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The Women Who Proposed Two-Step Flow: A Gendered Revisit to the Intellectual History of a Mass Communication Theory

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Women were a key component of the research teams that worked on the first proposal of the two-step flow theory in the Bureau of Applied Social Research (Columbia University) in the 1940s and 1950s. However, in a perfect stance of historiographical epistemic injustice, they disappeared from the history of the field. Through archival analysis and critical-hermeneutic approaches, we recover the contributions of female researchers to the Erie County and Decatur projects, published as *The People's Choice* and *Personal Influence*, respectively. These are the 2 projects that first proposed the two-step flow theory. In particular, we recover female contributions to both the fieldwork and theoretical debates. Ultimately, we analyze their work from a gender-informed perspective. To conclude, we argue that reinscribing women into the foundational narratives of communication research is a step toward a fairer, more pluralistic, and less individualistic comprehension of the historiography of the field.

Keywords: women, communication research, two-step flow, communication theories, gender, intellectual history, archival research

> One striking fact emerged: people who intended not to vote are more apt to attribute their crystallization to some personal contact . . .
> ([This] looks darned impressive)
> —Helen Schneider (1943, p. 2), on a memorandum to Paul Lazarsfeld

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It was 1943, and researchers at the Bureau of Applied Social Research (BASR)[2] were finishing the compilation of the results of the Erie County study, conducted three years prior—during the 1940 presidential election—to analyze opinion-formation among U.S. citizens. The results were soon to be published in a book, *The People's Choice* (*TPC*; Lazarsfeld, Berelson, & Gaudet, 2021), that would from then on change the field of communication research by proposing the relevance of interpersonal influence in the mass communication process. As he did with the Erie County project, BASR's director, Paul Lazarsfeld, was coordinating the edition of the book, while two of the Bureau's women, Hazel Gaudet and Helen Schneider/Dinerman,[3] were put in charge of interpreting and processing the enormous bulk of data retrieved in Erie County. A few months before the publication of *TPC*, Helen Schneider, by then a 23-year-old graduate student, wrote a memorandum to Lazarsfeld informing him of a finding made while going through the data. As shown by the quotation that opens this article, she was surprised by one thing: "Most of those who give reasons for their change of opinion admit that someone had convinced them (relatives, friends, colleagues);" this "looks darned impressive," she wrote (Schneider, 1943, p. 2).

This realization was a pivotal moment in the history of communication theories. It led to the development of a hypothesis, and, ultimately, to the inclusion of a reference to a "two-step flow of communication" in *TPC* (Lazarsfeld et al., 2021, p. 151), a theory whereby media influence on audiences is not direct, but rather exerted through "opinion leaders," producing a two-step process (media to opinion leaders, opinion leaders to people). The discovery was deemed so relevant that, in 1944, the Bureau received more funding to conduct a second study, now solely focused on understanding this two-step process and the nature of interpersonal influence. In the Decatur project—as the second study was known—more than 700 women from Illinois were interviewed in an attempt for understanding local patterns of interpersonal influence. The project's results would eventually turn into *Personal Influence* (*PI*; Katz & Lazarsfeld, 2006), the book where the "two-step flow" theory would be officially formulated for the first time, "a major milestone in the development of communication research" (Lang & Lang, 2006, p. 158). Through this groundbreaking perspective, *TPC* and *PI* together became one of the biggest milestones in the shift toward the limited effects paradigm, turning Columbia's BASR into the epicenter of such paradigmatical shift.

From 1939 through 1956, the analysis of personal influence was a fundamental part of BASR's agenda. During that same period, women constituted a significant part of Columbia's center. They were everywhere in the office: From students to senior researchers, from secretaries to project supervisors—their presence was outstanding. They were so meaningful to the center that Rowland and Simonson (2014) claim that female researchers at Columbia were the "most consequential cohort of women in the field before the 1970s" (p. 4), with more than 30 women consistently working in media research. Despite women being an essential part of the Bureau during the years in which interpersonal influence was being studied, "the extent to which [they] contributed to the 'two-step flow' theory of social communication is at present unknown"

[2] Named Office for Radio Research (ORR) from 1937 to 1941, it was renamed the Bureau of Applied Social Research (BASR) in 1941. For consistency, we will use BASR throughout this work, although this chronological distinction must be made.

[3] We will use Schneider's maiden name, which she used at the Bureau. She adopted her married name, Dinerman, for most of her career.

(Robinson, 2011, p. 31). More so, the main historiographies of two-step flow do not mention any female names linked to the theory (Summers, 2006). Our goal here is to reclaim their names and contributions for a more diverse intellectual history of the field of communication research. In particular, we recover the work of Helen Schneider (New York, 1920–1974); Hazel Gaudet (Washington, 1908–1975); Jeanette Green (New Jersey, unknown); Thelma Ehrlich (New York, 1921–2012); and Leila Sussmann (New York, 1920–2016). Their unpublished contributions help us reconsider the origins of the two-step flow theory, the gendered practices behind the projects, and early gender-focused interpretations.

