

Checking Up on *The Invasion from Mars*: Hadley Cantril, Paul F. Lazarsfeld, and the Making of a Misremembered Classic

JEFFERSON D. POOLEY
Muhlenberg College

MICHAEL J. SOCOLOW
University of Maine

This article reconstructs the bitter dispute over the authorship of, and credit for, *The Invasion from Mars*—the classic 1940 study of the mass panic sparked by the 1938 Orson Welles “War of the Worlds” broadcast. The conflict between Hadley Cantril (the credited author) and Paul F. Lazarsfeld (then director of the Princeton Radio Research Project) helped lodge *The Invasion from Mars* as the published rival, in the remembered history of communication research, to the subsequent work of Lazarsfeld’s Bureau of Applied Social Research. The article challenges this two-stage story, especially the typical Cantril-Lazarsfeld contrast. Based on archival evidence, we show that the “War of the Worlds” study should be read as an early installment in—as continuous with—the Bureau’s decade-long campaign to complicate media impact.

Perhaps the best-established origin story of U.S. communication research is that Paul Lazarsfeld’s Bureau of Applied Social Research (BASR), in the 1940s and 1950s, lanced earlier scholars’ naive belief in media potency. As narrated in the Bureau’s *Personal Influence* (Katz & Lazarsfeld, 1955), this “powerful-to-limited-effects” storyline celebrated the triumph of measured quantitative social science over the speculative work of pre-World War II scholars. The upbeat narrative was deployed in early readers, textbooks, and monographs as the new discipline of “communication” established itself within journalism schools and speech departments in the 1950s and 1960s. Over time, the putative prewar view hardened into evocative shorthand as the “hypodermic needle” or “magic bullet” theory (Chaffee & Hochheimer, 1985; Lubken, 2008; Pooley, 2006a, 2006b; Sproule, 1989).

Textbook accounts almost always cite Lazarsfeld and the Bureau for the limited effects breakthrough but often fail to mention a single prewar figure or work to illustrate its hypodermic needle predecessor (e.g., Hanson, 2013, pp. 29–31; Straubhaar, LaRose, & Davenport, 2013, pp. 418–419). When a study is named, the most common reference is to Hadley Cantril’s *The Invasion from Mars* (1940), which is said to document the “mass panic” caused by Orson Welles’ famed 1938 “War of the Worlds” broadcast (see Figure 1). Cantril’s *Invasion*, in other words, typically stands in for the first, “powerful,”

stage, while Lazarsfeld's Bureau work—*Personal Influence* especially—is positioned as exemplar of the second (e.g., Baran & Davis, 2011, pp. 136–139; Fourie, 2001, pp. 294–296; Giles, 2003, pp. 14–16; Perry, 2001, pp. 19–22; Sparks, 2012, pp. 57–61).¹



Figure 1. Orson Welles, March 1, 1937. Photograph by Carl Van Vechten.

Source: http://commons.wikimedia.org/wiki/File:Orson_Welles_1937.jpg

This article challenges the two-stage story, especially the Cantril-Lazarsfeld contrast. Drawing on Rockefeller Foundation archives, we reconstruct the messy backstory to *The Invasion from Mars (IFM)* study. We show that *IFM*, though credited to Cantril, is more accurately described as a collaborative product of the Princeton Radio Research Project (PRRP). The Rockefeller-funded PRRP was run by Lazarsfeld; it was the Bureau's direct institutional antecedent. Lazarsfeld and other PRRP figures (notably

¹ In each case, the citation is to the textbook's latest edition. Many of these texts cite Lowery and DeFleur's (1983, ch. 3) influential "milestones" book, which is organized around the progression from magic bullet theory to limited and more complex effects studies. Lowery and DeFleur, in a passing reference, cite *The Invasion from Mars* (p. 67) as a bridge text between magic bullet and more sophisticated studies, but the chapter, as a whole, positions *The Invasion from Mars* as a direct-effects example.

Herta Herzog) were crucial to the initial research and subsequent write-up of *IFM*. The book itself presents a far more nuanced account of media influence than the “magic bullet” label implies. Cantril and *IFM*, in short, are badly suited to stand in for the presumed powerful effects camp. If anything, *IFM* should be read as an early installment in—as continuous with—the PRRP/Bureau’s decades-long campaign to complicate media impact.

How was it, then, that *IFM* came to be remembered as the published antithesis to the Bureau’s subsequent work? The main reason, as we recount below, is that the book was produced at the peak of an already-bitter dispute between the two men. Archival evidence suggests that the pair reached a negotiated settlement in late 1939: Lazarsfeld dropped his and Herzog’s claims to *IFM* credit, and Cantril agreed to the PRRP move to Columbia University. In the scholarly equivalent of divorce proceedings, Cantril got the book, and Lazarsfeld the research institute. It is a fitting irony that *IFM* and the Bureau, twin born, came to represent rival camps in the discipline’s remembered past.

This article proceeds in four parts. First, we trace the formation of the Princeton Radio Research Project, with a focus on Cantril’s fraught recruitment of Lazarsfeld as PRRP director. Next, we document the mounting tension between the men over several overlapping issues. With the Project’s renewal in the balance, Lazarsfeld and Cantril scrambled to study the seeming panic that greeted Welles’ October 1938 “War of the Worlds” broadcast. In the third section, we describe the bitter disputes over control and authorship credit that afflicted the “Mass Hysteria Study,” as it was known internally. In late 1939, the Project’s impending renewal and *IFM*’s rushed publication schedule set the stage for the apparent deal to dissolve the two men’s troubled partnership. In concluding remarks, we discuss the consequences for the U.S. communication discipline’s historical self-understanding.

The Princeton Radio Research Project

The Rockefeller Foundation’s investment in radio research grew out of an earlier, sometimes-bitter struggle over the new medium’s public interest obligations—the so-called radio wars of 1927 to 1934, when federal communications policy was in flux (Buxton, 1994; McChesney, 1993). The radio industry won the legislative battle, beating back educational broadcasters’ proposal to set aside spectrum for public-interest programming. Still, the 1934 Communication Act did gesture toward the public interest, and the Rockefeller Foundation essentially took over the government’s underfunded effort to achieve a reconciliation (Buxton, 1994, pp. 158–161). The Princeton Project soon emerged as the centerpiece of Rockefeller’s peacemaking initiative.

John Marshall—easily the most important patronage figure in the field’s early history (Buxton, 2003)—played the key role in Rockefeller’s radio (and film) programs. Marshall, assistant director of the humanities division, had followed the broadcasting reconciliation effort closely. He interviewed a number of participants, including the young psychologist Hadley Cantril. Their May 1936 meeting began an awkward courtship that, through twists and turns, would result almost a year later in the establishment of the Princeton Radio Research Project.

