MNEMONIC MULTIPLES: THE CASE OF THE COLUMBIA PANEL STUDIES

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This article uses the Bureau of Applied Social Research's mid-century book-length panel studies—*The People's Choice* (1944), *Voting* (1954), and *Personal Influence* (1955)—to identify and illustrate a neglected phenomenon in the remembered history of social science: mnemonic multiples. The article describes the way that the Bureau books, originally published into a post–World War II interdisciplinary social science milieu, have since come to be remembered along distinct disciplinary tracks by sociologists, political scientists, and communication researchers. A contextual analysis of references to the Bureau studies in the flagship journals of the three disciplines, from 1960 through 2011, provides tentative support for the mnemonic multiples concept. © 2014 Wiley Periodicals, Inc.

There is little doubt that *The People's Choice* (1944), the book-length panel study conducted by Paul Lazarsfeld and his colleagues at Columbia's Bureau of Applied Social Research (BASR), is a classic. The question is, a classic of what? A cursory survey of textbooks and disciplinary handbooks suggests a patterned, field-specific answer. To political scientists, *The People's Choice* is a landmark of voting research; to sociologists, a breakthrough in survey methods; and to communication scholars, the founding document of the "limited effects" paradigm of media research.

Consider a trio of examples, this one from political science:

The fiftieth anniversary of the modern era in political behavior research was celebrated (quite silently) in 1994. We mark 1944 as the birth of the modern era because in that year Paul Lazarsfeld and his colleagues from the Bureau of Applied Social Research at Columbia University published the first academically inspired study of an election that focused primarily on individual voters. (Carmines & Huckfeldt, 1998, p. 223)

From sociology:

One famous early example of survey research was called "The People's Choice?" A study carried out by Paul Lazarsfeld and colleagues more than 60 years ago. This study, which investigated the voting intentions of residents of Erie County, Ohio during the 1940 campaign for the US presidency, pioneered several of the main techniques of survey research in use to this day. (Giddens, 2009, pp. 53–54)

And from communication research:

Evidence from [Lazarsfeld's] Erie County (Ohio) panel study of the 1940 presidential election found that the persuasive effects of media on voters' decisions were quite limited *in comparison with* the unsubstantiated claims made two decades earlier that World War I propaganda controlled the masses. (McLeod, Kosicki, & McLeod, 2010, p. 185)

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The textbooks authors seem to have read very different books. These distinct readings of *The People's Choice*, together with a pair of subsequent BASR panel studies, illustrate a neglected phenomenon in the remembered history of social science: *mnemonic multiples*. In suggesting the term, I am invoking the way that once-blended fields or research areas that since split off from one another come to remember their shared history in distinct ways. Textbook authors, seminar instructors, and even disciplinary historians look back on their disciplines' pasts through the subsequent evolution of their respective fields. Twentieth-century American social science is, after all, the story of progressive differentiation. When we glance back at our fields' pasts, we normally direct our gaze along the particular fork that leads to our present discipline.

There is a common complaint that historians of the individual social sciences all too often neglect the context of neighboring fields, most egregiously in treatments of those periods (such as the late nineteenth century) when the social sciences were unevenly differentiated. The schizophrenic memory of *The People's Choice* and its companion texts suggests a parallel class of cases, in which *the same work* is remembered along patterned and partial lines.

In this exploratory study, I tracked references to the Columbia panel studies in flagship journals of the three disciplines, *American Political Science Review (APSR)*, *American Journal of Sociology (AJS)*, and the *Journal of Communication (JoC)*.

The study is intended as a proof of concept. There are undoubtedly many other cases like this one that have not, however, been identified. Historians of science and social science might use the idea of mnemonic multiples to tease out similar cases, some of which may prove consequential for the remembered histories of differentiated fields. The idea may be a particularly helpful addition to the conceptual toolkit of scholars working on the history of disciplinary memory.

My discussion begins with the *context of creation*, centered on Lazarsfeld, the mid-century Bureau, and the three books themselves. I locate framing clues in the books that would, to later readers, suggest field-specific readings in political science, sociology, and communication research. Next, I attempt to make sense of the patterns by looking at the field-specific *context of reception* in the decades after the Bureau studies were published. Following this narrative account, I report on an analysis of the three disciplines' flagship journals. I tracked references to the Columbia panel studies in the *APSR*, *AJS*, and the *JoC*. Using keyword searches of full-text journal databases, I tabulated and analyzed each reference—however fugitive—with special attention to implicit or overt historical claims.

This flagship journal analysis serves, in part, to test the mnemonic multiples case proposed in the study's narrative sections. But the journal analysis is also intended to pick up where the narrative leaves off. It is a tricky business to follow the spread—the branching reception history—of intellectual artifacts, such as the Columbia books. Traditional narrative, with its capsule summaries and carefully chosen quotations, is too susceptible to proof-texting and cherry-picking. The reference-tracking strategy outlined here, a mix of citation analysis and close, contextualized reading, is better suited to the history of intellectual reception, with its dueling requirements of scale and nuance.

With the journals taken as proxies for the fields, the analysis supports the article's central claim that the Bureau books have been remembered along parallel mnemonic tracks. The interdisciplinary milieu of the books' production gave way, in the 1960s and after, to more or less segregated trajectories in political science, sociology, and communication research. Each of the three fields refracted the Bureau books through the lens of their subsequent evolutions. They were, in effect, reading different books.

THE CONTEXT OF CREATION

Lazarsfeld—in his background and intellectual style—made it easy for future scholars to remember his work in diverse ways.[1] When he arrived in the United States in 1933 on a Rockefeller fellowship, he was a psychologist trained in applied mathematics, with a self-taught expertise in market research. It is only due to a series of interventions by Robert Lynd that we know Lazarsfeld today as a sociologist; in a final beneficent act, Lynd championed Lazarsfeld for the now-famous compromise hire that also brought Robert Merton to Columbia in 1939 (Pooley, 2006a, p. 207, pp. 210–212, p. 237). Lazarsfeld was in the 1930s a self-identified "marginal man" who took advantage of whatever applied research work he could find in the Depression-era American academy (Lazarsfeld, 1969, p. 302). At his own fledgling Newark Research Center, and soon after at the Rockefeller-funded Office of Radio Research (ORR) at Princeton, Lazarsfeld adapted client-commissioned projects to his own intellectual interests in methodology and the psychology of the decision act. He was highly resourceful, and already packaging his findings in audience-specific ways. This came out most impressively in the ORR's hastily assembled, hodgepodge collection, *Radio and the Printed Page* (Lazarsfeld, 1940a), which managed to assuage a wary Rockefeller Foundation—but only by way of a carefully written introduction that reframed the ORR's disparate studies-to-date as contributions to the Foundation's interest in educational broadcasting (Pooley, 2006a, pp. 263–270; Stamm, 2010).

On the strength of the prepublication *Radio and the Printed Page*, the Foundation in 1939 had extended the ORR's funding and agreed to the Columbia affiliation (Morrison, 1998, p. 75). Lazarsfeld, working out of the ORR's new offices in Union Square, decided to test the impact of media messages on voting decisions in the 1940 presidential contest (Rossi, 1959, p. 316). He selected Sandunsky, Ohio, for its middle-American normalcy, and sent a team to conduct a study of 600 voters over the course of the campaign, using the then-novel panel method. The results were written up as *The People's Choice*, published in 1944 (Lazarsfeld, Berelson, & Gaudet, 1944)—the same year that the ORR was renamed the BASR (Sills, 1987, p. 260).

