

# Further Notes on Why American Sociology Abandoned Mass Communication Research<sup>1</sup>

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Communication research seems to be flourishing, as evident in the number of universities offering degrees in communication, number of students enrolled, number of journals, and so on. The field is interdisciplinary and embraces various combinations of former schools of journalism, schools of speech (Midwest for “rhetoric”), and programs in sociology and political science. The field is linked to law, to schools of business and health, to cinema studies, and, increasingly, to humanistically oriented programs of so-called cultural studies. All this, in spite of having been prematurely pronounced dead, or bankrupt, by some of its founders.

Sociologists once occupied a prominent place in the study of communication— both in pioneering departments of sociology and as founding members of the interdisciplinary teams that constituted departments and schools of communication. In the intervening years, we daresay that media research has attracted rather little attention in mainstream sociology and, as for departments of communication, a generation of scholars brought up on interdisciplinarity has lost touch with the disciplines from which their teachers were recruited.

The object of this speculative article is to reflect on why mainstream sociology in the United States may be said to have abandoned media research early on in spite of the centrality it occupied in the pioneering departments.

## The Chicago School and communication

For the first half of the last century, the Chicago School dominated sociology in the United States (Bulmer, 1984). We rather doubt that there was any subdivision in Chicago sociology that declared itself specifically interested in communication, although the label “communication” appears frequently.<sup>2</sup> In his listing of “substantive areas of research at Chicago,” Kurtz (1984) puts the study of “public opinion and communication” in second place—though in truth the interest was inseparable from the School’s broader inquiry into “collective behavior” and its part in social order and disorder. Robert E. Park, a former journalist and the department’s leading figure, had studied in Germany, like so many other American scholars. There Park was exposed to a wide-ranging European conversation on public opinion and the press in a rapidly changed social order. German press scholars like Karl Knies, Albert Schäffle, and Karl Bücher; French theorists including Gabriel Tarde, and Gustave LeBon; and thinkers from England like James Bryce and Graham Wallas were struggling, often in response to one another, to make sense of the new mass press and its barons, the apparent suggestibility of crowds, and the consequences of all this for social control.<sup>3</sup> One of

Park's American mentors—John Dewey—was pondering similar questions (Westbrook, 1991, pp. 13–115).<sup>4</sup>

With this loose tradition of thinking on communication as backdrop, Park (1904/1972) made a seminal contrast between “crowd” and “public” as modern forms of social organization, in his dissertation written under Wilhelm Windelband at Heidelberg.<sup>5</sup> Already in the dissertation, but later in more developed form, Park (1921, 1923, 1938) argued that the press and its reading public act as a healthy substitute for older, more traditional forms of social glue. This view, that mass media supply a kind of over-the-wire *gemeinschaft*, was shared, more or less, by Chicago-linked figures like Cooley (1909) and Dewey (1927) (Carey, 1996; Czitrom, 1982). Park (1922) saw the immigrant press, for example, as an integral part of the struggle for internal power and external acceptance in the new society, following the pathbreaking study of *The Polish Peasant in Europe and America* by Thomas and Znaniecki (1918). To crowd and public, Blumer (1939) later added “mass” in his well-known summary of Park's collective behavior approach. Blumer's (1933) interest in “audiences” was no doubt stirred by his contribution to the Payne Fund studies on film and children, to which Chicago student Cressey (1938) also contributed.<sup>6</sup> Further study of crowds, and of rumors, was led by Shibutani (1944, 1966), two of whose graduate students, Lang and Lang (1953), made the first-ever comparison of viewing an event on television and experiencing the same event in person. After World War II, Janowitz (1952) revisited the community press, and Wirth (1948) in his presidential address to the ASA equated the power of the mass media to save the world with the power of atomic weapons to destroy it.

Admittedly scattered, the Chicago School's reflections on communication were fundamental to its broader reflections on social order. This body of thought, however, suffered the same fate as the Chicago project as a whole—displacement by Columbia and Harvard just before, during, and especially after World War II (see Abbott, 1999, pp. 34–79; Turner & Turner, 1990, pp. 85–132). It is a complicated story, but for our purposes, it is enough to dwell on one source of that displacement—the interdisciplinary field of public opinion research that emerged in the mid-1930s.<sup>7</sup> Made up of sociologists, psychologists, and political scientists, and centered on new, sampling-based survey methods, this public opinion cluster took up the study of media-related questions, often at the request of private and public clients. Many of its key figures went on to assume posts in the federal government's World War II propaganda agencies and emerged from the war, improbably, at the center of empirical social science. From the beginning, the study of mass media was the opinion cluster's most pronounced topical research area—to such an extent, in fact, that the various communication-related labels, which emerged before and during the war, were often paired, or used interchangeably, with the “public opinion” moniker. But this was an accident of funding and world crisis and not the result of a conscious intellectual program or a received tradition of study. The field's mass communication focus was a straightforward outgrowth, rather, of media- and advertiser-sponsored research, Rockefeller Foundation intervention, and the federal government's wartime propaganda mobilization.