Cantril (1906–1969) was an ambitious, 30-year-old psychologist at Teachers College, Columbia University, set to join Princeton's psychology department in the fall (see Figure 2). He had studied with Gordon Allport as an undergraduate at Dartmouth and followed Allport to Harvard to pursue doctoral studies in 1930. While at Harvard, Cantril and Allport conducted a series of radio listening experiments, which formed the core of their 1935 collaborative book, *The Psychology of Radio*.



Figure 2. Hadley Cantril. Photograph by Frank Kane, Princeton Sunday News, April 14, 1940.

Source: Full folder, Cantril, Hadley, d. 1969, 1938–1951, Faculty and Professional Staff files, Princeton University Archives, Department of Rare Books and Special Collections, Princeton University Library.

Marshall had read *The Psychology of Radio*—later calling it “the historic moment” (Marshall, 1977)—and was especially intrigued by Cantril and Allport’s call for listener research (probably p. 271). The two men discussed radio research, and soon Cantril, together with Frank Stanton, submitted a proposal for what would become the Princeton Radio Research Project. Stanton, the future president of CBS, was in 1936 a discouraged researcher planning to depart the network for a career in academic research (Socolow, 2008a).

The proposal (Cantril [& Stanton], 1936), delivered to Marshall on New Year’s Eve, suggests an “objective analysis” of listener interests, in language that anticipates the “gratifications” approach to audience research that the Princeton Project would go on to establish: “What radio presentations do people really like and, above all, why do they like them and how are they influenced by them?”

In May 1937—a full year after Marshall and Cantril’s first meeting—the Rockefeller trustees approved a \$67,000 grant for a renewable 2-year Princeton Radio Research Project to be based at Princeton University (Rockefeller Foundation, 1937). To Stanton and Cantril, the Rockefeller approval was a mixed blessing. Five months had elapsed since their application, and in the interval Stanton had decided to remain with CBS. The Project—generously funded and ready to start—had no director. Cantril and Stanton approached Marshall with the idea of hiring someone else, leaving Cantril and Stanton as associate directors (Stanton, 1991–1996, session 3, pp. 105–106). With apparent reluctance, Cantril finally settled on Lazarsfeld, just weeks before the Project was scheduled to begin.

Paul Lazarsfeld (1901–1976), an Austrian emigre psychologist with an applied math doctorate, had recently established a fledgling research institute at the struggling University of Newark (see Figure 3).² He had modeled the Newark Research Center on an applied psychology institute he had founded in Vienna after his academic career there was blocked by anti-Semitism. With entrepreneurial pluck, Lazarsfeld and his staff produced a flurry of market research reports centered on interpreting consumer motives through careful questioning and analysis. An innovative 1930 study of working-class unemployment (published as Jahoda, Lazarsfeld, & Zeisel, 1933) caught the attention of Rockefeller Foundation officials, who urged him to apply for a traveling fellowship. The 32-year-old Lazarsfeld arrived in the United States in 1933 and spent the next 2 years traveling, building up a network of contacts in the social psychology and market research fields (Pasanella, 1994, pp. 10–12).

² There is a huge literature on Lazarsfeld’s life and career. On his early years in Vienna through his Newark Research Center, see especially Converse (1987, pp. 133–143); Fleck (2011, ch. 5); Morrison (1976, 2005); Zeisel (1979); and Lazarsfeld’s own (1969) memoir.



Figure 3. Paul Lazarsfeld.
Released by Bardwell Press to promote the biography
and collected research articles of Lazarsfeld.

Source: <http://en.wikipedia.org/wiki/File:Lazarsfeld.jpg>

After an attempted Nazi putsch in 1934, Lazarsfeld decided to emigrate from Austria permanently. With the help of Columbia sociologist Robert Lynd (1935)—whose *Middletown* (written with Helen Lynd) had partly shaped the unemployment study—Lazarsfeld accepted a post as director of a government-funded research project at the University of Newark. Just a few months later, in late 1935, he convinced the university's ambitious president to convert the project into a permanent organization, the Research Center of the University of Newark (Lazarsfeld, 1969, pp. 287–291; Morrison, 2005, p. 69). The Center, housed in an abandoned brewery nearby, had much the same character that distinguished Lazarsfeld's past and future organizational endeavors: a young, energetic and enthusiastic staff supported by commercial research contacts (Morrison, 2005, p. 107). Thus, when Cantril approached him, Lazarsfeld had only recently established a niche for himself in applied psychology.

Both Cantril and Stanton already knew Lazarsfeld. The Austrian had given a talk to Gordon Allport's Harvard seminar back in 1933 (Lazarsfeld, 1969, p. 298). There he met Cantril and discussed the radio lab experiments that Cantril and Allport were conducting for what would soon become *The Psychology of Radio*—a book that cited Lazarsfeld (Cantril & Allport, 1935, pp. 34, 94, 95). More significantly, Lazarsfeld and Stanton had established "friendly relations" (Lazarsfeld, 1969, p. 304), and by

1936 the two men were even discussing collaboration on a marketing book (Stanton, 1936). So it was natural, with the Princeton Project directorship suddenly vacant, that Lazarsfeld's name was floated.

After several senior figures turned him down, Cantril cabled Lazarsfeld with an offer in late July or early August, when Lazarsfeld was on a six-week visit with family back in Austria (Lazarsfeld, 1969, p. 305). Cantril was desperate; the Project's start date was just weeks away. Cantril's (1937b) cable began a brief but intense epistolary psychodrama that, in outline form, presaged the two men's conflicts to come.

In his reply by cable, Lazarsfeld wrote that he was "much interested," but "wondering if some connection of project with [Newark] Research Center" (Lazarsfeld, 1937a). His concern over the Center's fate reflected his broader sense of precariousness as a foreigner and as a Jew in Depression-era America. Indeed, he viewed the Center as a self-authored insurance policy, one that a native-born, Ivy League-affiliated scholar like Cantril could hardly appreciate.

Cantril and Lazarsfeld were divided by circumstance and background in ways that played out in the awkward courtship over the Project post. Cantril was privileged, if not by birth—little is known about Cantril's childhood in Utah and Oregon—then by Ivy League pedigree. He was valedictorian at Dartmouth (A. Cantril, 2004, p. 387), which, like other Ivy League schools, remained in the interwar years a bastion of WASP exclusivity (Karabel, 2006). At Dartmouth, Cantril had even roomed with Nelson Rockefeller (Glander, 2000, p. 85), a connection that later yielded a prominent wartime post.

Cantril was debonair and polished, as Converse (1987) concluded based on interviews with contemporaries: "He was unusually skilled and successful in maneuvering in the circles of wealth and power . . . tall, good-looking, charming, a commanding presence, he had an unflinching ability to 'be important'" (p. 144). Stanton remembered him as a "very bright star and a very attractive person" with a "lot of charm" (Stanton, 1991–1996, session 3, p. 119) appreciated by key funders like Marshall. When he approached Lazarsfeld, Cantril had just been promoted to associate professor at Princeton, secure in his perch at an elite university.