Lazarsfeld's penchant for resourceful framing was on display here again. Though the study had been designed to test media influence, the published book was put forward as a "voting" or "election" study. To Lazarsfeld's surprise and horror, the Sandunsky fieldwork had uncovered little evidence of direct media persuasion. Only 54—"few indeed"—of the 600 panelists, for example, appeared to have changed their voting preferences as a result of the candidates' media campaigns (pp. 94, 100). The findings were a grave disappointment, and for a year they paralyzed Lazarsfeld and his coauthors (Rossi, 1959, p. 316). To rescue the project, Lazarsfeld reframed the study's findings in political behavior terms. 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 campaign begins, and that these preferences are rather inflexibly grounded in voters' social backgrounds (e.g., p. 27). *The People's Choice* is, in short, a study on media persuasion that molted, due to hypothesis-denying data, into a book about voting. So neatly had Lazarsfeld and his coauthors reframed 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.

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1. There is a large and growing literature on Lazarsfeld's intellectual history and significance in the history of the social sciences. Among the most useful is Morrison (1976, 1998), Oberschall (1978), Barton (1979, 1982), Coleman (1980), Neurath (1983), Sills (1987), Merton (1998), Wheatland (2005), Converse (2009), and Fleck (2011, chapter 5).

If you read closely, though, you can find all three mnemonic strands in the book: the methodological breakthrough, the empirical research on voting behavior, and the finding that media have only limited effects.[2] But it was left to subsequent work—including the Bureau's two other book-length panel studies, *Voting* (1954) and *Personal Influence* (1955)—to suggest the three distinct interpretive lineages.[3]

Consider the theme of *methodological innovation*, which is already flagged in *The People's Choice*. In a characteristic Lazarsfeldian move, the book's 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" (p. 2). 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" (p. 2). The narrated claim to methodological novelty is even more prominent in Lazarsfeld's preface to the book's second edition (Lazarsfeld, Berelson, & Gaudet, 1948), and in his widely cited 1948 paper, "The Use of Panels in Social Research" (Lazarsfeld, 1948a):

> The following remarks are designed to draw attention to a fairly recent development in social research. In its bare essentials, the type of study to be discussed consists of repeated interviews made with the same group of persons. The people participating as subjects in such studies are commonly known as panel members and the whole procedure has become widely known under the name of panel technique. (p. 405)

Lazarsfeld's use of third-person, passive attribution—"commonly" and "widely known"—establishes the method as a genuine innovation, which sets up well *The People's Choice* example, which he cites in the article's second paragraph.[4] Future scholars were, in short, primed to read the book as a major breakthrough in survey-research technique.

The *election research* angle, the second mnemonic stream, was sharpened by the Bureau's publication of *Voting* in 1954. The book, based on a major panel study of the 1948 presidential campaign in Elmira, New York, was presented as a sequel to *The People' Choice* (p. viii). Its

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2. *The People's Choice* signals its status as a voting study in its first sentence: "This is a report on modern American political behavior—specifically on the formation of votes during a presidential campaign" (p. 1). The rest of the introduction takes up "the major contributions of the panel technique," the book's self-proclaimed methodological breakthrough (pp. 2–8). The book's closing chapter is devoted to the relative weakness of media influence: "In comparison with the formal media of communication, personal relationships are potentially more influential . . . . In the last analysis, more than anything else people can move other people. From an ethical point of view this is a hopeful aspect in the serious social problem of propaganda" (pp. 150, 158).

3. All three books addressed themselves to a wide, cross-disciplinary audience of social scientists. In each volume, generic references to "social research" and "social scientists" outnumber references to specific disciplines, and in no case is a disciplinary orientation declared. The 1948 preface to *The People's Choice* second edition, for example, opens with the wartime experience of "social scientists," and proceeds to list the complementary contributions of sociologists, social psychologists, anthropologists, and economists. Referring to "major trends in contemporary social research," the authors assert that "the reader will find the present study more useful if he reads it with these general developments in mind" (p. vii–ix). The Bureau was interdisciplinary by self-description, though dominated by sociologists owing to its directors' affiliation with Columbia's Sociology Department. Other social scientists—especially political scientists, but also figures from the other major disciplines—collaborated on Bureau reports and publications (see Barton, 1984 for a comprehensive overview). The authors of the three volumes reflected this disciplinary diversity: Berelson was trained in library science, Gaudet in psychology, and McPhee and Katz in sociology. Lazarsfeld himself only began to identify as a sociologist in the late 1940s (see Lazarsfeld, 1948b).

4. Lazarsfeld (1940b, p. 122) had introduced the "panel" label in similar fashion, in an earlier paper whose lead sentence reads: "Students of public opinion are showing increasing interest in the panel as a tool for studying the effectiveness of propaganda and, more especially, for gauging the effects upon people of specific promotional campaigns and devices."

core finding echoed the earlier book—"voting traditions are not changed much more often than careers are chosen, religions drifted into or away from, or tastes revised" (p. 17)—though more attention was paid to voters' rootedness in relatively homogeneous small groups. For our purposes, the important point is that the book was unambiguously framed as a voting study, and as heir to *The People's Choice.*[5]

The Bureau's *Personal Influence*, published the next year, was also positioned as inheritor of *The People's Choice* mantle.[6] A last conjectural chapter ("The Nature of Personal Influence") of *The People's Choice* had observed that face-to-face persuasion seems quite effective. The other, linked finding was that this "personal influence" was unevenly distributed, with various "opinion leaders" acting as nodes of guidance within small personal networks. Since these opinion leaders were also heavy consumers of media, Lazarsfeld had speculated about a "two-step flow of communication," in which media messages were passed on by the high-consuming opinion leaders. As with the social character of voting, this was an inadvertent finding: 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" (Rossi, 1959, p. 318). *The People's Choice* concludes with these ideas—but could only suggest them as stimulants to further research, since the original study design had only accidentally captured data that were not conclusive.

The "further research" came in 1945, with a modified panel study in Decatur, Illinois, led by then-Bureau associate C. Wright Mills. Funded by *True Story* publisher MacFadden, the study not only tracked consumer product, movie going, and political preferences among a large panel of women, but also asked them to identify influential acquaintances. Field workers then followed up with the designated influencers.

In a dispute that was at once personal, political, and intellectual, Mills and Lazarsfeld famously clashed over the Decatur data (see Summers, 2006). A full decade after the original field work, the findings were finally published in 1955 as *Personal Influence*, with Bureau graduate student Elihu Katz as Lazarsfeld's coauthor. *Personal Influence* framed the "two-step flow" argument as evidence that media influence is happily negligible—that opinion leaders stand as buffers between man and media. The book presents its finding of minimal effects as a repudiation of interwar scholars' naive belief in media potency (see Pooley, 2006b).

Lazarsfeld and his coauthors, in short, positioned the three books in distinct ways. The first book, *The People's Choice*, included cues for the mnemonic uptake of all three strands, including strong claims for survey-research innovation. The two sequels form a "V"-shape of suggested readings: *The People's Choice* on to *Voting* for election studies, and *The People's Choice* through to *Personal Influence* for mass communication research.