Two-Step Flow as Recounted by the Field's Historiography

In 1944, Lazarsfeld et al (2021) famously stated that "ideas, often, seem to flow *from* radio and print *to* opinion leaders and *from them* to the less active sections of the population," and so, one of the most important theories in the history of communication and media research was born (p. 151). Two-step flow remains one of the most used and cited theories in media and audiences research (Valkenburg, Peter & Walther, 2016, p. 319), with *TPC* and *PI* attracting over 18,000 citations more than 70 years after publication, according to a Google Scholar search in March 2024. The theory is also experiencing a revival with the growing interest in social media influencers' effects on public opinion.

Two-step flow soon "aroused considerable interest" among those researching audiences and the media (Katz, 1957, p. 61), and this interest was everlasting. It became a "milestone" in the field of communication, so much so that "it is difficult to find a work in communication studies that has been more rigorously fixed through secondary scholarship as a definitive component of the makeup of the field" (Park, 2008, p. 253). Two-step flow received substantial attention from the field's historiographers since the late 1950s (e.g., Douglas, 2006; Eulau, 1980; Katz, 1957; Lang & Lang, 2006; Park, 2008; Pooley, 2006; Simonson, 2006, 2024; Summers, 2006), including a special number of *ANNALS of the American Academy of Political and Social Science* devoted to specifically discussing its history. Two-step flow is, thus, one of the most *narrated* theories in the field.

Despite some exceptions mentioned above, most traditional historiographic approaches have adopted the coherent and easily communicable story about two-step flow in which the two foundational myths of the history of communication research converge. The first of these was the myth of the limited-effects turn, a myth that caricaturized prewar communication research as naïve, powerful-effects perspectives, holding that researchers at Columbia articulated the "powerful-to-limited-effects-storyline," and turning them, by default, into the "citational authority" (Pooley, 2006, p. 131). The second was the founding fathers' myth (Pooley, 2008; Rakow, 2008), an individualistic myth—first mentioned by Berelson (1959) and, later, by Schramm (1963)—according to which the origins of communications research were directly linked to the names of four male scholars. The founding fathers' myth helped to consolidate a traditional historiographical trend that communication research was founded by a small group of great individuals—often men—rather than by collaborative groups of knowers, reflecting "lone scholar" notions of knowledge production (Hood, 2019). Simultaneously, this myth erased women from the field's imaginary, cementing the idea that they were absent from the beginning, and have inhabited the field as outsiders ever since.

Ultimately, both myths combined turned into a "heroic narrative constructed retrospectively" (Livingstone, 2006, p. 233) in which the hyperbolic treatment of certain careers led to the invisibilization of many others. In a perfect stance of the Matthew/Matilda effect (Merton, 1968; Rossiter, 1993), the most renowned scholars would, from them on, receive even more recognition, perpetuating the association of the theory with specific names in our collective memory. This is the case as shown in one of the main encyclopedias of communication research, where two-step flow is simplified as being "initially formulated by Paul Lazarsfeld" and later "refined in a 1955 publication by Katz and Lazarsfeld" (Davis, 2009, p. 969). Communication research teaching also relies on these oversimplified storylines, suggesting that the field was built by a few prominent male scholars (Lazcano-Peña, 2014).

Now, in the light of a field that is recently revisiting its own history in a manner that reckons with decades of exclusion of women (Ashcraft & Simonson, 2015; Dorsten, 2012, 2016; García-Jiménez, 2021; Herrero & García-Jiménez, 2023; Hristova, 2022; Hristova, Dorsten, & Stabile, 2024; McCormack, 2009; Rowland & Simonson, 2014; Simonson, 2012), we ask a different question: What was female researchers' role in the proposal of two-step flow?

Women in Early Communication Research

As Rowland and Simonson (2014) claimed, "it's a little-known fact, but women made huge contributions to the field as it established itself in the 1930s and 1940s" (p. 4). Women were a constituent part of the foundational years of communication research, so much so that, despite the erasure these female pioneers have suffered in the field's historiography, their names and contributions are essential for understanding the earlier years of the field. Women were ever present in early communication research (Dorsten, 2012), from the critical-psychoanalytic perspectives of Frankfurt, with Else Frenkel-Brunswik as a main protagonist (Fleck, 2023; García-Jiménez, 2021), to the early interpretive-sociocultural-phenomenological-empirical-inductive influences (García-Jiménez, 2021), like the Chicago School, where pioneering female voices—like that of Jane Addams—were a foundational force (Fredriksen-Goldsen, Lindhorst, Kemp, & Walters, 2009). But, if one place was an actual haven for women, it was Columbia's sociopsychological-functionalist BASR.

As we mentioned before, the Bureau congregated a large group of female researchers who, for over two decades, conducted some of the earliest analyses of media effects and audiences. Often, they were among the graduate students and research assistants temporarily employed by the center, but in some cases, they held permanent, more stable positions as coordinators and research supervisors. Although roughly around 14% of researchers and professors in the U.S. social sciences of the 1950s were female (Rossiter, 1995, pp. 129–172), at least 34 women had a relevant role in the Bureau (Rowland & Simonson, 2014)—contributing to an estimated 40% of its projects between 1935 and 1955 (Herrero, 2024). Among them, Herta Herzog has received the most historiographical attention (García-Jiménez, Huertas, & Vera-Balanza, 2023; Klaus & Seethaler, 2016), being claimed as the first person to propose the "uses and gratifications" theory. Several other critical studies have examined the relevance of the "gendered assemblage" at the Bureau (Rowland & Simonson, 2014), or the essential role many women had in research (Hristova, 2022).