Lazarsfeld's place in the U.S. academy, by contrast, was hard-fought and unstable. Just after he had decided in 1935 to permanently immigrate to the United States, he learned that a promised job at the University of Pittsburgh was withdrawn (Pasanella, 1994, p. 11). Lazarsfeld opted to spend his last Rockefeller fellowship money on a third-class transatlantic boat trip, arriving in New York as "the classic immigrant, penniless" (Pasanella, 1994, pp. 303–304). It was only after arriving in the United States that the Newark opportunity emerged.

Parlaying a government contract into the Newark Center was, for Lazarsfeld, a survivalist improvisation—as was his assiduous cultivation of contacts and professional friendships. Even his abrupt and decisive abandonment of socialist politics can be read as an adaptation to strange new surroundings. If Lazarsfeld's integration into U.S. academic life was more "successful" than the often-wrenching experiences of other Nazi refugees (Neumann, 1953), the explanation lies in his careful, tenacious effort to build—brick by brick—his own institutional refuge.

In the summer of 1937, Cantril and Lazarsfeld were locked in a tense negotiation, with the Project and Lazarsfeld's career hung in the balance. In a pair of extraordinary letters written a day apart—which crossed in transit—the two men addressed objections they anticipated from the other in a shared tone of high ingratiation.

Cantril's letter (1937a) appealed to Lazarsfeld's uncertain future:

My thought was that with two years at good pay and with plenty of money and help, you could really put out a couple of first-class studies that would put your name at the top of the list not only for radio research but for this type of research in general.

He expressed confidence that Lazarsfeld could secure a post when the Project was finished—"perhaps back at Newark if you like, perhaps in the radio industry, perhaps with [George Gallup's] American Institute—nothing definite but all possibilities." Somewhat portentously in light of what was to transpire, Cantril added, "Anything that would be published . . . would be entirely yours."

Most striking of all is the letter's imploring tone. With Stanton out, Cantril writes, "I am stuck and will refuse to carry on myself since, as you know, my heart is not in this type of research as much as yours. Furthermore," he continues, "I am not too good at it. Lord, but I'd be relieved and happy if you would accept. . . . It's too bad," he added, "that I have to bother you at this time and that we can't light up a big cigar and talk it over leisurely." As a last inducement, he refers to the "research associate" title Lazarsfeld would have at Princeton. "Doesn't that appeal to your bourgeois soul!"

Lazarsfeld's equally long letter (August 8, [1937]), sent off the day before, is also revealing—filled with self-deprecating charm and barely suppressed anxiety. It is a "queer experience," he admits, to write sitting "in a mountain village . . . lying among flowers in front of glaciers." The letter tacks back and forth between gratitude and misgiving. He expresses concern about his current commitments and admits to worry over the "limited duration of your project," especially since the Newark Center would likely "collapse" in his absence. "I feel strongly that I don't want to go ahead alone," he writes, "that I want to stay for an institution and I try to build up an institution which is able and willing to stand for me."

Lazarsfeld elaborates the point with a sharp contrast to Cantril's relative privilege:

You see all comes back to an European attitude which might be not so easy to understand from your point of view. . . . Of course, I will have to do very different things, less glorious but about the same as you are a Prof in Harvard, then in Columbia, then in Princeton. But as my poise and my past and my name cannot compare with yours, I try to identify whatever I do with an institution which might after some time acquire the dignity which I myself ro [sic] reason of destiny and may be of personality can hardly aspire at.

Yet Lazarsfeld ultimately tacks back to gratitude. If Lynd advises acceptance, he writes, "I shall do it. After all I owe my whole American existence to him and to the fact that I always followed his leads, and I shall not stop now."

Mounting Tensions

With the Project under way, relations between Lazarsfeld and Cantril deteriorated quickly. No single incident or exchange was responsible. Newark was involved, as was the Project's basic direction; budgetary crises and research-topic anarchy contributed, too. Lazarsfeld's aggressive leadership style, and even his womanizing, stoked the conflict. When Orson Welles broadcast "War of the Worlds" in the fall of 1938, the two men had been sparring for more than a year.

The Newark Center was an obvious point of contention, and one that needed resolving right away. Lazarsfeld, freshly returned from Austria, met with Stanton and Cantril in early September, days into the Princeton Radio Research Project's official tenure. Stanton (1991–1996, session 3, p. 107) remembers "this hulking, cigar-smoking, heavy Viennese-accented male, who looked upon Cantril and Stanton as little boys who didn't quite understand what was going on in the world."

Determined to keep the Newark Center alive, Lazarsfeld convinced his two new associate directors that the radio project would operate out of Newark—with a small office in Princeton to keep up appearances. In an account sent to Lynd, Lazarsfeld boasted about Cantril's concession that Newark would serve as the Project's "actual headquarters," requiring trips to Princeton "only for incidental meetings" (quoted in Lazarsfeld 1969, p. 307). In practical terms, he continued, "the whole arrangement therefore means the Research Center has a huge new job" (quoted in Lazarsfeld 1969, p. 307).

In accepting the directorship, Lazarsfeld made it clear that he viewed the Project as a convenient vehicle with which to pursue his ongoing methodological inquiries. Throughout the fall, Lazarsfeld and his small staff continued work on preexisting Newark studies, drawing on Princeton Project funds. Even into the fall of 1939, the Newark–Princeton "symbiosis" (Lazarsfeld, 1969, p. 308) remained an irritant to Stanton and Cantril. "Who handles the telephone calls at the new office," wrote Stanton (1938) in a brief memo in September to Lazarsfeld, "—and have you considered a standard name since it is no longer the Newark Research Center?"

A second source of tension was Lazarsfeld's sometimes overbearing treatment of his fellow directors. His direct and forceful leadership style—a product in part of his Germanic academic training, but also a result of his experiences running the Vienna and Newark centers—clearly chafed at Cantril. It is striking to read the early Project memos from Lazarsfeld, which are filled with orders for Cantril and Stanton. Cantril (1939b), in one memo, refers to Lazarsfeld as "Herr Director."³

Early in the fall, Lazarsfeld decided to scrap the original Project plan that Cantril and Stanton had drafted. At one of their initial meetings, Lazarsfeld—referring to the original proposal as the Old

³ Lazarsfeld (1969, p. 300) later recalled the comment as a thinly veiled reference to his "foreignness."

Testament—told them he was going to write the New Testament (Pasanella, 1994, p. 12; Stanton, 1991–1996, session 3, p. 108). The plan (Lazarsfeld, 1938a) emphasized the re-analysis of existing polling and ratings data—a research strategy that Lazarsfeld, under the label of “secondary analysis,” would later make famous. Stanton’s CBS, along with George Gallup’s Institute, were major sources for the raw data (Lazarsfeld, 1969, pp. 308, 310–312). Notable, too, were the sweeping, all-inclusive plans for a methodologically diverse array of studies, most of which were never carried out.