There is nothing unusual about this: scholars regularly make decisions about how to package their raw findings, and the urge to narrate claims to originality merely echoes the modern

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5. Berelson and his coauthors begin *Voting* with, "This is a book about voting—how people came to vote as they do," and describe the study as one among "an increasing number of election studies of a fairly standardized type" (p. vii). The introduction refers to *The People's Choice* as the "first major study of 'votes in the making' during a campaign," and positions *Voting* as "another phase in a cumulative enterprise" (pp. viii–ix). See also "Appendix A (Summary of Findings from Similar Election Studies)", which identifies *The People's Choice* and *Voting* as "studies of electorate behavior," within the wider study of "political behavior" (pp. 327–347).

6. The opening pages of *Personal Influence* are addressed to the interdisciplinary field of "mass communications research" and "students of mass communications" (p. 1). The book is described as a follow-up study to *The People's Choice*: "During the 1940 presidential campaign the effect of radio and print on developing vote decisions was investigated in an Ohio community. The findings of that study indicated that the effect of the mass media was small as compared to the role of personal influences . . . . The investigation seemed to indicate what was called a two-step flow in the effect of mass media. . . . An obvious question to raise was whether these findings could be corroborated in another investigation more carefully centered around the problem of personal influence" (pp. 3–4).

university's incentive structure and value system. All scholars, in other words, introduce research in narrative terms, very often in order to anchor claims to novelty. Lazarsfeld, however, was particularly good at narrating his major projects, in part because he could afford to be rhetorically expedient:[7] his real interests were in methods and decision psychology. All the way back to Vienna, he had noticed "the methodological equivalence of socialist voting and the buying of soap" (Lazarsfeld, 1969, p. 279). It is no surprise that he and the Bureau left multiple frames—layabout narratives—waiting for others to extend or refute.

THE CONTEXT OF RECEPTION

I have lingered on the context of these books' creation, but the context of their reception is crucial too. With connections forged in the extraordinary social science mobilization of World War II, overlapping networks of elite postwar social scientists had, by the early 1950s, adopted the "behavioral sciences" moniker. Prominent sociologists, psychologists, and political scientists were drawn together by foundation and military funders, but also by personal ties, Cold War commitments and a shared enthusiasm for team-based, quantitative research (see Crowther-Heyck, 2006, pp. 422–426; Isaac, 2007, pp. 734–739). This is, of course, a grossly simplified picture of early Cold War American social science, but it is enough to claim that Lazarsfeld and his Bureau were embedded in a larger, interdisciplinary academic culture. In the 1950s, "political behavior" and "communication research" were recognized (and overlapping) cross-disciplinary fields, attracting contributions from psychologists, sociologists, and political scientists alike.[8] *Voting* and *Personal Influence* were published into these interdisciplinary fields.

What happened next is that "political behavior" and "communication research" were claimed, respectively, by political science and the new discipline of "communication" in the rapidly expanding 1960s university system. Michigan's Survey Research Center in effect adopted the erstwhile election studies tradition with its biannual election surveys beginning in 1952 and reported in major monographs, including *The Voter Decides* (Campbell et al., 1954) and *The American Voter* (Campbell et al., 1960). Though the Michigan studies were led by interdisciplinary teams, and though significant contributions to voting research continued to be made from outside the field, by the mid-1960s the academic study of election behavior was firmly established within the political science discipline (Converse, 2009, pp. 360–366).

Likewise, "communication research" as an interdisciplinary field gave way, over the 1960s, to a new would-be "communication" discipline housed in professional schools of journalism and departments of speech. The reasons for the handoff are complex, involving journalism schools' legitimacy needs and the tireless efforts of a few academic entrepreneurs (Pooley & Katz, 2008). Crucial too was a major shift in social science patronage, from the

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7. On Lazarsfeld's narrative talents, see Platt (1996, pp. 257–260), Pooley (2006b), and Oberschall (1978, pp. 202–203). Platt (1986), in an astute comparison of the divergent reputations of Lazarsfeld and Samuel Stouffer, credits the former's knack for pithy labeling and narration. Platt (p. 104) quotes a 1961 oral history of Lazarsfeld: "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, 1962, p. 339).

8. On "communication research" as an interdisciplinary social science field from the early 1940s through the early 1960s, see Gary (1996), Peters and Simonson (2004), Pooley (2008); and contemporary surveys, such as Berelson (1959). On the cross-disciplinary, if also political science–dominated, ethos of "political behavior" as a field, including the interdisciplinary SSRC Committee on Political Behavior, see Farr (1995), Adcock (2007), Hauptmann (2012); and contemporary surveys, such as Eulau, Eldersveld, and Janowitz (1956), and Dahl (1961).

foundation- and military-centric 1950s pattern to a post-Sputnik system of civilian agency grant-making that, in practice, dissolved the broker-driven interdisciplinary research culture that had incubated "communication research" (and "political behavior") as cross-disciplinary fields (Crowther-Heyck, 2006).[9]

Lazarsfeld, meanwhile, directed most of his scholarly energies, from the early 1950s until his death in 1977, to methodological topics, such as latent structure analysis (e.g., Lazarsfeld, 1954; Lazarsfeld & Henry, 1968). He was also, in these later years, a careful tender of his legacy as a major methodologist, building his case through historical work on the history of research methods (e.g., Lazarsfeld, 1972 [1958]), in presidential addresses (Lazarsfeld, 1950), in memoirs (Lazarsfeld, 1969), and in oral history interviews.[10]

So it is hardly surprising that, for communication scholars, *The People's Choice* and *Personal Influence* are remembered for establishing the "limited effects" paradigm of media research. With the aid of *Personal Influence*'s sweeping first-chapter narration, the nascent field's textbooks (e.g., DeFleur, 1966) and histories (Klapper, 1963; Schramm, 1963) told the story of an interwar "powerful effects" tradition supplanted by the Bureau's more measured and scientific conclusion that media impact is minimal. That two-stage emplotment was repeated by critics (such as Gitlin, 1978), and remains a staple of the discipline's textbooks (see Pooley, 2006b; Simonson, 2013).

Although the Bureau studies seem to occupy a smaller space within the remembered past of political science, much of the historiography on voting research begins with *The People's Choice* and *Voting* (e.g., Smith, 2001; Bartels, 2010). In the typical narrative, the "Columbia studies" are treated first, followed by the "Michigan studies," and on through revisionist critiques. When one of the Bureau studies shows up in a textbook or handbook chapter, it is very often in this voting research context.

Within sociology, the Bureau trilogy gets remembered more holistically, in part because Lazarsfeld and most Bureau figures were self-identified sociologists. Historical accounts, as well as textbooks and handbooks, often mention the voting- and media-related findings of the three books. But the treatment within sociology is still patterned, with the bulk of attention directed toward the books' methodological breakthroughs (e.g., Coleman, 1972; Oberschall, 1978).