This all matters because it was the field of public opinion that dislodged the study of media, ironically, Chicago sociology. It placed the sociology of media on unsure footing, for two major reasons. As already noted, the field was dependent on the

ongoing interest of government, foundation, and commercial funders, which waned in the late 1950s and early 1960s. The other reason has to do with the field's particular, and arguably unsociological, intellectual coordinates. With roots in attitude psychology and market research, survey-based public opinion research at Columbia's Bureau of Applied Social Research and elsewhere was individualistic and often centered on the social psychology of short-run persuasion campaigns. Of course, important sociological side effects surfaced in the bureau's voting studies (Lazarsfeld, Berelson, and Gaudet [1944] and Berelson, Lazarsfeld, and McPhee [1954]), for example, or Merton's (1946) war-bonds study (see also Simonson, 2004). Although a strong case can be made that the two fields—public opinion and mass communication—are rightly interconnected, the sociology of media was narrowed thereby to the social psychology of persuasion. So when the funding did drop off, so did the basis for work of larger sociological interest. As we will see, that is when journalism schools stepped in.

### **Public opinion research displaces Chicago**

The widespread sense that the new public opinion research had replaced the earlier currents at Chicago and elsewhere was, in certain respects, confirmed by Rockefeller Foundation funding patterns. In the 1920s, the Laura Spelman Rockefeller Memorial (LSRM) had provided massive, unrestricted block grants for social science research, and a substantial portion of the \$21 million that was disbursed went to the University of Chicago and its social science departments (Buxton & Turner, 1992, p. 382). The LSRM merged with the Rockefeller Foundation in 1929, and by 1932, the Foundation had officially turned away from social science block awards to applied, project-oriented grant making. The Hawthorne researchers' Industrial Hazards Project, with its practical focus, was an early benefactor of the new applied orientation—as was, a few years later, Paul Lazarsfeld's Rockefeller-funded Office of Radio Research.

The marriage of media research and polling technique in the mid-1930s had everything to do with timing and funding.<sup>8</sup> These were the years in which an academic-commercial coalition arose with roots in attitude psychology, market research, and the refinement of sampling methods. It was the migration of these techniques to national issues and elections, in 1936, that consolidated the formation of this relatively coherent cluster of researchers. But crucial too were the handful of entrepreneurial social scientists—notably Hadley Cantril, Samuel Stouffer, Rensis Likert, and Lazarsfeld—who recognized and exploited the potential of surveys and polling organizations to transform empirical social science. Just months after the famous *Literary Digest* upset, the embryonic field gained a publishing vehicle in the *Public Opinion Quarterly*, which was to long serve as the field's formal crossroads.<sup>9</sup>

The way that the field defined public opinion, in terms of a measurable aggregate of individual attitudes, was a striking departure from the various conceptions that had, before the polls, been dominant. That loosely structured and largely European tradition of thought on public opinion and the press that had influenced Park was an important symbolic contrast for the new scientists of public opinion—but not much else, as its more historical and theoretic character was all but overlooked. Figures as diverse as Bryce, Tarde, Ferdinand Tönnies, A. Lawrence Lowell, Walter Lippmann, Dewey, and Wallas—all of them concerned with the fate of democracy in modern times, with more or

less doubt—were swept away by the new empiricists who, moreover, claimed the public opinion mantle.<sup>10</sup> In an even more direct way, the field defined itself in opposition to that European tradition, as embodied by Chicago's studies of collective behavior, with their processual, multilayered account of the emergence of the public and public opinion from crowd and "mass" formations.

The opinion field, as it formed, had a number of unusual features. The first and most obvious distinguishing trait was its intellectual fixation on method and technique to the effective exclusion of any overriding substantive concerns.<sup>11</sup> It also had no stable disciplinary base and depended for many years on the POQ for institutionalized coherence. A regional web of interpersonal friendships and contacts was, at least until the founding of the American Association for Public Opinion Research in 1948, the field's main adhesive. Its researchers addressed themselves to a diverse set of audiences, many of them outside the academic arena. Fellow scholars within the cluster, of course, were one important audience, as were the psychological and, increasingly, sociological disciplinary publics. But other major audiences included the clients and funders in the business and foundation worlds, along with the news media and the lay public, through opinion polling itself. Some of this audience diversity was a predictable byproduct of the field's abnormal resource base: Far less reliant on undergraduate student enrollments—or any university source for that matter—the researchers subsisted instead on corporate contracts, in-kind aid from market researchers and pollsters, the Rockefeller grants and, once the war began, federal government largesse.