Although these women were central to one of the foundational schools of communication research, the simplistic and heroic (Livingstone, 2006) historiography of the field has dismissed, forgotten, and even undermined their intellectual contributions. More so, Rakow (2008) argues that the field's account for "the ideas, people, research, and concerns of those who constitute [it]" (p. 113) has perpetuated a narrow and masculinized historiographical discourse—that of the founding fathers—that, by action or omission, fails to address how gender has always been a constitutive force of communication studies (García-Jiménez, 2021). All in all, the history of the field of communication—a field otherwise feminine from its very beginning—has been built around a major Matilda effect.

Epistemic Injustice and Female Researchers in Communication

We use Miranda Fricker's (2007) concept of epistemic injustice to interpret this historiographic gender imbalance not just as a result of social meanings in science but also as a form of specifically epistemic oppression. In particular, for Fricker (2007), some subjects are systematically wronged in their capacity as knowers because of prejudices related to their social identity, thus creating an exclusionary epistemic environment. Fricker (2007) finds that epistemic injustice is conformed both by testimonial injustice and hermeneutical injustice, two concepts that are key for explaining how women—and many other subaltern subjects—have been erased not only in communication research but also in many other scientific fields.

According to Fricker (2007), testimonial injustice is the harm inflicted toward specific knowers when they are unheard, untrusted, or dismissed as epistemic authorities by their peers, something especially detrimental in a "credibility economy," like academia (pp. 30–35). As a result, when we only listen to certain individuals, we lose a much-needed plurality in our attempts to understand the (social) world. Ultimately, the knowledge produced is unidimensional, narrow, and biased, and the realities that only affect those silenced subjects remain unexplored. Fricker (2007) refers to this as "hermeneutical injustice." Although epistemic injustice primarily affects individuals who do not conform to the White, male, western, cis-heterosexual, upper-middle-class scientific ideal, for this work we will focus specifically on how it affected female researchers, particularly during the first half of the 20th century. Further explorations of the epistemic community of BASR, particularly those informed by decolonial or LGBT+ perspectives, could address the multiple layers of exclusion in the history of communication research.

When women's voices and contributions are erased from a field's historiography, often both forms of epistemic injustice (testimonial and hermeneutical) are committed at once. In fact, when losing historical female referents, we are also most likely to lose specific approaches to research or to assign those to their male colleagues. When Rossiter (1993) proposed the Matilda effect, she argued that women are not simply erased from the history of science but also perceived as exceptions: "She wrote it but she had help, she wrote it but she's an anomaly" (Russ, as cited in Rossiter, 1993, p. 334), constructing an androcentric notion of academia. We link Fricker's (2007) concept of testimonial injustice with Rossiter's (1993) historiographical look on women's erasure from science through the so-called Matilda effect. We argue that when women are deliberately unheard and marginalized by the main historiographical narratives in a discipline, a form of historical epistemic injustice is perpetuated.

Erasures in the history of science, furthermore, contribute to the lone scholar myth (Hood, 2019), an individualistic conception of science that denies the social nature of knowledge-construction, often by dismissing those who carry out the unseen work behind the big publications. This was particularly the case in early communication research, and specifically in the BASR, where work was gendered and, while women were in charge of—unpublished and restricted—commercial studies, men authored the academic publications that ended up receiving historiographic attention (Hristova, 2022). This gendering of research often occurred within the same project, with women performing the unpublished "secondary work," while men authored the books. This was the case with the two-step flow studies.

Methodology

To recover women's contributions to the first proposal of two-step flow, we combined a methodology of archival analysis and a critical-hermeneutical reading of texts. In particular, we analyzed a total of 68 documents (671 pages) archived in the Rare Book and Manuscript Library at Columbia University (New York), all of them belonging to either the Erie County (B-0080 & B-0105) or the Decatur (B-0218 & B-0240) studies. Additionally, the Fellowship Records were consulted at the Rockefeller Foundation Archives. The corpus of documents consisted of gray literature (338 pages), published results (104 pages), internal administration documents (15 pages), and correspondence (214 pages). The archival documents were put in dialogue with the two major publications of each project: *TPC* (Lazarsfeld et al., 2021) and *PI* (Katz & Lazarsfeld, 2006).