There is a certain logic, then, to the three mnemonic strands, with their partial recollections of the Bureau's trilogy of panel studies. The books are, after all, polyvocal: they invoke a handful of topics, all of which fit comfortably within the interdisciplinary space of 1950s behavioral science. Later readers—from the mid-1960s onward—encountered the books instead from their newly differentiated perches within political science, sociology, or communication

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9. These trends within communication and political behavior research—in the direction of discipline-bound specialization—reflect wider currents in the postwar American social sciences. See Geiger (1993, chapter 7); and the discipline-specific essays in Porter and Ross (2003, part 2) and Backhouse and Fontaine (2010).

10. Lazarsfeld typically placed his and the Bureau's methodological innovations in the third person. In a 1964 paper on "organized social research," Lazarsfeld (1964, pp. 10–12) included a historical capsule that, in part, highlighted the central role of the Bureau as well as its use of secondary analysis: "Beginning with the early 1930s and going up to the war, a second trend appeared, origining in data rather than problems. Commercial consumer studies had greatly contributed to the development of sampling methods and had given rise to public opinion polling. Radio had come on the scene and audience surveys were needed to parallel the circulation figures of magazines and newspapers. These data became the raw material for the new field of communications and opinion research. Many other applied topics suggested secondary analysis for scientific purposes. Thus a few institutes were created where social research for its own sake became the organizational motivation, most typically the Bureau of Applied Social Research at Columbia University . . . The tradition of secondary analysis which grew up in the second phase before the last war has been maintained ever since and is likely to grow." See also Lazarsfeld (1957, p. 43). Oberschall (1978) provides an excellent overview of Lazarsfeld's history-of-methods writings.

studies. The books were written and read in an interdisciplinary milieu that was itself history to the next generation of discipline-bound readers. And of course most of them did not read the books, but instead a passing textbook reference or a paragraph in the latest state-of-the-field essay.

<div align="center">FLAGSHIP JOURNAL ANALYSIS</div>

This study builds on the unsystematic review of textbooks and handbook chapters summarized above. To test my observation that the three Bureau books were remembered as the published trailheads to distinct scholarly pathways, I examined the books' treatment in each discipline's flagship journals beginning in 1960. Flagship journals are imperfect proxies for a field's memory, to be sure. Given the exploratory nature of the study, however, they are adequate stand-ins. The norms of scholarship in most fields call on researchers to credit forebears with at least passing reference. The stock literature review sections of standard journal articles are, as a result, iterative acts of self-proclaimed genealogy. Fleeting citations and casual shorthands—the "Columbia studies," for example—are very often rich memory deposits well worth mining.

Disciplinary memory—an elusive and slippery idea in the first place, like its parent concept collective memory—is notoriously difficult to track. After all, the stories that scholars carry around in their heads about their own fields vary by national tradition, subfield, department, and individual scholar. Even if we agree that there are such things as disciplinary memories, the task of finding and describing them is thorny. We can ask our scholar subjects, of course, but at best we learn about self-reported rememberings at the moment the questions are posed. If we are curious about the evolution of memory over time, as I am here, we are confronted with an impossibly elaborate capillary system of remembrances, most of which are not recoverable. All the conference-corridor conversations, classroom banter, and "oral publications" have disappeared into the ether.[11] We are left with textual traces, in the form of published scholarship, successive textbook editions, and time-stamped archival leavings: letters, lecture notes, syllabi, and oral history transcripts. These are our pottery fragments.

In both methodological and conceptual terms, the study is indebted to the tradition of citation analysis elaborated in the sociology of science, information studies, and applied linguistics. Scholars in these fields have long explored the complex ecology of referencing in academic writing, especially within the natural sciences. They have developed a range of quantitative and qualitative methods to measure citation practices, alongside an impressive conceptual toolkit (see Nicolaisen, 2007, for an excellent overview). This literature is a largely untapped resource for historians of the social sciences, who have tended to rely on narrative approaches rooted in archival research and the synoptic readings of texts. Only rarely, however, does the citation-analysis literature attend to the historical claims often embedded in reference contexts—a lacuna that the present analysis attempts to address.

The method employed here is a form of *citation context analysis*—the study of the text surrounding a reference (McCain & Turner, 1989; Small, 1982; White, 2004, pp. 99–100, 103). In conceptual terms, I draw on work that addresses the *symbolic role* played by citations, especially as persuasive rhetoric (Gilbert, 1977; Latour, 1987; Bazerman, 1988; Cozzens, 1989). Citations answer to a range of author purposes, from crediting colleagues for relevant contributions to establishing legitimacy and signaling allegiance. We can set aside

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11. Robert Merton defined oral publications as "the working out of ideas in lectures, seminars, and workshops—before finally converting their developed substance into public print" (Merton, 1996, p. 351).

the longstanding debate over whether citations work as advertised—in keeping with Robert Merton's (1968a) normative conception—or else function mostly as present-minded rhetoric (see Bornmann & Daniel, 2008, pp. 48–50). Scholars are plainly in the business of deploying *both* kinds, very often within a single citation context (Cozzens, 1989).

Following Henry Small (1978), I treat citations as *concept symbols*. Scholars impart meaning to their cited publications, as guided by the surrounding text. Referencing, as Small argues (p. 328), is a labeling process. The literal citation (an in-text parenthetical or footnote) is asked, by the citing author, to "stand in" for an idea—a theory, a method, an argument, or even a passing observation. It is the surrounding text—the citation context—that signals the citing author's intended meaning. Small (borrowing from Leach, 1976) designates as *standard symbols* those citation meanings that come to be shared by a community of academics. Such standard symbols are characterized by a "uniformity of usage" (pp. 328–329).

A single-cited publication, in theory, could come to have more than one standardized referent. Susan Cozzens (1982) identified such a case within economics, which she called a *split citation identity*:

> Scientists who are concerned with the same set of questions constitute an audience for each other. Their publications consist largely of the reinterpretation and reworking of findings and concepts already introduced in their collective discussion. In this process of reinterpretation and extension, it is not surprising that certain aspects of previously published work come up again and again, nor that those older works are mentioned in similar contexts. Since the questions which concern authors in one problem area are not the same as those which concern authors in another, it is therefore also natural that the particular aspects of older works which interest them should differ. (p. 235)

A small number of studies have identified other cases of split or multiple citation identities (McCain & Turner, 1989; Mizruchi & Fein, 1999). The idea is that the same publication can lead a double (or triple) life in the subsequent literature.

A contribution of the present study is to expand this notion to identify (and foreground) citation contexts that place cited publications in patterned historical relief. If citations are concept symbols, the "concepts" that they ascribe to referenced works may be—and, in the social sciences at least, often are—capsulized historical narratives. Hence, the idea of *mnemonic multiples* is to designate cases where the same works are historicized in distinct ways.

To test whether the Bureau trilogy qualifies, I conducted a citation context analysis of flagship journals. In citation context analysis, researchers examine the text that introduces and surrounds cited documents (Small, 1982). Though computer-aided approaches exist (see White, 2004, pp. 103–107), the method employed here entailed close, contextual reading of the surrounding text (alongside scrutiny of the publication's abstract, if available).[12]

Using keywords designed to pick up references to the studies, I searched the full text of the *AJS*, the *APSR*, and the *JoC* from January 1960 through December 2011.[13] Journal items that referred to one or more of the Bureau books were then analyzed with special attention to their historical claims.

