CHAPTER TWO

THE New History OF Mass Communication Research

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> The full story of mass communications research still waits to be written.
>
> —R. L. Brown (1970)[1]

Until recently, the extant literature on the history of mass communication research has been notoriously sparse, celebratory, and methodologically naïve. This chapter traces, and attempts to explain, a marked shift in that history over the past decade—from an airbrushed and unapologetically Whiggish rendering of the field's past to a rigorous, contextualist treatment with an altogether different story to tell.

This eruption of revisionist history resembles, in many respects, the historiographical self-scrutiny of other social science disciplines. The chapter's title, indeed, is an allusion to Robert Alun Jones's classic paper on "The New History of Sociology." That essay was published in 1983, and it documents a decade-long surge of accomplished historical work informed, Jones shows, by an engagement with Thomas Kuhn, Quentin Skinner, and debates in the history and sociology of science. Although the timing and contours varied by discipline, the other social sciences all experienced similar waves of critical historiography, beginning in the late 1960s or, more often, the early 1970s. By the early 1980s, when Jones wrote his survey, each discipline—sociology, political science, anthropology, and psychology—had attracted a small community of serious historians with, in many cases, subdisciplinary trappings like journals, archives, and divisional status within their fields' scholarly associations. These "new history" subdisciplines, owing to their post-Kuhnian intellectual coordinates and newly won autonomy, each produced a set of studies that lanced their discipline's self-serving origin myths.

These "new histories" emerged from the much broader tumult that had been reverberating around the social sciences since the late 1960s. This complex and overdetermined unrest reflected flagging confidence in the postwar social scientific elite, with its cocksure scientism, cold war liberalism, and federal government entanglements. Across the social sciences, youthful insurgent-scholars highlighted the gap between postwar social scientists' self-description as neutral observers, and their actual works' implicit support for the status quo. Self-identified radical factions formed in most of the social sciences, linked to the student New Left. This political radicalism overlapped, though unevenly, with a methodological backlash against pretensions to natural science status—often branded with the loose catchall "positivism"—in favor of various reflexive theories of knowledge.

The discipline-by-discipline leftist insurrection, together with the wars over method, lay the groundwork for the "new history" in a straightforward way: the effort to show that their disciplines' scientistic and progressivist self-descriptions were false required historical counter-narratives. In a related sense, the genealogical impulse was helped along by the state of upheaval itself—self-scrutiny often follows the disruption of taken-for-granted disciplinary norms. The skeptical and contextualist turn within the sociology and history of science fields, which reflected and contributed to the wider tumult, also fed the "new historians'" challenge to textbook fables and graduate seminar yarns.

Communication research experienced its own, now-notorious "ferment," albeit a few years later.[2] Many of the same political and methodological currents coursed through the communication research of the period. But unlike the other social sciences, there was, for communication, no real historiographical counterpart to the discipline's own "critical turn." There was some critical history, to be sure—exemplified by Todd Gitlin's 1978 "Media Sociology: The Dominant Paradigm." But Gitlin and other critical researchers largely adopted the standard history of the field, though that history was recast in negative terms. These were thinly sourced polemics, and not the kind of rigorous histories that Jones labeled "new."

This chapter asks the question: why did mass communication research fail to produce a body of contextualist history until the mid-1990s—more than twenty years late?

THE HISTORY OF COMMUNICATION RESEARCH, OLD AND NEW

Communication study in the United States, jury-rigged from the scraps of journalism schools and speech departments in the years following World War II, has from the beginning suffered from a legitimacy deficit. The would-be discipline,

in its newly institutionalized form, was flush with the resources that other, more established disciplines covet—research funds, students, and faculty jobs, all in abundance. But the field lacked legitimacy, and this threatened all of its material riches. Communication studies scrambled to justify its very existence—faced, as it was, with a kind of cultural lag. Its institutional gains had far outpaced its status.[3]

A body of disciplinary history was drafted, in the early 1960s, to carry some of this legitimacy burden—at least for that portion of the discipline that evolved from journalism schools. (It is an index of the field's schizophrenia that the speech- and journalism-derived traditions have developed distinct disciplinary histories, both of which, however, claim to represent the field as a whole.)[4] The mass communication field, busy colonizing journalism schools, had mnemonic entrepreneurs like Wilbur Schramm who took scraps of memory lying about in the postwar social scientific landscape, and assembled these into a coherent, and self-validating, narrative. This history was translated into a standard textbook formula soon after, and propagated without serious challenge for decades. Students of communication studies well into the 1990s were reared on its plot. Most still are, today.

The standard history has two strands, one lifted from postwar media sociology and the other self-consciously narrated by Schramm. The first was constructed by sociologists, notably Paul F. Lazarsfeld, affiliated with the Bureau of Applied Social Research (BASR) at Columbia University—who created a rather flimsy but exceptionally durable straw man with which to contrast themselves. The Bureau researchers, as crystallized in the enormously influential account offered in the first chapter of Elihu Katz and Lazarsfeld's culminating work, *Personal Influence*, presented their prewar scholarly predecessors as naïve, impressionistic, uninformed amateurs who mistakenly clung to a "hypodermic needle" or "magic bullet" theory of media influence—and who, what's more, were under the spell of European "mass society theory," itself an influential straw man construct.[5] This remarkably resilient caricature of prewar influence was contrasted with the scientific, methodologically sophisticated (and reassuring) "limited effects" conclusions of the Bureau.[6]

The second strand was a self-conscious creation of Schramm, a consummate academic entrepreneur who was almost single-handedly responsible for the mass communication field's institutionalization.[7] First elaborated in 1963, Schramm's genealogy credits the discipline's plucky emergence to four pioneers—"founding fathers," he labels them.[8] The text, though, renders the anointment in the passive voice ("Four men have usually been considered the 'founding fathers'. . .")—an act of audacious creativity that comes off as mere reportage.[9] Kurt Lewin, Carl Hovland, Harold Lasswell, and Lazarsfeld himself—two psychologists, a political scientist, and a sociologist, all eminent bearers of scholarly capital—are invoked

as predisciplinary forerunners of communication studies. Schramm's was a kind of involuntary draft: though these figures worked on "communication"-related problems at least occasionally, none would recognize himself in Schramm's communication pantheon—each had either died or moved on to other questions by 1963. Still, here they are; and if these four giants left distinct lineages, then happily their boundaries have since become porous: "These four strands of influence are still visible in communication research in the United States," writes Schramm, "but increasingly they have tended to merge."[10] Current "practitioners," for example, conduct "quantitative, rather than speculative" research—a legacy of the four founders.[11]

Like the account offered up by Katz and Lazarsfeld, Schramm's founders story is taut and Whiggish. The essay, which he was to publish in revised form at least five more times over the next three decades, is an unabashed origin myth.[12] It is neither history nor systematics, but something cartoonish in between—legitimacy on loan. The essay contains not a single footnote; its focus on convergence and recent institutional gains would make Herbert Butterfield blush. And like the *Personal Influence* history, Schramm's narrative was embraced by an insecure and newly institutionalized field.