The rift between Lazarsfeld and his associate directors, especially Cantril, was made worse by Lazarsfeld’s erratic management style and the Project’s chaotic financial state. The early Radio Project, like nearly all of Lazarsfeld’s past and future organizational efforts, was a highly improvisational affair, with missed deadlines, hasty report writing, and sloppy accounting. As Stanton (1991–1996, session 3, p. 113) recalled,

Paul would never keep an appointment. We’d have a luncheon engagement, he’d show up an hour-and-a-half late, for example. Exasperating. He’d do it to everybody. He forgot when he was supposed to be someplace and—he never had money with him. He wasn’t a very orderly person in his personal affairs.

Lazarsfeld was “bubbling with ideas,” but “didn’t know budgets” and “couldn’t find the mail he had received, nor the letters he had written” (Buxton & Acland, 2011, p. 197). In his memoir, Lazarsfeld (1969) admits that he was “rather rude to assistants and students” in these years, “barking at them when they fell down on an assignment” (p. 301).

The relationship between the Project’s three directors hit a crisis in the spring of 1938, as Lazarsfeld’s mismanagement of the budget forced Rockefeller to reallocate funds intended for the second year to cover overruns (Rockefeller Foundation, 1938). In a polite but charged memo referring to staff discontent and financial chaos, Cantril (1938a) admits to a “feeling of bewilderment and apprehension when I see our March expenses.” He insists on moving financial control of the project from Newark to Princeton, with palpable concern about his own standing. If Princeton or the Foundation heard of any “serious trouble, I am sure it would greatly jeopardize our chances for an extension of the grant, and would seriously reflect on the administrative ability of the directors of the project.”

Lazarsfeld apparently refused, and the crisis escalated. Cantril (1938b) followed up with a demand that Lazarsfeld sign a note of financial responsibility and agree to strict financial controls. “I am sorry,” he adds, “that I have had to be such a dictator . . .,” but if Lazarsfeld fails to cooperate, he “shall be forced to bring the matter to the attention of the authorities at Newark and Princeton.”

Evidently, a contentious meeting took place, and two weeks later Lazarsfeld (1938c) wrote a note to Stanton in which he refers to “this grotesque situation”: “Perhaps you have a good idea of how I should handle it in order not to keep my whole staff busy clearing up Had’s misunderstandings.”

In a pair of memos to Stanton and Cantril (1938b), he minces no words: “The misunderstandings of which Had was a victim have been so spectacular that I am afraid that we shall have to try to come to

the psychological basis of them." He demands a statement from Cantril "regarding his misunderstanding," conceding that "my way of working and handling people is certainly very different from his." He concludes with budget-related pique: "I estimate the costs of this memorandum and the time spent on preceding clarification to be about \$25."

Layered atop all of these management and personality conflicts was an alleged incident with Cantril's wife that is difficult to place in terms of time and referred to in only one source, an oral history interview of Stanton (1991–1996, session 3, pp. 110–111):

Well, Paul was a great womanizer and Hadley was not happy with Paul's behavior vis a vis Hadley's wife. I don't know what happened but I guess in the big world it would be given a mention or a footnote, but at that time Cantril pretty much turned on Lazarsfeld. . . . I don't know whether Hadley's wife, whose name was Mavis, was infatuated or whether she really was just playing with Paul, or whether she was beginning to fall in love with him for real, I just don't know. But certainly Cantril soured on Paul.

The Project's budget problems continued into the summer (e.g., Lazarsfeld, 1938d). Meanwhile, the University of Newark pulled its support for the Research Center (Lazarsfeld, 1969, p. 309). Forced to look for new offices, Lazarsfeld apparently ruled out Princeton, where, he later remarked, he had "never spent a night" (Lazarsfeld, 1973). The Project's operations—labeled "Field Headquarters" on stationery—moved, instead, to Union Square in New York City (Lazarsfeld, 1969, p. 309).

In the fall of 1938—in the midst of the Newark Center's collapse—the financial turmoil, the many unfinished and abandoned studies, and Lazarsfeld's managerial style all contributed to an overall sense that the Project was failing. The scholars working under Lazarsfeld had completed some initial, intriguing work. However, most of that work—such as the now legendary study of radio music completed by Theodor Adorno—did not seem particularly useful or even related to the stated goals of the project.⁴

Memos exchanged over the summer express more and more anxiety about, in particular, the Project's lack of a coherent plan for publication. As Lazarsfeld (1969, p. 317) remarked in his memoir, for all the Project's data riches and its staff's enthusiasm, the "image of the office was not good." No central theme was "visible," and Lazarsfeld and the rest of the office began "hearing rumors that important people questioned whether we knew where we were going." The Rockefeller grant was coming up for renewal the next year, and Lazarsfeld realized that he had to demonstrate results in some published form. "Something," he recalled, "had to be done."

It was in this climate of personal conflict and organizational chaos—the fall of 1938—when Orson Welles broadcast his infamous parody of a Martian invasion.

⁴ Adorno's stint at the Princeton Project has been extensively documented. See Adorno (1969); Fleck (2011, ch. 5); Jenemann (2007, ch. 1–2); Lazarsfeld (1969, pp. 322–326); Morrison (1978); and Wheatland (2005).

The War of the Words

Late in the evening of Sunday, October 30, 1938, Frank Stanton and his wife Ruth hurriedly drove down Madison Avenue toward CBS's headquarters building at the corner of 52nd Street. On the car radio, they caught the climax of "War of the Worlds." Stanton realized earlier in the hour that the excitement and reports of panic that had begun to circulate represented one of the most fortuitous research opportunities in the history of radio. Upon arriving at the CBS building, he parked his car, took the elevator to his office, and composed a questionnaire—as quickly and accurately as possible—on the effects of the program. He telephoned Lazarsfeld for a quick consultation, and then phoned the Hooper Holmes Company in Atlanta, Georgia. Hooper Holmes specialized in personal interviews for the insurance industry and, importantly, did not rely solely on telephones for its survey work. Stanton carefully went over the samples he was interested in—by economic class, rural or urban, and other demographic considerations—and the next morning fieldwork had commenced (Buxton & Acland, 2001, pp. 212–216; Stanton, 1991–1996, session 3, pp. 115–117).



Figure 4. The New York Daily News front page, October 31, 1938.

Source: <http://www.nydailynews.com/entertainment/tv-movies/remembering-war-worlds-gallery-1.25382?pmSlide=4>

Stanton (1991–1996, session 3, p. 116) recognized the unique research opportunity, but also “suspected that we [at CBS] were going to be charged with having stirred up the population.” As it turned out, the FCC did not file an official complaint, and the “firehouse” data Stanton had culled was never published. But Stanton’s study served as one of the main data sources for *IFM*, supplying the bulk of the evidence for the book’s important claim about “critical ability” and education.