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12. On the advantages of close reading grounded in researcher familiarity with the relevant domain knowledge, see McCain and Turner (1989), White (2004, p. 103), Golden-Biddle, Locke, and Trish Reay (2006, p. 240). Examples of context citation analysis include Hargens (2000), McCain and Salvucci (2006), and Chang (2013).

13. As a check against the arbitrary exclusion of other social science disciplines' flagships, I conducted searches in economics, anthropology, and psychology journals. The exclusion of these fields' journal was justified by the extremely low citation rates. The *American Economic Review* and the *American Anthropologist* (AAA's designated flagship) cited one or more of the Bureau books just twice, while the *American Psychologist* (the APA's flagship) made only a single reference over the 50-year span.

The journals selected for analysis (*APSR, AJS,* and *JoC*) are recognized as their discipline's flagship, at least in the United States.[14] Using the journal storage repository (JSTOR) and a publisher's proprietary database, I searched the three journals for mention of one or more of the Bureau studies from 1960 onward, with a Boolean keyword combination of book title (e.g., "Voting") and first author (e.g., "Berelson") for all three books. After discarding false matches, the search yielded 265 items (articles and book reviews) citing one or more of the Bureau books—136 (51 percent of total items) in *APSR*, 54 (20 percent) in *AJS*, and 76 (29 percent) in *JoC*.

For each item I conducted an internal search to locate references and their enclosing passages. After recording which of the books were cited or referenced, I read the relevant passages closely to establish the context(s) and historical claims, if any.[15] For each item, I made a judgment about whether or not the article or review was invoking an historical narrative related to the Columbia books. There is no bright line dividing the history-invoking articles from the others, but the call was relatively easy to make in practice. For example, Prothro (1967) refers to "pioneering work on electoral behavior by Paul Lazarsfeld and his associates," citing *PC* (p. 485)—a clear historical claim. By contrast, Cole (1969) merely credits *PC* and *V* for the idea of "cross-pressures" (p. 512), without any historical context. Just over a quarter (27 percent, or 72) of the items were classified as history-invoking.

I also assigned each item one or more *contexts* from a menu of six options that I compiled based on a sample of articles. Each article or review, in other words, was coded as invoking the Bureau studies in the context of (1) *voting studies*, (2) *methodological innovation*, (3) the *two-step-flow hypothesis*, (4) the *powerful-to-limited media effects account*, (5) the *selective exposure finding*, and/or (6) *other*. Shapiro (1969), for example, asserts that the "major original voting studies are of course those of the Columbia and Michigan groups ... " (p. 1106); the article was assigned the *voting studies* context. Scheufele and Tewksbury (2007) claim that:

> the early hypodermic needle and magic-bullet models of the 1920s and 1930s were quickly replaced by a paradigm based on the much more theoretically and methodologically sophisticated ideas that Lazarsfeld and his colleagues in Columbia University's Bureau of Applied Social Research put forth in *The People's Choice* ... and subsequent studies.

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14. American sociology has two flagships, *AJS* and the *American Sociological Review (ASR)*. At the suggestion of two anonymous reviewers, I conducted a follow-up analysis for the *ASR*, given that the journal is the ASA's official flagship, and generally recognized to have a more quantitative bent than *AJS*. Because the *ASR* analysis tended to echo the *AJS* findings—and because inclusion of two sociology journals would upset the parity of the analysis—I report the results of the follow-up *ASR* study in this extended footnote (with *AJS* results included for comparison). If anything, the substitution of *ASR* for *AJS* (or the collapsing of the two journals' results) would strengthen the study's findings, if only because the *methodological innovation* context appeared more often in *ASR* (22 percent of the sample) than the other three, including the next closest, *AJS* (13 percent). When considering the *history-invoking methodological innovation* context, *ASR* (38 percent of all history-invoking articles) again exceeded *AJS* (33 percent) as well as the other two journals (*APSR*: 8 percent; *JoC*: 0 percent). A breakdown of the results follow in the order they appear in the study: (1) *Distribution of book citations*: *People's Choice*—*ASR*: 34 percent (of 64 total articles in the sample); *AJS*: 46 percent (of 54 total articles in the sample); *Voting* (*ASR*: 56 percent, *AJS*: 48 percent); *Personal Influence* (*ASR*: 41 percent, *AJS*: 31 percent). (2) *Multiple citations:* both *PC* and *PI* (*ASR*: 9 percent, *AJS*: 6 percent); both *PC* and *V* (*ASR*: 20 percent, *AJS*: 19 percent); both *PI* and *V* (*ASR*: 9 percent, *AJS*: 2 percent); all three (*ASR*: 8 percent, *AJS:* 2 percent). (3) *Two-step flow context:* (*ASR*: 17 percent, *AJS*: 26 percent). (4) *Powerful-to-limited effects context:* (*ASR*: 5 percent, *AJS:* 0 percent). (5) *Selective exposure context:* (*ASR*: 0 percent, *AJS*: 0 percent). (6) *Collapsed media-related contexts:* (*ASR*: 19 percent, *AJS*: 26 percent). (7) *Voting studies context:* (*ASR*: 47 percent, *AJS*: 54 percent). (8) *Methodological innovation context:* (*ASR*: 22 percent, *AJS*: 13 percent). (9) *History-invoking references:* (*ASR*: 20 percent, *AJS*: 11 percent). (10) *History-invoking references to collapsed media-related contexts*: (*ASR*: 38 percent, *AJS*: 33 percent). (11) *History-invoking voting studies context:* (*ASR*: 62 percent, *AJS*: 83 percent). (12) *History invoking methodological innovation context:* (*ASR*: 38 percent, *AJS*: 20 percent).

15. On classification schemes for citation contexts, see Cronin (1994), Golden-Biddle, Locke, and Trish Reay (2006, p. 240–243), Chang (2013, pp. 537–538).

Media effects were much more complex in nature than previously assumed, they argued, and depended heavily on people's homogenous networks and their selective informational diets, which reinforced existing attitudes rather than changed them. (p. 10)

This article was assigned the *powerful-to-limited media effects account* and *selective exposure finding* contexts. The basic distribution of the three journals' citations to the Bureau books offers some initial support to the patterned remembering thesis. The *APSR* items were far more likely to cite *V* (72 percent) than *PC* (38 percent), and very few (9 percent) referenced *PI*—the 1955 Katz and Lazarsfeld book framed as a media study. For *JoC* articles, the pattern was inverted: *V* was cited in just a quarter (26 percent) of items, compared with over half for *PC* (51 percent) and *PI* (51 percent). The sociology journal, *AJS*, spread its citations around more evenly: nearly half cited *V* (48 percent) and *PC* (46 percent), while close to a third referenced *PI* (31 percent). The expected pattern, in other words, was confirmed by this admittedly blunt measure: the political science journal was far more likely to cite *Voting*, the book packaged as a voting study, than the media-focused *Personal Influence*. Though the disparity was less pronounced, the communication journal was twice as likely to cite *PI* than *V.*

The citation data also provide some support for my earlier suggestion that the disciplinary memories of the book form a "V"-shape—that is, *PC* to *V* for political science, and *PC* to *PI* for communication. *JoC* was far more likely to cite both *PC* and *PI* (10 items or 13 percent of the *JoC* total) than either *APSR* (two items or 1.5 percent) or *AJS* (three items or 6 percent). Along the other track—*PC* to *V*—the expected gap, however, did not appear: *APSR* was just as likely to cite both *PC* and *V* (22 items or 16 percent) as *JoC* (12 items or 16 percent), while *AJS* was actually the most likely (10 items or 19 percent).[16] As the "V"-shape thesis would predict, there were relatively few instances of items citing *V* and *PI*: just three for *APSR* (2 percent), one for *AJS* (2 percent), and three for *JoC* (4 percent). These citation frequencies offer tentative, but qualified, support for the expected patterning of Bureau-book treatment along the lines invited by Lazarsfeld and his coauthors.