The war was crucial here, since after the invasion of Poland in 1939, the Rockefeller Foundation essentially funded and deployed its network of public opinion researchers as a surrogate propaganda research bureau—before it was politically palatable for the Roosevelt administration to step in (Gary, 1996). At the same time, the foundation sponsored an important "Communications Seminar," populated by many of the same figures, which helped to establish opinion-related techniques at the center of wartime and postwar media research. Once the United States entered the war in December 1941, the Rockefeller projects were incorporated into rapidly expanding wartime bureaucracies (Gary, 1996). By the summer of 1942, most of the major government propaganda-related initiatives were in place and staffed to an astonishing degree by the main figures in the public opinion cluster. They emerged from the war with expanded ranks—many learned the survey techniques during their service—thicker personal networks, and a large measure of war-proven legitimacy. The opinion researchers' impact was most pronounced in sociology, where Stouffer's *American Soldier* volumes were greeted as important contributions—even by critics. The wartime and postwar publications of Lazarsfeld's Bureau of Applied Social Research, much of it media related but some of it focused on method, were widely touted.

A palpable sense of excitement greeted, at least for a time, the new survey techniques within postwar sociology. Lazarsfeld's media research, and the other opinion researchers' communication studies, were quite closely identified with the survey methods themselves. These methods, and their research shops (like Lazarsfeld's), would, somewhat surprisingly, establish themselves at the center of empirical sociology more broadly. It is in this sense that the postwar, interdisciplinary field of communication

research was, to a remarkable extent, indistinguishable from the elite core of American social science itself.

This elite was built on the foundation of wartime service—its infrastructural remnants, close personal ties, and nomothetic zeal—but fueled by the Cold War and the research needs of the new national security state (Gilman, 2003; Simpson, 1994; Solovey, 2001). There was also self-satisfaction over the near-term prospect of general, systematic knowledge, linked to the conviction that large and technically proficient research teams (often modeled on Lazarsfeld's Bureau) were the key instruments (see, e.g., Shils, 1948). The research was expensive and funds came from military agencies like the Office of Naval Research, affiliated institutes like RAND, the SSRC, and foundations—above all Ford, with its flush “Behavioral Sciences Program,” headed by opinion researcher and Lazarsfeld collaborator Bernard Berelson. It was, especially in the “behavioral sciences” heyday of the early 1950s, a tight-knit and interlocking group of “brokers,” as Crowther-Heyck (2006) calls them, who advised the major foundation and government funders. Many of these figures including Lazarsfeld, Berelson, Stouffer, and Likert—were veterans of the interwar public opinion cluster and continued to conduct (or direct funds to) communication research projects. Much of that communication study involved what was then called “psychological warfare” research, in the service of understanding effective propaganda (Glander, 2000; Pooley, 2008; Simpson, 1994).

For our story, the two important points are that communication research after Chicago, sociological or otherwise, was in many respects an accident of funding and geopolitics. The social scientific study of media, moreover, took on the rather narrow intellectual coordinates embraced by survey researchers (and supported by their succession of clients)—the social psychology of short-term persuasion. For all the postwar confidence, the sociology of media was in fact on wobbly intellectual and institutional ground. It was soon enough that key participants were reading the field's last rites.

## **Journalism schools to the rescue**

Berelson's (1959) eulogy for communication research is famous for its ill temper: “My theme is that, as for communication research, the state is withering away” (p. 1). In the article, Berelson outlines four major approaches to the field—those of Harold Lasswell, Paul Lazarsfeld, Kurt Lewin, and Carl Hovland—only to declare that all four are “playing out” (p. 4). Lewin is dead; Lazarsfeld, Hovland, and Lasswell have moved on to fresh interests. Not only are their innovations “wearing out” but also “no new ideas of comparable scope are appearing to take their place” (p. 4).

In his reply, published in the same issue of *Public Opinion Quarterly*, a wounded Wilbur Schramm (1959) cites his own frenetic day as evidence for the field's vitality. Schramm writes that he had just returned from a doctoral exam, had lunch with a pair of professors, and was off to attend a seminar with “scholars from eight countries.” Adds Schramm: “On the way to my office, just now, I was waylaid by an eager young research man who wanted to tell me of a new finding he has made concerning the messages that are received beneath the threshold of conscious perception” (p. 7). Yes, the “founding fathers” were “truly remarkable” but not all has been quiet in their

footsteps. Indeed, their greatness may lie, not in what they did, but in “what they got started” (p. 7). In place of Berelson’s withering state, Schramm proposed a new metaphor—communication as a great crossroads where “many pass but few tarry” (p. 8). Berelson’s corpse, to Schramm’s eyes anyway, seemed full of life.

Our hunch is that they were both right. Berelson’s field was withering away, even as Schramm’s days were getting busier. Communication research, to Berelson, was that interdisciplinary field populated by the postwar social scientific elite, linked together by methodological rigor, foundation support, and Cold War government service. This remarkable movement of interdisciplinary social research had, by the early 1960s, come to a gradual halt. The reasons are many, but one stands out: Funding for social science in this period shifted markedly in ways that undercut the field. As Crowther-Heyck (2006) recounts in much more detail, postwar social science experienced two distinct, successive funding regimes. The first, as we have seen, involved private foundations, the SSRC, and a series of military research agencies. Here, the emphasis was interdisciplinary, guided by the conviction that “basic” and “applied” research was inseparable and mutually beneficial. In this first regime, key “brokers” like Lazarsfeld worked on overlapping advisory panels and more informal networks to distribute funds. Berelson’s behavioral sciences program at the Ford Foundation epitomized this first funding regime and disbursed about \$40 million dollars from 1951 to 1957 (Crowther-Heyck, 2006, p. 437).

