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True Stories

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Abstract

If *Personal Influence* (Katz and Lazarsfeld 1955) has survived, it is thanks to Paul Lazarsfeld. Having stumbled on the idea of the “two-step flow of communication” in his study (Lazarsfeld, Berelson, and Gaudet 1944/1948, chap. xvi) of how voters made up their minds in the 1940 presidential election, it was typical of him, as empiricist and entrepreneur, to take the next step toward confirming the hypothesis that messages from the media are intercepted by “opinion leaders” who filter them, selectively, to their peers. Fieldwork for what was called the Decatur Study was begun toward the end of World War II, and its aim was to trace the flow of influence in the making of everyday decisions. I had no share in the design or fieldwork for the study, nor in most of the subsequent analysis. At the time, I was just beginning in Columbia College and, a year later, in the U.S. Army.

Disciplines

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Comments

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True Stories

By

ELIHU KATZ

If *Personal Influence* (Katz and Lazarsfeld 1955) has survived, it is thanks to Paul Lazarsfeld. Having stumbled on the idea of the “two-step flow of communication” in his study (Lazarsfeld, Berelson, and Gaudet 1944/1948, chap. xvi) of how voters made up their minds in the 1940 presidential election, it was typical of him, as empiricist and entrepreneur, to take the next step toward confirming the hypothesis that messages from the media are intercepted by “opinion leaders” who filter them, selectively, to their peers. Fieldwork for what was called the Decatur Study was begun toward the end of World War II, and its aim was to trace the flow of influence in the making of everyday decisions. I had no share in the design or fieldwork for the study, nor in most of the subsequent analysis. At the time, I was just beginning in Columbia College and, a year later, in the U.S. Army.

This was also the moment when the Bureau of Applied Social Research was being established at Columbia University, and Paul Lazarsfeld’s lifelong partnership with Robert Merton was being launched. The study enlisted Bernard Berelson as advance man, C. Wright Mills as field director, and a large posse of interviewers. Like *The People’s Choice* (Lazarsfeld, Berelson, and Gaudet 1944/1948), its predecessor, the Decatur Study was designed to zoom in on decisions in the making, employing (1) a panel method of repeated interviews with the same sample of respondents, (2) a method for weighting the relative impact of the several media in decision making, and (3) a method for determining whether interpersonal influence played a part in the process. Peter Rossi, David Gleicher, and Leo Srole each made major contributions to the analysis and interpretation of the data. And it took eight more years, until about 1953, for Lazarsfeld to invite me to undertake additional analysis, to put a theoretical wrapping on the findings, and to weave the whole together as a book. Using somewhat different methods, Robert Merton (1949) had long since completed his study of “Cosmopolitan and Local Influentials”—a 1943 study of interpersonal influence in the realm of public affairs in a town in New Jersey.

I am asked to look back to the creation of *Personal Influence* and to contemplate its career—past, present, and possible future. I began to do so in my new Introduction to the fiftieth anniversary edition (Katz 2005) and in the Lazarsfeld Lecture at Columbia University (Katz 1987). But inasmuch as our editor has asked a dozen or so distinguished colleagues to do the same, I find that my memory is no match for theirs. Historians, sociologists, political scientists—newcomers and old-timers—have reread the book. They dig deep into the sometimes shadowy past of the Decatur Study; contextualize it in time and space; and reconsider its standing in the sociologies of mass communications and small groups and its implications for the applied fields of marketing, advertising, and media. Their “true stories”—based not alone on memory, but on research—are sometimes different from mine. They tell me things that I don’t remember—or never knew. They reconstruct a past of which I was sometimes unaware, or hadn’t noticed, or for which I deserve no credit (or blame) at all. Often, though, our stories coincide. I take all the stories to be “true,” even my own.

Rashomon rides again. What I propose, then, is to consider these true stories, side by side with mine.

I will proceed under four headings: The first is called “Authorship, or the Cast of Characters,” especially in view of the complex logistics of fieldwork and analysis, as well as the elapsed time between the conception of the field study in 1944 and publication of the book in 1955. The second set of comments is called “Contexts, or Sociology and History,” in which I will explore the allegation that the study ignores its own place and time—the town of Decatur, Illinois, in 1945—preferring to “universalize.” The third section will deal with issues of “Theory, Ideology, and Their Consequences,” addressing allegations that the Decatur Study undermined the potential of communication research by understating the true power of the media, by lumping voters and consumers together, and by subservience to the media industry. The fourth section, “Some Things that Went Right,” will consider some of the things being said about the positive directions to which *Personal Influence* has pointed.