The distribution of *contexts* more closely matched the expected pattern than did the raw citation numbers.[17] As expected, *JoC* (18 items or 24 percent) was more likely to invoke the *two-step-flow hypothesis* context than *APSR* (11 or 8 percent), though *AJS* (14 or 26 percent) also employed the context relatively often. Recall that the two-step-flow claim—media influence works indirectly, via opinion leaders—was first postulated in *PC* and then tested in *PI*. As predicted, the context I have labeled the *powerful-to-limited effects account* was much more often invoked in *JoC* (15 items or 20 percent) than either *AJS* (zero) or *APSR* (seven or 5 percent). That is, the Bureau studies were referenced in the context of this media-related historical account far more frequently in the communication journal than in the others. The *selective exposure* context, likewise, was more often employed in *JoC* (nine or 12 percent) than *AJS* (zero) and *APSR* (one or 1 percent). These disparities are especially interesting, since selective exposure to media—that is, media consumers seeking out content that aligns with their preexisting beliefs—was a major conclusion of all three Bureau books.

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16. A third of the *JoC* items also cited *PI*, meaning that, for those articles, all three books were cited. *AJS* and *APSR* had no items that cited all three Bureau studies.

17. The context *other* was assigned when no other context applied: six items (or 11 percent) for *AJS*, one item (or 1 percent) for *APSR*, and 16 items (or 21 percent) for *JoC*. The higher percentage for *JoC* relative to *APSR* is likely a result of the menu of contexts developed for this study: *voting studies* was, in effect, a catch-all, as compared to the more specific media-related contexts. As a result, a number of *JoC* studies invoked a Bureau study on a media-related finding that was not, however, one of the explicit contexts developed for the coding.

To more clearly illustrate the variation among the three journals, I collapsed the three media-related contexts (*two-step-flow hypothesis*, *powerful-to-limited effects account*, and *selective exposure*) into a single *media* context. *JoC* was much more likely (42 items or 68 percent) to cite one or more of the Bureau books in reference to a *media*-related finding than either *ASPR* (19 or 14 percent) or *AJS* (14 or 26 percent). The communication journal usually referred to the studies' media conclusions; for the sociology and political science journals a media-related citation was less likely.

The Bureau studies were invoked in the context of *voting studies* relatively frequently across all three journals, but that context, as expected, was most often present for *APSR* (120 items or 88 percent), followed by *AJS* (29 or 54 percent) and *JoC* (21 or 28 percent). The political science journal, in other words, was three times as likely to employ the voting studies context as was the communication journal.

Though the *methodological innovation* context was less often invoked across the three journals, *AJS* (seven items or 13 percent) was more likely to highlight the books' survey-research ingenuity than either *JoC* (one or 1 percent) or *ASPR* (six or 4 percent). The analysis of contexts, in short, supports the predicted pattern of field-specific invocation. Still, there is considerable cross-pollination among the three journals' context use. The *methodological innovation* context, moreover, appeared less often in the sociology journal than expected.

For the purposes of this study, the most interesting and relevant references were those that explicitly place the Bureau studies in an historical narrative. Recall that just over a quarter (27 percent, or 72) of the total items under analysis were judged to make such historical claims. Among this historical subset, the patterns identified above were, if anything, more pronounced: 73 percent (or 19 items) of *JoC*'s historical references invoked at least one media context, as compared to just 13 percent (or five) for *APSR* and 33 percent (or two) for *AJS*. For voting studies, the proportions were reversed: 95 percent (or 38 items) of *APSR* items placed the Bureau studies in a voting studies historical frame, compared to 31 percent (or eight) for *JoC. AJS* did invoke the studies in the voting context 83 percent of the time, but the sociology flagship was far less likely to invoke *any* historical narrative than the other two titles: only six items (or 11 percent) total, as compared to 34 percent (or 26 items) for *JoC* and 29 percent (or 40 items) for *APSR.* Among the historical subset, research methodology was invoked relatively infrequently: 8 percent (or 3 items) for *APSR*, 20 percent (or one item) for *AJS*, and no references in *JoC*.

Taken as a whole, the analysis of the three journals—in terms of basic citations, contexts, and historical narratives—provides initial support for this article's core claim. When the Bureau studies get mentioned in the communication journal, the context is far more likely to be media-related. The voting studies context, by contrast, is employed in a large majority of the political science journal's references—three times as often as the communication journal. Though innovative methodology is employed less frequently in the sociology journal than expected, *AJS* was indeed much more likely to invoke the context. The same Columbia books, in other words, are appealed to in different—and patterned—ways.

The counting and coding is revealing, to be sure, but the plainest expression of patterned remembering is found in the relevant passages themselves—those journal article sentences that invoke and historicize the Bureau studies. In *JoC*, for example, the powerful-to-limited effects storyline is called up with relative regularity, especially since the early 1980s. Noelle-Neumann (1983), for example, refers to the "Lazarsfeld Institute [sic]" and its "minimal effects hypothesis," whose "point of departure" was "Lazarsfeld et al.'s admirable and still exciting study of 1940, *The People's Choice*" (p. 160). Likewise, Nord (1995)—citing *PC* and *PI*—points to the "classic political communication surveys conducted by Lazarsfeld and

other sociologists, a research tradition that culminated in the 'limited effects' model of mass media influence" (p. 85). In a similar vein, Bryant and Morin (2004) claim that "empirical research conducted in the 1940s and 1950s failed to support theories of powerful media effects (e.g., the magic bullet and hypodermic needle theories)"—referring here to two retroactive metaphors commonly ascribed to, but never used in, interwar media research (see Lubken, 2008). "[F]unctionalist Lazarsfeld and his colleagues," Bryant and Morin continue, citing *PC* and *PI*, "proposed that people's interpersonal communication with opinion leaders mediates the effects of mass communication" (p. 680). Drawing on mnemonic shorthands, such as "limited effects" and "hypodermic needle," these and other *JoC* authors invoke the Bureau studies with the matter-of-fact economy of assumed knowledge: references to *PC*, *PI*, and "Lazarsfeld" conjure up a shared narrative of the field's past that requires no elaboration.