By the mid-1960s, the two chronicles had merged, awkwardly, to form a single mnemonic stream: a powerful-to-limited-effects emplotting, welded to an equally upbeat forerunner-to-maturity narrative. Together, Katz, Lazarsfeld, and Schramm had furnished mass communication studies with a disciplinary memory—with a past that was eminently usable. The storyline supplied glue to a field with bricks but no mortar.

A challenge to this standard history did emerge in the 1970s and early 1980s, mounted by various "critical" strands of media research then ascendant. These critical currents, taken together, roughly paralleled the upheaval of the other social sciences in this period. But those other social sciences produced sophisticated revisionist history and emergent subdisciplinary communities; mass communication research did not. Indeed, the historical rethinking ushered in by the field's critical upsurge is notable for its embrace of the standard storylines—which are merely renarrated in a muckraking mold, but left otherwise intact.[13]

The agenda-setting text in the critical narrative was Todd Gitlin's 1978 "Media Sociology: The Dominant Paradigm," a scathing dismissal of the Lazarsfeld legacy and its "limited effects" conclusions. Gitlin attributed the Lazarsfeld circle's "limited effects" conclusions to its dependence on market research and especially media firm sponsorship, on the theory that Lazarsfeld and his Bureau colleagues were telling the media barons what they wanted to hear, that mass media exposure is harmless. Though the essay has other problems,[14] the crucial misstep is that it

accepted Katz and Lazarsfeld's own *Personal Influence* self-description—and their "powerful effects" contrast too—so that his account rests on rickety and misleading foundations from the start. Katz and Lazarsfeld, wrote Gitlin,

> conceptualize the audience as a tissue of interrelated individuals rather than as isolated point-targets in a mass society ... As a corrective to overdrawn 'hypodermic' notions, as a reinstatement of society within the study of social communication, the new insistence on the complexity of the mediation process made good sense.[15]

Gitlin reproduced the mythical "hypodermic needle" periodizing, and also the Katz and Lazarsfeld tradition's self-description as pioneers of the "limited effects" finding. He merely adds a third stage, in terms evocative of the Christian typology of Eden, the fall, and the second coming: Lazarsfeld's limited effects as a necessary interregnum before a higher and better critical paradigm emerged. Here history is being used as a weapon in paradigmatic succession. Though there are hints of an externalist sociology of knowledge approach, there is in Gitlin's influential paper very little actual historical digging.

Many of the highly charged essays in the "Ferment in the Field" special issue of the *Journal of Communication* (1983) made historical claims akin to (and often rooted in) Gitlin's account. Here again, the treatments largely mirrored the mainstream narrative, which is set up as an easily toppled contrast to emerging critical researchers.[16] So too with the nascent 1970s British sociology and cultural studies of media, which constructed *its* identity against the "American effects tradition."[17]

In the same period, a number of notable attempts at retelling the story of the field's origins, in whole or in part, were made outside of these critical currents. Kurt and Gladys Lang, Willard Rowland, David Morrison, Daniel Czitrom, and Jesse Delia: each drafted more or less serious accounts of the discipline's history, yielding some genuinely new insights. But these histories, too, stopped short of challenging the "limited effects" storyline that Katz and Lazarsfeld had so effectively narrated.[18]

ENTER THE NEW HISTORY

In the late 1980s and early 1990s, a new, growing body of critical history began to undermine the received narrative. This barbed, debunking history replaced the older version's internalist methods and progressive narrative with a robustly externalist approach. Drawing on the archives of the Rockefeller Foundation, on the files and letters of key postwar figures including Wilbur Schramm and Paul Lazarsfeld, and on various materials from the National Archives and documents secured through the Freedom of Information Act, this cluster of "new historians"

constructed an alternative narrative of communication study's genesis—one that stresses the conditioning role of Rockefeller, military, Central Intelligence Agency (CIA), and State Department funding, and also the tight interpersonal network of future communication scholars that coalesced during their common wartime government employment as overseas and domestic propagandists.[19]

There are six figures, with diverse backgrounds from American studies to the sociology of knowledge, who have stitched this new history together: Christopher Simpson, an investigative journalist; Timothy Glander, an education professor; Rohan Samarajiva, a development communication scholar; Brett Gary, a professional historian; William Buxton, a German-trained sociologist of knowledge; and J. Michael Sproule, whose roots are in speech communication. The six scholars, with an accidental and fortuitous division of historical labor, chronicle the emergence and consolidation of the mainstream effects tradition, in ways that differ strikingly from the dominant narratives I trace above.

Their histories tend to be methodologically reflective, though they distinguish themselves most by their archival digging; in contrast to earlier narrators of the field's past, the new historians have dirtied their fingernails. What they have dug up is that the mainstream effects tradition was crucially shaped, in the mid-1930s, by the Rockefeller Foundation's interest, first, in educational broadcasting and, after 1939, in anti-Nazi propaganda. The social scientists involved in this Rockefeller-funded effort, according to the new historians, formed the nucleus of the massive propaganda and "psychological warfare" bureaucracy set up in World War II. The new historians argue that, with much the same overlapping group of scholars, and with the wartime effort's infrastructural remnants, mass communication research matured in the early cold war. In a startling and incendiary conclusion, the new historians—notably Simpson and Glander—make the case that postwar media research was organized around the search for effective propaganda design on behalf of its State Department, military, and CIA funders. This is, of course, a long way from Katz and Lazarsfeld's self-professed "limited effects" finding. And the new historians' narrative doesn't just contradict the mainstream story, but also critical scholars' account of the field's past.

It is easy to paint with too broad a brush here: The six scholars differ in a number of important ways, and they would not all embrace the counternarrative, in its entirety, that I outline above. There is also a tonal and evaluative contrast worth stressing: Simpson and Glander, for example, tend to explicitly condemn the field's dalliance with government-funded propaganda, while Gary and Sproule are more forgiving. Though the six share a "family resemblance" in Wittgenstein's sense, there is a striking absence of cross-citation and other evidence of collaboration. The continuity I trace here is by ascription, in other words, and might not be recognized by those it describes.