Authorship, or the Cast of Characters

Let me begin with a parable, a blasphemous one. Allow me to recall the day when God summoned Moses to Mt Sinai. When he arrived, God is supposed to have explained that He had the outline of a Book in mind, written with the help of some favorite angels, but wanted one of the nations to adopt it, to live by its precepts, and for one of its sons to prepare it for publication. God revealed that He had been turned down by several nations, and by several potential collaborators. So Moses said OK, and his tribe said “we accept,” and he proceeded to transcribe the Book at God’s dictation, incorporating parts of the drafts prepared for Him by senior angels. When the Book was completed, and its abstract etched in stone, God was pleased. He invited Moses to add a section of his own and to bind it together with the rest. To his surprise, God then turned to Moses and said, “Moses, You be first author.” This was God’s way of rewarding Moses, but it is also true, in view of the Matthew effect (soon to be proclaimed),¹ that He had little to lose. And perhaps this was also His way of showing the angels who’s boss. Moses did more than his share in marketing the Book and in debating it, mostly with his own people. He even followed the example from which he had benefited, and bestowed first authorship on some of his own disciples. Moses never claimed first authorship; indeed, he spent a lot of energy explaining how it happened; but as time went by, God seemed firmly entrenched in second place. It wasn’t fair, but neither was it easy to fix—each time, over again. In the end, though, a lot of scholarship caught up with Moses, and he agreed that it would only be right if he were to be punished. Therefore, in addition to the honors that had been showered on him, he agreed to being buried in an unmarked grave in an unknown place.²

Paul Lazarsfeld was not God—even to his graduate students—and I am not Moses—but that’s what came to mind when I seated myself to write. As I proceed, I will try to relate my own story to the other stories recorded in these pages, weaving their arguments “in” and “out” as best I can. True stories all. Let’s now return to the question of authorship, which sounds straightforward, but isn’t. In fact, I will now propose four different kinds of authors.

Even those who believe that there are no authors, and that only cultures write books, cannot deny that the Decatur Study and its methodologies are the brainchild of Paul

Lazarsfeld. But on a more abstract level, it can also be argued that the final product may be attributed to a collective new look at postwar culture. One can point to the “rediscovery of the primary group” in the 1950s and the consequent decline of mass society theory. Scannell reads Riesman’s (1950) “other directedness” as harbinger of this change. It is in this sense that Zeitgeist may be dubbed the author of *Personal Influence*, with its latent message of the “communal,” or “conversational,” or “deliberative” that Peters sees diffusing from New York’s “Mercurians” (Slezkine 2004) to the Midwest.

And if we take a step down the ladder of abstraction, it can be said that the Bureau of Applied Social Research is the collective author. Like in a Broadway play, an infrastructure is at work in survey research “without whom” large-scale projects cannot be tackled. Behind the scenes, then, stood the Bureau of Applied Social Research, whose Golden Age of creativity in the 1950s and 1960s produced scholars, books, and papers that have been hailed by some as canonic. All of the contributing authors were junior members of the Bureau at the time, and the Acknowledgments to *Personal Influence* name seven or eight more professionals—not all academics—who made important contributions. As Morrison reminds us, the Bureau is the fruition of Paul Lazarsfeld’s dream of establishing an institute for applied social research that, financed by clients and other funding agencies, would address practical problems of politics, culture, and business and address academic issues as well. Before leaving Vienna, he had made a start in this direction, together with Hans Zeisel, Marie Jahoda, Hertz Herzog, and others. With support from the Rockefeller Foundation and prominent broadcasting barons, he turned attention to the new medium of radio, and leapfrogged his institute from Princeton to Newark to Columbia, which, soon after, formally appointed Lazarsfeld as well as Merton. In spite of the much-remarked differences between the two men, they became fast friends and lifelong partners. When Lazarsfeld completed his classic study of how voters make up their minds, the partners—now directors of the new Bureau—decided to mount a further study that would expand on the same theme in the everyday areas of marketing, fashions, movie going, and local politics. The book was completed in 1955, when my Part One, relating interpersonal and mass communication, situated the Decatur Study in the context of (what today might be called) social networks, very different from the image of mass society and mass manipulation that underlay early thinking on the mass media. In addition to the star-studded cadre of doctoral students who were charged with analyzing the data, it could not have been accomplished without the infrastructure of the Bureau. From the contract with Macfadden Publications, to the design of the sample, to the posse of interviewers who descended on Decatur, through the decade of stick-to-itiveness that kept the study alive, it could not have been accomplished without an organizational structure. And what an organizational structure (Morrison 1976; Barton 2001)!