Results: Women's Contribution to Two-Step Flow

One of the few times in which women are mentioned in some of the previous two-step flow historiographies is an episode where Merton and Lazarsfeld were trying to persuade Mills to join the Bureau at a Manhattan restaurant. According to Summers (2006), Lazarsfeld invited "several handsome young women" who "spoke in sugary voices" in an effort to "seduce Mills into joining the team" (p. 29). They "seduced" him and he joined. The rest of the story is mostly told through men's names and feats. The restaurant episode reflects the very gendered, very ambiguous, and very complex interpersonal dynamics at play at the Bureau (Ashcraft & Simonson, 2015). Although illustrative, when decontextualized, the anecdote helps reproduce the male reason vs. female emotion (Lloyd, 2002) long-standing epistemic trope—or, more specifically, the "male rationality vs. female erotic" one (Lorde, 2007, pp. 53–56). These stereotypes perpetuate traditional gender roles in academia, making it more difficult for women to succeed, even as the field develops (Herrero & García-Jiménez, 2023). Ultimately, this consolidates the insider/outsider historiographical dynamics that have excluded or underestimated women's voices in communication research, contributing to the materialization of broader conceptions of women as otherness in academia (Acker, 1983).

By reinscribing women into the epistemic history of the field, we can further contextualize anecdotes like these. Otherwise, there is a risk of perpetuating the biased and false impression that women in the Bureau were merely passive "seductresses," whereas men—only a selected handful of them—did the actual work. To counteract this widespread narrative, our goal is to recover the epistemic contributions of women to the proposal of two-step flow. To accomplish this, we examine the presence of women in both

projects where the two-step flow was first introduced—Erie County and Decatur. We then analyze their contributions and lived experiences, identifying three levels of female participation in the projects: (1) fieldwork and codification, (2) theoretical approaches and data interpretation, and (3) gender-informed approaches. Finally, we propose the overall relevance of female researchers in the early study of interpersonal influence.

"Getting the Circle": Women Working in the Field and Doing Codification

The first level we identify is that of fieldwork and data analysis, encompassing all tasks related to data retrieval and processing, from the very first in-the-field testing to the data coding done back in the Bureau office. Referring to this type of work, Bureau researcher Joan Doris Goldhamer remembered "us women working, coding, making tables, and taking percentages" (McCormack, 2009, 00:00:20), jobs where their presence was more tolerated because of the impact of gender stereotypes on epistemic communities (Herrero & García-Jiménez, 2023).

Although indispensable, this type of work is a form of academic invisible labor (Acker, 1983); the kind of labor that does not withstand the test of time in historiographical terms. The general lack of attention paid to cooperative research teams ultimately reinforces the lone scholar myth (Hood, 2019), which falsely suggests that single author(s) alone are responsible for all the work behind major contributions. As we will see, in the two-step flow projects, those conducting secondary work were also engaged in the initial interpretations of interpersonal influence. That women have been historically deemed more suited for these background roles, as well as the general lack of attention paid to them, perpetuates a double exclusion of female scholars in the history of the disciplines. Thus, the individuals who did the "dirty work" in both the Erie County and the Decatur studies are barely acknowledged in the books. Most individuals were women, whose roles ranged from interviewers to field supervisors.

Supervision in the Field: Women as Chief Interviewers

In 1940, a team of 16 interviewers based in Sandusky (Ohio) was actively conducting interviews with people from Erie County, analyzing how they formed their voting opinions during an election year. Unlike the Decatur study, which would only include women as interviewees to determine how interpersonal influences shaped their consumption habits (Douglas, 2006), the sample of respondents in Erie County was generally diverse in terms of gender. The interviewers, however, were predominantly women, with 13 of 16 (81% of them) being female (BASR, ca. 1940). Even though the teams of interviewers were often recruited on-site or subcontracted through other research organizations,[4] the Bureau's researchers in the field directly supervised, coordinated, and trained the staff. Hazel Gaudet was the Bureau's designated supervisor in Erie County, where she was joined by Elmo Wilson as a representative of Elmo Roper & Associates. At the time, Gaudet was a research assistant at the Bureau, and recent collaborator of *The Invasion from Mars* (Cantril, Gaudet, & Herzog, 1940)—one of the first major contributions to communication research from Lazarsfeld's centers. Although she is recognized only as an assistant on the book's cover, she had been "among the most active staff members at the project" (Pooley & Socolow, 2013, p. 39). Gaudet had worked with

[4] Elmo Roper & Associates was a polling organization subcontracted by BASR for the Erie County interviews.

Lazarsfeld since 1936, when he was at the University of Newark. She had recently published two methodological papers, focusing on the use of interviews for studying media influence. This consolidated her recognition as an interviewer and put her in a privileged position for supervising the Erie County study.

Although the methodology was being designed in 1939, Gaudet coordinated a trial set of 1,000 interviews in Erie County and used the results to systematize the identification of opinion changes. She wrote "Model for Assessing Changes in Voting Intention," a memorandum designed for internal circulation that would be key during the fieldwork and, 15 years later, published as an example of early communication research methodologies in a book edited by Lazarsfeld (Gaudet, 1955).

Gaudet not only helped design the panel methodology but also trained and supervised the fieldwork team. She simultaneously reported any issues back to the Bureau, becoming the formal liaison between the interviewers in Erie County and the researchers in New York. Continuous correspondence between Lazarsfeld and Gaudet shows how direct the communication between the center's director and the chief interviewer was, as well as how Gaudet had an agency for suggesting potential reformulations: "I might add I feel very strongly that if the question had originally stressed the phrase since the last interview, they would have had less trouble with meaningless responses . . . I feel I should talk to you about this" (Gaudet, 1940a).