THE INTERWAR YEARS: JOHN MARSHALL
AND THE ROCKEFELLER FOUNDATION

Buxton, Sproule, and Gary focus on the 1930s and the emergence of the main-stream "effects" tradition. Their histories, in line with many standard accounts, highlight the importance of Paul Lazarsfeld's Office of Radio Research (ORR), established in 1936 with funding from the Rockefeller Foundation.[20] Unlike previous accounts, however, the new history reconstructs in detail the context around that Rockefeller intervention. In particular, the foundation's John Marshall—who had been the most neglected major figure in the historiography of mass communication research—emerges as a key protagonist.

Buxton, a sociologist of knowledge who wrote a notable, somewhat muck-raking study of Talcott Parsons in the 1980s,[21] provides the richest treatment of the foundation's early 1930s involvement in then-heated debates over commercial and educational radio.[22] Buxton's research is the most informed about the existing historiography of the field, which he masterfully critiques.[23] Gary, an intellectual historian, charts the immediate prewar years in terms of "nervous" liberal intellectuals and their Rockefeller-organized effort to promote intervention and plan a propaganda defense against the Nazis before it was politically palatable for the Roosevelt administration.[24] In an earlier essay, he presents an excellent, compact treatment of the Rockefeller role in organizing the "Communications Seminar" in the lead-up to the U.S. entry to war.[25] Sproule, finally, is a speech communication scholar who has mounted a multiyear project to recover the forgotten "propaganda analysis" paradigm which, as he details in a number of fascinating essays and a recent book, was self-consciously pushed aside in the Rockefeller-sponsored prewar propaganda mobilization.[26] Without any coordination, their research is nevertheless complementary, and together they provide a remarkably coherent picture of the foundation's formative role.

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

After the anarchic and interference-plagued airwaves of the 1920s had been brought under initial federal control with the 1927 Radio Act, a vigorous public debate broke out. Educational broadcasters, in particular, worried that the Act's new technical standards would drive out noncommercial stations.[27] There was, as Buxton describes in detail, a critical split in the educational camp—between a moderate group backed by John D. Rockefeller, Jr., and the Carnegie Corporation, and a

less conciliatory group funded by a small Payne Fund grant. The Rockefeller-backed group urged cooperation between the networks and reformers, and proposed to develop high-quality educational programs that commercial broadcasters would find attractive. The Payne-backed group set out, instead, to lobby Congress and build public support for a fixed-percentage spectrum set-aside.[28] The 1934 Communication Act that emerged from the legislative and public battles was, of course, a victory for the commercial broadcasters, which included no mandated set-aside. But the Act did gesture toward the "public interest," and the new Federal Communications Commission (FCC) called a meeting to reconcile differences between the educators and the broadcasters, which ended in further acrimony.[29]

Buxton documents how the Rockefeller Foundation's interest in the study of radio emerged from the meeting and its bitter aftermath. The Foundation's Humanities Division (HD) essentially took over the government's underfunded effort to achieve a reconciliation.[30] John Marshall, the HD assistant director, played the key role in this and other communication-related Rockefeller initiatives.[31] After the post-Act meeting, he interviewed the main players in the debate, including broadcasters. As his diaries reveal, he came to share the moderate group's belief that commercial and educational goals were ultimately reconcilable.[32] Accepting commercial radio as a given—indeed, largely adopting the Rockefeller-backed moderate group's stance—Marshall sought to convince the networks to voluntarily embrace some educational programming.[33] The Foundation, in this vein, funded fellowship appointments at NBC and CBS to train public broadcasters.[34] But Marshall, by 1936, came to believe that only objective audience data would persuade the networks that some educational programming is in their interests.

It was Hadley Cantril, then an ambitious, thirty-year-old psychologist, who convinced Marshall that polling techniques, newly prominent after the infamous *Literary Digest* upset, could yield valuable data on audience interests and motivations. Cantril, then at Columbia's Teachers College, had recently published *The Psychology of Radio* (1935) with his Harvard mentor Gordon Allport. As Marshall later recalled in an oral history interview cited by Buxton, the "historic moment" came when he read Allport and Cantril's book—based mostly in experimental laboratory studies, but including some survey data—which in its conclusion urged that more research be conducted on listeners.[35]

When Marshall interviewed him, Cantril invoked the polling procedures and proposed to Marshall that he, Cantril, conduct laboratory and sampling-based research into listener tastes.[36] (Cantril had been serving on a committee, formed in early 1936 by the FCC to mediate between educators and broadcasters, which Marshall was closely following.) Impressed, Marshall urged Cantril to submit a request for funding; his initial proposal called for ongoing research

into "what listeners find of interest in radio programs and . . . why these interests exist"—research, he argued, that would not be trusted if it emanated from either the broadcasters or the educators.[37]

A series of complicated maneuvers followed—which Buxton documents—that resulted in Cantril's revised proposal, this time with CBS's Frank Stanton as a partner, for a radio research bureau, which Marshall openly supported.[38] In his statement to the Rockefeller trustees, Marshall framed the research center as the key to bringing the broadcasters around to educational programming:

> If the present project succeeds, as I expect it will, in demonstrating the feasibility and significance of studying the actual and potential public service of radio to its total audience, it will set a style which the broadcasters cannot afford to disregard.[39]

In early 1937, the trustees approved a grant of $67,000 over two years to fund a "Princeton Radio Research Project," whose charter explicitly forbade research that questioned the commercial basis of the broadcasting.[40] It was Marshall who arranged that the Project be located at Princeton, and he seems to have played the crucial role in securing a post for Cantril in the University's psychology department. At around the same time, Cantril became one of the founding editors of the Princeton-based *Public Opinion Quarterly*—which the foundation also funded.[41]

When the Radio Project grant was awarded, Stanton was designated director, with Cantril as associate director. But Stanton, as he later recalled, was so "completely involved in what [he] was doing at CBS" that he declined to leave the network.[42] Cantril, too, was unwilling to assume the directorship, and the pair went looking for an appropriate candidate by asking around in psychological and sociological circles, and settled on Paul Lazarsfeld.[43] Though the Foundation was initially reluctant—concerned, as Marshall recalled later, that Lazarsfeld's interests were too broad—the appointment was made. The Rockefeller Foundation's expressed desire to get educational programming on commercial radio had, through this circuitous route, issued in the Office of Radio Research. Lazarsfeld's ORR (later the Bureau of Applied Social Research at Columbia) would proceed, of course, to help set the field's intellectual and methodological agenda during and after the war.

