History and Political Communication:
An Introduction

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This essay introduces a series of articles that explore the relation of history to political communication research. It is shown that as a field of study political communication has tended to ignore historical methods and sensibilities. This tendency is traced to the field's roots in social psychology, political science, and early mass communication research. However, although political communication tends to ignore historical research, it often depends upon implicit, unquestioned historical narratives. Thus, a more robust historical imagination is encouraged not only because it may produce more and better historical research, but also because it may assist in the development of tools for reflecting on the way political communication already deploys historical narratives.

Keywords   history, methodology, narrative, political communication

If there has been a "return to history" in the human sciences, analysts of political communication apparently have missed their cue (McDonald, 1996). Histories of politics and political communication are written. Some of the best of this work is represented by the three contributors to this issue. And there are vibrant historical literatures in fields contiguous to political communication, such as rhetorical studies, journalism history, media history, cultural history, and political sociology. But history—defined broadly as an effort to understand behavior, actions, and/or events in context—is not an institutionally recognized interest of political communication as a field of study. Its journals do not typically include historical work, and its self-identified practitioners do not recognize historical methods as their own.

This is evident in a number of ways. For instance, in the last ten years the three flagship journals of communication, politics, and political communication (*American Political Science Review, Journal of Communication*, and *Political Communication*) have published a total of ten historical essays (six in *Political Communication*, four in *Journal of Communication*, and none in *American Political Science Review*). Similarly, a brief search of ComAbstract, one of the most widely used computer databases in communication, found 1,507 articles spanning several decades coded under the keyword "political communication." Of these, only two used historical methods or asked historical questions.[1] At least as defined by journal editors and writers, and despite the fact

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that most social sciences have seen a resurgence of historical writing, history plays a very small role in the collective imagination of political communication as a field of study.

From a historian's perspective, it is easy to blame the field for this lapse of judgment—how dare it ignore history! But this kind of blanket indictment distorts the situation. Historians have ignored the interests of political communication quite as much as the other way around. Having said that, this introduction and the set of essays that follow are directed at analysts of political communication, not historians. I believe that history—its methods, concerns, and sensibility—ought to be more central to the agendas that animate mainstream political communication research. As a step in this direction, I have asked three prominent historians of political communication, Susan Herbst, John Durham Peters, and Michael Schudson (although, typically, only one [Herbst] would label herself in this manner), to complete an impossible task: to present a piece of historical research while reflecting on why and how they do this work, the assumptions that guide it, and how it might contribute to political communication as a field of study— all in the space of a single essay! They have done a far better job of realizing these demands than I had a right to expect, and a reading of their essays at all of these levels is a profitable and enriching experience.

In this introduction, I wish to tie together some themes that arise in their essays with a discussion of the history of the field, how it has evolved such that historical research tends to be undervalued, the implicit ways in which historical narratives are often used in the field, and how historical inquiry might invigorate the field's core research agendas. As a field of study, political communication has got along just fine without history. But it could extend its range and depth greatly by making history a more explicit aspect of its research agenda.

The History of Political Communication

Political communication's disinterest in historical questions and methods began early in its development. The field has been recognized as a coherent area of study only since the early 1970s, with the creation of the Political Communication divisions within the International Communication Association (ICA) and, much later, the American Political Science Association (APSA). But its roots stretch back to the very earliest research on mass communication (Chaffee & Hochheimer, 1985). It is in the theoretical and methodological commitments of this early research that the field's stance toward history took shape. These commitments, in turn, were shaped primarily by three disciplines: social psychology, mass communication research, and political science.

Of the three, perhaps social psychology exercised the most profound influence. It was from this discipline that the field inherited its interest in attitudes, opinions, and beliefs. Social psychology emerged as a field of study in the early 1900s, but by the 1920s its basic perspective was employed by a host of communication scholars (Delia, 1987, p. 39). Of Berelson's (1959) "four great men" of early communication research— Harold Lasswell, Paul Lazarsfeld, Kurt Lewin, and Carl Hovland—all were trained in the social psychological tradition. Not surprisingly, the earliest studies of political communication, from Lasswell's (1927) analysis of political propaganda to Hadley Cantril and Gordon Allport's (1935) study of persuasion and Walter Lippmann's theory of public opinion (1922), were heavily influenced by social psychology.