It was the response to Sputnik in 1958 that set the second postwar patronage system in motion. For a few years—until the early 1960s—the two funding patterns overlapped. But already in the late 1950s a new, far more prominent role was given to civilian federal agencies, including the National Science Foundation (now expanded to the social sciences) and the National Institutes of Mental Health (NIMH). This second funding regime emphasized, in practice, disciplines and specialized knowledge and increasingly hewed to a formal distinction between “basic” and “applied” research (Crowther-Heyck, 2006).

The conditions that had supported the behavioral sciences moment—interdisciplinarity, the conscious merger of applied and basic inquiry, the informal networks of brokers, and Northeastern U.S. foundations—all these were gradually displaced by the second, far less hospitable funding system. The fate of communication research was, as we have seen, closely tied to the fortunes of the behavioral sciences more broadly. Those fortunes were fast receding. That communication research as such was never an intrinsic interest of most of the scholars themselves made matters worse still.

A subplot within this wider story helps illustrate the decline we have just outlined and involves a classic Cold War irony. As Morrison (2008) details, Ford in 1955 pulled out of a major commitment to fund a series of television studies at Lazarsfeld’s Bureau, even though the project was well underway. Attacks on Ford and other “fellow traveling” foundations from Congressional McCarthyists in 1952 and, notably, 1954’s Reece Committee, prompted the Ford trustees to back out of the potentially controversial Bureau television project (Morrison, 2008). Were it not for the McCarthyite stirring of Cold War waters, a whole body of television research might have found its ways onto sociologists’ bookshelves to complement Lazarsfeld’s pioneering radio research. As it was, the first major book on television was written years later—and by a British

psychologist at that (Himmelweit, 1958). More of the same—right-wing congressional assaults on the “collectivism” and “amorality” of social scientists and their funders—led the Ford trustees to shutter’s Berelson’s Behavioral Sciences Program altogether in 1957.

Yet another index of the decline was especially close to Berelson. The cross-disciplinary Committee on Communication, which he had helped found at the University of Chicago in 1947, closed its doors shortly after the *POQ* exchange (Wahl-Jorgensen, 2004, pp. 557–560). The irony here is that, in some respects, the thinking that emerged from the Committee had been rich and heterodox—an echo, perhaps, of the prewar Chicago tradition.<sup>12</sup>

Like nearly all interdisciplinary initiatives, the behavioral sciences movement was fragile, reliant on the uninterrupted commitment of its funders. That commitment got interrupted, so it’s no wonder that Berelson saw a corpse.

Berelson’s death sentence struck Schramm as premature because his communication\ research—the sort establishing itself within professional schools of journalism across the U.S. Midwest and beyond—was just getting started.<sup>13</sup> By 1959, Schramm had labored for over 15 years to seed journalism schools with Ph.D. programs in “mass communication” and with gathering success. Conferences, institutes, book-length readers, doctoral tracks, symposia: all of it “social capital” in Oakes and Vidich’s (1999) sense, invested with the intent to institutionalize the young field. Schramm and his fellow “chi-squares,” as the social scientist colonizers of journalism schools were sometimes known, embraced the quantitative social psychology of the behavioral sciences, even as the interdisciplinary movement itself faded (There was, in one ironic respect, a literal borrowing from that 1959 exchange: Schramm [1963, 1980, 1983, 1985, 1997] adapted Berelson’s four-fathers construct as an origin myth for his aspiring discipline, in a succession of “four founders” histories.) Thanks to Schramm and his allies, journalism schools across the country adopted the Journalism & Mass Communication moniker, and made room for the orphan science of communication. Hence Schramm’s busy day.

Political science, and especially psychology, were better adapted to the new, post-Sputnik funding regime and more closely aligned, anyway, with the behavioral sciences coordinates that the interdisciplinary field had passed along to journalism schools. Many psychologists, in particular, remained active participants in the federally funded media research of the 1960s and 1970s, much centered on violence and funded by the NIMH (Rowland, 1983, pp. 116–170). The fact that the NIMH was pouring money into medical sociology in these same years helped create one of the discipline’s largest subfields (Bloom, 1986)—pulling at least some would-be communication researchers into its well-off orbit. Freidson (1953, 1954), a Chicago graduate student in the 1950s, published a pair of important articles on communication research, but unable to secure a communication-related sociology post, opted for medical sociology instead.<sup>14</sup>

## **The social psychology of persuasion**

One consequence of this handoff to the journalism schools is that American sociology, more decisively than psychology or political science, abandoned the study of mass

communication. Flourishing from the mid-1930s to the early 1960s, the interdisciplinary communication field had, after all, conceived of social research in a narrow and (truth be told) unsociological manner. With roots in attitude psychology and market research, the field's survey-based social psychology was individualistic and often centered on short-run persuasion campaigns. This orientation did not bother sociologists like Lazarsfeld and Stouffer, who had helped locate survey methods and campaign studies at the core of postwar sociology. Still, when funding for their brand of interdisciplinary social science dried up, there was little reason to preserve its psychological approach at the heart of the discipline. Sociologists who had been doing survey-based communication research moved on—or were hired by the journalism schools.