As Buxton, Sproule, and especially Gary document in great detail, the Foundation's next venture into media research was motivated on entirely different grounds, with arguably more lasting consequences for the communication field. With the rapid Nazi conquest of continental Europe as backdrop, Marshall in 1939 organized a "Communications Seminar" that was, in effect, a self-conscious precursor to the government propaganda campaigns of the war—convened in the knowledge that explicit government efforts, at that time, were not

politically feasible.[44] One of the outcomes of the Seminar was the consolidation of the "communications" label itself, which as Sproule details was put forward as a deliberate alternative to the "propaganda analysis" tradition—whose Progressive emphasis on propaganda inoculation, in the new context of intervention and defense against the Nazis, risked obscuring the crucial distinction between (German) propaganda and (American) morale building.[45] Many of the Seminar participants—including Lazarsfeld, Cantril, and Harold Lasswell—would go on to leading roles in the government's wartime propaganda activities. As it evolved, the Seminar came to define the study of mass communications in largely quantitative terms, and identified the question of media effects as its driving problem. As both Sproule and Gary document, the field of communication was conceived in the Seminar as both an emerging scientific field and as a crucial instrument of effective propaganda design.[46]

The Seminar—a truly fascinating episode in the sociology of knowledge—was initially conceived by Marshall in terms of his interest in media study as a potential bridge-building between educators and commercial broadcasters. In late 1938, he proposed, to the Rockefeller trustees, a series of conferences centered on the prospects for the public's media-led education.[47] In August 1939, just before the outbreak of war, the foundation agreed to fund the series, which, in the proposal's language, was designed to develop a disciplined approach to the study of mass communication, through such media as radio, motion pictures, and print." One of its explicit charges was to identify a "general body of theory about mass communications in American culture.[48]

Before the first conference—the meetings only later came to be called the "Communications Seminar" or, less often, the "Communications Group"—the Nazi conquest of Central Europe was already underway. The Seminar's work over the following two years was forged by two competing, but ultimately merged, agendas: to map out the scientific study of mass communication, and to design an extragovernmental plan for combating Nazi propaganda and mobilizing war support. Most of the Seminar members, at least by 1940, had come to adopt a robustly scientistic view of communication research that was, significantly, also conceived as a major weapon in the world struggle.[49] Sproule has aptly labeled this seemingly schizophrenic scientific instrumentalism as an "ideology of service and science."[50] Before this rough consensus was formed, however, the Seminar's debates split along two axes that were not, moreover, clearly parallel to one another. Some of the participants were resistant to the others' stress on quantitative techniques. Along another axis, Seminar members disagreed about whether media research should be used for propaganda design. In the end, as Gary and Sproule show, the group's momentum and the developments in Europe brought

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

The Seminar issued its first group report in July 1940, "Research in Mass Communications," which laid out the famous "*who* says *what* in which *channel* to *whom* with what *effect*" formulation—which, however, has long been credited solely to Lasswell, probably because this initial, group formulation was kept secret.[51] The report's collective call for war-related opinion management was unambiguous:

> We believe . . . that for leadership to secure that consent will require unprecedented knowledge of the public mind and of the means by which leadership can secure consent. . . We believe. . . that we have available today methods of research which can reliably inform us about the public mind and how it is being, or can be, influenced in relation to public affairs.[52]

The Seminar's early ambivalence and qualifications were missing from the report, and some members openly decried its "fascistic" implications.[53] In response to the complaints, the group's final report, issued in October, was far less brazen in its language and recommendations. Titled "Needed Research in Communication," the document called for "two-way communication" between the government and the people; without it, the report warned, "democracy is endangered." The report concedes that the new mass communication research might be used for propagandistic ends, but asserts that such "authoritarian" cooptation could and should be avoided.[54]

The document's disclaimers, however, are more than a little disingenuous, as the foundation was already building an elaborate network of propaganda-related research projects, in lieu of a government-directed campaign. At the Seminar's September meeting—just a month before the report was issued, notes Gary— Marshall reported that the foundation's projects would engage "the threefold task of maintaining civilian morale at home, of maintaining good relations with friendly countries, and of waging propaganda warfare with countries hostile to us."[55]

Even as the report was distributed to a number of scholars, university presidents, foundation officers, publishers, and government officials,[56] Marshall and Lasswell were approaching government officials and, in Gary's words, "quietly made it known that foundation monies might be available to facilitate government-needed communication research."[57] From early 1940 until the U.S. entry into the war in December 1941, the foundation served, in essence, as an unofficial arm of the state when the Roosevelt administration—hampered by a public culture still wary of propaganda, and a somewhat isolationist Congress—could

not feasibly do so itself. Marshall was quite candid about this in his proposals to the Rockefeller trustees:

> Whether or not this is something for the foundation to consider, I do not know. . . But the early neglect of this type of study [by others] may mean that it is the only agency as yet ready to recognize its importance and to provide the necessary funds. . . for the type of work which later may be needed in national defense.[58]

Fellow Rockefeller officer Stacey May, who was at the time also working with the Office of Emergency Management, wrote to Marshall to warn him that the "last war left the country suspicious of propaganda" and that, as a result, the government would be "slow to develop 'morale' activities for fear of being accused of propagandizing."[59]

Gary documents that, in his response, Marshall agreed and observed that even "communications research" was plagued by propaganda fears. Despite the "growing recognition of the need for such research," he continued, any Roosevelt-led efforts "would not be looked on favorably by Congress."[60] By the end of 1940, the foundation had set up and funded an elaborate bundle of propaganda-related projects; even those research initiatives, such as the ORR, that were originally conceived with other purposes were, by this time, brought into the propaganda fold. By 1940, Rockefeller-backed projects included Cantril's Princeton Public Opinion Research Project (which the foundation had funded after Cantril split with Lazarsfeld); the Princeton Shortwave Listening Center; the Graduate Library Reading Project at Chicago; the Film Library of the Museum of Modern Art (which included Siegfried Kracauer's studies of Nazi film propaganda); the Library of Congress Radio Project; the Totalitarian Communications Project at the New School; and Lasswell's content analysis operation at the Library of Congress.[61]

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

WORLD WAR II AND THE EARLY COLD WAR:
STATE-FUNDED "PSYCHOLOGICAL WARFARE"

Timothy Glander and Christopher Simpson pick up where Gary, Buxton, and Sproule leave off: the massive wartime propaganda effort. Based on extensive archival research, Glander and Simpson show that the extraordinary social scientific mobilization for "psychological warfare" work fostered social networks and an intellectual framework that shaped the communication field long after the Axis powers were vanquished. Their argument is that the scholars (such as Lazarsfeld, Schramm, Lasswell, and Daniel Lerner) and the questions (concerning effective propaganda design) from the war period were, in essence, redeployed in the early cold war. Both scholars uncover an extensive set of once-classified or long-forgotten studies, funded by the military, State Department, and CIA (with some foundation collusion) throughout the 1950s and into the 1960s, preoccupied with the task of making propaganda work.