During the fieldwork, Gaudet applied for a Rockefeller Foundation fellowship, sponsored by Lazarsfeld and Stanton (Gaudet, 1940b). She proposed a subsidiary study in Erie County to evaluate media uses and audiences using the interviews that were already being conducted. In her proposal, she stated:

> The principal opportunity for pursuing this study lies in conjunction with the survey now being made in Erie County, Ohio, by the Office of Radio Research . . . by utilizing much information already collected, it would thus be possible to collect much valuable research data . . . with a minimum of effort and expense. (Gaudet, 1940b, p. 1)

Her proposal was approved, and Gaudet received personal funding to coordinate a study herself. Her project, internally known as the "Shortwave Radio Project" (Barton, 1989), used the data from Erie County to examine differences in media use based on educational and socioeconomic status, and showed how different media influenced different sectors of the population. In particular, Gaudet (1940b) was interested in how "radio is a potential force for education in the lower classes" (p. 1). Her findings eventually became part of the BASR's studies on media (Barton, 1989), and she soon received support from Lazarsfeld to conduct follow-up interviews to corroborate and expand the data. In a letter to Professor Harwood Childs, Lazarsfeld stresses the relevance of Gaudet's findings: "The following paragraphs describe the kind of work done . . . by the Office of Radio Research" (Lazarsfeld, 1941, April 22, p. 1). Gaudet contributed to articulating the methodology of the study, and her work was essential for defining the different changes of opinion among voters.

Women continued supervising the fieldwork when the Decatur study began in 1944. After his restaurant encounter with Lazarsfeld and Merton, Mills was offered the role of field supervisor for the Decatur project. Despite the commonly held belief that he was solely in charge (Summers, 2006), he did not manage the team alone. Instead, he worked alongside two women, Jeanette Green and Thelma Ehrlich, and the

three formed a trio of supervisors who settled down in Decatur for the study's duration. As Ehrlich (2007) recalled, "it was great fun . . . I think we all liked each other" (p. 9).

Jeanette Green was among the most prolific researchers at the Bureau: despite barely publishing any academic work, she was the default chief interviewer—especially once Gaudet had left Columbia—(BASR, 1944), as well as responsible for a vast amount of the center's commercial, unpublished work (Barton, 1989). Younger than her, Thelma Ehrlich was a graduate student studying the impact of film supervised by Lazarsfeld (Department of Sociology, 1945, p. 1). When assigned the field supervision of the Decatur project, Ehrlich joined Green in the training and supervising of the staff. Later in her life, Ehrlich (2007) remembered checking the interviewer's work on-site "every day" (p. 8).

Mills' disagreement with Lazarsfeld over the scope of the Decatur study delayed data interpretation for at least two years, until Mills was eventually fired and the coordinating team reorganized (Lang & Lang, 2006). As a result of the reorganization, Ehrlich and Green were reassigned a leading role in the administration of the project. In 1947, once the project resumed, Jeanette Green processed the data from the interviews. Meanwhile, Ehrlich reorganized the instructions written two years prior and put together a document entitled "Additional Instructions for Interviewers" (Ehrlich, 1948). Because of its meta-reflection, the document is an excellent piece for understanding how the fieldwork was carried out. In particular, Ehrlich recalls that interviewers were instructed to "Get the circle!" (Ehrlich, 1948, p. 2); that is, to engage enough with the interviewees to be able to draw their networks of influence.

Identifying Opinion Leaders: "Not Aggressive, Not Convincing"

The work done in the field was also key for further theoretical proposals. In fact, interviewers not only interviewed people but also evaluated them psychologically. They were trusted to identify either (a) each person's potential opinion leader traits, or (b) their network of personal influences. Most of the proposed descriptions of influence in *PI* were based on the careful work of interviewing teams, who had worked, ever since Erie County, on perfecting the detection of influence patterns among the interviewees.

Something particularly fascinating happened in Erie County: after completing the fieldwork, the team of interviewers was asked to prepare profiles of those interviewees who fitted into the opinion leader depiction. They wrote a total of 132 profiles, each of them including a short description of an individual, a summary of their psychological traits, and subjective impressions of the interviewers. These profiles were not intended as formal categorizations of influencers, but rather as informal notes for internal communication. Interviewers often judged individuals: "I imagine she was ashamed of the way she was living. She apologized for things . . . I think she is a sincere sort of person, there is something wrong though with a person who lived the way she lived"; "I don't know why he would be in there as an opinion leader—it's beyond me" (BASR, ca. 1940, pp. 2540, 2969). They also shared specific episodes that they felt were relevant for understanding each person: "He once yanked the plumbing out of his house and threw it out the window" (BASR, ca. 1940, p. 0202). After a brief description, each interviewee was classified as "aggressive/not aggressive" and "convincing/not convincing." Based on this information, they were considered opinion leaders or not: "NOT AGGRESSIVE but MIGHT BE SLIGHTLY INFLUENTIAL WITH A

LIMITED CIRCLE . . . Maybe he is an opinion leader, he seemed quite dependable to me" (BASR, ca. 1940, pp. 0202, 2421). Eventually, among the 132 profiles, only 53 individuals were identified as opinion leaders.