As we have seen, not all *JoC* items call upon the powerful-to-limited effects narrative. Indeed, a sizable minority position the Columbia books as milestones in a voting research tradition that also includes subsequent University of Michigan studies. Wyatt (1998), in a review essay that cites *PI*, refers to "Lazarsfeld and the Columbia group," as well as the "Michigan School," in the context of voting research (p. 844). Some *JoC* authors mixed media- and election-related themes in their citations of the Bureau studies. Rogers and Chaffee (1983), citing *PC* and *V*, argue that Lazarsfeld's studies "concluded prematurely after the 1940 and 1948 presidential election studies that mass communication isn't very powerful." Continue Rogers and Chaffee (with a dubious chronology): "Then they studied the role of interpersonal communication, as a mediator of media impact, in the Decatur project"—citing *PI* (p. 19). Even when the voting studies context is employed, then, the discussion of the Bureau books is often refracted through a media-centric lens.

In the political science journal, by contrast, the media angle is far less frequently invoked. Narrative markers like "the classic voting behavior studies of Paul Lazarsfeld and his associates" (Sigel, 1968, p. 216), "the Columbia school of electoral analysis" (Zuckerman, 1996, p. 193), and "the early Columbia studies" (Beck et al., 2002, p. 58) are common. The typical *APSR* item positions the Bureau trilogy as forerunner to the Michigan studies and subsequent political science research. Strong (1965), for example, refers to a book under review as a "study of voting behavior in the tradition of the studies of Lazarsfeld, Berelson, and the Michigan Survey Research Center" (p. 159). Likewise, Kirkpatrick (1971) refers to "a long series of electoral studies running from Lazarsfeld's efforts in Elmira and in Erie County to the [Michigan] Survey Research Center's most recent work" (p. 972). Pairings of the "Columbia school" and "Michigan school" recur frequently (e.g., Brady & Sinderman, 1985, p. 1073).

A substantial minority of *APSR* items do mention the two-step-flow or limited-effects finding, but very often with "voting research" as backdrop. Tellingly, *PI* is almost never cited in these items. Mutz and Martin (2001), for instance, cite *PC* and state, "Beginning with the Erie County election study this assumption became part of the conventional view that the media have limited effects on political attitudes" (p. 98). Wildavsky (1987) refers to "Berelson, Lazarsfeld, & McPhee's (1954) well-known two-step flow of communication from activists to less attentive citizens" (p. 16). Not only has credit for the two-step-flow transferred to *V*, but the very idea is reframed in explicitly political terms. Likewise, Bartels (1993) observes—citing just *PC* and *V*—that the "field of electoral politics has produced some of the most influential early findings of 'minimal effects,' especially in the classic Columbia studies of presidential campaigns in the 1940s" (p. 269). The voting studies framing is even more pronounced in Dalton, Beck, and Huckfeldt (1998), which invokes "the Columbia studies of voting" with reference to *V* and *PC*. The two Bureau books—"the first empirical studies of

the media's influence in elections"—gave "rise to the so-called minimal effects view: The media do not appear to exert a major persuasive influence on partisan preferences" (pp. 111–112). Thus, even when the media-related themes in the Bureau trilogy—limited effects and the two-step flow—make an appearance in the political science journal, the enveloping word choice positions the claims in voting-research terms. And *PI* is almost always excluded, despite its far more substantial analysis of the two themes.

While the political science and communication journals hewed, more or less, to discipline-centric memories of the Bureau trilogy, the sociology journal was more promiscuous.[18] As the article coding already revealed, the *methodological innovation* context surfaced less frequently than expected. Close readings of the *AJS* items confirmed that the journal spread its (relatively rare) references to the Bureau studies among the voting, media, and research contexts. Some items *did* invoke the books in the context of survey methodology. Goodman (1962), for example, reanalyzes the *PC* panel data in a pure methods piece, devoted to developing statistical methods to better gauge attitude change (p. 58). In a passing methodological reference to snowball sampling, Wallace (1964) cites *V* for the "usual technique for measuring interpersonal influence in a large social group . . . asking respondents to name their 'best' or 'close' friends" (p. 316). Relatively rare, however, were framings of the Bureau books as prominent methodological breakthroughs. One exception is Coleman (1986), a prominent theorist and former Lazarsfeld student who cites *PC* (along with the American Soldier studies and Stanton, & Lazarsfeld's, 1941, *Radio Research 1941*) as defining works of the "watershed in empirical research in sociology," characterized by survey research. Adds Coleman: "The watershed was brought into existence in part by new empirical methods" in the three works (p. 1314). Here the Bureau books (or at least *PC*) are held up as methodological milestones in the expected sense.

Still, many other *AJS* items refer to the Columbia studies in media or voting research related contexts. Gottdiener (1985), for example, makes passing mention of the "Lazarsfeldian school's study of media effects," citing *PC* and *PI* (p. 980). With voting studies in mind, Guetzkow (1965) refers to "sophistication in the study of voting behavior, after the manner of Lazarsfeld and Berelson" (pp. 106–107). In a few cases the media and voting contexts are combined: Burt (1987) first cites *PC* and *V*, "the seminal studies of informal social pressures on voting in the 1940 and 1948 elections." In the very next sentence he invokes *PI* in reference to "the study of opinion leaders in the two-step flow of mass media diffusion" (p. 1290).

Measured by *AJS*, at least, the Bureau trilogy seems to occupy a relatively small space in the memory of sociologists—and when the books do surface their embedding contexts are spread about. Recall that *AJS* was the least likely to cite one or more of the Bureau books; there were two-and-a-half times as many *APSR* items in the analysis. And the gap widens when history-invoking articles and reviews are considered: *AJS* registered just six such items, as against 26 for *JoC* and 40 for *APSR*. There is an irony here: the Columbia books were authored by self-identified sociologists and addressed to the field.

Why the relative neglect by sociologists? One explanation could be the comparative power of the media and voting frames, as narrated in the two successor books, *PI* and *V*, respectively. The innovative methods frame is strongest in the original book, *PC*, but that originality claim competes with the substantive (and counterintuitive) voting-stability findings, as well as the tantalizing last-chapter allusions to a possible two-step flow. The successor books are, in

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18. As an anonymous reviewer observed, the fragmented nature of sociology as a discipline may contribute to this context diversity, though this explanation needs qualification since communication research has even greater coherency challenges (cf. Corner, 2013).

short, more coherently framed. When, in the 1960s, the existing interdisciplinary fields of "political behavior" and "communication research" were claimed by political science and the new "communication" discipline, sociologists largely abandoned these topics. All that remained, for sociologists, was the narrated claim to survey-research innovation. And there is good reason to believe that methodological narratives are less amenable to mnemonic uptake. Methodological discussions, after all, tend to be ahistorical—preoccupied with the current state of the art. As Lazarsfeld's panel innovation receded into the past, the originality claim suffered a kind of obliteration by incorporation (cf. Merton, 1968b, pp. 27–28). (That Lazarsfeld is remembered primarily as a methodologist by sociologists may help explain the rapidly shrinking space he has occupied in the discipline's consciousness since his death in 1976.) Well-narrated substantive findings, by contrast, are more useful to scholars working on similar topics: they permit researchers to identify themselves within (or against) a recognized research tradition.

The flagship journal analysis, at any rate, provided much stronger support for the two other disciplines. As expected, the political science and communication journals read the same Bureau books in contrasting terms.[19] When the trilogy was cast in historical narrative, moreover, the two fields told relatively distinct—and differently inflected—stories.