Because of his crushing workload at CBS, Stanton could play only an advisory role in the Project’s November scramble to secure emergency funding for follow-up research. It was a fateful month: While Lazarsfeld and Cantril orchestrated a successful effort to obtain \$3,000 from Rockefeller’s General Education Board, Herta Herzog (Lazarsfeld’s second wife) conducted a series of in-depth interviews with frightened listeners. Based on the interviews, Herzog (1938) proceeded to draft a memo of preliminary analysis whose themes *IFM* later echoed with remarkable fidelity. Nevertheless, her contributions—along with those of Project staffer Hazel Gaudet—were barely recognized in the published study. It is a revealing irony that, as Cantril and Lazarsfeld battled over credit and oversight for more than a year, much of the actual intellectual work was conducted, invisibly, by these two women, as we document in a companion article (Pooley & Socolow, 2013).⁵

The key probe in Herzog’s memo was her extensive discussion of “checking up,” a concept she apparently invented (1938, pp. 9–11, 14). The published book’s most celebrated finding was its linkage of some listeners’ “critical ability” with their tendency to seek out and confirm the broadcast’s fantastic nature against other evidence. Although Herzog did not, in this early memo, tie checking up to education or critical ability, she identified the immense significance of checking up to any further study.

Meanwhile, Lazarsfeld and Cantril plotted to convince Rockefeller to release the emergency grant. Lazarsfeld hoped that publications deriving from the Welles studies would shore up the Princeton Project’s shaky case for renewal. In a mid-November memo to Cantril with the playful “From: Orson Welles, Director of Publications, PRRP” heading, Lazarsfeld (1938e) admitted that he was “much worried about the fact that the prolongation of the project will come up with Marshall and the Foundation at a time when no major initiative of the project will be finished.”

Though Lazarsfeld apparently deputized Cantril to direct the Welles studies, archival records make abundantly clear that he never intended Cantril to take sole authorship credit for any eventual publication.⁶ Cantril (1938c) did draft the Rockefeller proposal, which he sent in late November. Perhaps at the suggestion of Rockefeller officials, Cantril submitted his proposal to the Rockefeller Foundation’s sister fund, the General Education Board (GEB). Cantril and Lazarsfeld had learned that the Institute for Propaganda Analysis (IPA) was also interested in a study that could be customized for distribution to schools. Cantril, during his short stint at Columbia’s Teachers College, had been the IPA’s founding board chairman and remained a member of its board (Sproule, 2005, ch. 5). Despite his close ties to the

⁵ This article and the companion chapter share a number of passages. Reprinted with permission.

⁶ Pasanella (1994, p. 15), in her guide to Lazarsfeld’s papers, refers to a “November memo,” which she quotes: “Had will be in charge of the study and will draw a compensation of \$400 for it.” The memo could not be located in the Lazarsfeld papers.

Progressive antipropaganda education group, Cantril would maneuver, in the months ahead, to block the IPA from any direct say in the Welles study. In his initial proposal, however, he played up the IPA connection, perhaps with the General Education Board's K-12 mission in mind.

The panic over the Welles broadcast, Cantril (1938c, p. 1) wrote, provides an "almost unparalleled source of data" for both social psychologists and educators concerned about propaganda. He promised a complete report to the GEB in "2 to 3 months," and added that the findings would be published by the Princeton Project. He emphasized that the Project directors ("all trained psychologists interested in radio") had "already cooperated on a preliminary survey" (ibid., p. 3)—thereby rendering invisible Herzog's crucial contribution. Though he touted a planned school report to be distributed by the IPA, he insisted that he, Cantril, approve any such booklet. He also stressed that "this report shall be above all else a presentation of the data obtained and the conclusions reached by the Princeton Radio Project rather than an interpretation of the data by the Institute itself" (ibid., p. 4).

Cantril's (1938c, p. 3) proposal referred to a "written report by the [Project] directors." The clear implication that the Welles study would be coauthored by Lazarsfeld and Cantril (and possibly Stanton) was confirmed at a meeting with a GEB official the next day. According to the GEB official's account (Havighurst, 1938), the write-up would be "published by the Princeton group." Lazarsfeld, the official recorded, "estimates that he could do a good job with \$3,000," and has "already had members of his staff make thirty interviews." The focus on Lazarsfeld (and not just Cantril) is notable, as is the second erasure of Herzog's contribution.

In late November—less than a month after the Welles broadcast—Rockefeller's General Education Board (1938) awarded the \$3,000 grant. In the months following the award, Cantril seems to have decided to assert control—and ultimately authorship credit—over the Welles study. The evidence for this is, in part, Cantril's move against the IPA's involvement, but also, ironically, his own self-promotion.

Although cooperation with the IPA had been written into the GEB grant, Cantril pushed back against the Institute's attempt to play a role in the Welles study. With obvious pique, the IPA's education director Violet Edwards (1939a) contacted Cantril about a missed meeting. "I'm sorry, particularly," she wrote, "that we could not meet and discuss the possibilities of the Institute's participating in the proposed study." She then asked Cantril if he had any reactions to a list of proposed "cooperators" on the "complete study—statistical data and interpretation based on data," naming 13 prominent social scientists (including Cantril himself). In his answer, Cantril (1939a) writes that the "names of the possible 'cooperators' somewhat puzzle me." The Institute could distribute educational literature based on the Princeton Project's findings, though he, Cantril, would need to sign off, "to prevent any distortion of facts that might slip in quite honestly." But "any 'organized' cooperation" on the Welles study itself "would complicate our own already complex research job." The IPA's Edwards (1939b) pushed her case again, noting that she had met with Lazarsfeld and that he was sending Hazel Gaudet to further discuss the study's findings.

The correspondence dies off, but Cantril—ostensibly still an IPA board member—seems to have prevailed, since no further record of IPA cooperation exists, nor did the propaganda-education group ever distribute school materials based on the Welles findings (Sproule, 2005, ch. 5).

Cantril's increasingly brazen efforts to publicize his role directing the study soon led to another stormy confrontation with Lazarsfeld. Since the first days after the Welles broadcast, Cantril had been feeding stories about his role in the study to Princeton University press outlets. A November 2 story in *The Daily Princetonian* centered on the Project's planned study, with Cantril as the unmistakable source. The piece ("Welles' Broadcast Aids Psychologist," 1939) concludes with the time and room location of Cantril's social psychology course, directing readers to a "lecture today touching the Orson Welles broadcast and the aspects of mob behavior that were brought out by it." A follow-up piece the next day ("Martian Invasion' Treated by Cantril," 1939) recounts the lecture.