These opinion leaders' profiles are useful for understanding how ambivalent female experiences were in early communication research. As a result of compulsory femininity, women have often been stereotyped as possessing traits such as carefulness, empathy, and social proficiency, which led to the assumption that they were better suited for conducting "productive interviews" (Rowland & Simonson, 2014, p. 19) where interviewees shared the most personal information possible. Women interviewers did, indeed, build personal relationships with a good number of interviewees, especially other women. As some profiles show, their role as women helped them in the interviews: "She was interested in talking to me because I had children . . . she calls me 'honey' and 'dear'"; "He treated me as a daughter of the family and insisted on taking me back to town so I wouldn't have to walk" (BASR, ca. 1940, pp. 0741, 1005, 1657). Many interviewees felt close to "the girls," as evidenced by a comment included in one profile: "She asked me about some of the other girls who interviewed her and what they were doing" (BASR, ca. 1940, p. 0607). Nevertheless, exclusionary dynamics were not absent from the fieldwork: In other cases, female interviewers faced gender-related rejection. One interviewer wrote about a man: "He refused me so nastily"; other profile includes the following note: "He would not be interviewed by women, only men" (BASR, ca. 1940, pp. 0723, 2791). It was in those cases that the few male interviewers took the lead.

Years later, in Decatur, female chief interviewers continued writing descriptions of opinion leaders. However, their understanding of interpersonal influence was more advanced, and their profiles focused on the specific techniques used by influencers. Jeanette Green wrote several memoranda using psychological concepts to study how influence is exerted: "[There is] a 'taking-for-granted' attitude on the part of the influencer that the person to whom he is talking agrees with his point of view"; Green (1948) stated that influencers adopted "the role of an expert" (p. 2). These were among the first detailed descriptions of the mechanisms of influence: "[Influencers] set the stage for putting across their own points of view by *flattering* the recipients of their opinions into a more receptive mood" (Green, 1948, pp. 1–3). Whereas in Erie County, influencers were identified because of their aggressiveness and convincing traits, Green's complex analyses show the first hints of *PI*'s deeper understanding of influence patterns.

The Interpretive Contributions of Female Researchers to Two-Step Flow

Widespread gender meanings in academia reinforce the prejudice that women are better suited to secondary, supportive, and technical jobs, not for the actual production of knowledge—which has been stereotypically assigned to men (Herrero & García-Jiménez, 2023). For this reason, women working in theoretical roles tend to be seen as exceptions and erased from history (Rossiter, 1993).

Figure 1. Women and men working on the Decatur study (BASR, ca. 1947). Courtesy of Columbia University.

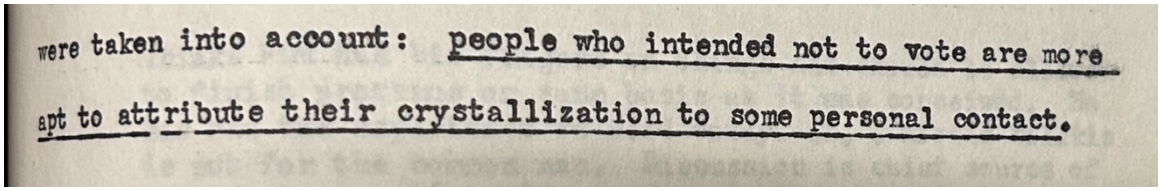
Even if their names rarely appear in major academic publications, women made significant contributions to the Bureau's theoretical work. In the Erie County and Decatur studies, female researchers wrote hundreds of pages of interpretive memoranda, most of which remained unpublished in the form of gray literature. These internal documents, however, provide a good starting point for reconstructing the intellectual history of the two-step flow theory. They are the testimony of the collaborative construction of knowledge in a center whose daily life consisted of "a continuous flow of memoranda" (Lazarsfeld, 1975, p. 68) that provided the basic structure upon which ideas were written for later editorial publication. Women thoroughly participated in the early theoretical discussions about interpersonal influence, conforming gender-mixed teams—as shown by internal pictures of the Decatur study (see Figure 1). We now explore their contributions to the early interpretations of Erie County; to the study of influence in Decatur; and, finally, we read their contributions from a gender-informed perspective.

"Darned Impressive:" Early Interpretations of Interpersonal Influence in Erie County

We began this text with a quote from an internal memorandum by Helen Schneider to Paul Lazarsfeld, written in 1943, that places the 23-year-old graduate student at the forefront of the Bureau's theoretical debates about interpersonal influence. Schneider's exchange with Lazarsfeld not only points us toward an essential contribution of female researchers to the proposal of two-step flow but it also provides a first-ever scope into the backstage, collaborative proposal of one of the most important theories in the history of the field.