A few limitations of the study should be acknowledged. The most obvious is the reliance on flagship journals, since the Bureau books were invoked in a range of other publication types (including nonflagship journals and books) as well as conferences, courses, letters, and conversations. Because of the study's disciplinary focus, full-text search limitations, feasibility requirements, and the exploratory nature of the analysis, the focus on flagship journals is arguably justified. Still, the transmission of academic memory is plainly a complex, multimodal process that this study only incompletely measures.

Another, thornier problem is endemic to the scholarly differentiation process that—at the disciplinary level—the study takes as its principal explanatory dynamic. All three disciplines—especially sociology and communication research—are themselves fragmented into subfields and research areas with isolated reference ecologies.[20] It is possible, or even likely, that the Bureau books are remembered variously *within* disciplines—that election researchers within political science, for example, have referenced (and recalled in a patterned way) *PC* and *V*, while international relations specialists, presumably, have taken little notice. An analysis that included a sampling of specialist journals would better isolate such within-discipline imbalances, with potential consequences for the parent-discipline framework employed here. Along similar lines, future studies might incorporate the disciplinary affiliations of the publications' author(s) to check the assumption that flagship journals are populated by authors trained in, and/or affiliated with, the journal's discipline. Finally, further research should attempt to measure changes in the patterned remembering *over time*, as a test of the assumption, only

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19. This contrasting treatment of the Bureau books holds, despite the prominence of a relatively interdisciplinary field, "political communication" that has a jointly recognized journal and formally linked professional association sections, and draws researchers from both disciplines.

20. In Whitley's (1984) classification scheme, all three disciplines would qualify as "fragmented adhocracies," characterized by intellectual variety and low levels of knowledge integration (pp. 169–176). As Whitley (p. 173) observes, however, the diffuse nature of these fields often orients scholars, at least in identity and reputation-seeking terms, to their parent disciplines: "In most cases it is the disciplinary identity which is more important to practitioners than their, often short-lived, adherence to a particular topic or object of concern. Thus reputations in sub-units are usually complemented by attempts to gain the approval of scientists in other areas, especially when resources are scarce, through publishing in the more central journals or claiming more general significance for a study in a particular sub-field."

suggested here, that these memories should increasingly diverge as they are iteratively invoked within disciplinary literatures.[21]

CONCLUSION

Scholarly memories, however nebulous and difficult to track, plainly matter. Widely shared norms of social science require published scholarship to engage with past research; these same norms, however, tend to permit that engagement to be rather loose, perfunctory, and unapologetically presentist (Cole, 1975; Moravcsik & Murugesan, 1975; Hargens, 2000). Works that acquire the "classic" label, like the Bureau trilogy, may not be often read. They may indeed act as dusty bookshelf ornaments. But they do get mentioned often enough, and the very half-baked hastiness of most reference makes them more, not less, interesting.

As we have seen, the Bureau books are frequently invoked by what might be called *mnemonic shorthands*—pithy identifiers meant to activate researchers' shared memories. Scholars routinely deploy such shorthands because of the resolute present mindedness of most social science fields. The scenarios are familiar: the literature review narrative of previous research, the allegiance-signaling invocation of a school or scholar, the puffed-up predecessor contrast in service of an originality claim, even the relegation of contemporary disputants to a discredited past. Shorthands, in other words, do important work in the present.

Just *because* shorthands—mnemonic or otherwise—are pliant and often tossed-off, historians should pay closer attention. To be sure, labels like the "Bureau voting studies" have complicated careers across space and over time that make for difficult tracking. But the task is not impossible, especially given new database and search technologies. Indeed, it is possible to imagine a kind of golden age for "sociological semantics," the phrase Robert K. Merton used to describe his and others' studies of linguistic forms over time.[22] Searching is not enough, of course, no matter how many pottery fragments are digitized. It remains crucial to contextualize a term's origins and initial reception, and to trace its complex afterlife across and between fields.

Some subset of prominent publications of the past, perhaps a small minority, qualify as mnemonic multiples, as defined here. These works, against the backdrop of shifting academic topographies, take on more than one relatively stable meaning. But even if mnemonic multiples exist, why should we care?

Within information studies and the sociology of science, the concept extends and arguably deepens an important finding: that cited works may take on multiple, and relatively distinct, meanings within particular research areas. Scholars have explored only a few cases of such split citation identities (Cozzens, 1982), and none of these has considered the implications for disciplinary or subdisciplinary memory. The focus on mnemonic multiples foregrounds remembered history by isolating *history-invoking* citation contexts. The concept, and the attendant coding strategy, should prove useful for researchers exploring the legitimacy-establishing, field-specific rhetoric of referencing behavior.

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21. Indeed, the commemorative nature of many history-invoking references invites careful analysis of how prominent references may reproduce or even reshape the cited works' memory in response to earlier references—akin to the iterative memory-making that Jeffrey Olick (1999) has observed in anniversary commemorations of the Federal Republic of Germany.

22. The phrase appears in the title of Merton and Barber's (2004) brilliant, posthumously published history of "serendipity," preceded, of course, by Merton's (1965) classic *On the Shoulders of Giants*. See Zuckerman (2010) and Camic (2010) for complementary histories of Merton's sociological semantics.

For historians of social science, the notion of mnemonic multiples may be especially relevant—in part because social scientists are more likely to cite "classics" to "establish the literature" than their natural science counterparts (Line, 1981, p. 74; Bazerman, 1988, pp. 278–288; Hargens, 2000; Becher & Trowler, 2001, p. 114). Brief historical claims, as this study suggests, are very often used to orient research as continuous with, or as a break against, the "focal works" of the past (Golden-Biddle, Locke, & Reay, 2006). The prevalence of mnemonic multiples may testify to the relative plasticity of these prior-research invocations.

There is a large and intelligent literature on the sociology of cross-national intellectual translation, with a focus on the reshaping of a foreign author's work within the new national context (e.g., Lamont, 1987; Platt, 1995; Scaff, 2011, part 2; see Bielsa, 2011). In a sense, the phenomenon of mnemonic multiples tracks a similar process *over time*, against the backdrop of disciplinary (or other kinds of) differentiation.

Though the unit of comparison need not be the discipline—subfields within sociology, for example, may each reproduce, through iterative referencing, their own "Weber" (cf. Scaff, 2004)—the disciplinary focus maintained here points to a fruitful application in the historiography of social science. The main social science disciplines only emerged, in the American context, in the late nineteenth century (Haskell, 1977)—and remained unevenly differentiated into the interwar years (e.g., Young, 2009). Other social science claimants—quasi-applied fields, such as communication, management, and criminology—have broken off (and then only partially) since World War II (Abbott, 2010, p. 127). One result is that more-or-less segregated collections of scholars may share a common set of intellectual references, but assign to those works particular (and separately evolving) meanings. Thorstein Veblen's *Theory of the Leisure Class*, for example, or George Herbert Mead's *Mind, Self and Society* may be claimed—and distinctively remembered—by their descendants in disciplines that have since stabilized. More recently, works by field-spanning scholars, such as Richard Rorty or Clifford Geertz, may have field-specific reception histories worth telling.

The Columbia panel studies are one such case. Published into interdisciplinary fields, the Bureau books have come to be remembered in discipline-specific ways with slotted and iteratively affirmed meaning. As political science, communication research, and sociology went their separate ways, memories of the trilogy—invoked by shorthand—became segregated too.

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