Still at the institutional level, it is worth recording the contribution of the GI Bill, which accounts for more than a few of our careers, and Jeremiah Kaplan’s Free Press of Glencoe, pioneer publisher of social science. But the organization that got more than it dreamed of is Macfadden Publications. In 1945, Macfadden invested a reputed \$20,000 in the Decatur Study, hoping that it would confirm that “opinion leaders” were to be found among readers of *True Story*, the kind that would advise their working-class peers about what to buy and how to vote. *Time* magazine, *Life*, and other media moguls made similar investments in other Bureau studies, and for much the same reasons. The preliminary findings presumably pleased

Macfadden and its advertisers, even if the full report would take years to produce. In fact, more than ten years passed before the publication of *Personal Influence* in 1955, whose Part Two publicly unveiled the Decatur findings.³ The book evoked wide interest as well as angry response from those who were convinced that it understated the power of the media. Fifty years later, the name Macfadden lives on in *Personal Influence*, even if the company is much less visible. Not bad for \$20,000.

At a more concrete level, even if Lazarsfeld and the Bureau are inseparable, real people did the work. The aforementioned doctoral students, not sufficiently acknowledged in *Personal Influence*, did good and hard work on the sections of the study to which they were assigned. My job was to put the whole together while composing a Part One that would identify those aspects of small groups that might have bearing on mass communication and vice versa, unlikely as that sounded at the time. All I know—perhaps I should know more—is that Lazarsfeld had a falling out with Mills over the conception of the study (whether interpersonal influence moved vertically or horizontally through the class structure?); over commercial sponsorship of the study; over the conduct of the analysis; and even earlier, says Summers, over the administration of the fieldwork. Subsequently, Peter Rossi, Robert Gleicher, and Leo Srole were commissioned to analyze and report on (1) the relative impact of the several media and interpersonal influence in the making of decisions (Rossi, Part Two, Section Two of *Personal Influence*) and (2) the matrices of who influences whom (Gleicher and Srole, Part Two, Section Three). After substantial sections of the report had been prepared, and even more time had elapsed, Lazarsfeld—still apparently unsatisfied as to how to paste the pieces together—turned to me, his newest doctoral student, to complete the work, perform additional analysis—and to propose a qualitative introduction to the study as a whole. Even at this point, all is not clear. Small-group research and group dynamics research had a prominent place in the sociology and social psychology of the 1950s. Lazarsfeld's discovery of the mediating role of interpersonal influence in the flow of mass communications suggested to me that it would be interesting to review the flourishing literature on small groups for its possible bearing on the dynamics of consuming media. I recall that I proposed this project to Lazarsfeld, but Pooley tells a different story. His claim is that Edward Shils (1951) was there first, and that Lazarsfeld found in Shils's studies of the primary group a possible theoretical wrapping for his repeated findings that the power of the media to influence everyday decisions might be contingent on the intervention of interpersonal influence, an idea that I then developed.

[F]rom the very first, [Personal Influence 's]

collaborators themselves divided over

interpretation of the data.

At the bottom of the ladder stand the readers, to whom theorists of reception would attribute authorship. Indeed, from the very first, the collaborators themselves divided over

interpretation of the data. More cogent examples are closer at hand. For example, several contributors to this volume read *Personal Influence* as a study of postwar community, or as a study of the everyday life of Midwestern women—leading us directly into the second theme of this Afterword.