In 1943, Hazel Gaudet and Helen Schneider wrote more than 120 pages of memoranda that would eventually be used for the publication of TPC. Although Gaudet had a consolidated background as a Bureau researcher and had taken the lead in the project, Helen Schneider was a very young graduate student who was invited to work as an assistant in the interpretation of the Erie County materials (see Figure 2). Schneider authored only four memoranda (40 pages), but her work is among the most significant in the project's gray literature. She specifically explored how people changed their minds because of interpersonal influence, finding that "a 'conversation with' or 'discussion with' friends or relatives" was one of the main reasons given for opinion changes (Schneider, 1943). "This . . . looks darned impressive," she wrote, "the one clear finding is that most of those who give any reason for their decision admit that someone persuaded them to vote"[5] (Schneider, 1943, p. 2; emphasis in original).

Schneider was recognized in the book's acknowledgments for being "in charge of the statistical analysis" and guiding the team "indefatigably" (Lazarsfeld et al., 2021, p. xli), but a review of the project's gray literature reveals her role extended beyond that of a statistical analyst. She also significantly contributed to the first theoretical debates on interpersonal influence, and she continued to specialize in this topic in the Decatur study, being one of the very few individuals to work consistently on both projects. Ultimately, she became a key figure without whom a complete understanding of the intellectual history of two-step flow would not be possible.


were taken into account: people who intended not to vote are more

apt to attribute their crystallization to some personal contact.

Figure 2. Helen Schneider's memorandum to Paul Lazarsfeld (Schneider, 1943). Courtesy of Columbia University.

Meanwhile, Hazel Gaudet interpreted most of the data from Erie County, writing 13 memoranda (104 pages) where the reasons for changes in voting intention were exhaustively studied. She also coordinated the data interpretation, presenting her results to Lazarsfeld. Gaudet's memoranda proposed potential analysis categories: "I think some more exciting way should be devised to bring out the contrasts between men and women . . . you might get some subtle nuances of differences" (Gaudet, 1943, p. 5), and Lazarsfeld annotated the margin, complementing her findings. Although none of Lazarsfeld's responses are found, the communication between them was fluent, as shown in a note she wrote: "You asked me the other evening what reasons people gave for becoming undecided" (Gaudet, 1943, p. 5). Eventually, Hazel Gaudet's interpretive work would inform complete chapters of TPC.

Movie-Going, Marketing, Fashion: Female Researchers in Decatur

Years later, when the Decatur data was being interpreted, four women (Helen Schneider, Thelma Ehrlich, Jeanette Green, and Leila Sussmann) contributed to the analysis of interpersonal influence in the

[5] Underlined in the original.

context of movie-going, marketing, and fashion choices. The analyses, which would comprise the second half of *PI*, were written between 1947 and 1948 to decipher the ways in which interpersonal influence conditioned women in each of those realms.

After being an integral part of the Erie County interpretation, Helen Schneider had a supervisory role in the Decatur study. By this time, however, she was beginning to make a name for herself in the field of public opinion and left the Bureau when she was hired by the American Jewish Committee. Nevertheless, at Lazarsfeld's request, Schneider continued working on the project (Lazarsfeld, 1947, January 23). She authored several memoranda exploring the link between interpersonal and media influence—work that was essential to the formulation of two-step flow: "Is the personal influence the 'initiating force' to a change? . . . perhaps [it] operates as a 'catalytic agent' . . . [that] hastens the change" (Schneider, 1947, September 29, p. 3).

Schneider's work was broader and theoretical, while the rest of the BASR women focused on specific sections of the Decatur study: movie-going, marketing, and fashion. Together, they wrote over 55 pages of memoranda exploring specific interpersonal dynamics, and they proposed several conceptual definitions that informed the second half of PI. Overall, these women led teams and worked alongside many male researchers, actively contributing to what ultimately was a collaborative theoretical proposal.

"The Patriarchal Concept of the Husband": Gender Meanings and Influence Networks

Although the Decatur project primarily examines women's influence networks, Douglas (2006) notes that there is no reference to gender dynamics in *PI*. According to her, the study constantly refers to women as "people," which ultimately ignores the impact of gender on influence networks. *PI* addresses this issue in just one paragraph: "For married women, the important male influentials are their husbands, while for single women, fathers play the important role in opinion changes" (Katz & Lazarsfeld, 2006, p. 498). No further explanation is given in the book, which seems more interested in identifying family influence networks than in exploring gender.

Thus, *PI* missed an opportunity to explain the role of women and their relationship with media in the mid-20th century. But this is surprisingly not the case when we look at the project's gray literature. Previous Bureau projects exploring women as audiences—like Herta Herzog's (1941) "On borrowed experience"—had mostly considered women as inhabitants of the private sphere. Herzog's (1941) "mediated experience" explained that women used the media as a replacement for actual contact with the world, "accepting [stories] as a substitute for reality" (p. 145). This changed radically when the Decatur project positioned women as integral to interpersonal and mediatic networks, and gender-informed analyses permeated discussions among Bureau researchers attempting to explain influence among American women.

