that mass media can and occasionally do contribute to development of better taste in individual members of the audience.”

7. Katz and Lazarsfeld (1955, 33): “We might say, perhaps, that as a result of investigating and thinking about the opinion leader, mass communications research has now joined those fields of social research which, in the last years, have been ‘rediscovering’ the primary group.” The footnote to the passage relates that this “rediscovery” is “an accepted term by now, referring to the belated recognition that researchers in many fields have given to the importance of informal, interpersonal relations within situations formerly conceptualized as strictly formal and atomistic. It is ‘rediscovery’ in the sense that the primary group was dealt with so explicitly (though descriptively and apart from an institutional context) in the work of pioneering American sociologists social psychologists and then was systemically overlooked by empirical social research until its several dramatic ‘rediscoveries’. . . . For an account, see Shils 1951.” There was, too, a local source for Katz and Lazarsfeld’s “rediscovery of the primary group” narrative: Robert K. Merton. In the same footnote (p. 33), Katz and Lazarsfeld quote the phrase from Merton’s (1948/1949) “Manifest and Latent Functions” essay. Merton’s reference, however, is merely a passing one—a footnote on page 114, along with a brief discussion of the Hawthorne studies on pages 120-21. Indeed, the second reference is anchored by a long quote from Shils’s (1948) *Present State of American Sociology*, referring to the interwar dropoff in small group research (p. 121). It is possible, as Peter Simonson suggested to me, that Merton expounded on the “rediscovery of the primary group” in lecture—in what Merton referred to as an “oral publication.” On Merton and the importance of “oral publication,” see Simonson (2005).

8. For example, “Several sections of this and the following chapter will draw extensively on Shils’ excellent essay” (Katz and Lazarsfeld 1955, 17). See also *ibid.*, 28, 33, 37, and 45. Shils, Katz and Lazarsfeld write

in another note, “in his excellent discussion of the antecedents, as well as the contemporary flowering, of small group research” (ibid., 45).

9. Katz and Lazarsfeld (1955, 17). Shils is extensively credited in the footnote: “In ‘The Study of the Primary Group,’ Shils (1951) discusses this main trend in 19th century European sociology which was reflected in the notion that ‘any persistence of traditionally regulated informal and intimate relations was . . . an archaism inherited from older rural society or from a small town handcraft society.’ Discussing early American sociology, Shils indicates that there was a comparatively greater interest in the primary group as a subject for study. He points out, however, that Cooley’s well-known contribution and the interest displayed by American sociologists in voluntary associations, pressure groups, etc were counterbalanced by an emphasis on the disintegration of the primary group in urban society such as may be found in the work of W.I. Thomas, Park and his associates, and others. Several sections of this and the following chapter will draw extensively on Shils’ excellent essay.”

10. Shils has a remarkably small place in the memory of American sociology. 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. For a much more exhaustive treatment of Shils and his intellectual context, see Pooley (2006, 25-184).

11. Though Shils’s concern with the constituents of social order comes out, in the *Wehrmacht* essay (Shils and Janowitz 1948), 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 survey of American sociology, for example, explicitly recommends the “fundamental problem of consensus” as the central research agenda for the field (Shils 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 (Shils 1949, 239-40). For a much more exhaustive treatment of Shils and his intellectual context, see Pooley (2006, 25-184).

12. For a brief discussion of the post-*Personal Influence* uptake of the “powerful-to-limited-effects” storyline, see note 2.

13. The Treasury war-bond studies were conducted by Rensis Likert’s Division of Program Surveys at the Department of Agriculture, under Dorwin Cartwright’s direction. On the studies and their personal-influence findings, see Converse (1987, 174, 225) and Cartwright (1949). Merton and Lazarsfeld were consulting for Likert’s outfit at the time, and Merton’s classic study of the eighteen-hour Kate Smith radio war bond drive, *Mass Persuasion* (1946), was a piece of the Program Surveys’ Treasury studies, and Lazarsfeld’s suggestion (see Converse 1987, 174).

14. 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 reanalyzed as part of the Carnegie-funded *American Soldier* series; Hovland’s mass communication research was summarized in volume 3 (Hovland, Lumsdaine, and Sheffield 1949).

15. On secret propaganda work overseas, see, for example, Samarajiva (1987), the flawed but informative Simpson (1994), and Gilman (2003).

16. 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 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” (Lazarsfeld, Berelson, and Gaudet 1944, 27).

17. 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, held at the University of Illinois, was funded by the Rockefeller Foundation and organized around the founding of Schramm’s new Institute of Communication Research (see Chaffee and Rogers 1997, 139-40). Lazarsfeld’s (1948b) own contribution to the conference is a revealing, advice-filled meditation on the communication researcher’s “tightrope walking” between criticism of, and dependence upon, the media industry. The conference talks were published in a collection edited by Schramm (1948), who, as dean of the College of Communication at the University of Illinois, directed the University of Illinois Press.