Simpson's 1994 *Science of Coercion* is the more exhaustively researched effort. Simpson, an investigative journalist who has produced a number of blistering exposes in other areas, does not conceal his leftist political commitments.[62] Despite its polemical tone, Simpson's book convincingly shows that the traditional "limited effects" storyline is woefully inadequate to explain postwar communication research. Indeed, he suggests (though does not develop) the point that, especially after the mid-1950s when the campaign for third world hearts and minds heated up, prominent published research was based on secret propaganda work that was repackaged as disinterested science. Glander, an education scholar, covers much of the same territory as Simpson in his 2000 *Origins of Mass Communications Research during the American Cold War,* based on a 1988 dissertation.[63] Glander's work is useful mainly as a supplement to Simpson's, as it includes some new detail and archival sources[64] but in other respects confirms the *Science of Coercion* narrative.

Both scholars stress the crucial importance of social scientists' wartime service. Hundreds of social scientists temporarily left their academic posts to take up direct employment or consultancies for dozens of government and military agencies—an always-evolving acronymic tangle of programs and departments including, most prominently, the Army's Research Branch, the Office of Strategic Services (precursor to the CIA), the Office of Wartime Information, and the Library of Congress, but also the Departments of Justice and of Agriculture, the FCC, and many others. Perhaps more importantly, the employment overlap of constantly shuffled scholars produced networks of contacts, friendships, and acquaintances that proved, after the war, to be of extraordinary importance to

many disciplines and to various lines of scholarship—including, as it turned out, mass communication research itself.

Much of that wartime social science was concerned with the design and testing of propaganda—"psychological warfare," as the bundle of techniques and theory came to be known.[65] Simpson and Glander show that, when the cold war heated up in the late 1940s, the federal government in effect reconstituted its World War II propaganda infrastructure. Scholars who had been working in Washington were, by the early 1950s, spread about at research institutes modeled on Lazarsfeld's BASR. Federal money—from the military, CIA, and State Department, often in close coordination with foundations such as Carnegie and Ford—poured into these university-based research institutes, as Simpson meticulously documents.[66] Throughout most of the 1950s, and with no public acknowledgment, government funds made up more than three-quarters of the annual budget at Lazarsfeld's Bureau, Cantril's Institute for International Social Research at Princeton, Ithiel de Sola Pool's Center for International Studies at MIT, and similar research shops.[67] Simpson concludes that these Bureau-style institutes grew up as "de facto adjuncts of government psychological warfare programs."[68]

The detailed findings of Simpson and Glander are so startling in part because they drastically contradict the field's "limited effects" self-narration—the claim that postwar media research had discovered that mass media influence is happily negligible. Even while "limited effects"-style conclusions were published, in *Personal Influence* for example, research outfits like Lazarsfeld and Katz's Bureau were under federal contract to design effective propaganda campaigns overseas.[69] Though critics of the "dominant paradigm" like Gitlin had stressed the influence of funders, the critics' indictment had pointed to media industry patrons who, according to the argument, were let off the hook by the "limited effects" conclusion. Simpson and Glander draw altogether different conclusions: The Bureau was hardly concerned to show that media influence is limited, since it was in the business of making persuasion work for its commercial and government clients.

Much of the federally funded research, Simpson and Glander show, was directed at third world populations deemed susceptible to Soviet influence. Rohan Samarajiva, a respected development communication scholar and the final "new historian" I identify here, has exposed a significant example of the wider pattern described by Simpson and Glander. Samarajiva, in a brilliant though little-noticed 1987 paper, revisits Daniel Lerner's classic, *The Passing of Traditional Society: Modernizing the Middle East* (1958). In the paper—aptly titled "The Murky Beginnings of the Communication and Development Field"—Samarajiva reveals that the book was spun-off from a sprawling and largely secret audience research project funded by the Voice of America.[70] The project, awarded to Lazarsfeld's Bureau in 1949, was explicitly tasked with identifying target audiences for U.S.

propaganda in the Middle East.[71] During World War II, Lerner had worked with Edward Shils and Morris Janowitz in the Psychological Warfare Division of Eisenhower's Allied command, and after the war wrote a dissertation (published as *Sykewar* in 1949) on the anti-Nazi propaganda effort.[72] Lerner (and a number of other Bureau figures) helped oversee the field interviews in 1950 and 1951—nearly 2,000 were conducted across the Middle East.[73] In *Passing*, Lerner acknowledges that the book is based on Bureau surveys, but says nothing about government funding or the study's original purpose.[74] Indeed, Lerner claims he is motivated by the "historic shift. . . of modernist inspiration from the discreet discourse of a few . . . to the broadcast exhortations among the multitudes."[75] To Samarajiva, Lerner's failure to disclose the study's original context amounts to "willful suppression," a "lie by omission"—and helps to demonstrate how "modernization" was largely a euphemism for ongoing cold war psychological warfare.[76]

THE NEW HISTORY: AN ASSESSMENT

The new history, as a body of work, presents an astonishing indictment of the field's intellectual progenitors and their whitewashed remembrances. One inescapable conclusion is that the field's remembered history—of four founders and the reassuring "limited effects" findings—obscures a major thrust of postwar mass communication research: "psychological warfare" studies on behalf of, and funded by, various government agencies for use in overseas and domestic cold war propaganda campaigns.

Each of the new histories takes into account the array of external pressures—intellectual and otherwise—that have helped shape the would-be discipline. Most of the preexisting historiography, in contrast, has been resolutely internalist, neglectful even of intellectual influences from outside the field. The bulk of that earlier history is concerned with legitimating the field from within and without. Significantly, the new history revises even the *revisionist* history (of Gitlin and others) that had, as we have seen, accepted core elements of the mainstream story.

The new history is distinguished, too, by its rigor and archival burrowing, and here again the contrast to a long line of footnote-less digests is striking. To varying degrees, the new history is acquainted with the rich methodological reflection in the history and sociology of science fields. And, perhaps more importantly, the new history by and large embraces a laudable explanatory eclecticism—a refusal to settle on a single mode of analysis. There is simply too much complexity and diversity in the field's past for any unitary scheme to bear much explanatory

burden—without, at least, inflicting major violence on the empirical reality that it purports to explain. The new history, for the most part, submits to a case-by-case explanatory agnosticism that lets the empirical evidence "assert" itself first.

These features of the new history, to borrow Alun Jones' 1983 description, "bear a suspicious resemblance to what most professional historians would identify as simply 'good history.'"[77] Even so, the six historians' work has its limitations—most notably in scope—and we would do well to consider their research as a foundation on which to build.