In December and January, these stories became more explicit about Cantril's leading role. A December 19 Associated Press story ("Men From Mars' Not a Dead Issue Yet—Savants Enter Case," included in Cantril, 1938–1951) describes him as the study's director, as does a January 6 piece appearing in *The Daily Princetonian* ("Cantril Directing Hysteria Analysis," 1939).

Lazarsfeld had apparently not seen these stories, but did come across yet another article, running in *The Princeton Alumni Weekly* in mid-January ("Psychologists to Study Martian Hysteria," 1939). The story, referring to "Dr. Cantril's study," states that the Welles project "will be greatly aided by work already performed at Princeton by Dr. Cantril in the Princeton Radio Project." Lazarsfeld was not mentioned, and he wrote Cantril about the oversight. Though Lazarsfeld's letter of complaint does not survive, it is clear from Cantril's reply (quoted in Morrison, 2005, pp. 73–74) that Lazarsfeld had reacted angrily to the *Alumni Weekly* article. Cantril took obvious umbrage at Lazarsfeld's accusation:

I am glad you expressed yourself on the release, but I must say that the reaction seems a bit infantile. Perhaps we should have directors' uniforms with differential insignia. It is hard to imagine people like [Lawrence K.] Frank, [George] Gallup, [Gordon] Allport, [Daniel] Katz, [Samuel] Stouffer would maintain petty jealousies, and I should like to think that you, too, would have sufficient perspective not to let such trivia bother you. . . . In the official university release I clearly indicated that the whole project was under your direction.

Cantril writes that "I seldom see the sheet," and that the "report seemed quite harmless." He continues:

If the project could go on completely without me I should honestly be much happier. But apparently I am a strategic link in the chain. I am willing to play the role only for two reasons: (1) [Princeton President] Dodds feels that we should not tell the Foundation outright that we do not want a renewal; (2) I am anxious to help you make a reputation and attain some sort of eventual security in these highly insecure days. Please believe me that these are my only motives. . . . If I have to become involved in many emotional reactions, I may reconsider my whole position.

That the professed concern for Lazarsfeld's "eventual security" is juxtaposed to the issue of the Project's renewal is ominous, since Cantril's letter carries the unmistakable implication that Princeton was no longer

interested in serving as host. With the Newark Center shuttered, Lazarsfeld's security depended on the Project's renewal.

At stake here, too, was Lazarsfeld's sense of the prerogatives of the directorship: he, and he alone, should decide who directs a Project study. This extended to authorship as well: When another Project book was issued, in 1940 as *Radio and the Printed Page*, it was solely credited to Lazarsfeld—despite the fact that many of its constituent chapters were written by Project subordinates (including Herzog). It is likely that *The Invasion from Mars*, had Lazarsfeld succeeded in maintaining control over its destiny, would also have followed this director-as-author practice.

The Project's fate was hanging in the balance. Just days before the nasty exchange between Lazarsfeld and Cantril, the Foundation's John Marshall initiated a review to consider the Project's renewal (Rockefeller Foundation, 1939a). Marshall's committee, a mix of academic and industry representatives, issued a report in March recommending renewal, but with a renewed focus on the "detailed analysis and interpretation of some of the material collected to date" (quoted in Morrison, 2005, p. 79). Rockefeller officials, however, opted to delay the renewal pending a more coherent write-up of the Project's research to date. Marshall cabled Lazarsfeld in mid-March: "DISCUSSIONS IN OFFICE INDICATE RELUCTANCE TO INVEST IN NEW RESEARCH PENDING FORMULATION OF PRESENT FINDINGS STOP FEELING HERE THAT NEED IS FOR BREATHING SPELL TO SAVE PROJECT FROM BEING VICTIM OF ITS OWN SUCCESS [*sic*]" (quoted in Morrison, 2005, p. 79).

Marshall gave Lazarsfeld until June 1 to assemble the Project's eclectic research portfolio into a summative manuscript. The Project staff threw themselves into the project—"day and night literally" (Lazarsfeld, 1969, pp. 328–329)—and submitted the draft on the morning of the deadline. Marshall was satisfied, and the manuscript was published the following year as *Radio and the Printed Page* (Lazarsfeld, 1940; see also Stamm, 2010).

Cantril (1939c) successfully used the Project's "breathing spell" to press the Foundation to accept a delay in his delivery of the Welles write-up. He took at least some of the time, however, to chart out a new solo project independent of Lazarsfeld and unconnected to radio.

Just two weeks before the Project's June 1 deadline, Cantril (1939d) sent off a funding query to Stacy May, assistant director of Rockefeller's Social Sciences Division. Cantril was "hoping to play much less an active role in the Radio Research," and proposed instead a large-scale re-analysis of George Gallup's vast store of polling data.

The Foundation (May 1939) initially turned Cantril down. The Nazi invasion of Poland on September 1, however, abruptly changed the Foundation's funding priorities. Marshall's media-related portfolio, in particular, mobilized to support morale and propaganda activities that were politically off-limits for the Roosevelt administration (Gary, 1996).

September 1 was also the date that Cantril's affiliation with the Project officially expired. Both Cantril and Lazarsfeld perceived the Foundation's priority shift and swiftly adapted their respective plans

to match the new climate. The two men were soon fighting over CBS and Gallup data and vying for new Rockefeller funds—all of it layered atop the ongoing Welles conflict.

Cantril (1939e) wrote to Marshall in mid-September that he had just received a Lazarsfeld memo “regarding an extension of the Project to cover some of the radio problems arising out of the present European situation.” He had also “gathered the impression that you and the others in the Foundation were interested in having some studies made very shortly on the effects [*sic*] of war propaganda, changes in attitude, and the like.” He was, he wrote, “very dissatisfied” with Lazarsfeld’s memo, for “the new problems are far too important to become mere appendages of research already in progress.” With considerable brio, Cantril proceeds to outline in great detail his Gallup proposal, reframed as a study of Americans’ attitudes toward the war. He concludes:

Please forgive me for butting into any plans you and Paul may have. Naturally, I have not written this to Paul and should prefer that you do not mention it to him. But I think one should definitely take a fresh start on so important a matter and, if possible, not be arbitrarily limited by a “communications” category.

In his reply, Marshall (1939) confirmed that he is “still holding strictly to the position” that the Princeton Project “undertake no fresh investigation until the present work of formulation is virtually complete.” That could only change, Marshall added, with your “full concurrence and in all probability only on your initiative.”

As for the Gallup proposal, Marshall continued, “I am of course particularly interested. . . . As a matter of fact, the whole question which underlies your letter is now being canvassed as rapidly as possible.” Marshall, who remained supportive of the Project under Lazarsfeld’s leadership, was now poised to take advantage of Cantril’s new independence. In follow-up correspondence Marshall encouraged him to submit a revised proposal, which Cantril (1939f) delivered in mid-November.