Take Lazarsfeld's media research program, which, within the much broader Bureau agenda, concentrated on short-term persuasion. This reflected, in part, Lazarsfeld's interest in decision making, but also the fact that the Bureau's clients, the wartime government and various commercial firms, wanted only this kind of information. Lazarsfeld maneuvered adroitly within these confines—designing the research in such a way that it would yield academic results of interest. It's also true that a number of impressive studies (including Lowenthal, 1944; Herzog, 1944; Arnheim, 1944; and Berelson, 1949) emerged from the Bureau in the 1940s wide-ranging media sociology with few debts to the short-term persuasion framework. But the fact remains that the Bureau's research findings were often centered on short-term campaign effects and summarized (in Klapper, 1960; Katz & Lazarsfeld, 1955; and elsewhere) as concluding that media have only “limited effects” (see Pooley, 2006b).

### **Was limited effects limiting?**

Communication research was, in short, tethered to a narrow (and questionably sociological) approach—fixed on the short-term social psychology of persuasion, which in mass-mediated form, moreover, turned out to be ineffective. It is this last point that Kurt Lang stresses. Although he is well aware that the findings of limited effects derive from the study of media “campaigns,” aimed at changing opinions, attitudes, and actions in the short run, Lang considers the finding discouraging (personal correspondence, 2007; also see Lang & Lang, 2006, p. 172). He believes that other scholars turned away from media research for this reason. If true, this is ironic. First of all, for the reason that Lazarsfeld himself had a much broader map of media effects in which he was interested, ranging from the influence of *Uncle Tom's Cabin* on the Civil War to the influence of soap opera on housewives (Lazarsfeld, 1948; also see Katz, 2001). Nevertheless, Lazarsfeld himself seems to have been discouraged, or distracted, from the study of the media at more or less the same time.

A second response to Lang's critique stems from the fact that the Bureau's continued exploration of the reasons for limited effects led to the idea of audience “self-selection” in the process of communication and to the idea that the influence of the media was itself mediated by interpersonal influence circulating in the small groups in which individuals are embedded. The power of these “intervening variables” redirected media research to the study of what is now known as “reader reception” and “social networks.” Specifically, the idea of “selectivity” coincided with so-called “gratifications



research,” in which the balance of power is partially transferred from medium to receiver and from there to “reception.” In parallel, the so-called “two-step flow of communication” (Lazarsfeld, Berelson, & Gaudet, 1944; also see Katz, 1957) led to a search for the links between mass media and interpersonal networks in the study of diffusion. The frustration of limited effect, in other words, proved to be creative, but sociologists did not seem to have the patience to wait. And hardly anybody seems to have remarked that campaigns were not the only place to look for media effects (Katz, 1989).

Perhaps also the historical contrast that Katz and Lazarsfeld (1955) had proposed— between prewar, intuitive research that had clung, mistakenly, to a picture of “powerful” effects, and the Bureau’s own more scientific finding of limited effects had served to blot out (or at least distort) the interwar Chicago contribution in the field’s memory of itself. Even the Bureau’s more diverse body of 1940s media research is, arguably, overwhelmed by the mnemonic boundaries of the limited effects construct (see Pooley, 2006b).

## **Conclusion: Prospects for a revival**

So why did American sociology abandon mass communication research? How is it that the discipline committed to a holistic understanding of modern life has long neglected one of modernity’s central institutions? Our speculations so far, in these two companion papers, relate to possible answers that are both external and internal to mainstream sociology.

To summarize: (a) We see Chicago sociology as heir to the rich but scattered reflections on communications and the media that characterized European thought. At Chicago, as in Europe, interests were broad: media professionals and media organizations, media as agents of social integration and deviance, media as contributors to a public sphere of participatory democracy, and media as implicated in social change and in the diffusion of ideas, opinions, and practices. (b) We think that this sophisticated approach to the study of public opinion and communication was supplanted, almost inadvertently, by survey-based opinion research and its behavioral sciences successor— neither of which was ever truly rooted in the discipline. Although survey methods were espoused temporarily at Columbia and Harvard, they also took root in the world of academic/commercial survey research and, later, in rejuvenated schools of journalism and communication. (c) These relocations coincided with a changed focus on media as agents of short-term persuasion and opinion change. (d) We associate this new focus with the methodological innovations in the social psychology of survey research and to the support for this work from government, business, and foundations interested in the power of the media (especially radio) before and during the Second World War. (e) We agree that the much-touted limited effects of media campaigns were mistakenly generalized to media research in general and led to Berelson’s (1959) obituary and to Gans’ (1972) proclamation that there was a “drastic famine” in media research “with no signs of abating” (p. 697). (f) Interdisciplinary initiatives, we suggest, are inherently unstable and rely on the ongoing good graces of outside funders. When the funders drifted off in the late 1950s, the whole