The new history, for example, does not adequately place the study of communication in the context of "public opinion research," the interdisciplinary social science field that grew up after 1936 around polling methods and emerged, after the wartime propaganda effort, at the center of postwar empirical social science. From 1936 until the "communication" field had substantially migrated to journalism schools by the early 1960s, public opinion (or survey) research was, indeed, hard to distinguish from "communication" study.[78] The new history also neglects the fascinating relationship between 1950s communication research and the public intellectual debates over "mass culture" then raging. Most significantly, the synthetic account that emerges from the bundle of histories ends rather abruptly, in the early 1960s—just as the slow march through journalism schools had picked up pace. We remain almost wholly ignorant of the field's history as an institutionalized "discipline," and here the new history provides little relief.

More generally, the new history is not informed well enough by the substantial and growing body of historical research on postwar social science.[79] Historians of communication research should be immersed in this literature, if only because the field, especially before the migration to journalism schools, was nothing but a loose assemblage of sociologists, psychologists, and political scientists. Most of the other social sciences have subdisciplines devoted to their disciplinary histories, and benefit from the attendant cross-pollination and organizational momentum. The new history of communication research, as with the rest of the field's historiography, does not seem to be self-conscious of itself as a community of collaborating scholars. Few of the historians cite one another, and in many instances seem unaware of the others' work. The infrastructure of scholarship—conferences, journals, and associations, and the linked intangibles of friendships and a sense of common knowledge goals—these are missing.

There are specific, substantive problems too. Gary, for instance, is too taken with the quite genuine dilemmas of interwar liberalism, and as a result downplays the crucial, and interwoven, roles of funding and opportunism. Simpson and Glander err in the opposite direction, in their overcommitment to an otherwise laudable resource-based sociology of knowledge. One of the drawbacks of such an approach is that the complex and distinct motives of key researchers get neglected. It

is important, for example, to distinguish between zealous cold warriors like Wilbur Schramm or Daniel Lerner and apolitical funding opportunists like Lazarsfeld.

It's also true that the new historians—especially Simpson, Glander, and Sproule—could make a more strenuous effort to narrate the field's past in its own terms, rather than through present concerns. Simpson's muckraking zeal to uncover the field's original sin, for example, leads him to underplay the elective affinity between genuinely held cold war liberalism and the goals of the national security state. Here George Stocking's 1965 call for an "enlightened presentism," in which a rigorous effort to understand the past "for its own sake" is tempered by acknowledgment of the attempt's limits, should be our guide.[80]

CONCLUSION

Kurt Danziger, the historian of psychology, described the state of the subfield before a wave of revisionist historiography in the 1970s and 1980s: "Historical scholarship," he wrote, "came a distant second to the primary function of the field which was pedagogical, imparting an appropriate group image to aspirant members of the discipline."[81] Danziger's description could readily apply to communication studies and its remembered history today, were it not for the recent contributions of what I have here called the "new history."

Still, the question remains: why did this body of work emerge so "late" relative to the other social sciences? How is it that, as early as 1966, Stocking could speak of an "upsurge" of interest in the history of anthropology—when, decades later, no such surge had materialized within communication research?[82]

In conclusion, I want to propose a possible answer. In a field with little in common save a label, selective memory and forgetting play outsized roles in holding the discipline together. John Peel once observed that there is an inverse correlation between our ability to narrate the past faithfully and the functions that a remembered "past" performs for a community.[83] Communication research, as a field, badly needs the glue of tradition, however invented.

This is true because of the field's peculiar (and intellectually retarding) institutionalization—in journalism schools, speech departments, and other sites scattered across the university.[84] Faculty who work under the "communication" label are normally expected to produce scholarship and, at the same time, impart career skills to industry-bound students. In practice, this means polarized departments or else schizophrenic faculty. "The fact that a single individual can teach courses in, say, magazine editing and research techniques in social psychology," observed Jeremy Tunstall back in 1983, "is a tribute to human adaptability, not to a well-conceived academic discipline."[85]

For all its incoherence as an institutionalized discipline, communication is endowed with abundant resources, including an enormous supply of undergraduate would-be celebrities. But because of its vocational taint—and its messy and recently formed institutional trappings—the field has from the beginning endured a deficit in legitimacy. Even internal to the field, there are very few shared conceptual underpinnings. In short, the field has (in Andrew Abbott's terms) a social structure without much cultural coherence.[86]

Much rests, then, on the field's self-narration of its past. Whiggishness and intellectual continuity are crucial, as is a kind of forgetting—notably, of the field's checkered institutional roots.[87] The rigorous scrutiny of communication's past might fray the discipline's fragile bonds. Recall Stephen Brush's famous question, whether the history of science should be "rated X" for its potential to undermine students' faith in science.[88] Perhaps the new history of communication research took so long to emerge—and even now registers so weakly in the field's consciousness—because the discipline has needed all the faith it can muster.

NOTES

1. "Approaches to the Historical Development," 41.
2. The reference is to the 1983 special issue of the *Journal of Communication*, in which more than two dozen scholars challenged (or, less frequently, defended) the mainstream "effects tradition" of the postwar field (see "Ferment in the Field").
3. The best analysis of this institutionalization remains John Durham Peters's "Institutional Sources of Intellectual Poverty in Communication Research." But the field's institutional history remains strikingly understudied (see Introduction, this volume). There are bits and pieces of such a history in various accounts, especially Delia, "Communication Research: A History," 73–84; Chaffee and Rogers, "The Establishment of Communication Study in America"; Weaver, "Journalism and Mass Communication Research"; Marjorie Fish's underappreciated dissertation ("A Study of Mass Communication Research," 54–70); Cartier, "Wilbur Schramm and the Beginnings of American Communication Theory," 243–76; King, "A History of the Department"; Katzen, *Mass Communication*, 19–55; and Sproule, this volume.
4. This mutual neglect was on vivid display in 1994, when two book-length histories (Rogers, *A History of Communication Study* and Cohen, *The History of Speech Communication*) were published, rooted in the journalism- and speech-derived traditions, respectively. As Robert Craig observed, the books have almost no overlap (review, 181). My focus is on the history remembered by the mass communication tradition.
5. Katz and Lazarsfeld do not use "hypodermic needle" or "magic bullet" in *Personal Influence*, but these became the standard shorthands for the "limited effects" narrative. Katz did first employ the "hypodermic" metaphor, in this context, in an unpublished 1953 report (see Simonson, Introduction, 16–17). On the evolution of the "hypodermic" label, see Lubken, this volume.
6. On the formation of this limited effects storyline, see Pooley, "Fifteen Pages"; and, in more detail, Pooley, "An Accident of Memory." See also Pietilä, "Perspectives on Our Past," 347–50.