Contexts, or Sociology and History

A critical chorus echoes through the commentaries ingathered here to the effect that *Personal Influence* takes pains to disguise itself for what it isn't and to conceal what it really is. Why do they conceal the fact that the respondents are women, ask Douglas and Glickman? Why do they obscure the survey respondents' femaleness by referring to "people" (in the subtitle of the book) or, worse, why do they speak of "he" (Part One)? Why don't the authors tell us that these women have just returned from wartime jobs to their roles as housewives and consumers? Why don't they tell us more about the lives of these women—and their other roles? Why don't they tell us more about the town in which the study was conducted—especially after taking such pains to establish its (statistical) representativeness? Why don't they tell us about the cultural turn to other-directedness that dates to the postwar period (Scannell)? Why don't they remind us that their conclusions about mass communication are based on radio broadcasting and that television might soon change all that? And if they're so smart, why didn't they predict—as Bennett and Mannheim now do—that opinion leaders would be put out of work by the custom-tailored messages of the new personalized media?

In a word, *Personal Influence* is criticized for its universalizing pretensions, that is, for overgeneralizing its findings as if they were applicable anywhere and anytime. While some of these "complaints" are regrets over research opportunities forgone, they are not postmodern calls for "local" narrative. These are not simple arguments that small is beautiful or that sociology has forgotten history, or pious calls for taking account of the specificities of time and place and technologies. Rather, I read them as expressions of concern over the situational limits of social science. They have to do with the perennial problem of accounting for those contextual aspects of research—whether in the laboratory or in the field—that are likely to limit its generalizability. I believe them to be asking whether there may be something about postwar Decatur (the resurgence of consumerism?), or the prospect of television, or, indeed, about female respondents that should give pause to those who would generalize from the Decatur Study to the proposition that people everywhere and always will consult other people prior to forming opinions. Of course, such warnings should indeed limit conceit, but, equally important, they may lead to the legendary (but not often implemented) kind of further research that will confirm or disconfirm the proposition and specify whether and how it varies under conditions specified by the objections. Consider how tempting it was to dismiss Sherif's (1952) classic experiment on the workings of interpersonal influence because it was based on the optical illusion that a pinpoint of light in a darkened room will appear to move. The beauty of the Bureau of Applied Social Research is that it took such thinking seriously, moving from study to study in the search to confirm or disconfirm, as does Robert Hornik in this volume.

So it was with Decatur. Lazarsfeld had little interest in a “community study.” His aim was not to revisit Middletown but to revisit a hypothesis that arose in the course of his earlier study of voting decisions to see whether it would be confirmed in other domains. It is a fair guess that he had no special interest in the site of the study, even if his near-ritualistic concern with representativeness led him there; it is certain, in any case, that a larger study or a national sample would have been organizationally and financially unmanageable. On the other hand, he was, indeed, concerned about context, not for its own sake but because the culture of the town might have bearing on the findings, as he himself points out (Lazarsfeld, Berelson, and Gaudet 1944/1948, xxvii). It is also true that major Bureau studies were almost all community based, as Simonson points out.

[W]omen were referred to as “people” in the subtitle of the study . . . because they were, indeed, making decisions in their presumed areas of specialization.

It seems doubtful that he had any special interest in female respondents except insofar that they were more likely than men to be decision makers in the realms of everyday shopping and fashion, and perhaps in deciding which movie to see. (And, of course, he was interested in women because Macfadden was.)

That’s why women were referred to as “people” in the subtitle of the study. Not to disguise the possibility that they limited the study’s generalizability, but because they were, indeed, making decisions in their presumed areas of specialization. It follows that there is room for the objection that women and men are not interchangeable. *The People’s Choice* finds that husbands influenced the voting decisions of wives (maybe still do), and it is probably correct to guess that men’s fashion decisions are more influenced by women than vice versa. Such questions are at the very heart of the Decatur Study, which asks who influences whom in which domain, and the findings certainly imply that women cannot “stand in” for men in certain respects, just as older women—influential in marketing decisions—cannot stand in for younger women. In this sense at least, the objection is sustained. Women, like men, are special kinds of people, and it is the circulation of influence—the flow—among these differences that were of interest in the Study.

Nor is there reason to believe that Lazarsfeld took account of the changing roles of women at the end of World War II, even if one may assume—with the critics—that these changes affected their interests and activities. Might the findings of the study have been different during the war or prior to it? Probably. But Lazarsfeld was not so much interested in these probabilities. He had no interest in producing an index of consumer behavior.