18. The two readers, one edited by Wilbur Schramm (1949), and the other edited by Berelson himself along with Shils’s coauthor Janowitz (1950), were especially important given the complete absence of mass

communication textbooks at the time. Berelson's paper was included in the two additional editions of Schramm's reader, in 1960 and 1972; and included, too, in the three additional editions of Berelson and Janowitz's reader, in 1953, 1966, and 1981.

19. Shils, Janowitz, and Berelson had close contacts from 1946 on. The three worked together to form the University of Chicago's short-lived Committee on Communication ("Current Items" 1949, 429; on the Committee, see Wahl-Jorgensen's [2004] flawed but revealing history). And there is ample evidence that Berelson was dependent on Shils for intellectual guidance. Berelson's 1952 presidential address to the American Association of Opinion Researchers, and the last, theoretical chapter of *Voting* were written with Shils's significant help (see the acknowledgments: Berelson [1952, 313]; Berelson, Lazarsfeld, and McPhee [1954, 305]). Shils (1957b) himself referred to the help on *Voting* (p. 143). Berelson and Shils, moreover, planned to cowrite a volume on mass communication standards, for Lazarsfeld's *Foundations of Communication Research* series (see Berelson 1948/1952, 52).

20. A piece of possible counterevidence 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.

21. The footnote, which is appended to the passage above, is to the 1950 reprint of Berelson's 1948 article (in Berelson and Janowitz 1950, 51).

22. The lecture (Lazarsfeld 1948c) was published in Norway in pamphlet form. It was later reprinted in a Norwegian sociology journal (Lazarsfeld 1948/1990). My page references are to the 1990 reprint.

23. Shils (1948, 42) 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."

24. Indeed, the phrases "small group" and "primary group" do not appear in the text.

25. Indeed, Lazarsfeld, along with coeditor Merton, commissioned a lengthy Shils reanalysis of primary group data from the *American Soldier* studies for their *Studies in the Scope and Method of the American Soldier* (see Shils 1950). The Shils paper included an explicit restatement of the "rediscovery" of the primary group (Shils 1950, 16, 19).

26. Former Bureau student Peter Rossi, based on interviews with Lazarsfeld, reports that Lazarsfeld and his coauthors set aside the 1940 data for a full year after conducting the preliminary analysis, on account of their disappointment at the lack of media persuasion findings (Rossi 1959, 316).

27. For a complementary analysis of Lazarsfeld's rhetorical expediency, see John Durham Peters, "The Part Played by Gentiles in the Flow of Mass Communications" (2006 [this volume]). It is worth, also, quoting Lazarsfeld's introduction to his friend and colleague Samuel Stouffer's collected papers. There, Lazarsfeld gently reprimands 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" (quoted in Platt 1996, 32).

28. Katz himself, in a follow-up essay (1957, 61), referred to *Personal Influence's* populism: "The [two-step flow] hypothesis aroused considerable interest. The authors themselves [referring to Katz and Lazarsfeld] were intrigued by its implications for democratic society. It was a healthy sign, they felt, that people were still most successfully persuaded by give-and-take with other people and that the influence of the mass media was less automatic and less potent than had been assumed." Compare this to the altogether different tone in *The People's Choice*, in which the finding of limited media effect was recruited to the book's thesis that the democratic ideal falters in practice: "The real doubters—the open-minded voters

who make a sincere attempt to weight 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” (Lazarsfeld, Berelson, and Gaudet 1944, 100). The fact that the same bundle of evidence can be so easily put forward as reassuring evidence of media noninterference 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. For a more detailed discussion of *The People’s Choice*, see Pooley (2006, 283-94).

29. See epigraph. See also, for example, Wood (2005, 303), Wimmer and Dominick (2006, 8), Grossberg et al. (2005, 302), McQuail (2005), and Klapper (1960).

30. 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. See, for example, Schramm (1963, 1980, 1997).

31. Lazarsfeld and Merton (1948) is in Schramm (1949); Berelson (1949) and Herzog (1944b) are reprinted in Schramm (1954); Lowenthal (1944) and Herzog (1944b) reappear in Berelson and Janowitz (1950); and Arnheim (1944) and Berelson (1949) are reprinted in Daniel Katz et al. (1954).

32. Peter Simonson has, fortunately, brought out a reprint edition with an excellent introduction (Merton 1946/2004).

33. The Decatur field research for what would become *Personal Influence* was completed in 1945; the book was published in 1955. See note 1.

34. See, for example, Shils’s (1948) own postwar survey of American sociology, with its subheading: “Communications Analysis and Public Opinion” (p. 34).

35. Educational and antidiscrimination groups were also important, though secondary, funders.

36. It is true that Lazarsfeld continued to publish intermittently on mass communication issues even after *Personal Influence* (see, for example, Lazarsfeld 1961, 1963; Lazarsfeld and Menzel 1963). But the bulk of his research energies had, by *Personal Influence*’s publication in 1955, long since been redirected to other areas, especially research methodology and its academic history.

37. Gitlin’s 1978 critique of the Lazarsfeld “dominant paradigm” is an important case in point (p. 207).

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