Social psychology's focus on attitudes and opinions did not necessarily mean that questions of history would be ignored. For instance, one can imagine historical work

that seeks to contextualize the formation of political opinions (Herbst, 1993). But social psychology's substantive interest in individual attitudes was combined with a deep methodological concern for precise measurement and experimentation. The two most common instruments of data collection in political communication, surveys and experimental studies, were developed by social psychologists. Moreover, the first great proponent of behaviorism—a scientistic "mood," as Robert Dahl (1961) described it—which gripped the social sciences in the twentieth century, was psychologist John Watson (1930). Key aspects of this mood include a visceral reaction against the nonsciences of philosophy and history, a fervent effort to model the social sciences after the natural sciences, a corresponding interest in developing general laws of political communication, and a high regard for research that tests such laws with quantifiable, observable facts.

The behaviorist mood of political communication research has been reinforced by the influence of political science. Except perhaps for psychology, the discipline of political science has embraced this mood more than any other social science (Almond, 1990; Dryzek & Leonard, 1988; Ricci, 1984; Robertson, 1994; Smith, 1996). From the 1920s forward, but especially after World War II, political scientists accepted the notion that they were scientists developing testable theories of politics on the basis of observable, quantifiable facts.

This self-image paralleled the development of a view of politics as process. Once imagined by political philosophers and historians as a kind of organic evolution of cultural habits and sentiment, twentieth-century political scientists began to see politics in terms of group competition for scarce resources. The notion was first announced by Arthur Bentley (1908/1967). Through careful observation of Chicago politics, Bentley concluded that the essence of politics was action—not the action of individuals, because they were rarely effective in politics, but the action of groups. Bentley defined groups by their interests. Thus, Bentley conceived of politics as a process of interaction among interest groups. Although received skeptically at the time, this view of interest-group politics became pervasive in postwar political science, especially in the pluralist model advanced by political scientists such as David Truman (1951/1962) and Robert Dahl (1956).

This politics-as-process image marks an important parameter of political communication research. From its inception, the field has defined itself in terms of the study of governmental processes and institutions—elections and campaigns, Congress and the presidency, parties and interest groups. Ironically, this focus has remained steady despite repeated claims like Nimmo and Swanson's (1990) that the field is "interdisciplinary," a "contested terrain" that possesses little more than a "rough and ready identity" (pp. 7–8). Indeed, just after issuing this description, Swanson and Nimmo (1990) conceded that the field's core interests are much less volatile, settling as they do on the "proposition that communication in election campaigns constitutes the field's paradigm case" (p. 8; see also Denton & Woodward, 1998; Stuckey, 1996). Other approaches derived from varieties of critical and cultural theory have challenged this focus (cf. Fiske, 1996; Gitlin, 1980; Hartley, 1992; Miller, 1998). Tellingly, however, individuals working in these latter traditions remain isolated from the core agendas of political communication research. They publish in separate journals, often work in different departments, and tend to identify themselves as scholars of political rhetoric, political culture, political sociology, or mass communication rather than "political communication."

Finally, the field has inherited from mass communication research a language of "effects" and "influences." When analysts of political communication investigate the role of mass media in politics, they typically ask research questions that seek to measure

the "effects" of mass media (for a summary of theories in this vein, see McQuail, 1994). This orientation is in perfect keeping with the behaviorist mood of the field and with its interest in examining influences on and changes in attitudes, beliefs, and opinions. The most important and long-standing theories of political communication—from two-step flow to agenda setting to framing and priming—have their roots in the language of effects. Indeed, much of the contemporary literature responds to and seeks to extend these theories (cf. Hart & Shaw, 2001).

To be sure, this is a brief, stylized description of political communication. But it is, I believe, a fair one. Early social psychology, political science, and mass communication research provided a series of terms that continue to mark the boundaries of the field: opinions, attitudes and beliefs, politics as process, media effects. These terms conjure research agendas that seek to measure the effect of mass media on attitudes, beliefs, and opinions expressed within the political process. But they are flexible rather than absolute boundaries. For instance, one may find in political communication a line of research stretching from the Chicago School of Sociology through Murray Edelman (1964) to Robert Denton (1982), Bruce Miroff (1998), and others that examines symbolic environments rather than individual opinions and beliefs. Still another body of work stems from rhetorical theory, particularly the work of Kenneth Burke (Stuckey, 1996). Having said this, the boundaries still produce a hierarchy of research values.[2] Interests that lie outside them are possible, but they are not rewarded, valued, or respected in nearly the same way as research that accepts the core terms of the field. That is why one finds so little research in the major journals that lies outside the research agendas defined by these boundaries. And it is why scholars who do work outside these boundaries rarely identify themselves as doing "political communication" research.