interdisciplinary enterprise could hardly stay afloat. Wilbur Schramm sent out a flotilla of lifeboats, and mass communication research sought refuge in legitimacy-hungry journalism schools. There was a waning of communication research by sociologists, even at Columbia's Bureau and elsewhere.

## **Feast or Famine?**

There are indications, decades later, that sociology is finally recovering from that famine, but we remain skeptical (if not resigned). It is true that there is, at present, evidence of renewed interest in sociological study of mass media. But for media research to take root again in the discipline, would-be media sociologists will need to clear a major organizational hurdle: established programs in "communication," housed in former journalism schools and elsewhere in the university.

As a rule, subfields lost are very hard to lure back. This is because subfields require not just self consciousness and a shared set of topics but also much more mundane goods: core journals, for example, or divisional status within a scholarly association—perhaps also required courses to teach and a textbook market to service.<sup>15</sup> Without this sort of scaffolding, subfields eventually die off. Why, then, have sociologists not built up the infrastructure over the years, despite occasional upswings of sociological interest in media? We propose that communication schools and programs are the reason. "Communication" became "communication," as we have seen, when journalism schools (and speech departments) picked up the slack that social scientists let drop. Ever since, the organized field of communication has exerted a gravitational pull on sociologists of media, not least because of plentiful and well-paid faculty jobs funded by enormous cohorts of undergraduate majors. Most U.S. sociologists who have maintained an interest in media—such as Charles Wright, Melvin DeFleur, and more recently Michael Schudson, Vincent Mosco, Todd Gitlin, Sandra Ball-Rokeach, Muriel Cantor, and Rodney Benson—have taken up residence in communication programs. Some maintain ties to sociology, it is true, but nearly all spend their time teaching communication Ph.D. students. Sociology graduate students are not even in the same building.

Consider, too, the contrast: Those sociologists of media who maintained residence in sociology programs—most of them anyway<sup>16</sup>—have more or less stopped doing media research. If we are right, the magnetic pull of communication programs is to blame here as well. What kind of conversation can you carry on with colleagues who have left for a new field and who publish in their own journals? The internal demands of sociology, for peer recognition and its rewards, are powerful too and pull the left-behind toward established subfields that do have students, conferences, collaborators, and research dollars.

You might imagine that the contributions of communication scholars (by trained sociologists or otherwise) would find their way back to sociology proper. But few disciplinary sociologists are familiar with the literature of communication studies. Here, communication's legitimacy problems play a role, and the irony is that the field's low status has everything to do with its abundant resources (cf. Peters, 1986). The field's roots in, and ongoing commitment to, vocational training is what attracts all those

students, but it is also the source of the field's status problem. Call it the tyranny of the undergraduate professional major: resource-rich, but legitimacy-poor, scholars of communication toil away in well-heeled obscurity. Sociologists do not read communication research, and so have even less reason to invest themselves in media questions.

All this may help explain the otherwise-baffling course that the sociology of culture subfield has taken over the past 30 years. For the most part—and with one major exception, which we discuss below—this flourishing area of sociology has taken up every facet of expressive culture except the mass media. Art, fashion, academic publishing, orchestral music, the theater, and religious practices, all these have long occupied leading sociologists of culture like Howard S. Becker, Richard Peterson, and Paul DiMaggio. But only rarely are media institutions the object of study, and even when they are—as in Peterson's (1990, 1997) research on the record business or Long's (2003) study of book clubs—the work tends to be cut off from related scholarship coming out of communication programs. So sociologists and communication researchers seem to have divided up the labor of studying “culture,” and when their work overlaps they remain in silo-like ignorance of one another. The single step with the greatest potential to help along the ascent resurgence, we suggest, would be to bring the sociology of culture subfield into conversation with communication scholars.

Our hunch is that the divide, once again, is an unintended consequence of the handoff to the journalism schools. By the time the sociology of culture had developed as a self-conscious subfield in the 1970s, media research was already turf claimed by communication studies. Even so, a major strand of the sociology of culture did take up media questions. There was, recall, a remarkable burst of sociological interest in journalism from the mid- to late 1970s, led by Molotch and Lester (1974, 1975), Tuchman (1978), Gitlin (1980), Schudson (1978), Gans (1979), and Fishman (1980)—but its equally dramatic falloff is what is telling. Some of the figures—Gitlin and Schudson, for example—were drawn into the orbit of communication programs. Molotch, Tuchman, Gans, and Fishman all stayed in sociology and drifted (or returned) to other subfields within the discipline. No one was left to reproduce the next generation of media sociologists.