Up until the publication of *The Invasion from Mars* in March 1940, Lazarsfeld and Cantril kept up their interlocked fight over Rockefeller money, the Project’s future, and the Welles study itself. At the same time, both men needed the other’s cooperation. Cantril discovered that, as a practical matter, he could not get the Welles manuscript published without Lazarsfeld’s clearance. Lazarsfeld, likewise, came to realize that his plan to relocate the Project to Columbia could not move forward without Cantril’s tacit cooperation.

In a mid-October memo to Cantril, Lazarsfeld (1939) pressed his case that *IFM* should center on “checking up,” the theme that Herzog had highlighted almost a year earlier. Lazarsfeld wrote that he has “a still stronger feeling that the emphasis of your study should be very strongly upon checking up.” The fact that people panicked, Lazarsfeld continued, is not compelling. “However, what is so extremely interesting and deserves all generalization is the fact that after people were scared they were not able or not willing to check up to see whether it was true or not.” Lazarsfeld, an especially savvy packager of concepts (Platt, 1996, ch. 7), urges Cantril to find a better phrase than “checking up,” so that the idea

"could be more easily merchandized." Though collegial, Lazarsfeld's memo also served a tactical purpose: he was angling to bring Herzog back into the study, presumably to secure her coauthorship credit.⁷

Cantril, previously eager to maneuver Herzog away, was suddenly receptive. In an undated reply to Lazarsfeld, Cantril (1939j) wrote that he "simply MUST" submit the Welles manuscript by mid-November. Although he had only recently disparaged Lazarsfeld's war-related memo to Marshall, Cantril assumed a chummy tone. "So COULD Herta go at the job in the very near future?" he asked, estimating three days of "rather concentrated work." In the same jovial tone, he proceeds to nullify any future credit Herzog might claim: "God knows what her reward will be—except my continued admiration for her ability and a eulogistic footnote in the last chapter." There is no record suggesting that Herzog agreed to help under these conditions.

Lazarsfeld and Cantril continued to spar over the Welles manuscript. In a late November exchange, Cantril (1939g) flatly refused to make substantive changes suggested by Lazarsfeld, citing the impending deadline.

Since you and Frank [Stanton] have both read it carefully once, since I am satisfied that I have taken account of your suggestions, and since [Gordon] Allport—as a complete outsider—has caught no errors or misinterpretations . . . I have reached a stage where I must stop any major revisions.

Lazarsfeld scribbled angry challenges to Cantril's deadline claims in the margin. Needless to say, Cantril's request, in the same letter, for Lazarsfeld's foreword was never answered—as the book was published without one.

Lazarsfeld answered with an apparently bitter memo, judging from Cantril's (1939h) curt reply: "I shall refrain from answering your classic letter. But it is hard to do so."

Just days later, however, the two men met face-to-face in a fascinating yet mysterious denouement to their long struggle. No account of the meeting survives, though Cantril (1939i), in coordinated letters to Lazarsfeld and Marshall, strikes a surprisingly conciliatory tone. I am writing, Cantril explained, to repeat what "I told you at the end of our discussion today—that I now for the first time honestly see what has been bothering you about the Invasion from Mars study." Your methodological criticisms, Cantril writes, are finally clear. Your failing is not the "common charge of 'mismanagement,'" but instead impossibly high standards. "So I can admit—since I now understand—that many of your troubles have been over genuine methodological procedures."

The letter comes off as a less-than-genuine statement, a suspicion confirmed, perhaps, by its transparent performativity. Indeed, the letter closes on a note of saccharine harmony that the two men's

⁷ In his memoir, Lazarsfeld (1969) acknowledges his effort to secure Herzog recognition: "at that time I had hoped Dr. Herzog would receive a major share of the credit for her imaginative work on that study" (p. 313).

history renders implausible: "Personally, I am enormously relieve [*sic*] that we at last know what has been the cause of our minor difficulties. In order that John Marshall should know how I feel, I am sending him a copy of this letter." That their difficulties were "minor" and mendable seems a message intended more for Marshall than for Lazarsfeld.

The accompanying letter to Marshall doubles down on the first letter's praise:

Today, however, [Lazarsfeld] was able to verbalize for me in a really brilliant way his objections to my study and other studies of the project (including his own). I can see now for the first time the fundamental reasons for the delay we have been worried about with respect to the project. And in all fairness to Paul, I did want you to know at once that the delay now makes sense to me and would, I feel sure, make sense to anyone if Paul explained it to them the way he explained it to me.

Though impossible to prove, it is plausible to read Cantril's sudden camaraderie as evidence of a deal between the two men—a reconciliation of mutual expediency. After all, Cantril could hardly move forward with the Welles submission over Lazarsfeld's vociferous objections. Likewise, Lazarsfeld was days away from learning the fate of his proposal to relocate the Project to Columbia University—a move that Cantril could attempt to block. Both men, moreover, depended very much on Marshall's near-term favor: Lazarsfeld for the Columbia move, coupled with a three-year extension, and Cantril for the pending war-opinion grant. Cantril's closing paragraph, at any rate, strengthens this interpretation:

In a conversation with [Princeton] President Dodds the other day, I gathered that he and [Rockefeller official] Mr. Stevens were quite worried about the slow rate of productiveness of the project. I can quite understand their point of view. But I should be very glad to discuss with Mr. Stevens, if you think it at all advisable, what I have finally learned about Paul's difficulties and the reasons for what has seemed a publication blocking.

Three days later, Cantril won the \$15,000 grant (Rockefeller Foundation, 1939b). Soon after, he formally established a new Office of Public Opinion Research at Princeton to host his war-opinion polling and Gallup re-analysis. Just over a week later, Lazarsfeld learned that his proposal to extend and move the Project was approved (Morrison, 2005, p. 75). It is likely that the pair of December grants were coordinated, enabling the two men to disentangle their long-fractious union.