Given this core conception, it is possible to see why the study of history sparks so little interest in political communication. As a child of social psychology, political communication is shaped by a methodological interest in quantitative data on attitudes, beliefs, and opinions. But, of course, such data did not really exist before about the 1930s. Moreover, the methodological individualism inherent in this perspective often leads analysts to ignore the role of exogenous factors—such as historical context—that may shape individual cognition. The language of process and effects also reduces interest in historical questions and methods. As a process, politics is conceived as a timeless game of group interests which participants seek to maximize in competition with other interests. Since one expects to find this game played in essentially the same way across time and space, questions of historical context tend to recede in importance. Mass communication research adds the language of "effects" and "influences" to the core research agenda. Armed with this language, analysts seek to make generalizable inferences about how mass media influence beliefs and behaviors in the political process. Taken together, social psychology, political science, and mass communication have constructed a crucible through which historical work on political communication rarely survives.

The lack of historical awareness in political communication research can also be seen in a wider context. The role of an "administrative" mind-set in communication and political science research is well documented (cf. Gitlin, 1979; Ricci, 1984). Throughout the twentieth century, governmental and economic actors sought techniques and tools for manipulating public attitudes. More important, they were willing to pay researchers to develop such tools. This funding pattern exercised a pronounced influence on core research agendas. But analysts of political communication also responded to a more general social anxiety about the mass media's effect on politics and government. Implicit in the question "What are the media doing to us?" is the temporal assumption,

"now." In its effort to respond to this anxiety, political communication research has tended to favor questions of immediate rather than historical concern. Indeed, each new medium has spawned another generation of ahistorical studies that seek to discern its impact on the political process—the Internet being a current example (Wartella, 1996).

Together, these disciplinary and social forces have made political communication resistant to the historical turn in the human sciences. Where even the highly behaviorist disciplines of political science and economics have responded to this turn (in the form of neo-institutionalism, of which I say more below), political communication has remained unresponsive. The boundaries of "good" or valued research in the field continue to be set by terms developed long ago—opinions, attitudes, beliefs, politics as process, media effects—and by a behaviorist mood that makes a firm distinction between science and nonscience. These boundaries are reinforced by exogenous groups such as grant-making agencies and by the impetus provided by general social anxieties concerning the effects of new media on political institutions.

Why Is History Important?

Of course, long-standing research agendas are not necessarily a bad thing. All fields must have some identity or they will produce little consistency or coherence. But political communication, as so many commentators have noted, is by definition a far-reaching subject. It involves aspects of culture, society, politics, economics, and psychology—and yes, history. In the face of this complexity, the effort to develop variable-analytic research designs that focus on the causal relation of a few independent and dependent variables can be seen as heroic. When done well, it produces valuable clarity. However, it has another, unintended consequence: Aspects of political communication—like history—that are squeezed out of explicit focus via rigorous research designs often reappear in implicit, unrecognized form. Despite its explicit ignorance of history, the field finds itself working with and within historical narratives. At least one of these narratives should be recognizable to any moderately attentive consumer of political communication research. It is what I will call a "usable past" (Lowenthal, 1985), a normative conception of the past that structures and animates much of the field's research.

In broad strokes, this narrative goes something like the following. In the twentieth century, politics and public life progressively worsened, and mass media have been a primary culprit. Their influence may not be wholly or uniformly bad (as very early research implied), and their effect may be limited by the role of primary groups (as Lazarsfeld and others argued) or aspects of individual information processing (as more recent scholars have claimed), but, taken as a whole, they have had deleterious consequences for the political process. They have (not necessarily in this order) contributed to the demise of parties, helped to produce ignorant and apathetic citizens, depressed social capital, warped the election process, heightened public cynicism and disrespect for political authorities and institutions, and severed the crucial connection between citizens and the political process. Moreover, historically there is a linear relationship between the presence of mass media in the political process and their negative impact: The greater their presence, the greater their general negative impact.