The contrast with the British case is revealing. In the United Kingdom, media research was never housed in professional schools of journalism—there were not any—and instead emerged in the 1960s in various strands, one with literary roots (at Birmingham) and another self-consciously sociological (at Leicester; Redal, 2008). As the field spread in the 1970s, it was more often than not in sociology programs or sociology inflected research institutes, at Leeds, Glasgow, Liverpool, and elsewhere (Lodge, 2008)—and even the Birmingham cultural studies project, as it evolved over the 1970s, took on a heavily sociological (and theoretical) cast (Schulman, 1993). The British media studies scene has grown far more complex since the 1980s—the rapid growth of media and cultural studies programs (especially at the polytechnics) has produced resource-based and intellectual tensions with disciplinary sociology. But it remains true today that many leading British media researchers identify as, and are housed within, departments of sociology (see Webster, 2001; Wolff, 1999). It is certainly not possible to imagine a schism—communication research on the one side, sociology on the other—like the one that obtained in the United States. There simply were not

preexisting journalism programs to colonize, so the American pattern of professional school self-segregation was not an option.<sup>17</sup>

## Future prospects

There does, for all that, seem to be a mild resurgence of sociological interest in media study in recent years. The media-themed sessions at the 2007 American Sociological Association meeting were, for example, well-attended and lively. Sociologists with appointments in sociology departments are publishing important books, including Gamson (1994, 1998), Klinenberg (2007), Eliasoph (1998), Starr (2004), and Grindstaff (2002). Though promising, the trend is still embryonic and has not taken root in any organized way. Sociologists-in-exile like Schudson, Katz, and Benson, joined by non-sociologists like the late Timothy Cook, Daniel Hallin, Robert Entman, and Lance Bennett, still produce far more sociology of media than do traditional departments.

If the resurgence gathers real strength in the years ahead, however, it will be the Internet and digital culture that draw in a critical mass of sociologists. The study of media is thrown into flux every time a major new medium arrives on the scene, and the disruption this time has been creative. With no established jurisdictional claims, an interdisciplinary field of internet research—intersecting with science and technology studies—has flourished, and sociologists are very well represented (Elesh & Dowdall, 2006). In 2002, the ASA's "Sociology and Computers" section changed its name to "Communications and Informations Technologies," and the section is growing rapidly. Perhaps the famine is already letting up.

## Notes

1. This article is the second of two exploratory papers on the topic; the other will appear as Elihu Katz, "Why Did Sociology Abandon Mass Communication Research?" in *The American Sociologist* (2009). The present article, overlapping with the first, includes new sections and editing by Jefferson Pooley. Strange as it sounds, the authors differ on certain points in the present article. They plan to expand these initial, speculative papers into a longer research article in the future—hoping, at least, to convince one another.

2. If sociology and mass communication research have parted ways, the fact of their estrangement has made it hard to reconstruct their once-interwoven history. This is because the two fields remember their pasts—the portions that are ostensibly shared—in distinct ways. The result is two accounts of a common past that have little in common. The "Chicago School" of sociology, for example, looms large in the memory of both fields, though in a patterned way. Sociologists' treatment of the pre-World War II Chicago department is often rich and sophisticated but neglectful of the School's research and reflection on communication (Abbott, 1999; Bulmer, 1984; Matthews, 1977). Browse the communication literature, however, and you get the impression that Chicago personalities such as Robert E. Park thought about nothing else (Belman, 1975; Carey, 1996; Czitrom, 1982). Within communication studies, moreover, the School has been deployed as a proxy for the post-1970s interpretivist wing of the field, associated with the late James W. Carey. That is a partial view, of course, though not an uncommon one. As Abbott describes in his excellent history, the idea of the Chicago School was a retroactive creation of the early 1950s, when the department briefly embodied the traits it projected onto its past: "Meadean, dogmatically qualitative, and perhaps even dogmatically ethnographic" (p. 14). The prewar department was, by contrast, far more complex and eclectic—a "melange of contradictory viewpoints," in Becker's (1999) phrase (p. 8). See Pooley (2007) for a short treatment of the Chicago School's place in the memory of communication research.

3 The best English-language overview of the German tradition is Hardt (2001). See also Lang (1996) for a pan-European survey; and Nye (1975) for a superb reconstruction of the French scene.

4 On Park and Dewey, see Raushenbush (1979) and Matthews (1977).

5 Tarde (1901), who made the same contrast earlier in *L'opinion et la foule*, defines crowd and public along somewhat different lines than Park. Still, *L'opinion* is a clear influence on Park's dissertation.