Pooley gives us reason to believe that Lazarsfeld had taken notice of the American sociology of the “primary group” as refracted in the writings of Edward Shils (1951) and of

the variety of ties that bind, from primordial to civil. If true, it might have suggested to him that the character and extent of interpersonal influence might vary by time and place. Gabriel Tarde, the French social psychologist, suggested as much in his essay on “Conversation” and the public sphere (in Clark 1969). However reasonable, no evidence suggests that he was concerned that what he found in Decatur might not hold true in New York; nor did he give thought, as Peters does, to the nature of the exchange between the two. For the purposes of the study, it is unlikely that he (or I) gave much thought to the image of Decatur as a smallish town occupied with local gossip, in comparison to the cosmopolitan concerns of New York, as Tarde might have done.

What surely did occupy him was the different effects of the several media. He had early studied “radio vs. the printed page” (Lazarsfeld 1940) and differentiated between the two in the voting study. In the Decatur Study, the media were differentiated according to their relevance to each domain (fashion, public affairs). Researchers took an important step to determine the characteristic constellation of media, and their relative position in the sequence that led to the making of a decision. Lazarsfeld named this mapping procedure “an accounting scheme” (*Personal Influence*, pp. 189-91).

If Lazarsfeld had been asked to predict how his main findings might change in the era of television, I believe that he would have stood his ground. Would he have been proven wrong? Have women discontinued their consultations with peers because fashion and household goods are represented so much more vividly on television than they were on radio and magazines? Now that television has brought politics into the living room, has the flow of political influence between men and women become more symmetrical? Will the new media of personal tailoring finally annihilate the second of the two steps? Bennett thinks yes, and one infers that Mutz (1998), too, thinks that television might be having more direct influence than its predecessor. I doubt it. But let’s not hold all this against Lazarsfeld, sixty years later, even if they’re right.

Theory, Ideology, and Their Consequences

More troubling than the critiques of the universalistic pretensions of *Personal Influence* are the allegations that the book had crippling consequences for the then-developing field of media studies. These critiques assert (1) that the focus on effects of the media in the short run and on everyday decisions distracted scholarly attention from the study of media impingement on bigger things, (2) that the focus on interpersonal influence and limited effects shields (intentionally?) the captains of the culture industry from the blame they deserve, (3) that the theory of mass society and its presumption of powerful media was introduced as a straw man to set the stage for the “discovery” of interpersonal influence and limited effects, and (4) that the lumping together of decision making in the domains of voting and shopping is reprehensible.

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Influence’s focus on the short run of media

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To these critiques, one can immediately concede that media studies at the Bureau of Applied Social Research, *Personal Influence* among them, did indeed focus on the short run of media influence on opinions, attitudes, and actions. This relates to the interest in the dynamics of decision making, to the accessibility of such research—methodologically speaking—as well as to the (perhaps naive) question of whether the mass society can be easily manipulated by the elites of business and politics. Fortunately or unfortunately, this emphasis probably did give currency to the conclusion that the ostensibly omnipotent media have only “limited effects.” This conclusion still holds for well-designed evaluations of “campaigns” as evidenced in Hornik’s careful study of an expensive antidrug campaign. Indeed, academic research has no evidence of massive media effects in the short run, even in advertising. But that conclusion should not be applied to domains in which media power may be (or may not be) much greater. Did not media rally public opinion for the current war in Iraq (packaging Saddam Hussein and al-Qaeda)? Did not the media make impeachment “thinkable” during the Watergate affair (Lang and Lang 1983)? Did not the media hasten the retreat from Vietnam, or the much-delayed U.S. intervention in Kosovo? Answers to these questions are less than clear-cut, research-wise (see Hallin 1986), but the questions are of a different sort, obviously, than those that were asked at the Bureau in the 1950s. Lang and Lang are right in “blaming” the Bureau for defining media-effect studies so narrowly—or, indeed, for failing to call more explicit attention to the typology of other kinds of effects that Lazarsfeld had early published (in Lazarsfeld 1948; also see Katz 2001), even if his actual research was more narrowly focused. Too bad.⁴