This narrative's usability is confirmed by its pervasiveness, just as its pervasiveness is guaranteed by its usability. It serves as a backdrop for hundreds of studies on cognition, voting, elections, political advertising, presidential communication, news media, Congress, television, and other facets of the political process. It works not because it imposes meaning, but because it serves as a standard historical trope around which

political communication research is organized. As Hayden White (1979) argues, the function of historical narrative is to organize events into a meaningful order. This is to say, narrative produces legitimacy and, ultimately, authority. It sets the terms on which we will speak about the past, and through this discourse, the ways we imagine the present. The "declinist" narrative of media's influence on the political process represents this kind of narrative. Its use lies in the way it authorizes certain meanings around which both adherents and detractors organize. The narrative is productive in the sense that Foucault (1988) uses the term. Foucault argues that discourses both constrain and enable. They constrain by restricting what is sayable and doable, and at the same time they enable by providing the conditions for making meaning in the first place.

The declinist narrative of media's role in politics functions in this manner. One might choose many examples to illustrate this process. For the sake of convenience, I have selected an article published in a recent issue of *American Political Science Review*, Diana Mutz and Paul S. Martin's (2001) "Facilitating Communication Across Lines of Political Difference: The Role of the Mass Media." Mutz and Martin's essay is an excellent example of contemporary political communication research. It uses national survey data to evaluate the extent to which various sources of political information, from interpersonal interaction to mass media, influence the diversity of views to which individuals are exposed. But why is this question important? That is, why do Mutz and Martin, and the editors of *American Political Science Review*, believe it is a question worth investigating? Because, as the authors observe, there has been a "recent trend toward residential balkanization based on shared lifestyles" that "heightens concerns about communication across lines of political difference in the United States" (p. 97). This is to say, in line with the declinist narrative, political discourse has become more tenuous over time as people have ceased interacting with others different from themselves. Moreover, "much of what is known about the structure and news gathering practices of American media suggests that they are unlikely to play a very useful role" (p. 97). Translated, this means that media exacerbate, if not directly cause, political balkanization. Mutz and Martin's study is animated by their desire to investigate the validity of this narrative.

Importantly, Mutz and Martin find that the narrative does not hold. In fact, people are exposed to different viewpoints more often through the mass media than through interpersonal communication. This contradiction is precisely what makes the essay interesting, even "sexy," to political communication researchers. It defies "common sense" to suggest that mass media may benefit rather than erode democracy. As Mutz and Martin put it, "the sheer idea that mass media might serve to the benefit of the public sphere strikes most as heretical" (p. 110). But in defying "common sense," Mutz and Martin must first accept its basic premise: that the proper beginning point for research is the commonsense normative assumption that media tend to negatively influence the political process. In this way, political communication analysts often rely upon a usable past to assign value and meaning to their research.

Put in these terms, history is important not only because it is rarely an explicit focus of the field, but also because it often serves as an implicit, unquestioned, usable past.[3] Informed by the declinist narrative, a great deal of research is devoted to measuring more precisely the extent of decline, qualifying or revising its implications, and showing that media are detrimental in this case but not nearly so much in that case. The result looks remarkably like an eternal return of the same: theories and studies that verify the media's deleterious consequences for politics met by theories and studies that qualify or refute this conclusion. This is not (at least not directly) the fault of the field's

focus on political process, media effects, and attitudes and beliefs. But to the extent this focus limits the field's historical imagination, it contributes to this dilemma.

Perhaps the most interesting and influential recent example of this process is Robert Putnam's *Bowling Alone* (2000). Although Putnam is not commonly identified as a scholar of political communication, his work's substantive focus, methodology, and use of the declinist narrative link it to the mainstream of political communication research. Putnam uses survey data to trace cause-effect relationships in changes of social capital over time in the United States. Despite his insistence that he does not accept the declinist narrative ("It is emphatically not my view that community bonds in America have weakened steadily throughout our history—or even throughout the last hundred years"; p. 25), that is the story he tells in tables, charts, and graphs. Heroic efforts were made during the progressive period to create enduring voluntary associations intended to thicken social connections between Americans. A "long generation" raised during the depression remained loyal to these associations. But Putnam traces a steady, post–World War II decline in association membership and a corresponding decline in social capital. Typically, he finds that television is a primary cause of this decline: "More television watching means less of virtually every form of civic participation and involvement" (p. 228).