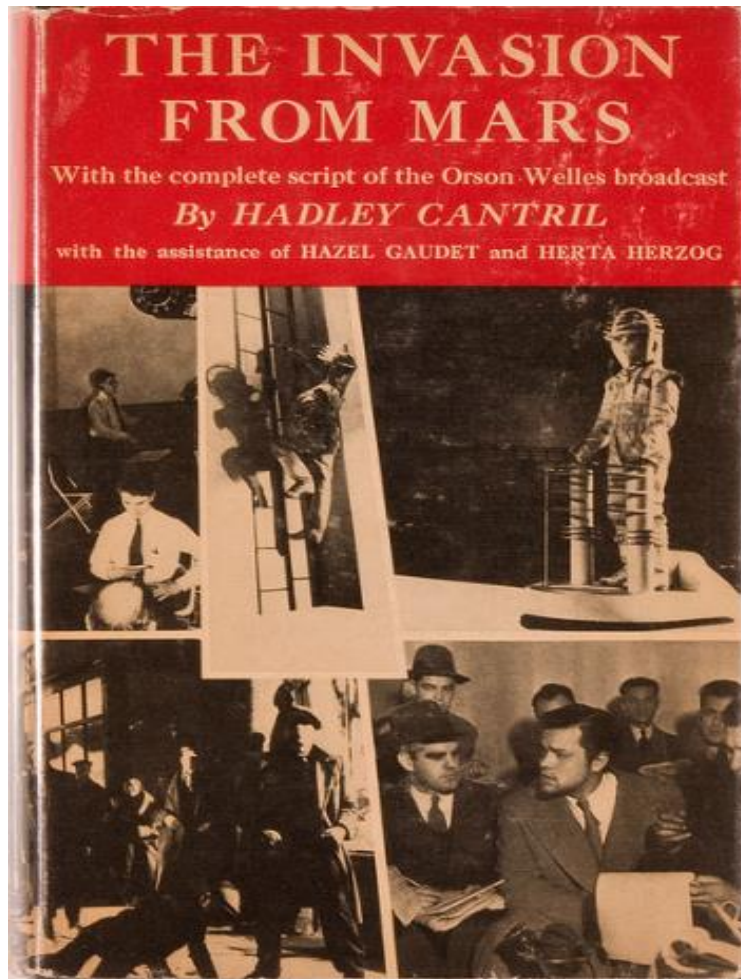


Figure 5. Cover of *The Invasion from Mars*, Princeton University Press, 1940.

Source: <http://www.flickr.com/photos/44823747@N02/4725954329>

Conclusion

The “Mass Hysteria” study was finally published in March 1940 as *The Invasion from Mars: A Study in the Psychology of Panic*, with Cantril listed as sole author—though there is a “with the assistance of” credit for Herzog and Hazel Gaudet (see Figure 5). The *Invasion’s* popular style, and its arresting topic, made for rapid sales, and the book was later issued as a mass-market paperback. In the remembered history of media research, the book is exclusively associated with Cantril, and most bibliographic references drop the “with the assistance of” credit altogether. The book’s ties to Lazarsfeld—as well as Herzog, Gaudet, and Stanton—have long been forgotten.

Lazarsfeld, of course, took the Radio Research Project to Columbia University. Recast as the Bureau of Applied Social Research, the institute served as a model for other postwar research shops and was widely recognized as a leading center of American sociology (Barton, 2001; Morrison, 1976). The Bureau's three book-length panel studies, culminating in *Personal Influence* (Katz & Lazarsfeld, 1955), helped establish Lazarsfeld and the Bureau as an intellectual foundation for the newly organized discipline of communication.

Cantril's and Lazarsfeld's parting of ways had the effect, we conclude, of obscuring the commonalities between *IFM* and the rest of the PRRP/BASR output. The book's principal argument, developed in its core chapters and conclusion, is that some listeners—those with "critical ability"—successfully "checked up" on the Welles broadcast's veracity. The checking up thesis, first elaborated by Herzog, was featured prominently owing to Lazarsfeld's prodding. In that sense, *IFM*—along with the PRRP's other book-length publication from the period, *Radio and the Printed Page* (Pooley, 2006a, pp. 265–275; Stamm, 2010)—anticipated the Bureau's later, more celebrated emphasis on the selective audience. *IFM* should be read as an early installment in a long-running PRRP/BASR's research effort to understand the audience's complex and differentiated experience of mass media.

IFM has been remembered instead as discrete from—indeed, discontinuous with—the rest of the PRRP/BASR corpus. In the field's memory, *IFM* and the Bureau, along with Cantril and Lazarsfeld themselves, are not just disconnected; they are, more to the point, positioned as anachronistic tokens in the story of the field's research maturation. *IFM*, as a typical treatment reports, "opened the door to a host of similar studies, leading to what is now called Hypodermic Needle Theory (HNT)" (Danesi, 2010, p. 136). A few years later, the account continues, Lazarsfeld found that "the media had virtually no ability to change people's minds" (Danesi, 2010, p. 136). Similar two-stage portrayals—with Cantril and *IFM* in the magic bullet camp, superseded by the Bureau's limited effects findings—abound in handbook chapters (e.g., McDonald, 2004, pp. 187–189), methods texts (e.g., Riffe, Lacy, & Fico, 2005, pp. 6–8), and historical accounts (e.g., Scannel, 2007, ch. 1). *IFM* and the Bureau, in short, are remembered as a two-stage mnemonic set piece.

In an important sense, Cantril and Lazarsfeld are responsible for the field's distorted memory. Their dispute—and its peculiar, clean-break resolution—furnished an incentive to downplay the collaborative context of *IFM*'s production. Cantril was plainly intent on securing primary credit for the book; Lazarsfeld acquiesced in the service of the Radio Project's renewal and relocation. Both men ensured that their paths would never again cross. Though Cantril avoided writing about the conflict, it was widely known among his colleagues in the opinion research field that he considered Lazarsfeld his archrival, and he made frequent disparaging comparisons between his new research institute and Lazarsfeld's Bureau (Converse, 1987, p. 150).

For his part, Lazarsfeld was still bitter about Cantril's self-serving behavior years after the book's release. In a 1942 letter to a government official, he wrote that Cantril has "hardly done any original research," adding "I just want to be sure that in the field of research, moral and intellectual standards are not set by him" (quoted in Glander, 2000, p. 84). In a 1943 interview with a Rockefeller official, Lazarsfeld (1943) called Cantril "pathologically ambitious" and dismissed his Welles work as "laughable." As late as

1975, he was still writing of his "justified complaint" against Cantril, that he "forced me to make him co-author of the *Invasion from Mars* while he had practically nothing to do with it" (quoted in Pasanella, 1994, p. 30). Of course, Cantril had not settled for co-author. Stanton (1943), too, savaged Cantril to a Rockefeller official. He claimed that Cantril had refused to revise a "completely unsatisfactory" draft and insisted on sole authorship even after Stanton and Lazarsfeld had rewritten the manuscript.

Another reason that the book's continuity with later Bureau work is overlooked is that Cantril himself insisted on a dramatic prose style and chapter structure and exaggerated the extent of the panic (Socolow, 2008b). *IFM*'s original marketing, moreover, pitched the book as a "mystery-adventure story of the American people" with "leading parts by H.G. Wells, Orson Welles, a million or more radio sets, and some apparently normal men and women." The book, to some extent, mimicked the theatricality that was its ostensible subject.

Lazarsfeld's skill at establishing claims to research originality in narrative terms was never so successful as with the first 15 pages of *Personal Influence* (Pooley, 2006b). By recounting a taut "powerful-to-limited-effects" story—but without specifying any "powerful-effects" works—Lazarsfeld (and Elihu Katz) unwittingly created the narrative vacuum that *The Invasion from Mars* would, years later, come to fill.

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