The Langs’ objection may have had more than theoretical consequences. It differs, however, from the more ideological approach that characterizes Gitlin’s (1978) famous attack. Gitlin (correctly) sees *Personal Influence* as part of the Bureau’s reapportioning of power between media and mass, and declares this a travesty. Opinion leaders, argues Gitlin, are mere conduits for the messages of the media and are, therefore, unwitting collaborators in the hegemonic process.⁵ Attributing independence to interpersonal influence obscures its reinforcing role, says Gitlin, thus deflecting criticism from the media as powerful agencies of social control (to which, ironically, Noelle-Neumann [1984] would agree). Powerful institutions, Gitlin implies, are only too pleased to find their power understated. Moreover, the decisions studied by Lazarsfeld are mere decoys, he says, because they conceal genuine alternatives. Rather than an exercise in democracy, the study of consumer choices between the two major colas or voters’ choices between the two major parties is, yet again, a collusion with the economic and political establishments, which are only too happy to suppress “third parties.” Lazarsfeld’s (1940) brilliant defense of “administrative” research—helping responsible institutions to diagnose and confront the problems they face—takes explicit

account of such biases, while acknowledging the complementary role played by “critical” research in making the “client” (of administrative research) a part of the problem.

To the allegation that the Bureau dismissed the theory of mass society too quickly, there are at least two sides. One side says that the term *mass* does not characterize any society and that the concept itself is hardly useful (Williams 1958/1989). This side also might say that early theorists of mass communication did not have direct, powerful effects in mind, and hence the “discovery” of interpersonal influence and limited effects is only a kind of grandstanding. Others disagree. The term *mass* is certainly applicable to the first half of the twentieth century says Scannell, and David Riesman (1950) is the herald of its demise. Theorists and researchers of propaganda in the early days of radio expected powerful effects, and retreated only much later.

True, the “theory of mass society” was discredited by Shils and others who saw the primary group as underpinning large institutional structures. This is the theme that I pursued in Part One of *Personal Influence*, abetted by Shils, Homans, Lewin, Bales, Festinger, and the social psychology of small-group research. The question of whether Shils influenced Lazarsfeld who influenced me to move in this direction, or whether the routing proceeded differently, is addressed above. But that is a small question compared with the big one of whether the retreat from mass society and powerful effects was a capitulation to the powers (and clients) that be.

Finally, consider Gitlin’s (1978) objection that it is frivolous to treat consumers and voters in the same breath, as if decision making in the two domains were commensurable. Yet what is one to do if they are roughly comparable behaviorally— even if they “ought” to be different, as Schudson says? In fact, there *is* some difference, as the critics—and the authors—point out in that the extent of direct media influence *is* greater in the domain of public affairs than in the other domains; but that only exacerbates the problem.⁶ Schudson confesses to being plagued by this problem, and together with Glickman, proposes that there is a (partial) way out. They argue, each in his own way, that certain consumer decisions are political (as was the Boston Tea Party, for an ancient example) and that certain political decisions are about consumerism (whether to dig for oil in Alaska, for example).

Some Things that Went Right

It’s been mostly argument so far, pro and con. Let’s now try for a happier end by turning to a few nominees for praiseworthy outcomes of the Decatur Study and of *Personal Influence*.

A series of three Bureau studies may be described methodologically, as attempts to introduce sociometry into survey research. It began in *The People’s Choice* when voters verbally described the persons who had influenced them, continued in *Personal Influence* where influencee and influential were paired and interviewed, and culminated in the so-called Drug Study (Coleman, Katz, and Menzel 1966), where the social and professional relationships of communities of physicians could be sociometrically mapped. This progression led from the study of dyads to the study of networks and from the study of decision making to the study of diffusion.

In this sense, *Personal Influence* deserves kudos for being among the forerunners of the current interest in network theory and the revived interest in tracing the epidemiology of ideas and practices—and diseases! Hornik is well aware of how the social embeddedness of individuals contextualizes media effects. Some of the foremost sociologists of social networks (Burt 1992, for example) are carrying the torch.

Livingstone goes on to propose that the tempering of media power implicit in the networks of *Personal Influence* jibes with the redistribution of power to audiences implicit in the gratifications theory of the Bureau (Blumler and Katz 1974) and the resistance theory of the Birmingham School (Hall 1992). These ideas, she says, belong to the history of audiences, acknowledging that research on audiences long preceded the upstart claims of cultural studies, as Curran (1990) has long maintained. The “active audience” has had its ups and downs both in theater and in journalism, as Livingstone shows in recent work, and it would be no surprise to Gabriel Tarde, for one, that “press-conversation-opinion-action” (Clark 1969; Katz 2006) might still (or again) help to describe the public sphere.