Putnam's intention, of course, is noble. Like many analysts of political communication, Putnam is motivated by a desire to change our political culture for the better. "In small ways . . . and in larger ways," he writes, "we Americans need to reconnect with one another. That is the simple argument of this book" (p. 28). However, in fashioning a "usable past," one that conforms quite closely to the declinist narrative, Putnam's history employs a highly truncated historical imagination. Most prominently, his history is curiously devoid of context. Associational membership is traced over time with little regard for whether the term has different meanings in different contexts. He blames television for a decline in social capital but does not consider the case of radio, a medium that kept Americans home during the heyday of associational life, the 1920s to the 1940s. He lauds the progressive generation for building national associational structures but does little to explore the role religion played as an inspirational force in their efforts. In broad strokes, he decries the loss of associations in contemporary America but fails to balance his portrait with a discussion of what Americans have gained in individual rights and freedoms.

Of course, it is difficult to address these questions through survey data. But that is exactly the point. Putnam's preference for quantifiable data limits the kinds of historical questions he may ask. He relies upon a narrative of decline—against his expressed intentions—because it is the kind of past that is "grasp-able" and usable in the terms of his preferred data and methods. It is not that this narrative is necessarily wrong; but it is partial. And in its partiality it places limits on the kinds of questions that are addressed and addressable by the field.

Toward a More Robust Historical Imagination

How, then, to open the past to more vigorous historical inquiry? What advantages would this move provide? And, even if it would advantage the field, is it possible to link such inquiry to the field's dominant research agendas? The last question is perhaps the most important. A demonstration that historical inquiry could be improved, and that it would be advantageous to the field, is little more than a call to arms. Either one agrees, and thus eschews the mainstream of the field, or one does not and simply ignores the point. Such a confrontational stance is not helpful—or necessary. The call for more sensitive

historical inquiry can be one of evolution rather than revolution, *if* one shows that more sensitive historical inquiry is possible, advantageous, *and* not wholly antithetical to the mainstream enterprise.

I think this challenge can be met—and the essays that follow point the way. Susan Herbst, John Durham Peters, and Michael Schudson are three of our most interesting and important historians of political communication. Here, they write on very different subjects: Herbst presents a new model of public opinion; Peters discusses the intellectual history of quantification as an idea and ideal in the study of politics; and Schudson elaborates a view of politics as a form of cultural practice. And they develop their themes using different methods and points of view. However, taken together they offer, if not answers, then clues to answers to our questions. These clues can be summarized in three propositions.

History Is Not Efficient

Much of political communication research assumes that history works quickly and rapidly: Processes are set in place that inevitably lead to a given outcome. The declinist narrative I discussed above fits this view. Since the present is merely the culmination of past events, it is important to the extent that it allows one to trace backward, in a kind of reverse engineering, from the present to the past. In this way, history is represented as a singular path from the past to the present. Grasping the essential processes of that path (such as, say, the decline of parties, or the decline of associations, or the rise of the mass media) enables one to ignore the details of historical events.

Herbst, Peters, and Schudson reject this representation of history. For them, there is nothing inevitable about historical change. For instance, Schudson looks across the 200-year expanse of American citizenship and finds great discontinuity. The past did not inevitably produce contemporary understandings of citizenship. Not only is the past not really past—earlier models continue to hold some sway today—but the current notion of citizenship as an intellectual exercise is the product of great conflict. Similarly, Herbst views the history of public opinion as a series of "infrastructures," some of which arose and disappeared in the past, and so are foreign to us, and others of which continue to be deployed and fought over. Finally, Peters describes the history of the idea of quantification in terms of "fossil traces of . . . forms of political life" that form something like semantic layers of ideology. Here, past and present ideas of quantification become jumbled as actors resurrect and confront old ideas in new guises. History, the three authors agree, is permeable across time periods and punctuated by conflict.

Since history is relatively inefficient, the details of history matter as much as outcomes. Working inductively, historians search for the "nuggets" of fact, as Schudson terms them, that illuminate the meaning of a given behavior or event. Making much the same point, Herbst encourages scholars of public opinion to get on the ground floor of opinion formation, where people "living in the real world" make sense of public opinion. And Peters weaves together an argument about the significance of quantification from a detailed analysis of the term's use. One might argue that a focus on details mistakes the forest for the trees. For the social scientist intent on explaining outcomes, structures—whether economic, social, or cultural—are more important than the details of individual action. But this misstates the way in which Herbst, Peters, and Schudson imagine individual action. For the historically sensitive scholar, structures are important. However, outcomes are an expression not merely of structure but of the way in which actors struggle with and against the conditions that structure their experience. This is to

say, order is produced in the *interaction* of structure and agency in particular contexts. It is not imposed mechanistically by structures. Since the interaction of agents and structures takes place differently at different times and places, history by definition is inefficient. Since the struggle itself is crucial for understanding, its details are paramount.