7. Schramm, who before World War II was a professor of English at the University of Iowa (where he helped found the Iowa Writers' Workshop), volunteered for the war propaganda effort in December 1941. After he left government in late 1943—excited about the possibilities for communication as a new field—he transformed Iowa's journalism school into a school of "Journalism and Mass Communication" complete with a PhD program, leading other Midwestern universities (including Minnesota and Wisconsin) to follow suit. Schramm moved to the University of Illinois in 1948 and founded the first of three institutes of communication research (modeled after Lazarsfeld's Bureau). This story has been told many times, usually in a celebratory fashion. See, for example, Chaffee and others, "The Contributions of Wilbur Schramm"; McAnany, "Wilbur Schramm, 1907–1987"; Cartier, "Wilbur Schramm and the Beginnings of American Communication Theory"; Keever, "Wilbur Schramm"; and Chaffee and Rogers, "Wilbur Schramm, the Founder."

8. "Communication Research in the United States," 2. The four were first designated as distinct lineages by Bernard Berelson in 1958 and 1959 ("Present State" and "State of Communication Research").

9. "Communication Research in the United States," 2.

10. Ibid., 5.

11. Ibid.

12. "The Beginnings of Communication Research"; "The Unique Perspective of Communication"; "The Beginnings of Communication Study"; and (posthumously) *The Beginnings of Communication Study*.

13. I am indebted to Veikko Pietilä's excellent essay, which makes this point convincingly ("Perspectives on Our Past," 150–51).

14. Gitlin's reliance on commercial funding simply cannot perform the heavy lifting that he asks it to do—despite the fact that Lazarsfeld did indeed package some of his findings in a media-friendly way, especially if the executives were the intended audience. And certain Bureau-linked figures, most prominently Frank Stanton of CBS and Joseph Klapper, who by the late 1950s had also joined CBS, were indeed carrying the industry's water in a more-or-less shameless way. But Lazarsfeld, with his genuine scientific interests and aspirations, was much more complicated. (See Pooley, "An Accident of Memory," 179–299.) As Christopher Simpson and Timothy Glander show, moreover, government propaganda funding during the war and especially in the 1950s was a more important source of funds (see discussion, below).

15. "Media Sociology: The Dominant Paradigm," 207.

16. As Pietilä observes, critical scholars did not draft a new history as much as judge the standard history "in an entirely different way" ("Perspectives on Our Past," 151). Pietilä: "one gets the impression that the New Left version is not motivated by an ardent interest in the past as much as creating a weapon in the struggle for hegemony in the field during the ferment of the late seventies and early eighties" (Ibid.).

17. The key diffusion figure here was James Halloran of the University of Leicester, whose 1964 *Effects of Mass Communication* and other writings in the late 1960s and early 1970s framed the American sociology of media for the two major traditions of 1970s British media research, political economy and cultural studies.

18. See, for example, Lang, "The Critical Functions of Empirical Communication Research"; and Lang and Lang, "The 'New' Rhetoric of Mass Communication Research." Lang and Lang do assert, in these works and elsewhere, that the "magic bullet" contrast is largely a straw man, but do not challenge the "limited effects" emplotting. Rowland's *The Politics of TV Violence* is a rich and detailed study of the field's entanglements in public policy debates over TV violence, but

the book also largely accepts the "limited effects" self-description. Morrison's dissertation ("Paul Lazarsfeld") and follow-up work ("The Beginnings of Modern Mass Communication Research") reproduces (with terrific, interview-based detail, however) the *Personal Influence* storyline. (On Morrison's more recent work, see n. 19.) Czitrom's treatment ("The Rise of Empirical Media Study"), like the Langs', asserts that the putative "powerful effects" contrast is a straw man, but otherwise accepts the "limited effects" narrative. Delia's exhaustive "Communication Research: A History," likewise, accepts the standard "limited effects" self-characterization.

19. I exclude here some recent work that, by the criteria of rigor and sophistication alone, would be included, but that does not substantially challenge the field's received history. In this excluded-but-worthy category, recent work by David Morrison (e.g., "Late Arrival," "Influences Influencing"), Peter Simonson (e.g., Introduction, "Serendipity"), and Karin Wahl-Jorgensen (e.g., "Rebellion and Ritual," "How Not to Found a Field") stands out.

20. For a history of the ORR that draws on Buxton, Sproule, and Gary, see Pooley, "An Accident of Memory," 179–299.

21. *Talcott Parsons and the Capitalist Nation-State.*

22. See especially "The Political Economy of Communications Research" and "From Radio Research to Communications Intelligence." "Reaching Human Minds" and "John Marshall and the Humanities" add some more detail. Much of Buxton's more recent work has focused on excavating the thought of Canadian economist Harold A. Innis (including "The 'Values' Discussion Group").

23. For an example, see "The Emergence of Communications Study."

24. *The Nervous Liberals.*

25. "Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words."

26. See especially "Propaganda Studies in American Social Science"; "Progressive Critics and the Magic Bullet Myth"; "Propaganda and American Ideological Critique"; and *Propaganda and Democracy.*

27. "Political Economy of Communications Research," 154–56.

28. Ibid., 154–55.

29. Ibid., 155–56.

30. "In effect, the Rockefeller Foundation assumed a task that neither broadcasters, educators, nor state officials were willing or able to undertake" (Ibid., 153).

31. It is only a slight exaggeration that, as Buxton argues, Marshall "almost single-handedly gave coherence and direction to the assorted Rockefeller projects related to the relatively new media of mass communication" (Ibid., 156). His crucial contributions were not so much intellectual but financial and organizational. As Gary observes, his interests in radio were "largely derivative and synthetic, and not especially original. His importance should be measured by his role as an administrative catalyst and agent for scholars, and not for the questions he asked or the problems he framed" ("Communication Research, the Rockefeller Foundation," 130.)

32. Buxton, "Political Economy of Communications Research," 158.

33. Buxton: "While the Act set the framework for the incorporation of educational interests into the commercial broadcasting system, it did not provide the resources, programs, and expertise through which this reconciliation between educators and broadcasters could take place . . . the role of charting the path of cooperation between educational and broadcasting interests fell to the Humanities Program of the Rockefeller Foundation" (Ibid., 153).