In 1947, Schneider and Green wrote two memoranda exploring interpersonal influence about politics and fashion. At some point, they questioned why women tend to mention only men as their main influence for politics:

> Many of the Decatur women adhere to the patriarchal concept of the husband as the head of the house, dictator of thoughts . . . women with acceptance of woman's traditional place in the home explain their political dependence by the fact that their husbands have a better opportunity for contacts. (Schneider & Green, 1947, p. 8)

The 11-page memorandum introduces ideas such as gender inequality and hegemony as key factors in interpersonal influence. The authors conclude that men need not fit into the opinion leadership profile to be influential: rather, they are essentially influential over women because of "patriarchal" values and women's adherence to their traditional place in the home. Although men hold strong political influence, women have authority in fashion and marketing, considered "women's realms" (Schneider & Green, 1947, p. 9). Something similar happened when Ehrlich studied women's relationships with cinema. On a sidenote, she wrote about movies as a social activity and about housewives being "trapped" away from the world:

> Movie-going appears to be an activity for those who are "up in the world"—women who are young . . . and older women who are "richer" and better educated. (One has the feeling from these findings that movie-going is an out-going activity—a sign for hope or aliveness or satisfaction. A practice which is . . . unbearable to the woman who is resigned to, or trapped by, a less fortunate living). (Ehrlich, 1950, p. 11)

She explored the social situation of women through their movie-going habits: "Why do we find more movie-going among employed women? Being employed implies . . . she is more exposed to the world outside . . . she has fewer household responsibilities, or she is unmarried"; "Two interpretive themes offer themselves—one dealing with the responsibilities and freedom of women . . . and the other with the qualitative differences between women who work and those who don't" (Ehrlich, 1950, pp. 14–15). These interpretations, although common among the gray literature, never made it—or only made it slightly—into the major academic publications. A look at the intrahistory behind two-step flow, however, can uncover a whole other dimension of the interpersonal influence studies.

Not an Exception, Not an Anomaly: Gendered Patterns of Recognition

Probably, the most surprising finding in reviewing the Erie County and Decatur records is how active female researchers were in both projects. Women's contributions to two-step flow were neither "exceptions" nor "anomalies" (Rossiter, 1993) but rather a foundational component of the proposal of the theory, as we see in the connection between the memoranda mentioned above and the content of some empirical chapters of *TPC* and *PI*. Previous work has shown how traditional authorship conventions at the Bureau favored scholars who brought the studies "together into publishing form" rather than those who conducted the empirical work and made initial theoretical interpretations (Simonson, 2024, p. 2008). These conventions produced gendered patterns of recognition that often diminished the empirical and interpretive work done by women within the projects. Thus, we argue that gendered dynamics were at play at the Bureau not only in terms of the division of work (Hristova, 2022) but also in terms of the economy of recognition.

Earlier drafts of *PI*, however, did include some of these women as coauthors. In 1947, Peter Rossi wrote an outline for an "interpersonal influence book" that was never published and included chapters by

Lazarsfeld, Rossi, Schneider, and Sussmann: "Dear Helen: An outline of the [Decatur] book we are writing . . . Third section: 'Speculations on Personal Influence' (Schneider). Fourth: 'The model of change' (Sussmann)" (Rossi, 1947). Internal correspondence, such as a letter from Lazarsfeld to McFadden—the company that funded the Decatur project—reveals the complex roles of these women, who were often recognized internally, but whose recognition rarely translated into academic authorship. When Schneider left the Bureau, Lazarsfeld wrote to McFadden:

> I felt miserable . . . After you left for the coast, Helen Schneider got such an attractive offer from another research organization . . . I couldn't honestly advise her to refuse it . . . The only good feature is that the people Helen is working for now are good friends of mine . . . we can borrow her in an emergency. Fortunately, the movie article is being finished under her supervision. (Lazarsfeld, 1947, January 23)

Conclusions

The role of the women who worked at the Bureau between the 1940s and 1950s was nothing but dynamic. From fieldwork to data interpretation, to project-supervision, women's contributions constantly and consistently permeated Columbia's early communication studies. They were particularly active in the projects that explored and proposed two-step flow, despite that traditional historiographical narratives have forgotten and dismissed the "invisible" but indispensable work done by women.

Responding to the demand of complexifying the stories we tell about communication research, we recover women's work and (re)claim gender as a constitutive force in communication studies: a field that cannot be understood without addressing the gendering dynamics shaping it from its very origins. In particular, we argue that our historiographical conversation about two-step flow remains incomplete without addressing the invisible work behind the theory, especially that done by women. Future research must also address the foundational years of our field from an intersectional approach, uncovering the complex epistemic patterns of exclusion/inclusion that have historically shaped it.

Overall, these approaches to our shared history are calls for more complex, collaborative narrations of communication studies; narrations that avoid individualistic and masculinized myths, and, rather, address the communicative and cooperative nature of the socioepistemic networks we inhabit. The women we reclaim here were, consciously or not, part of those very collaborative research teams hidden behind the "great men, great ideas" historiographies. By incorporating them into our shared memory, we reclaim a more communicative conception of knowledge. Ultimately, pluralistic narrations are a way of rejecting the "lone genius myth," substituting it for a less exclusionary conception of our field, one that approaches it as a network of interpersonal relationships, both epistemic and social.

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