It seems fair to say that the uphill efforts to resurrect participatory democracy and community—in the struggle against the increased alienation noted by Putnam (2000), Eliasoph (1998), and others and against the prophets of the new media and their fragmentation of society—find support, even today, in the everyday fraternity of *Personal Influence*, its predecessors, and successors. The Bureau itself continued along these lines, mounting the Berelson, Lazarsfeld, and McPhee (1954) study of voting and Wright and Cantor’s (1967) study of opinion leadership, and culminating in the study of physicians’ decisions to adopt new drugs (Coleman, Katz, and Menzel 1966). Thus, the idea of opinion leadership—which implied interception and negotiation of media messages—helped to restore the spotlight to “conversation,” and from conversation to deliberation and debate—of the kind that is supposed to nourish the public sphere, even if Schudson (1997) doubts it.

This is a good time to recall, with Kadushin, that Lazarsfeld’s guiding interest was in the psychology of decision making, or to switch frames slightly, in the dynamics of choosing. Kadushin invokes a larger frame still, alluding to Lazarsfeld’s well-known sorting of the various types of influence (“the art of asking why”; Lazarsfeld 1935), in his striving for “a theory of action.” These labels allude to the comforting messages of *Personal Influence* that (1) democracy is about choice, not about imposition; and (2) choices are made in the informal deliberations of small groups of family and friends, not by a tyrannical majority or by hegemonic duplicity. One does not have to be a critical theorist nowadays to suspect that these small comforts may be manipulative, or to be a neocon to applaud them. There is a lot of room in between—then, and maybe even now. Half a century ago, they were a relief from the threat of mass society (Kornhauser 1959), real or imagined. Lazarsfeld seems to have found pleasure in these thoughts. Livingstone agrees with Peters that “the genius of *Personal Influence* was to rescue the public sphere from the media’ [Peters 1989, 215] and thereby to permit an alternative approach to participatory democracy even in a media age.”

Elsewhere, I have elaborated on the extensions of certain of these ideas and methodologies (Katz 2006, 1987). But here and now, these thoughts have been greatly enriched by the privilege of reading the essays contributed to this volume and to the honor of networking with their authors, both virtually and in person. Our greatest debt on this

occasion—if I may be allowed to speak for both authors—is to Peter Simonson, editor and convener of the symposium; to Peter Bearman of Columbia University, director of the Institute that succeeded the Bureau; and to Michael Delli Carpini, dean of the Annenberg School, for their sponsorship and encouragement. It seems fitting, ceremonially speaking, to add that these reflections are being written at the Center for Advanced Study in the Behavioral Sciences, in Stanford, California, where Paul was in residence when *Personal Influence* was, at last, put to bed.

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Notes

1. “For unto every one that hath shall be given, and he shall have abundance,” quipped Robert Merton citing the Apostle Matthew, “but from him that hath not shall be taken away even that which he hath” (Merton 1968/1973, 445).

2. In this parable, only the angels are not consonant with tradition. In his introduction to *The Influentials*, Gabriel Weimann (1994) also invokes Moses. But he assigns him the role of “opinion leader.”

3. Elmo Roper, the pioneer pollster, was closely associated with Lazarsfeld and the Bureau and wrote a Foreword to *Personal Influence*. Interestingly, his heirs have recently published a book on the power of interpersonal influence (Keller and Berry 2003).

4. Ironically, Dayan and Katz’s (1992) *Media Events* is accused of ascribing too *much* power to television by Couldry (2003). Parenthetically, it might be noted that these events may also be classified as “short run.”

5. Gitlin’s (1978) allegation that opinion leaders simply forward what they see and hear in the media was nourished, admittedly, by the sometimes misleading formulations in *Personal Influence* and elsewhere. The thrust of the concept is otherwise, however, at least in later years. For an evenhanded analysis of this ostensible contradiction, see Pooley (2006, 291).

6. The area of public affairs was very poorly operationalized in *Personal Influence*, whereas the other three domains dealt with very specific decisions to change. This is a very likely explanation for some of the differences observed in the area of changing opinions about public affairs.

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