34. Ibid., 160.

35. Marshall, "Reminiscences."

36. "Political Economy of Communications Research," 161–64.
37. Ibid., 160–61.
38. Ibid., 164. From the Cantril and Stanton proposal: "If radio in the United States is to serve the best interest of the people, it is essential that an objective analysis be made of what these interests are and how the unique psychological and social characteristics of radio may be devoted to them" (164).
39. Ibid., 167.
40. Ibid.
41. Ibid.; and Gary, "Communication Research, the Rockefeller Foundation," 132.
42. Oral history interview, quoted in Pasanella, *The Mind Traveller*, 13.
43. Ibid., 12.
44. Gary: "With war breaking out in Europe, Rockefeller officers and the founding fathers of communication research were galvanized by the recognition that the Roosevelt Administration, hamstrung politically, could not adequately prepare for war on the propaganda front. Isolationist sentiment and bad memories from World War I limited the administration's ability to influence domestic public opinion or control foreign and domestic antidemocratic propaganda. The Rockefeller Foundation, whose university-, museum-, and library-based projects had more room to experiment with potentially controversial activities, took up the slack. With Marshall and the Foundation providing funding and serving midwife roles, Rockefeller-funded research laid the groundwork for a wide range of national security projects that were eventually absorbed by the state" ("Communication Research, the Rockefeller Foundation," 125).
45. See Sproule, "Propaganda Studies in American Social Science."
46. Gary's broader argument, laid out in most detail in his 1999 book *The Nervous Liberals*, is framed in terms of two major debates among post–World War I liberals. He describes, in impressive detail, the debate, from the early 1920s on, between chastened realists like Walter Lippmann and progressive populists as to the competence of the public in terms of democratic theory and practice. Gary argues that, with this debate in the background, a second major conflict came to the fore with the rise of fascism, World War II, and the possible U.S. entry: a debate that pitted traditional liberal concerns for civil liberties against emergency-context national security concerns. Many liberals, even those who had in the 1920s opposed Lippmann's view, came to help in the building of what Gary rather generously calls a "propaganda prophylaxis"—a set of state-driven defenses against fascist propaganda that involved propping up U.S. domestic morale and countering fascist propaganda at home and abroad. Gary makes it quite clear that this "propaganda prophylaxis" and the wartime service of communication scholars was an honorable, good faith effort—retroactively justifiable given the uniquely "just war" context. He allows, though, for a kind of unintended consequence of well-intentioned action: this good faith, emergency effort became routinized, through inertia and the subsequent cold war context, into the national security state, whose propaganda activities are less defensible.
47. See "Communication Research, the Rockefeller Foundation," 131, for a summary of the proposal.
48. Quoted in Ibid., 132.
49. The Seminar's nonfoundation participants were Lazarsfeld; Harold Lasswell; Robert Lynd; Hadley Cantril; Geoffrey Gorer, an Oxford-trained anthropologist; Lyman Bryson, an adult education specialist; Donald Slesinger, former dean of the Social Sciences at Chicago and director of the Rockefeller-funded American Film Center; I.A. Richards, the prominent Canadian literary theorist; Douglas Waples of the University of Chicago's library school (and mentor to Bernard Berelson, who began his academic career there); Charles Siepmann, a communication analysis

for the BBC; and Lloyd Free, the once and future Cantril collaborator who would, in 1940, take over the editorship of the *Public Opinion Quarterly* and after the war participate centrally in the cold war propaganda efforts (Ibid., 133).

50. "Propaganda Studies in American Social Science," 75.

51. As Gary observes: "Normally attributed solely to Lasswell, the paradigm was the product of months of paper exchanges, seminars, and oral and written dialogue, among diverse members" ("Communication Research, the Rockefeller Foundation," 138.)

52. Quoted in Ibid., 139.

53. Ibid., 140.

54. Ibid., 141.

55. Quoted in Ibid.

56. Including, as Gary reports, Robert Hutchins, Louis Wirth, Henry Luce, Talcott Parsons, William O. Douglas, and Archibald MacLeish (Ibid., 142).

57. Ibid.

58. Quoted in Ibid., 143.

59. Quoted in Ibid.

60. Quoted in Ibid.

61. Ibid., 125.

62. See, for example, *War Crimes of the Deutsche Bank* and *The Splendid Blond Beast*.

63. "The Battle for the Minds of Men." See also Glander, "Wilbur Schramm."

64. Glander establishes, for example, that Schramm was almost certainly on the CIA payroll and the secret informant that nearly scuttled Dallas Smythe's appointment at the University of Illinois in 1948 (170–72).

65. The best overview of social scientists' government propaganda service is Converse, *Survey Research*, 162–228.

66. *Science of Coercion*, 4, chaps. 5–6.

67. Ibid., 4.

68. Ibid.

69. This contrast between a *frontstage* "limited effects" finding and a *backstage* search for workable propaganda strategy is suggested, but undeveloped, by Simpson and Glander.

70. "Murky Beginnings," 6. See also Samarajiva, "Tainted Origins of Development Communication."

71. "Murky Beginnings," 6–8.

72. Samarajiva quotes the final paragraph of *Sykewar*: "In its 'cold war' with the Soviet Union. . . the United States is offering mainly dollars. . . to produce more 'good things of life.' . . . Should it turn out that . . . the 'good things' we offer are not adequate competition against the 'better world' offered by the Soviets, we shall need some new policy decisions. . . Here we shall need to consult the intelligence specialists (the social scientist) and the communication specialist (the propagandist)—rather than, or in addition to, the diplomat, the economist, and the soldier" (6–7).

73. Ibid., 7.

74. Ibid.

75. Quoted in Ibid., 7–8. Adds Samarajiva: "The investigation that led to this paper was sparked by the difficulties experienced by this writer in attempting to evaluate *Traditional Society* as part of a graduate course. The natural question as to what the original research questions, were, proved difficult to answer" (14).

76. Ibid., 3, 14.

77. "The New History of Sociology," 458.

78. See Pooley, "An Accident of Memory," 216–30. Converse's *Survey Research* is an excellent overview.
79. The literature is far too broad to cite, but see Crowther-Heyck, "Patrons of the Revolution," 421–24, for an excellent review.
80. Stocking, "On the Limits of 'Presentism,'" 8–11.
81. "Concluding Comments," 208.
82. "The History of Anthropology," 281.
83. Peel, *Herbert Spencer*; cited in Jones, "New History," 456.
84. Peters ("Institutional Sources") makes this case convincingly. As Robert Craig has noted, "The field or discipline of communication as we presently know it has sprung from no single source and has no coherence" (review, 178).
85. "The Trouble with U.S. Communication Research," 93.
86. Abbott, *Chaos of Disciplines*, 122. Abbott's distinction mirrors Richard Whitley's contrast between "social" and "cognitive" institutionalization ("Cognitive and Social Institutionalization").
87. Here the parallel with national identity, in Ernst Renan's famous stress on the importance of forgetting, is striking ("What is a Nation?").
88. "Should the History of Science be Rated X?"

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