This "inefficient" view of history has several advantages. One advantage is its suggestion that there may be more varied relationships between past and present than simple linear causal statements. The past may be a fossil, a memory, a practice, or infrastructure. Many pasts may exist in a kind of skeletal closet of the present, jutting up against one another in uneasy familiarity. In a kind of "presentism," the past may be defined by terms in the present, reversing the assumed relationship between the two. Or the past may be wholly foreign to the present, alien to contemporary attitudes and behaviors. Dismantling the "efficient" conception of history encourages a greater appreciation of the many ways that the past and present may coexist. As such, it opens the way for more imaginative comparative and contrastive analysis.

Political Communication Is Cultural as Much as Attitudinal

Consistent with its methodological individualism, political communication research often assumes that meaning is rooted in the preferences of individuals. For this reason, scholars tend to limit themselves to the study of individual expressions, the preferences that underlie these expressions, and how these preferences are shaped by larger structures. Since data on the preferences of individuals in the past are severely limited, analysts of political communication tend to ignore history. The result is a little like the man who looks for his keys under the lamp post because that is where the light is: Political communication scholars overlook history not because it is unimportant, but because their methods render it invisible.

Our three contributors invite scholars of political communication to expand their understanding of where meaning in political communication resides. "Politics," Schudson tells us, "is not a category unchanging through time and space . . . [it] is culture, too. Politics is a set of symbols, meanings, and enacted rituals." Peters makes much the same point when he notes that the "lifeblood of politics is ideas, images, and words." Herbst labels these things—symbols, rituals, images—as "cultural artifacts" to suggest how they crystallize meaning in a given context. For all three, meaning is rooted not in the preferences of individuals but in the cultural forms in which those preferences take shape.

There is a sense in which this notion directly contests the methodological individualism inherent to mainstream political communication research. If individuals do not form preferences based on an independent calculation of their interest but instead merely borrow from a cultural "toolkit" of meanings, symbols, and artifacts (Swidler, 1986), then mainstream analysts have been looking for the dynamics of preference formation in the wrong place. There is, of course, a raging debate in social and political theory on precisely this issue: To what extent are individuals socially constructed? The debate has a kind of chicken and egg quality, in my estimation producing much heat but little light. For our purposes, it is not necessary to insist that one *must be* either a social constructionist or a methodological individualist. Instead, it is better to suggest the following.

The study of political communication as culture promises to expand the vision of the field. It does so in several ways. First, it enlarges the range of data that count as registers of political communication. Instead of confining themselves to the study of what people think, analysts may study what people do. Voting, parades, demonstrations, town hall meetings, and the many other ways that Americans have practiced politics all

become ripe for analysis. Moreover, media forms not typically included in political communication research, such as movies and songs, television shows and radio programs, become visible expressions of political communication—cultural artifacts that crystallize political meaning. The latter not only extends political communication research beyond the study of news media, it also builds a bridge to media studies, a field with which it has many affinities but few concrete connections. Second, a stress on culture widens the theoretical prism through which political communication in and around governmental institutions is conceived. Typically, this communication is understood as a game of strategy in which individual actors seek to maximize their preferences. Understood as culture, it becomes something more like a ritual, practice, or performance. As such, it loses its veneer of inevitability and timelessness. Rituals gain meaning in particular contexts; practices are rooted in specific times and places. Viewed in this way, the core agenda of political communication research—the explanation of communication in and around government—becomes amenable to historical comparison, contrast, and inquiry.

Political Communication Is Social

Writing in 1962, William Riker dismissed historical research as a "traditional method" capable of producing great wisdom, but not science or knowledge (p. viii). If I have not lost the confirmed behaviorists yet, they are probably feeling much the same way. One may accept that history matters (as Herbst says, "of course it matters"), that details matter, that political communication clearly is cultural as well as attitudinal, and yet still doubt that this vein of research is "scientific." How can one operationalize something so fuzzy as culture? How can one build theoretical knowledge from the bottom up? How can one develop causal inferences within this perspective?