6 The diverse and fecund series of book-length studies by leading social scientists, published in the early 1930s with support from the Payne Fund, have been neglected, or else caricatured, by media researchers since—in part because the studies were organized, and then popularized, by anti-Hollywood moral crusaders. For a fascinating history, including previously unpublished work by Cressey, see Jowett, Jarvie, and Fuller (1996), as well as Buxton (2008).

7 See Pooley (2006a) for an attempt to sort through public opinion research, Lazarsfeld and the evolution of media study (pp. 179–299).

8 The best history of public opinion research before, during, and after the war is Converse's (1987). Platt (1996), in her broader history of social science methods, provides an excellent narrative as well.

9 Founded by the Princeton political scientist Harwood Childs with Cantril's help, its editorial board and its first-volume roll of authors included leading figures from the worlds of polling and market research, empirical sociology, psychology, and, to a lesser extent, political science—the figures, many already linked, who would soon form the small world of public opinion research. Among the contributors and board members: Floyd Allport, Harold Lasswell, Bruce Lannes Smith, Elmo Roper, Archibald Crossley, Harold Gosnell, George Gallup, Harry Field, Clyde Hart, Daniel Katz, as well as Lazarsfeld, Cantril, Stouffer, and Likert (Davison, 1987).

10 Berelson's (1956) state-of-the-field article—written while still director of the Ford Foundation's Behavioral Sciences Division—is a somewhat startling, muscular defense of the older thinkers' consignment to the mnemonic dustbin. Berelson celebrates their supersession as one of seven stages on the way to full behavioral science status. With unblushing Whiggism—and using “scholarship” as a pejorative—he writes that “[t]wenty years ago the study of public opinion was part of scholarship; today it is part of science.” His catalog of the field's progress is somewhat jarring to our rather less confident ears: There has been, he writes, “revolutionary change in the field of public opinion studies: the field has become technical and quantitative, theoretical, segmentalized, and particularized, specialized and institutionalized, ‘modernized’ and ‘groupized’—in short, as a characteristic behavioral science, Americanized” (Berelson, 1956).

11 The methods are those of “survey research,” though the term is anachronistic and was first applied in its field-defining sense only after the war (Indeed, Lazarsfeld claimed credit for the term.). Included under the “survey” umbrella were various interview techniques, a mix of structured and nonstructured types, usually but not always using sampling and scaling procedures, and almost always including standard socioeconomic status and other demographic variables. In market research and media-based surveys, quantitative content analysis—as developed by Lasswell but refined by many others including Berelson—was a typical supplement. The point is that these scholars shared a body of methods united by nonexperimental survey techniques of a generally quantitative character. There was a belief, moreover, among the researchers that the methods constituted a coherent bundle.

12 The Committee was established as a loosely federated group of faculty mobilized, initially, by Douglas Waples, Dean of the Library School, known for his research on reading. Kenneth Adler, later of the USIA, coordinated the group, which was augmented by David Riesman—who introduced the study of leisure into the mix—reinforced by a coterie of newly minted Ph.D.s from Columbia. They included Rolf Meyersohn and Philip Ennis, who worked with Riesman, as well as James Coleman, who was marginally involved, and one of the present authors (Katz). Other Columbia novitiates also arrived in Chicago at the same time—notably Peter Rossi, who would soon head the NORC, and Peter Blau, but they were not part of the communications committee. Neither were the old-timers, except perhaps for Donald Horton, Nelson Foote, and Reuell Denney. The Committee began publication of *Studies in Public Communication*, while Berelson and Janowitz (1950) produced the first of a number of editions of their classic, *Reader in Public Opinion and Communication*, as the department was exporting brilliant graduate students elsewhere—inspired by Everett Hughes and Morris Janowitz. These included Erving Goffman, Howard Becker, Gaye Tuchman, Herbert Gans, and the Langs.

13 The best analysis of this institutionalization remains Peters' (1986) classic article, "Institutional Sources of Intellectual Poverty in Communication Research." But the field's institutional history remains strikingly understudied. There are bits and pieces of such a history in various accounts, especially Delia (1987), Dennis (1996), Chaffee and Rogers (1997), Weaver and Gray (1980), Fish's (1984) underappreciated dissertation, Cartier (1988), King (1990), Katzen (1975), and Sproule (2008).

14 Freidson (1978): "As a graduate student in sociology at the University of Chicago, I became involved in exploring the problems of understanding the effect of mass communications on audiences, and the social character of language and symbolism. By the time I collected my data and looked for a job, however, those topics were singularly without any academic market value." After a pair of postdoctoral fellowships and a withdrawn offer from the Russell Sage Foundation, Freidson moved into medical sociology, where he made his career (pp. 118–119; J. Pooley, personal communication, March 2003).

15 We have in mind here Whitley's (1974) contrast between "cognitive" and "social institutionalization."

16 The Langs, William Gamson, and his son Joshua Gamson, are notable exceptions.

17 One irony is that, when "cultural studies" jumped the Atlantic, the interdisciplinary project found a hospitable home in communication departments, as well as various programs in the humanities.

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