Historians of political communication have not been of much help in answering these questions. Most accept a distinction between "understanding" and "explanation" developed by the philosopher Edmund Husserl (1958), among others. For Husserl, and those who follow him, one may understand human affairs but not explain (in the sense of infer causes about) them, because the observational position of the objective, neutral bystander that is necessary to achieve explanation is simply not available to us. As scholars, we are as deeply caught in webs of culture as those we purport to study. Accepting this view, historians have tended to be more interpretive, to read political communication as a kind of textual practice, and therefore to produce research that looks more like literary analysis than behavioral social science. In short, if mainstream political communication research has ignored history, the reverse is also true: Historians have ignored political communication research.

This cannot be said of the essays written by Herbst, Peters, and Schudson. They recognize that while political communication is cultural—it is composed of symbols and language, rituals and performances—it is social as well. Symbols do not float free form in society, and individuals are not free to make just any meaning they wish. Symbols and meanings are linked to, and gain significance from, institutions, processes, and structures that exist in a given place and time. Perhaps it is this intuition that makes their work so useful. For example, Schudson suggests not that politics is culture, but that it is cultural *practice*. A practice, Scribner and Cole (1981) tell us, is a "recurrent, goal-directed sequence of activities" consisting of three variables—technology, knowledge, and skills (p. 236). This conception of practice looks remarkably similar to Herbst's definition of public opinion infrastructures. Like Scribner and Cole, Herbst defines infrastructures as a composition of three variables: conceptions of public opinion (knowledge),

measurement tools (skills), and media (technology). In adopting terms such as practice and infrastructure, Schudson and Herbst respond to the sense that the organization of culture matters, that this organization takes specific forms in historical context, and that, for this reason, it is analyzable as a set of variables. This is to say, for Schudson and Herbst, interpretation is necessary, but it is possible to make interpretation serve the needs of explanation.

It is worth linking this view with neo-institutionalism, a body of work that has taken Schudson and Herbst's intuition farthest in the social sciences. A response to the failed "scientism" of behaviorist—particularly rational choice—theories of behavior, neo-institutionalism has become influential in economics, political science, and sociology (cf. March & Olsen, 1984, 1989; Orren & Skowronek, 1994; Powell & Dimaggio, 1991; Skowronek, 1982, 1997, 1998). It has even made some inroads in the study of political communication (Cook, 1998; Ryfe, 2001; Sparrow, 1999). Like our contributors, neo-institutionalist theory suggests that individual preferences are shaped by culture and that cultural meanings embedded in recurrent institutions (Schudson's practices and Herbst's infrastructures) shape the preference formations that are likely to take place in a given historical context. To analyze any particular context, it is necessary to know something about how institutions have evolved over time (that is, to turn to history). This turn reveals that individuals form preferences in a recursive, reflective struggle within and against the institutional conditions they face. Since the outcome of that struggle is unpredictable, history tends to be punctuated by conflict and, as a result, inefficient.

Neo-institutionalists take this perspective in many directions, and none of them is without its critics. Even when using concepts such as institutions, practices, and infrastructures, for instance, it is difficult to define variables rigorously enough to satisfy some behaviorist scholars. But their arguments, and those made by our contributors, deserve to be heard. They register a dissatisfaction with prevailing behaviorist theories that conceptualize individuals as preference-maximizing agents existing in timeless strategic games. They offer convincing arguments in support of a robust historical imagination, one that captures the way in which structures and agents interact in specific contexts. They call attention to the way in which historical narratives are often smuggled into mainstream political communication research. And they hold out the promise of a field invigorated by new kinds of data and methods. If, as Peters tells us, we are "fated to count and recount," perhaps it is time to develop research agendas that do justice to both.

Notes

1. I had similar results in searches of JSTOR, another widely used computer database of journal articles, and WorldCat, perhaps the most comprehensive computer database of scholarly books. Also see the bibliographies of political communication research compiled by Johnston (1990a, 1990b) and Stuckey (1996).

2. I cannot resist a personal anecdote on this point. As a graduate student, I once visited a prominent scholar of political communication for advice on the subject matter and research design of my dissertation. His primary advice came in the form of a warning: Choose my audience well, he said. If the dissertation veered too far toward rhetorical or cultural theory, "political communication" specialists—especially those trained in political science—would not likely be interested. Since perspectives (behaviorism, politics as process, etc.) favored by political science dominate the field, he warned that this could have grave consequences for my career.

3. Even the few historical studies of political communication, from Jamieson's (1988) study of electronic eloquence to Kenneth Cmiel's (1990) history of nineteenth-century American rheto-

ric, seek a "usable past." Written in the form of "social criticism," such studies begin from the premise that something has gone wrong with American political communication and seek the origins of this condition in the past.

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